AN ABSTRACT OF THE THESIS OF

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Mary Jo Nye

This thesis will address the transformation of biological sciences during the 1930s and 1940s and it effects on fisheries science. It will focus on Oregon State College and specifically the Department of Fish and Game Management and the interaction with the Oregon Game Commission. Support for mutation theory and neo-Lamarckism lasted throughout this study's time frame. The resulting belief that the environment can directly affect species fitness could have been a factor in fisheries managers' support for fish hatcheries.

Throughout this time frame the science of ecology was emerging, but the dominant science of agricultural breeding science within wildlife management took precedence over ecology. Two case studies show changing ideas about agricultural breeding science as applied to wildlife management. In the first case study, the debate concerning fishways over Bonneville Dam shows that fish hatcheries were counted on to mitigate the loss of salmon habitat due to construction, and to act as a failsafe should the fishways fail. When the 1934 Oregon Game Commission members failed to enthusiastically support the construction of the dam and the fishway plans, this

thesis argues that the commission members were dismissed in 1935. The second case study addresses the actions of the Oregon Game Commission in placing some high dams on tributaries of the Willamette River, the Willamette Valley project. This thesis shows that the inclusion of ecological principles in the evaluation of fish hatcheries led the commission to oppose this project. For their opposition, this thesis argues that the 1949 Oregon Game Commission members were dismissed.

In both cases, this thesis concludes that the federal funding of water development projects played an important role in the dismissals of both Oregon Game Commissions. In addition, the evolving nature of biological science during the 1930s and 1940s shows that lingering beliefs in mutation theory and neo-Lamarckism would have supported the use of fish hatcheries as a scientifically acceptable solution to declines in fish runs within the scientific tradition of agricultural methods of breeding wildlife.

In the Midst of a Revolution: Science, Fish Culture, and the Oregon Game Commission, 1935-1949

by

Linda Hahn

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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.

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Linda Hahn, Author

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Abbreviations for Archival Collections

MSS Roland Dimick Papers	**DIMICK**
OSU Archives	
Memorabilia Collection, Roland Dimick	**MCDIMICK**
OSU Archives	
Memorabilia Collection, Nathan Fasten	**MCFASTEN**
OSU Archives	
MSS William Finley	**FINLEY**
OSU Archives	
MSS E. E. Wilson Papers	**WILSON**
OSU Archives	
SG IV, Series I	**WILSON/date**
(Game Commission minutes)	
SG IV, Series II	**WILSON/FISH**
(Fisheries)	
Oregon State College Catalogs	**CATALOGS**
OSU Archives	
Oregon State Game Commission minutes	**MINUTES/date**
State of Oregon Archives	

This thesis is dedicated to the memory of

Ona June Ivers,

June 8, 1910 - June 26, 1999,

one who daily reveled in the beauty and diversity of Oregon's flowers, birds and animals.

In the Midst of a Revolution: Science, Fish Culture and the Oregon Game Commission, 1935-1949

Introduction

Many factors have been cited as contributing to the decline of salmon runs in the Pacific Northwest: overfishing, pollution, the effects of mining, logging and farming, urban growth, loss of habitat, construction of dams and fish hatcheries, as well as cultural factors that distance people from nature. Historically, concern over dwindling runs has been ameliorated with the belief that science and technology could solve the dilemma. Repeated studies that have shown declines in salmon runs prove otherwise. A 1986 study by the U. S. Department of Fish and Wildlife analyzed 25,500 escapement records for naturally reproducing populations of five species of Pacific salmon from Alaska, Washington, Idaho, Oregon and California. While chinook, sockeye and pink salmon showed some increase in Alaska and decreases in other regions, coho and chum showed decreases in all regions.[1]

In 1991 Willa Nehlsen, Jack Williams and James Lichatowich documented that 214 native, naturally-spawning Pacific salmon stocks faced a high or moderate risk of extinction in the Northwest, proving to fisheries managers that technology was not providing long-term solutions. The study emphasized the importance of native, naturally produced fish with genetic diversity that made them more capable of withstanding ecological and climatic changes. Fish artificially produced in hatcheries lacked this genetic diversity and could not sustain salmon runs. In fact the 1991 study indicated that hatchery fish may have contributed to declines in native populations. The study asked for an emphasis on restoration of salmon habitat over the historical emphasis on fish hatcheries to supplement salmon runs.[2] A 1999 study of hatchery operations submitted by an independent panel of scientists to the Northwest Power

Planning Council further indicted the fish hatchery system and called for a massive overhaul of fish hatcheries in the Northwest. The study charged that hatchery fish lacked genetic diversity, mated with native fish and weakened their genetic diversity, spread disease among native fish, spawned at the wrong time, and were not equipped to live in the wild after being raised in an artificial environment. Non-native species produced by hatcheries for distribution in Oregon's streams and lakes were taking the place of native species. By 1999, 90 percent of salmon in the Columbia River basin were from hatcheries.[3] How had fisheries managers come to rely on a process that would prove to be so destructive to the fish runs it was supposed to supplement?

Fish hatcheries first appeared in Oregon in the late 1800s to supplement runs that had been overfished by commercial fishers. The practice expanded when sports fishers demanded that Oregon's fishable waters provide adequate sport for a growing number of licensed fishers in the early 1900s. A major growth spurt for fish hatcheries came in the 1930s when water resource development projects proposed for the Columbia River Basin involved the construction of dams , some of which would block the passage of anadromous fish. Fish hatcheries were proposed as a means to mitigate the loss of habitat resulting from the construction of dams, as well as a failsafe should fish passage attempts over dams fail. While politics and economics played major roles in water resource policy decisions, this thesis examines the role science played in supporting, and in some cases, refuting those decisions. The dams systems in the Columbia River Basin were proposed during the 1930s-1950s when biological science was in a state of flux. I argue that because of this timing, the primarily quantitative arguments made regarding fisheries the first half of the century are vastly different than the qualitative arguments made regarding fisheries at the end of the twentieth century.

Scientific investigation is an evolving process in which not only the facts change with time, but also the methods and the theoretical constructs change as well.

The science that evaluated fish hatcheries in the 1930s and 1940s was different from 1990s science in fundamental ways: the fisheries knowledge base has vastly expanded; the types of evidence used to support scientific theories has changed; and the theoretical base for the life sciences has changed. During the period from 1930-1950 a biological revolution was in progress. In the 1990s the theory of evolution and natural selection affect all areas of the biological discipline. However, not that long ago biology was a term little used, and the life sciences functioned as separate disciplines. Prior to the 1930s and 1940s life sciences did not function under the umbrella of one scientific theory. The theory of evolution and natural selection when joined with Mendelian genetics brought to the life sciences a theoretical structure that applied to all biological fields thereby uniting them as a discipline. That process of unification within biology is often referred to in the literature as the modern synthesis.[4] It fundamentally altered the understanding of how hereditary information is transferred, how new species are formed and what factors control evolution. This biological revolution introduced genetics to the life sciences and solidified laboratory experimentation and population systematics in support of the traditional natural history of observation, description and classification.

However, from 1930-1950 many different theoretical concepts about species development were competing against each other. This thesis addresses the competition between natural selection theory and mutation theory, as they were commonly understood in the early 1900s. According to natural selection, individuals that are born with favorable characteristics or variations suited to the local environment survive and those that are born with unfavorable variations die out. Favorable variations, minor in character, are passed on to progeny, gradually resulting in the emergence of new species. In contrast to natural selection, the early mutation theory argued the role in evolutionary change of sudden, major alterations or mutations in individual traits, followed by the inheritance of these mutations in the next generation. The source of mutations, like the source of variations, was a matter of controversy, with a neo-Lamarckian hypothesis often favored, assuming that the environment causes changes in the individual and that such change is heritable.[5]

Work done in the midst of this transformation reflects old standards mixed with new, producing a confusing and often contradictory body of knowledge on which to base decisions. The complexity of science as well as the complexity of social and economic factors must be considered when analyzing elements which led to the Pacific Northwest salmon crisis. This thesis will present a historical look at the biological science of the early twentieth century in order to provide some new insights into the background of the salmon crisis. This thesis takes national and international theory and developments and, by presenting local case studies, examines how those developments were expressed in parts of Oregon.

Species fitness, a major topic of evolutionary theory, is the heart of the recent qualitative arguments made about fish hatcheries. Chapter 1 focuses on the development of Charles Darwin's theory of evolution and natural selection and how it relates to species fitness. Many theories about species change were generated during this timeframe and many aspects of Darwin's theory were debated. The thesis will focus on one of those conflicts: the debate over the stimulus for species change. Garland Allen presents a general historical account of the developments of the modern synthesis through the emergence of molecular biology in *Life Sciences in the Twentieth Century* (1975).[6] Julian Huxley presents an earlier version of the molding of biology as an academic discipline in *Evolution: The Modern Synthesis* (1943).[7] William Provine specifically details the competition between the Biometricians and the Mendelians in England that form the basis of the genetical theory in the 1930s in *The Origins of Theoretical Population Genetics* (1971).[8] In *Transforming American Traditions in Biology, 1880-1915* (1991)[9] Jane Maienschein provides biographies of four biologists, Edmund Beecher Wilson, Edwin Grant Conklin, Thomas Hunt

Morgan and Ross Granville Harrison, whose work in separate fields was central to the development of biology as a science. Of considerable utility, too, in understanding the American contexts are *The American Development of Biology* (1988),[10] and *The Expansion of American Biology* (1991)[11] edited by Ronald Rainger, Keith Benson and Jane Maienschein. Keith Benson (1988)[12] provides insight into the development of professional societies within biology, and the influence of Mendel at agricultural colleges is discussed by Diane Paul and Barbara Kimmelman (1988).[13] The role that agricultural colleges played in the development of genetics as a scientific discipline is detailed by essays in *The Right Tools for the Right Job: At Work in Twentieth-Century Life Sciences* (1992),[14] edited by Adele Clarke and Joan H. Fujimura.

The application of mutation theory and Mendelian genetics to fisheries management represents the field of breeding science within agriculture. Two factors aided the growth of this science: the creation of land grant colleges, and the interpretation of Mendelian genetics that gave the craft of breeding scientific legitimacy. Agriculture moved from a vocation to a science in the United States when the 1862 Morrill Act created land grant colleges to investigate and promote improvements in agriculture, provide educational opportunities to the underclasses, and provide professional training programs in the military and industrial arts. U. S. President Thomas Jefferson had initially lobbied for such colleges to halt the loss of farmland caused by inefficient and destructive agricultural practices. The land grant college complex includes educational programs dedicated to agriculture, agricultural research stations and an extension service. Jefferson envisioned a system that would be responsive to local needs by offering agricultural research equal to industrial research programs. However, Wendell Barry asserts in *The Unsettling of America: Culture and Agriculture* (1977) that the research programs of agricultural land grant colleges instead help agribusiness.[15]

Chapter 1 of this thesis shows that through joint efforts to aid agricultural development, land grant colleges and the U. S. Department of Agriculture worked to breed superior forms of domestic plants and animals that would increase the quality and production of agricultural commodities. The breeding of domestic plants and animals had historically been executed by breeders with a "good eye." Once breeding could be based upon the theoretical structure found within Mendelian genetics in the early 1900s, the craft of breeding became a science. That science found its first home within land grant colleges' agricultural programs and agricultural research stations. Wildlife were treated like agricultural commodities, as Chapter 2 will show, and the breeding programs that were used to breed domestic plants and animals were extended to wildlife as well. From reading archival papers produced in the 1930s-1940s, the language that described fish hatchery programs further supports the contention that wildlife were agricultural commodities. Hatchery fish were not "put" in streams, they were "planted" in streams. When one caught fish, the descriptive term used was "harvesting" fish.[16] These attitudes and terminology were not used occasionally, but can be found in documents and game commission minutes regularly. The Oregon Game Commission's mission was to supply fish on the demand of the fishers, and agricultural breeding programs offered a "scientific" way to meet that demand. Therefore, for the purposes of this thesis agricultural breeding science should be understood as the science of breeding promoted by agricultural educational programs and research, and extended to wildlife management programs.

Because the Game Commission relied on scientific experts at Oregon State College (OSC) for wildlife management advice, it is essential to understand the extent to which decisions were informed by current biological knowledge. The previous books about the modern synthesis focus on the exceptional people at the national and international levels who affected evolutionary debates. Examining the work of Professor Nathan Fasten, a zoology professor at Oregon State College from 1920-

1945, shows how one unexceptional person interpreted the debate regarding the stimulus for change in species development. Despite his unexceptional status within evolutionary circles, on the local level he was not without his own venue of influence. Fasten served as chair of the OSC Department of Zoology for 24 years and regularly taught introductory and advanced classes in zoology and genetics. In addition, his field of expertise was fisheries and he had consulted with the Oregon Game Commission. Unpublished personal papers from Fasten's memorabilia collection were used to explain his viewpoint in addition to books that he authored. Fasten's 1935 textbook, *Principles of Genetics and Eugenics: A Study of Heredity and Variation in Plants, Animals and, Man*[17] is compared to the 1930 textbook of Harvard Professor William Castle, *Genetics and Eugenics: A Text-Book for Students of Biology and a Reference Book for Animal and Plant Breeders.*[18] Biological science of the 1930s and 1940s was also affected by the growth of eugenics as becomes clear in Daniel Kevles *In the Name of Eugenics: Genetics and the Uses of Human Heredity* (1985)[19] and Diane Paul's *Controlling Human Heredity, 1865 to the* Present (1988).[20] Ute Diechmann's 1996 book, *Biologists under Hitler,*[21] confirms the connection between Mendelian genetics, agriculture and Nazi racial hygiene policies.

While the term eugenics was applied to many different kinds of programs, primarily two definitions of eugenics were used in the literature. Positive eugenics described programs that encouraged reproduction among people deemed to be superior specimens. Negative eugenics supported programs that worked to either eliminate weaker human specimens, or at least prevent them from reproducing.[22] Castle supported positive eugenics whereas Fasten supported negative eugenics. Fasten rationalized his stand with Mendelian genetics supported by mutation theory and neo-Lamarckism. Had Fasten supported natural selection, that would have changed the role Mendelian genetics could have played in breeding superior specimens, and his belief in negative eugenics and breeding science would have been seriously challenged. Consequently, from 1920-1945 Fasten taught zoology students at Oregon State College about the strengths of mutation theory and the weaknesses of natural selection.

What we find is that mutation theory, which supports the breeding policies of agricultural science, and natural selection, which would eventually support ecology, functioned as competing fields of scientific knowledge and practice in the period of the ongoing modern synthesis. The continuing sway of neo-Lamarckian ideals, explains how fisheries managers could find fish hatcheries acceptable replacements for salmon habitat and natural propagation. With respect to fish hatcheries, giving the environment a major role in species development meant that good food and a favorable environment would produce good fish. Whereas once the modern synthesis was established and supported by the discovery of the structure of DNA in the 1950s, this attitude would have been modified.

An evaluation of the OSC fisheries and wildlife management program through examination of college catalogs, unpublished agency reports and personal correspondence shows that agricultural breeding science took precedence over ecological science during the 1930s and 1940s. When a wildlife management program began in 1935, it was not housed within the College of Science but within the College of Agriculture under the division of Animal Industries. That placement as well as the funding and research schemes worked to extend agriculture's breeding techniques to wildlife. Fish and game were viewed as agricultural commodities raised to satisfy the needs of growing numbers of fishers and hunters. The quantity of wildlife produced was the emphasis, not the quality. Since fish hatcheries were viewed as an improvement over nature, artificially producing massive quantities of fish in hatcheries was an accepted form of agriculture production. However, when the numbers of fish produced in hatcheries did not provide plentiful runs for commercial and sportsfishers, the hatchery techniques came into question as did the problems of overfishing and pollution. As fisheries biologists investigated the problems, environmental concerns increasingly called into question the ability of fish hatcheries to compensate for degraded habitat. This thesis then argues that a Kuhnian conflict was set up between the scientists who adopted the agricultural breeding techniques for wildlife management as opposed to those scientists who took a more ecological approach. Aldo Leopold's 1949 book, *A Sand County Almanac,*[23] sets up the competition between agriculture, the field that was to improve upon plants and animals through better breeding, and ecology, the field that studies the interrelationships between species and the environment. Peter Bowler's *The Norton History of the Environmental Science* (1993)[24] provides the backbone for a brief history of ecology supplemented by articles by Joel Hagen[25] on plant ecology and Gregg Mitman and Richard Burkhardt, Jr.[26] on animal ecology.

With this scientific context established, Chapter 2 discusses the development of fish culture and fisheries science. In *Scaling Fisheries: The Science of Measuring the Effects of Fishing, 1855-1955* (1994)[27] Tim Smith argues that fish culture and fisheries science were estranged from ecology at the time when fisheries researchers began working to solve fisheries fluctuations and depletions. A Canadian and American study of Fraser River sockeye salmon in the early 1900s led by Stanford fisheries biologist Charles Gilbert shows that salmon catches did vary but with a predictable pattern, indicating different runs of salmon and different life cycles for each species. Using the techniques of fish scale analysis and tagging studies Gilbert worked to substantiate the home stream theory. *The Fisherman's Problem: Ecology and Law in the California Fisheries, 1850-1980* (1986)[28] by Arthur McEvoy tells of the failure of fisheries managers to protect overfished waters off the California Coast. Documents that forewarn the salmon crisis in the Pacific Northwest are presented with commentaries in *The Northwest Salmon Crisis: A Documentary History* (1996)[29] edited by Joseph Cone and Sandy Ridlington. A 1977 essay by Dan Bottom notes a lack of salmon life history studies in Oregon and argues the view of fish as agricultural commodities permeated Oregon fisheries manager's decision-making.[30] Two recent books, *Northwest Passage: The Great Columbia River (1995)*[31] by William Dietrich and *The Organic Machine (1995)*[32] by Richard White discuss the effects of dams on the Columbia River to salmon runs. *Saving the Salmon: A History of the U. S. Army Corps of Engineers' Efforts to Protect Anadromous Fish on the Columbia and Snake Rivers* by Lisa Mighetto and Wesley Ebel (1994) and *Army Engineers and the Development of Oregon: A History of the Portland District U. S. Army Corps of Engineers (1980)*[33] by William Willingham tell the story of water resource development in the Pacific Northwest from the perspective of the Army Corps of Engineers.

The most recent additions to a growing literature on the fisheries problems in the Northwest are *Making Salmon: An Environmental History of the Northwest Fisheries Crisis* (1999)[34] by Joseph Taylor and *Salmon Without Rivers: A History of the Pacific Salmon Crisis (1999)*[35] by Jim Lichatowich. Taylor addresses the cultural, political and economic factors that preceded the crisis from the nineteenth century to the present. He believes that the formation of a solution lies within the political realm rather than in the technological and engineering schemes that have been offered. Lichatowich argues that a worldview that disconnects humans from the natural world created the salmon problems. Both Taylor and Lichatowich offer detailed accounts of the interconnections of the salmon conundrum with science, especially with regard to the development and promotion of fish hatcheries as mitigation for lost salmon habitat due to the construction of dams throughout the Pacific Northwest. Both support the view that too much reliance was placed on fish culture to solve problems of depleted fisheries, and that discussion of ecological principles and population systematics did not take place in Oregon during the 1930s-1940s.

Research for this paper began with general histories and environmental histories about the Pacific Northwest. A number of recent books describe nineteenth century westward migration and the environmental impacts the industrialization of natural resources, especially water resource development, brought to the west. *The Pacific Northwest: An Interpretive History* (1989)[36] by Carlos Schwantes provides a comprehensive account of the settlement of the Northwest, natural resource development and its environmental impacts. Bill Robbins fine tunes the history of Oregon in *Landscapes of Promise: The Oregon Story, 1800-1940* (1998),[37] an environmental history that pursues impacts on the landscape from the first changes wrought by Native Americans to twentieth-century engineering feats on the Columbia River. Robbins also provides a unique discussion of the Willamette Valley Project, a water resource development scheme on the Willamette River. The damming of rivers of the West to provide water for irrigation are discussed in detail in other environmental histories. *Rivers of Empire: Water, Aridity, and the Growth of the American West* (1985)[38] by Donald Worster tells of a scarcity of water in the West that led to development of rivers for irrigation. *Cadillac Desert: The American West and Its Disappearing Water* (1985)[39] by Marc Reisner documents the environmental impacts of water development projects in the western desert from the early 1800s to the present, emphasizing the competition between the U. S. Army Corps of Engineers and the Bureau of Reclamation for water resource development projects. *Crossing the Next Meridian: Land, Water, and the Future of the American West* (1992)[40] by Charles Wilkinson discusses nineteenth century policies and laws meant to encourage westward expansion and now encumber environmental protection for the West.

Chapter 3 presents a 1934 case study of discussions regarding the fishways proposed for Bonneville Dam on the Columbia River. Unpublished sources from the minutes of the 1934 Oregon Game Commission from the State of Oregon Archives show that fish hatcheries figured prominently as a fail safe measure in the event that the fishways proved ineffective in allowing fish passage past the dam. The Oregon Game Commission participated in discussions in which the participants were expected to support fishway proposals, and instead they expressed opposition to the dam and also made demands of the federal government to protect salmon runs on the Columbia. Due to their opposition of water resource development, I argue that newly elected governor Charles Martin dismissed the members of the Oregon Game Commission in January of 1935. The dam was completed in 1938 and the fishways declared a success when salmon used the fishways to climb past the dam on their way to spawn. Due to this success, policy-makers declared that dams and anadromous fish could co-exist.

Chapter 4 presents the work of E. E. Wilson on the Oregon Game Commission from 1935-1949. Unpublished sources from the Wilson manuscript collection at the Oregon State University Archives show how Wilson came to oppose the Willamette Valley Project that would put dams on tributaries of the Willamette River, the primary salmon seed stream located directly 40 miles below Bonneville Dam. The project was proposed to make more land within the Willamette Valley farmable by constructing dams to provide flood control for bottomlands and protection of property, and water for irrigation of farmlands in the summer. Proponents hoped to attract refugees from the Dust Bowl and the Depression to the Willamette Valley. Wilson voiced opposition to the project because some of the dams would block anadromous fish passage and would mitigate the loss of natural salmon habitat with fish hatchery production, which he claimed was a scientifically unproven technology. Opponents to Wilson charged that he was not utilizing modern scientific procedures like fish hatcheries to supplement depleted salmon runs and was therefore unfit to lead the Oregon Game Commission. Records from his manuscript collection show that he was ahead of his time in questioning the efficacy of fish hatcheries and dam construction based on scientific fact-finding which employed techniques emblematic of the discipline of ecology rather than agricultural breeding science. Like the 1934 commission that voiced opposition to water resource development on the Columbia, this thesis argues that Wilson and the entire Game Commission were dismissed in 1949 by newly elected Governor Douglas McKay for their opposition to water resource development.

Introduction Endnotes

[1] Gregory Konkel and John McIntyre, "Trends in Spawning Populations of Pacific Anadromous Salmonids," U. S. Fish and Wildlife Service, Technical Report 9, 1986, p. 5.

[2] Willa Nehlsen, Jack Williams and James Lichatowich, "Pacific Salmon at the Crossroads: Stocks at Risk from California, Oregon, Idaho, and Washington," *Fisheries*, 16(2):4-21, on p. 4.

[3] Jonathon Brinckman, "Hatchery salmon unfit for life in the wild, report says," *The Oregonian*, p. 1, col. 3, December 3, 1999; for methods of changing hatcheries see Robert Bugert, "Mechanics of Supplementation in the Columbia River," *Fisheries*, 1998, 23(2):11-19; Robert Behnke, "About Trout: Do We Learn From History?' *Trout*, 1998, 31(2):55-57.

[4] Julian Huxley, *Evolution: The Modern Synthesis,* (New York, NY: Harper & Brothers Publishers, 1943). Huxley wrote on p. 1 that in 1943 biology was in a state of synthesis, and on pp. 25-26 he described the synthesis as the unification of life sciences; see also Peter Bowler, *The Eclipse of Darwinism: Anti-Darwinian Evolution Theories in the Decades around 1900* (Baltimore, MD: The Johns Hopkins University Press, 1983) in which Bowler used the term "modern synthesis" to describe a unified science on p. 5, the joining of genetics and selection theory on p. 117, as a refutation of orthogenesis on pp. 146 and 178, and on pp. 216-217 he described the scientists whose work affected the synthesis, and two steps of reconciliation between biometry and Mendelism, and between heredity science and Darwinism that led to neo-Darwinism.

[5] Bowler (1983) defined the term as the inheritance of acquired characters popular from 1885-1910, especially with the American school on p. 59, he discussed an alternate use to describe use-inheritance on p. 81, and in the discussion on the popularity of the term in the American school he wrote that neo-Lamarckism was used to denote a school of thought opposed to Darwinism on p. 135.

[6] Garland Allen, *Life Science in the Twentieth Century* (New York, NY: John Wiley & Sons, 1975).

[7] Huxley (1943).

[8] William Provine, *The Origins of Theoretical Population Genetics* (Chicago, IL: The Univ. of Chicago Press, 1971).

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[10] Ronald Rainger, Keith Benson and Jane Maienschein, eds., *The American Development of Biology* (Philadelphia, PA: Univ. of Pennsylvania Press, 1988).

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[12] Keith Benson, "From Museum Research to Laboratory Research: The Transformation of Natural History into Academic Biology," in Rainger, et. al. (1988), pp. 49-77.

[13] Diane Paul and Barbara Kimmelman, "Mendel in America: Theory and Practice, 1900-1919," in Rainger, et. al., (1988), pp. 279-303.

[14] Adele Clarke and Joan Fujimura, eds. *The Right Tools for the Right Job: At Work in the Twentieth-Century Life Sciences* (Princeton, NJ: Princeton Univ. Press, 1992).

[15] Wendell Barry, *The Unsettling of America: Culture & Agriculture* (San Francisco, CA: Sierra Club Books, 1986/1977), pp. 143-169; see also William Cronon "A Place for Stories: Nature, History, and Narrative," *The Journal of American History* 78(4) 1992: pp. 1347-1376.

[16] For example, see Dorothy O. Johansen and Charles M. Gates, *Empire of the Columbia: A History of the Pacific Northwest* (New York, NY: Harper & Brothers, Publishers, 1957) Chapter 25, "Harvests of the Sea," pp. 475-487.

[17] Nathan Fasten, *Principles of Genetics and Eugenics: A Study of Heredity and Variation in Plants, Animals and, Man* (Boston, MA: Ginn & Company, 1935).

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[21] Ute Diechmann, *Biologists under Hitler* (Cambridge, MA: Harvard University Press, 1996).

[22] Bowler (1983), pp. 77-83, and Paul (1995), pp. 40-45.

[23] Aldo Leopold, *A Sand County Almanac, With Essays on Conservation from Round River* (New York, NY: Ballantine Books 1973/1949).

[24] Peter Bowler, *The Norton History of the Environmental Sciences* (New York, NY: W. W. Norton & Company, 1993/1992), pp. 503-535.

[25] Joel Hagen, "Organism and Environment: Frederic Clement's Vision of a Unified Physiological Ecology," in Rainger, et. al, (1988), pp. 257-278.

[26] Gregg Mitman and Richard Burkhardt, Jr., "Struggling for Identity: The Study of Animal Behavior in America, 1930-1945" in Benson, et. al. (1991), pp. 164-189.

[27] Tim Smith, *Scaling Fisheries: The Science of Measuring the Effects of Fishing, 1855-1955* (Cambridge, MA: Cambridge Univ. Press, 1994).

[28] Arthur McEvoy, *The Fisherman's Problem: Ecology and the Law in California Fisheries, 1850-1980* (New York, NY: Cambridge Univ. Press, 1993).

[29] Joseph Cone and Sandy Ridlington, *The Northwest Salmon Crisis: A Documentary History* (Corvallis, OR: Oregon State Univ. Press, 1996).

[30] Dan Bottom, "To Till the Water: A History of Ideas in Fisheries Conservation," in D. J. Strouder, P. A. Bisson, and R. J. Naiman, eds., *Pacific Salmon and Their Ecosystems: Status and Future Options* (New York, NY: Chapman and Hall, 1977), pp. 569-597.

[31] William Dietrich, *Northwest Passage: The Great Columbia River* (New York, NY: Simon & Schuster, 1995).

[32] Richard White, *The Organic Machine* (New York, NY: Hill and Wang, 1995).

[33] William Willingham, *Army Engineers and the Development of Oregon: A History of the Portland District U. S. Army Corps of Engineers* (Portland, OR: Army Corps of Engineers, 1980).

[34] Joseph Taylor, *Making Salmon: An Environmental History of the Northwest Fisheries Crisis* (Seattle, WA: Univ. of Washington Press, 1999).

[35] Jim Lichatowich, *Salmon Without Rivers: A History of the Pacific Salmon Crisis* (Covelo, CA: Island Press, 1999).

[36] Carlos Schwantes, *The Pacific Northwest: An Interpretive History* (Lincoln, NA: Univ. of Nebraska Press, 1996).

[37] Bill Robbins, *Landscapes of Promise: The Oregon Story, 1800-1940* (Seattle, WA: Univ. of Washington Press, 1997).

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[40] Charles Wilkinson, *Crossing the Next Meridian: Land, Water, and the Future of the West*, (Covelo, CA: Island Press, 1992).

Chapter 1 - The Biological Scientific Revolution

Debates regarding the move from special creation to organic evolution gained momentum after Charles Darwin proposed the theory of evolution and natural selection in 1859. This chapter will discuss the modern synthesis and various interpretations of evolutionary theory prevalent during the 1930s-1940s. Ichthyologist David Starr Jordan and William Castle's views on the move from mutation theory to natural selection taken from their biology textbooks highlight their opinions regarding neo-Lamarckism. Finally the viewpoint of Oregon State College Professor Nathan Fasten will be contrasted to Castle's. Professor Nathan Fasten chaired the Department of Zoology, he was a fisheries biologist who consulted with the Oregon Game Commission, and he taught regularly for 25 years from 1920-1945. Fasten's viewpoints as a consultant, as a department chair and as a teacher contrast with the development of modern genetics at other institutions. His 1935 textbook, *The Principles of Genetics and Eugenics,* represents a version of species formation worthy of examination because it reflects the confusion of opinion about evolution prevalent at that time. Fasten's account shows that as late as 1941, breeding techniques common to heredity science, as well as a neo-Lamarckian explanation for variation, did not easily give way to natural selection and the foundations of genetics.

A brief overview of events critical to the modern synthesis will be presented and will emphasize the factors that weigh heavily in Fasten's 1935 textbook. It is impossible here to give a complete exposition of 45 years worth of scientific work done during the modern synthesis. Therefore, detailed description will be given up to 1935, and later events of the modern synthesis have less prominence in this portrayal. A brief discussion of eugenics and views on breeding from that time period will further show disparate opinions about evolutionary theory.

The Modern Synthesis

With the publication of *Origin of Species* in 1859 came hotly debated religious disputes over the existence of evolution. Many people, including many scientists, believed that divine creation was responsible for new species. Other groups, like domestic breeders of plants and animals, accepted various forms of seemingly contrary notions regarding evolution: they accepted that species may evolve, and conversely that like individuals produced like individuals. However, most theoretical scientists by 1870 did not dispute evolution, but asked fundamental questions about how evolution was accomplished and what guided the process. During the modern synthesis, the life sciences were separate disciplines with a wide array of evolutionary theories supported by many different standards of evidence. A wealth of material exists describing the people, theories and events between 1900-1944 that produced a biological revolution. The standard story describes the modern synthesis as a linear process from 1900-1944 that unified the life sciences under one theoretical umbrella that embraced evolution pushed by natural selection. This evolution synthesis was supported by the new science of genetics and by verifiable, material forms of evidence as proof. Along the way, this process established legitimacy for biology by developing modern statistical and experimental laboratory techniques as standards of evidence, as opposed to philosophical constructs sustained by logic.[1] While many scientists followed this pattern, the conversion of science and scientists was not uniform.

Lamarckism

The modern synthesis was preceded by a variety of materialist theories and evolutionary ideas. Most of the theories on evolution were supported by philosophy and a non-material force that directed the activity. Special creation charged that the complexity of nature could not have happened by chance and could only be explained by God's intervention. Theories from the seventeenth and eighteenth centuries include various degrees of divine design, order and adaptation: the preformation theory proposed that miniature models of organisms existed perfectly formed within the reproductive material and were stimulated to grow by fertilization; the epigenesis theory stated that undifferentiated material grows and takes shape after fertilization.[2] William Paley's *Natural Theology* (1802) proposed a static system where adaptation of organisms was part of God's design, and Jean Baptiste Lamarck proposed in *Philosophie zoologique* (1809) an entire theory of evolution that included design, adaptation and a direct role of the environment in stimulating change.

Lamarck's version was popular because it validated what biologists witnessed in the field, adaptation of plants and animals to the environment, and it included a directed form of evolution in the production of variations that would stimulate change prior to the organism's formation. For example, should a person lift weights to build muscles, Lamarckism implied that the weight lifter's children would then be born with highly developed muscles. Under Lamarckism, physical and mental abilities acquired during the lifetime of the individual, such as muscles, intelligence or musical expertise, were heritable by succeeding generations. Lamarck's proposed inheritance of acquired characters was a widely accepted idea that proved difficult to scientifically refute.[3] In contrast, Darwin's theory of evolution argued that natural selection acts on chance variations which were heritable for organisms living long enough to reproduce. The role of chance in Darwinism disturbed both religious and scientific practitioners because it denied humans a special position in the eyes of God, and its competitive and materialistic nature described a cold and inconsequential existence. As scientific research advanced, both theories experienced transformations when portions were accepted by some groups and discarded by others.

Crucial to Darwin's argument was the efficacy of natural selection as the mechanism that drove evolution. Darwin theorized that each species produced individuals with subtle, chance variations, yet he did not indicate how variation was accomplished. Those individuals with variations that were best suited to their environment survived and individuals with variations poorly suited to the environment died out. The survival of individuals with beneficial variations was the product of natural selection. Because those individuals with beneficial variations lived and reproduced, those variations were then passed on to succeeding generations. The continuous process of the gradual accumulation of small modifications present in the selected individuals that survived from generation to generation coupled with isolation eventually produced new species.[4] The question of how variation was produced challenged many evolutionary theorists. While Darwin did not adequately provide a means by which variation was produced in his 1859 book, he addressed the omission in his 1868 book, *The Variation of Plants and Animals under Domestication*. In this book he offered the theory of pangenesis which incorporated a Lamarckian theory of the inheritance of acquired characters as the source of variation. Pangenesis stated that changes caused by environmental influences could become heritable. Minute particles called gemmules carried hereditary messages from the body cells (somatoplasm) through the circulation system and implanted them into the reproductive material (germ plasm) which then became heritable information.

Continuous versus Discontinuous Evolution

Points of contention surfaced regarding Darwin's evolutionary theory from two of his allies who preferred evolution by leaps (discontinuous) as opposed to gradual evolution (continuous). Thomas Henry Huxley indicated that continuous evolution could not be reconciled with the gaps in the geologic record. While he accepted that natural selection played an important part in evolution, he suggested that discontinuous variations caused leaps in evolution which resulted in new species, explaining why the geologic record did not show intermediate fossilized forms in between species.[5] The case for discontinuous evolution also was made by Darwin's cousin, Francis Galton, who questioned the ability of the gradual accumulation of variations alone to produce new species. He based his arguments on statistical evidence from hereditary studies of sweet pea plants. These studies showed that despite the accumulation of small variations, and despite the production of exceptional qualities within some individuals, the population would regress towards the mean, never varying beyond established limits, making new species creation impossible. Galton proposed that evolution proceeded through the production of discontinuous variations, and that selection, either natural or artificial, then processed those factors as well as the continuous variations to produce new species.[6]

In 1883, a year after Darwin's death, August Weismann's experiments on mice argued against Lamarckian inheritance thereby invalidating pangenesis. Weismann's germ plasm theory of inheritance stipulated that the germ plasm was separate from the somatoplasm, and that only the germ plasm could transfer hereditary information.[7] Without an acceptable mechanism to explain variation on which to base natural selection, Darwin's theory fell into stagnation and the mutation theory was to gain popularity. The debate between either gradual evolution or evolutionary leaps as the crucial factor in species development formed the backbone of contentious disagreements between two schools of thought that gained strength when the work of Gregor Mendel was rediscovered by Dutch biologist Hugo de Vries, in conjunction with Carl Correns and Erik Tschermak in 1900. Both de Vries and Correns used Mendel's 1866 work on pea plants to show that hereditary traits were passed onto progeny in discrete units, contradicting the theory of blending inheritance. While this theory served to show how heredity worked, it did not explain species formation. De

Vries added insight from his previous experiments on the evening primrose plant that indicated that breeding resulted in new hereditary characters causing species formation. De Vries' previous hereditary studies demonstrated that evolutionary leaps driven by discontinuous mutations motivated species formation and he interpreted Mendel's work as further support for mutation theory. Additionally, his work showed that recessive traits expressed in the first generation, would show up in the second generation in a ratio of three to one. This interpretation exacerbated the conflict between the Biometricians, who believed in Darwin's views on continuous evolution, and the Mendelians, who believed that Mendel's work supported the discontinuous form, mutation theory.[8]

A series of experiments in the early 1900s attempted to establish whether or not natural selection could produce new species, thereby validating or refuting mutation theory. In 1902 Wilhelm Johannsen proposed that pure lines of plants, those whose germinal plasm was identical, could not be altered by artificial selection even when selecting individuals exhibiting extreme variations. Like Galton's experiments, Johannsen's work supported the theory of regression, that the stability of types within a population would remain constant. Johannsen agreed with de Vries that the only way to affect change was through discontinuous evolution driven by mutations[9]. Johannsen's results were confirmed by Herbert Spencer Jennings in 1908-1910, and by Raymond Pearl in 1908.[10]

William Castle also initially adhered to the pure line/mutation theory, but later selection experiments executed from 1907-1919 produced new types, supporting Darwin's gradual evolution by selection, not evolution by mutation.[11] In 1903 T. H. Morgan also supported mutation theory, but by 1909 his experimental work with drosophila fruit flies led Morgan to suspect that the way de Vries had interpreted Mendel's work might be wrong. He found within his work with drosophila minute modifications that carried Mendelian characters leading Morgan to slowly change his mind. In 1915 the Morgan research group published *The Mechanism of Mendelian Heredity* which unified Mendel's work with the chromosome theory of inheritance. Morgan proposed that the Mendelian-chromosome theory could be extrapolated to general biological principles that applied to other animals and plants.[12] In 1908 H. Nilsson-Ehle reported that his experiments on wheat showed that sexual reproduction produced unlimited variations through genetic recombination. He concluded that natural selection combined with unlimited variety produced by recombination could yield new species; Edward Murray East confirmed these results in 1910.[13] In light of work regarding genetic recombination, Jennings began a new series of experiments. In 1917 he changed his mind about mutation theory and wrote that Mendelian recombination coupled with Darwinian selection would produce new varieties that could respond to environmental changes, thereby affecting evolution of species.[14] In addition to this work in favor of Darwin, in 1918 H. J. Muller found that the species of plant that de Vries had used in his experiments that supported mutation theory had a predisposition to mutation, casting further doubt on the strength of the mutation theory.[15]

Research from the next two decades cemented the belief that the rearranging of Mendel's hereditary units was the mechanism for natural selection.[16] In addition to providing the basis for altering theoretical evolutionary views, this work established experimental laboratory techniques as standards of evidence for biological questions, and the use of statistical evidence laid the groundwork for the development of population systematics within the life sciences. In 1937 the production of a unified theory of biology that reduced evolution to a process dominated by adaptation through natural selection, supported by laboratory experimental and statistical evidence began and continued through the 1950s. While the major factors of evolutionary theory may have been solidified for many participants by the 1920s,[17] other groups did not so readily accept natural selection and clung to mutation theory through the 1940s.

Agriculture and Eugenics

A discussion of evolutionary theory is not complete without discussing how eugenics contributed to the theory's progress. Some expositions of the Modern synthesis do not include a discussion of eugenics.[18] Standard science has worked to disassociate itself from eugenics for two good reasons. The "science" of eugenics did not conform to the standards of modern science in which laboratory experiments supported theory. Eugenics theory addressed the breeding of humans. Because humans were not appropriate laboratory subjects in most countries, eugenics theory was only supported through subjective statistical evidence. Primarily eugenics has been eschewed by standard science because it was associated with the justification for the Nazi extermination camps of World War II. Within science of the 1920s and 1930s, eugenics was complex and took many paths, leading to support for racist policies, as well as providing impetus for studies of heredity and genetics.[19]

Eugenics concepts within agriculture fit well with the breeder's belief in the ability to improve nature. Facets of eugenics were an extension of the belief that if plants and animals could be improved through breeding, so could humans. Generally, breeder's relied on a "good eye" to produce superior species. With the introduction of Mendel's work, a more scientific system of production was adopted as dominant and recessive traits were understood and could be applied to laboratory experiments that would result in more precise predictions regarding animal breeding techniques.[20] Mendel was enthusiastically embraced by agricultural programs and experiment stations throughout the U. S. because it gave their craft scientific legitimacy. The first genetics departments within academia were in agricultural colleges and the affiliated experiment stations where research and instructional programs addressed Mendel's work. However, the links between the chromosome theory of inheritance and Mendel transformed the agricultural breeder's applied science into one that increasingly addressed problems foreign to experiment stations and agricultural colleges. With the growth of genetics as a biological program, the science of heredity that had been claimed by agricultural scientists was being usurped.[21] While the mutation/natural selection theory debate remained the province of academic evolutionary biologists,[22] many agricultural breeders in practice preferred a theory of mutation or a theory of neo-Lamarckism because these theories seemed to promise some control of rapid changes by breeding or eugenics. Therefore, when practitioners chose to support mutation theory over natural selection they were siding not only with a personal socio-political belief, but also with a professional belief in the ability to breed superior products.

Many of the Biometricians and the Mendelians considered themselves eugenicists at some point and were motivated by the positivist sentiment that science could improve nature. Concern over the degeneration of the human species led scientists as well as sociologists to argue that breeding techniques that had been used to improve plant and animal species should be applied to improve the human species as well. The heredity studies by mathematician Francis Galton in 1865 laid the groundwork for his 1869 book on race improvement, *Hereditary Genius*. This work challenged Darwin's theory of pangenesis as well as gradual evolution in favor of evolution by leaps. By 1875 he had developed his own theory of heredity which concentrated heritable information only in the germ plasm. By 1894, he confirmed that new breeds or species were created by mutations which were then selected either artificially or naturally to achieve evolution. The source of mutation was provided by a Lamarckian heritable organic change supplied by nature.[23]

Eugenics was popular in academia and many biologists adopted various forms of eugenics theory.[24] While there were many different theories on eugenics that were often contradictory, two viewpoints emerged. Positive eugenics dictated that individual with good traits should thrive and reproduce using Mendel's system to breed superior human beings. Negative eugenicists believed that ability was decided at birth and could not be altered. Since the unfit could not improve themselves, negative eugenicists proposed to eradicate defective human characters through isolation, sterilization and extermination.[25] At this point, neo-Lamarckian factors were invoked to explain the unexplainable on either side. Pregnant women were encouraged by eugenicists to positively mold the character of their children during pregnancy through their own educational efforts, as well as to guard their germ plasm by eating well.[26]

Natural Selection and neo-Lamarckism

Once natural selection was combined with Mendel in early twentieth century science, many scientists who claimed to advocate eugenics dropped support for mutation theory and Lamarckian theories of acquired characters, while others vacillated. David Starr Jordan, President of Stanford University and a fisheries biologist, wrote about eugenics in *Evolution and Animal Life* (1916) in which he supported natural selection, but not as the sole determinant of organic evolution. "Few investigators question the far-reaching influence of natural selection, but there are many phases in organic evolution which cannot be ascribed to it."[27] His stand on neo-Lamarckism as the source of variation was that it hadn't been proven in the laboratory, but that the concept should not be wholly discounted. He believed that the inheritance of acquired characters may have some bearing on heredity. "And there are to-day many Lamarckian evolutionists. So that in our list of possible evolution factors the so-called Lamarckian factor should not be omitted."[28]

Environmental influences were important, especially during growth and development of offspring. However the lack of a mechanism to prove Lamarckism contradicted materialist science. "We have no proof to show that the environment of one generation determines the heredity of the next - and yet perhaps most naturalists feel that the effects of extrinsic influences work their way into the species, although a mechanism by which this might be accomplished is as yet unknown to us."[29] He cited the inheritance of instincts as a factor that had been debated by selectionists and Lamarckians. "(Herbert) Spencer believes instinct may be explained as the inheritance of habits of the individual. Neo-Darwinians call it natural selection acting on endless variations."[30] Jordan believed that evolutionary factors should be materialist and should be "grounded upon inductive demonstration, not logic." However, in the case of instinct, even Jordan seemed to support Lamarck.

> Meanwhile we may regard the theory of the inheritance of acquired characters as a piece of scaffolding which has served its purpose...At present most of it-perhaps all of it-must be taken down, but it may be that from that same base will arise a better constructed theory which will again serve a purpose in the study of organic evolution.[31]

William Castle, a professor of genetics at Harvard University, wrote in his 1930 biology textbook *Genetics and Eugenics: A Text-book for Students of Biology and a Reference Book for Animal and Plant Breeders* that "the three processes, heredity, variation and natural selection, account fully for evolution."[32] Castle distinguished between de Vries' mutation theory and a "modern" mutation theory. De Vries' version depended on large, discontinuous mutations that immediately produced a new species without evidence of intermediate groups. In this version, de Vries held that the continuous, fluctuating variations of Darwin were not permanent. The modern mutation theory, Castle maintained, confirmed that small genetic changes are as permanent as large ones and when taken in the aggregate can result in species change. Castle defined mutation as any heritable change. He also indicated that in the modern mutation theory, fluctuating variation "is not to be regarded, as it was in the view of De Vries, as something impermanent and fleeting." The small variations of the modern mutation theory were in line with Darwinian natural selection.[33] He expanded on Jordan's views regarding Lamarckism and negated Lamarck with respect to direct modification of the environment.

However, Castle indicated that in higher animals germ cells may be modified by soma cells and cited parallel induction as a possible means of indirectly transmitting phenomena such as memory and reactions to internal secretions (hormonal influences). "For Weismann admits that the environment may cause *parallel* modifications of soma and germ plasm...That a mechanism for the transmission of acquired characters from soma to germ-cells has as yet not been demonstrated, does not of course disprove the existence of such a mechanism."[34] He finally concluded that neither the extreme Lamarckian position that all acquired characters are inherited, nor the extreme position of Weismann that absolutely no outside influences may modify the germ plasm were correct. Castle also espoused eugenics and devoted five chapters to the subject in this book. However, he viewed the utility of eugenics as a social remedy best promoted through education, not a biological one that demanded unrealistic social controls in a democracy.[35]

Nathan Fasten's Genetics

Contrary to many of Nathan Fasten's contemporaries, he did not accept natural selection's role in evolution but supported mutation theory and remained an advocate of Lamarckism. Fasten seems to have been stuck in a time warp in which the heyday of breeders techniques dominated by Mendel's work from 1900-1920 had never been challenged by the advances of genetics. However, if Fasten began teaching at OSC in 1920, then looking backwards, he would have attended college during the height of the rediscovery of Mendel, mutation theory, and the growth of eugenics. Often, the concepts learned during college are the most difficult to overcome. Once Fasten started teaching he would have been exposed to the USDA experts who would have further supported the utility of Mendelism for its practical use in selective breeding, hybridization and evolutionary implications. In analyzing Fasten's genetics, eugenics coupled with breeding played an important role in his 1935 interpretation of natural selection and Mendelian genetics.

Nathan Fasten[36] began teaching in the Department of Zoology in 1920 and chaired the department from 1921-1945. Fasten's areas of expertise were fish and shellfish, he had previously consulted with the Game Commission on two projects,[37] and he regularly taught classes on genetics. He taught four classes on a yearly basis: two classes, Principles of Zoology and General Zoology he taught with staff; the classes, Genetics and Evolution and Eugenics he taught alone.[38] The genetics class was described in the 1935-1936 catalog as,

> ...a study of heredity and variation in plants and animals. Special emphasis on such topics as heredity versus environment, the inheritance of acquired characteristics, the glands of internal secretion and development, Mendelian principles of heredity, newer developments in heredity, and heredity in man.[39]

Fasten wrote three books: *Origin Through Evolution* (1929), *Principles of Genetics and Eugenics* (1935) and *Principles of Zoology* (1941). A 1931 listing of his publications shows 29 articles primarily on fish and shellfish; some were published in scholarly journals, some in experiment station bulletins, and some in newspapers. Included with this group were also articles on eugenics: "Eugenics and War" (1929), and "What is the aim of Eugenics?" (1930).[40] Two of Fasten's books, *Genetics and Eugenics*, and *Principles of Zoology* are textbooks. In the genetics book Fasten devoted four chapters to eugenics; in the zoology book eugenics was discussed briefly.[41] In the bibliography of his book, *The Principles of Genetics and Eugenics: A Study of Variation in Plants, Animals and Man,* works were listed from most of the people involved with the development of the modern synthesis. William Castle's 1930 textbook was listed and much of Fasten's book seems to have been patterned after Castle's. The title is almost identical, the structure of the book includes four chapters at the end which address eugenics, and many of the topics and examples cited are the same. The substantive differences are that Castle accepted a role for natural selection whereas Fasten supported de Vries' mutation theory. In addition, Castle promoted eugenics on sociological principles whereas Fasten promoted biological uses for eugenics. Regarding Lamarck, only subtle differences divide the two.

Fasten supported Hugo de Vries' mutation theory to explain species formation rather than Darwin's theory of evolution and natural selection. Unlike Castle, Fasten did not differentiate between an older and newer mutation theory, and he did not differentiate between an older and newer meaning for fluctuating variations. He wrote that the only types of permanent changes were those that affected the germ cell and according to him, only the discontinuous mutations, be they small or larger, were the variations that were heritable.[42] Fasten indicated that Darwin's continuous, fluctuating variations were not permanent, whereas de Vries' discontinuous variations were permanent.[43] Fasten then used Johannsen's pure line theory to show that natural selection was incapable of establishing new variations but did not indicate that pure line theory fell from favor. "The experimental work on pure lines has led most modern biologists to the conviction that selection has no effect on the genes responsible for the production of unit characters."[44] Fasten used Castle's work from 1911 to support pure lines, but indicated that Castle's later work which supported natural selection was faulty.[45] He cited Weismann's research to disprove Darwin's use of pangenesis as the source of variation necessary for natural selection to operate.

Fasten showed that he was familiar with the work of Morgan with respect to sex linkage and chromosome mapping, and most of Morgan's books and articles were listed in the book's bibliography. Nevertheless, Fasten never indicated in his book that Morgan's work, coupled with the work of Castle, Jennings, Nillson-Ehle and East, supported the view that Mendelian genetics was the mechanism for natural selection.

Morgan's biographer, Garland Allen, wrote that Morgan's conversion from de Vries and mutation theory to Darwin and natural selection was known as early as 1916, and that it was complete by 1925 when Morgan published *Evolution and Genetics,*[46] a book also listed in Fasten's bibliography. It is as if the Mendelism that the USDA pushed in the first decades of the century at agricultural colleges had never been eclipsed. Without pangenesis and without Mendel, natural selection did not have a source of variation that would drive evolution. Fasten then dismissed natural selection without a mechanism as a fanciful, philosophical construct, similar to vitalism, without factual foundation. "Natural selection was considered of the greatest importance to Darwin. This is a blind force existing in nature which automatically chooses the best-fitted organisms that present themselves for survival."[47]

In discussing Darwin's theory of evolution, Fasten took great pains to indicate that while natural selection may be unacceptable, other tenets, such as the fertility of the organic world, operation of heredity and variation, and survival of the fittest, were still valid. "Because mutations rather than fluctuating variations are now considered responsible for species formation, one must not infer that the other general factors of evolution as enunciated by Charles Darwin must therefore be discarded."[48] He then indicated that Darwin had not understood hereditary terms.

> The trouble with Darwin's conception was that he did not distinguish carefully between changes that were permanent, or germinal, and those that were transitory, or somatic. Had he done this, he would have realized what every present-day biologist knows, that no matter what the agency is that is doing the selecting, whether it be nature or man, it must single out only those new acquirements for perpetuation that are germinal in origin and of the nature of mutations.[49]

Fasten steadfastly supported a neo-Lamarckian role for the environment in the production of variation within organisms even though he was aware of August Weismann's 1883 experiments which appeared to disprove the inheritance of acquired characters. Fasten accepted the majority of Weismann's work but at the same time discounted Weismann because he did not explain the source of variation, if it was not environmental. "What makes variations occur spontaneously? What are the influences which cause germ cells to vary . . . These are the things we are interested to know, but the language used by the Weismannians, while consisting of nicely spun words and phrases, nevertheless explains nothing."[50] While Fasten was sure that discontinuous variations were the source of heritable change, he did not know how those variations surfaced. Throughout this book Fasten continually and persistently returned to the problem regarding the source of variation within organisms. If only biologists understood the source of variation, Fasten wrote, all of the mysteries regarding speciation would be solved. He did not, however, refer to the work done by Nillson-Ehle and East from 1908-1910 indicating the sexual recombination produced an unlimited source of variation. Instead, he attributed recombination to Weismann's germ plasm theory.[51] He also discussed the environment as the source of variation and cited work on geographic isolation, Lamarck's theory of use and disuse and the inheritance of acquired characters. Fasten cited recent research that had failed to prove the inheritance of acquired characters. He characterized these efforts as a failure of science, not a failure of the theory. Like Castle, Fasten wrote that because science hadn't been able as yet to prove Lamarckism did not mean that the theory was invalid, just unproven for the moment.

> In spite of the fact that there is no more vital problem than this one, the results on the whole have been very disappointing. Nevertheless there is no basis for the claim that the arguments in favor of the inheritance of acquired modification are absolutely without foundation. All that one can say to this point is that no one has succeeded in proving such inheritance. In passing, it is well to remember that all variations are really of the nature of acquired characters...[52]

Fasten also cited parallel induction as a means by which variation was caused by environmental factors which work in a parallel fashion upon somatic cells and germ plasm thereby inducing heritable differences. Even though this was a variation upon the Lamarckian theme, he distinguished it from neo-Lamarckism.[53] Because Fasten did not link Mendel's genetics with natural selection, he evaluated Mendel's work alone. According to Fasten, Mendel's work gave breeders of domesticated species a more efficient way to predict beneficial matings and improve on plants and animals.

The book's presentation on eugenics then focused on the ability to improve the human species through the use of Mendelian genetics. Fasten's chapters on eugenics cover discussions of physical, physiological and mental traits eugenics could improve. To justify eugenics programs he cited pedigree studies of noteworthy individuals contrasted with family studies of the Jukes, the Zeros, the Tribe of Ishmael and other examples of weak constitution. His solutions included both positive and negative measures. He supported segregation and sterilization of defectives, as well as education and promotion of better human breeding techniques to produce a stronger population in the United States. One of the techniques he promoted included inbreeding, exemplified by the pure line doctrine, to improve the constitution and vigor of progeny. He did not envision a problem with close family members marrying as long as the couple's ancestry was known well enough to predict in good Mendelian fashion the dominant and recessive characters.

> In general there has been a grave objection to inbreeding of related individuals in the human family, because in many instances such matings have yielded deficient or defective offspring. Where the heredity is good and there are no undesirable covered-over traits, there is no objection to the intermarriage of such relatives.[54]

For Fasten, the aim of eugenics was to develop a social consciousness through "various means - educational, civic or otherwise -" that would humanely decrease defectives and increase the number of gifted individuals. "The eugenicist...does not aim to establish a new race of supermen but desires, rather, a race of sturdy, intelligent, and healthy individuals..." He indicated that a combination of heredity and environment were the keys to success.[55] Fasten chose to believe that the stimulus for variation was environmental as this was a basic tenent of eugenics theory, that improvement of the human race may be achieved through good breeding and favorable environmental factors which could be translated into heritable changes. Should the source of variation come from within the organism the end product could not have been easily controlled by breeders. Fasten held tightly onto mutation theory, the neo-Lamarckian and the pure line doctrines because they buttressed his belief in eugenics.

The Journal of Heredity

Morgan's work combined chromosome theory and Mendel. It served to unify the biological sciences. Genetics was transformed and became a part of biology more so than agricultural science, signaling an end to agricultural science as the primary venue for genetics research.[56] In addition to challenging the theoretical basis of agricultural breeders, Morgan's work also affected professional journals that published genetics material. The Association of American Agricultural Colleges and Experiment Stations supported The American Breeders' Association (ABA), organized in 1903, and the USDA published the ABA Proceedings in which Mendel and Morgan's work were discussed.[57] Prior to 1915 Morgan had supported the eugenics movement and was a member of the ABA Committee on Animal Breeding, a group that addressed animal breeding and eugenics. In 1910 the ABA began publication of the *American Breeders Magazine* which included articles on plants, animals and eugenics.[58] By 1915 the ABA had changed its named to the American Genetics Association, and the *American Breeders Magazine* had changed its name to *The Journal of Heredity*.[59] Also in 1915, Morgan resigned from the committee, citing unscientific and unreliable statements regarding eugenics made by the magazine, but remained a member of the journal's board.[60] In 1916, Morgan founded the journal *Genetics* which was dedicated to "longer more detailed accounts" of "fundamental investigations in genetics."[61] In addition to this journal, in 1913 the USDA began publishing the *Journal of Agricultural Research*, which presented articles on applied agricultural problems. Not only was the research venue changed, but the means by which work was disseminated was altered as well. Morgan's journal addressed genetics theory, the USDA journal addressed applied agriculture, and the *Journal of Heredity* increasingly addressed matters of eugenics, mixed with agriculture.

Fasten published an article in *The Journal of Heredity* in 1932 and his genetics book was reviewed by this journal in 1935. Many of the same factors of evolutionary theory Fasten supported were echoed in this journal. A 1933 retrospective on Hugo de Vries declared him the founder of the science of genetics as he discovered the "essential harmony between Mendelism and the Mutation Theory." He was credited with finding problems with Darwin's fluctuating variations. "He was the first to see clearly the inadequacy of the older methods of attack on these (evolutionary) problems, and to recognize that something more was involved than the gradual accumulation of quantitative variations." The article made two vague references to criticisms of de Vries' work but in no way indicated that the mutation theory had been seriously challenged by natural selection.[62]

In every issue, plant and animal breeding articles were curiously mixed with articles on eugenics programs throughout the world. While articles from the early 1930s extolled the eugenics efforts in Germany, articles in opposition appeared from the mid to late 1930s. While an emphasis was placed on supporting mutation theory and the role of environmental influences, articles supporting natural selection increasingly appeared as well. A 1934 book review by William Castle, a member of the journal's editorial board, discussed both Darwinian natural selection and environmental influences transmitted through parallel induction. "He (the author) is still an ardent selectionist and Darwinian but like Darwin, has Lamarckian leanings which make him regard the riddle of evolution insoluble without a slow and gradual inheritance of environmental effects."[63] In a 1937 book review, Castle evaluated a book on evolution and genetics written by a paleontologist. The author wrote that the geologic gaps could not be explained solely by natural selection and indicated that phylogenic evolution involved two distinct phases of development. The phases explained by Castle were "...(1) a usually brief phase of the discontinuous and explosive origin of new types and (2) a subsequent phase of longer duration in which the types change progressively and more slowly through the origin and selective elimination or retention of minor modifying gene mutations." Castle wrote that this example involved both natural selection and mutation, but denied Lamarckism.[64]

A Kuhnian Analysis

Both Thomas Kuhn in *The Structure of Scientific Revolutions* (1962)[65] and Allen (1979) note that scientists are resistance to change. Allen points to good reasons for this reluctance.

> Yet it is important to realize that there resides in the scientific community a certain conservatism, a certain reticence about immediately jumping behind any and every new idea. New ideas appear almost daily; consistent and well-integrated theories could not be build [sic] up over the course of time if old ideas are scrapped immediately on the advent of every new one. Discounting the conservatism of particular individuals, the scientific community as a whole is not as fast to change its shared beliefs as popular legend has it...Scientists, both as individuals and as a collective body, also share in a reluctance to abandon cherished ideas.[66]

Kuhn details specific criteria for analyzing scientific change. Since Kuhn's and Allen's books, the notion of revolutionary change within science has been challenged. This is not the forum for that debate. However, much of the criteria Kuhn cites about scientific change fits particularly well with this study regarding the retention of Lamarckian attitudes, and especially when referring to Nathan Fasten.

Kuhn pits cumulative scientific change, which had been the standard for analysis of scientific development, against revolutionary scientific change. Beyond looking at objective criteria to account for adoption of new theories, Kuhn considered subjective criteria and upheld the growing use of sociological principles to evaluate the history of science. Thomas Kuhn was a graduate student in physics when his interests changed to the study of the history of science. Many of the examples from his book are drawn from physics. However, he wrote that the criteria for a scientific revolution may be applied to any scientific discipline, including biology.

A scientific revolution is the movement of scientific practitioners from one scientific paradigm to another scientific paradigm. The definition of a paradigm was the point with which Kuhn was most fuzzy because he adopted a circular argument to define the term. "A paradigm is what the members of a scientific community share, and, conversely, a scientific community consists of men [sic] who share a paradigm."[67] A scientific community consists of those who work within the framework of a scientific specialty. They have been educated within a tradition that defines specific data and methodologies as the means by which one investigates natural phenomena. The people read the same journals, attend the same professional meetings, and share a common language specific to the specialty. The group performs "normal science," a term Kuhn referred to as a scientific tradition that directs research. Normal science means "...research firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice."[68]

These achievements are often described in scientific textbooks that serve to uphold the traditions inherent in the achievements and build a scientific community that agrees on basic fundamentals of their specialty.

> Men [sic] whose research is based on shared paradigms are committed to the same rules and standards for scientific practice. That commitment and the apparent consensus it produces are prerequisites for normal science, i.e., for the genesis and the continuation of a particular research tradition.[69]

However, scientific anomalies, those problems that do not fit within the confines of normal science, challenge the researcher to search beyond their scientific tradition to find explanations for the natural phenomena. This challenge leads to competition between different scientific traditions as each attempts to provide an explanation for anomalies that were drawn from their specific specialties and paradigms. As a result, often a new specialty is born and the older traditions slowly fade. Because practitioners of specific scientific traditions often have careers based within a tradition, to deny that tradition would deny their life's work and they resist assimilation. In addition, when parts of a theory are deemed suspect, often the entire theory is rejected. Such was the case in moving from special creation to organic evolution.

The patterns of species development Charles Darwin saw during his voyage on the Beagle challenged the normal science of special creation. When Darwin supplied pangenesis, a Lamarckian explanation for the production of variation, within his theory of evolution and natural selection, the theory fell into disrepute when Weismann argued against Lamarckism. Various forms of species formation competed to explain heredity and the creation of new species. Eventually, with the connection to Mendelism, evolution by natural selection took precedence. Heredity science and mutation theory were left in the wake of this competition, and the new field of genetics was born. Yet some scientific practitioners, like Nathan Fasten, whose careers and socio-political beliefs were tied to the older tradition, resisted adoption of evolution based on natural selection. The one persistent problem enunciated by Jordan, Castle and Fasten was the source of variation. For Jordan and Castle, they adopted natural selection but did not wholly discount a reliance on Lamarckism to produce new varieties. Once the structure of DNA was unlocked in 1953, the intermixing of base pairs could be pointed to as materialist proof for the source of variation. Prior to 1953, Lamarckian questions surfaced regularly within the international, national and local scientific communities that dealt with species development.

Unusual events at the Seventh International Genetical Congress held in Edinburgh, Scotland in August 23-30, 1939 show the strength of the Lamarckian question as well as the confusion regarding the factors that controlled evolution during the 1930s and 1940s. Internationally respected Russian plant geneticist N. I. Vavilov had been selected as the President of the Congress but just prior to the meeting the entire Russian delegation withdrew from participation. In a letter that explained their withdrawal, Vavilov claimed that since a Russian site for the conference had not been chosen the Russian delegation could not attend. However, the heart of the controversy lay with the Soviet interpretation of Darwinism, as well as the imminent threat of war. The Soviet government believed that real Darwinism included a neo-Lamarckian role, but did not include a Mendelian role or the work of T. H. Morgan. According to the Soviets, an interpretation of genetics that included Mendel was inconsistent with dialectical materialism and tainted with Fascism and Nazi race theories. Unfortunately, Vavilov supported Darwinian evolution, the work of Morgan, and the theory that Mendelism proved natural selection. Shortly after the Edinburgh Conference, Vavilov was arrested and died in a labor camp in 1942.[70] The disruption of the conference did not end with the withdrawal of the Russian delegation. Soon after the meeting began, war was declared in Poland, and the German, Hungarian, Scandinavian and Swiss delegations immediately left Scotland. Remaining participants from Britain and America composed "The Geneticists Manifesto" in response to a question posed at a session regarding means by which the world's population could be genetically improved. The document reacted to eugenic claims of race improvement by insisting that betterment of the world's population was as much a social and economic problem as well as a biological one. The Manifesto cited six changes for improvement, the fifth of which was a declaration that the neo-Lamarckian doctrine that the environment directly affected species development was "fallacious." The document was composed and signed mainly by British and American and was not included in the proceedings for the conference. But because the document addressed questions regarding human breeding, it was published in the *Journal of Heredity.*[71]

Deichmann (1996) confirms that Mendel was the basis for Nazi racial hygiene policies, and that agricultural programs that utilized Mendel's breeding techniques played an important role in supporting those policies. Of support for Mendel she wrote, "…the representatives of National Socialism in the party and the state legitimized their racial doctrine with the help of scientific insights of Mendelian genetics…"[72] However, biologists in Germany were not all of one mind. Prior to World War I, many biologists maintained vitalistic "emotions" and eschewed the reliance upon mathematics to prove the biology of Mendel. In the 1920s, many German scientists did not embrace Morgan's work as it represented the crass materialism of the United States. With a few exceptions, genetics as an academic discipline did not reach the universities until the 1930s.[73]

Biology in Germany did not have firmly established theories to buttress the field as physics did, but it did manage to influence research programs. At institutes of applied and agricultural zoology, Deichmann wrote that animal breeding experiments investigated the production of twins. "During the Nazi period, twin research itself was carried out above all to prove the heredity of all essential traits of a person, including intellectual and emotional characteristics; it thus served to provide scientific legitimization for the National Socialist racial doctrine."[74] Heinrich Himmler, the Nazi SS officer in charge of scientific investigations, had an interest in animal breeding and herbal medicines since childhood. He took a degree in agriculture in 1922 and used animal breeding techniques on humans in Nazi concentration camps to carry out experiments that would support his belief in the superiority of the Nordic race.[75] Deichmann notes that much of the research and practical application of racial hygiene was carried out by physicians and jurists, not biologists. However, biologists "played an important role initiating and propagating eugenic and racial-hygiene concepts within the racial hygiene movement."[76] As Kuhn pointed out, groups that had a lot to lose by adopting new scientific theories were reluctant to change their ways. While most international research done within the timeframe of the modern synthesis pointed to the gene as the source for production of new varieties, materialist proof for this supposition was not available. Therefore, looking inward did not hold any stronger sway than the Lamarckian belief that the environment directly affected species fitness. Despite strong statements like The Geneticists Manifesto, belief in the inheritance of acquired characters survived.

Retaining a Lamarckian belief in the inheritance of acquired characters had important consequences in fisheries science during the 1930s and 1940s. If environmental factors could be used to produce heritable improvements in people, then certainly environmental factors like good food and agreeable water temperature in fish hatcheries would produce bigger and stronger fish. For fisheries science, the competition between mutation theory and natural selection was a metaphor for artificial propagation and natural propagation. If one accepts that the application of mutation theory to fisheries management represents artificial propagation in fish hatcheries, and the application of natural selection represents the emerging field of ecology that supported natural propagation, then, in Kuhnian fashion, the two disciplines function as competing entities. However, during the 1930s and 1940s, ecology was a new field that had not gained prominence among the scientific establishment. In a competition between the two at that time, ecology fell short when compared to agriculture's established base of legitimacy.

Ecology

Noted ecologist Aldo Leopold explained the competition in *A Sand County Almanac* (1949), a narrative about the value of wilderness and the emerging field of ecology. After graduating from Yale University, Leopold worked as a forester for the U. S. Forest Service in the early 1900s. He founded the Wilderness Society, is credited as the founder of the field of game management, and wrote one of the first texts on the subject. In *A Sand County Almanac,* Leopold objected to the move towards specialization in science that reduced natural phenomena to separate parts for examination. As an ecologist, Leopold emphasized the importance of interactions between populations, yet he did not associate ecology with evolution.[77] He described Darwin's evolution as a mechanistic science which, when "even bolstered by mutations," could not explain the diversity apparent in the natural world.[78] He disliked wildlife management programs that preferred artificial propagation to natural propagation transforming wildlife managers into production managers.[79] As opposed to agricultural husbandry, Leopold preferred wildlife husbandry where managers allowed flora and fauna to interact naturally.[80] He disdained the "land bureaus, the agricultural colleges and the extension services" that promoted conservation education as a means to an end rather than as a process.[81] He regarded many conservation remedies like flood-control dams and fish hatcheries as superficial.[82] He viewed competition between scientific agriculture and the emerging field of ecology part of the problem in conservation, in which he believed that conservation should be "a state of harmony between men and land." But since agricultural science was actively developed and taught in educational systems before ecology was born, agricultural science remained dominant.[83]

Bowler (1993) wrote that while previous scientists had studied natural processes, the systematic study of ecology, the interrelationships between plants, animals and their physical surroundings, didn't begin until the 1890s. After shaky beginnings, the fields within ecology did not gain legitimacy until the middle of the twentieth century. While scientists in other fields moved to the laboratory to complete scientific experiments, ecologists preferred field studies that were executed in an experimental manner. Darwinism did have an effect on the study of ecology as it showed how fragile natural systems could be, but most ecologists were not Darwinians. Bowler described most ecologists as "still dealing with an essentially static world view,"[84] lacking a consistent theoretical structure. Theories that were based in materialism were just as prevalent as vitalistic works. Most people who began such work were not interested in evolution, but in ways that the natural environment could be modified to allow sustainable exploitation. By the end of the nineteenth century, the U. S. Biological Survey of the U. S. Department of Agriculture mapped the country to determine the areas were plants and animals could be found. Plant and animal ecology progressed within the university system adopting various schools of thought. However, marine ecology evolved separately. Different funding schemes for fisheries research allowed fisheries scientists at the international level to execute studies regarding the health of the oceans before the ecology of terrestrial species gained legitimacy, as Chapter 2 will explain.[85]

Work in the field of plant ecology began with botanists studying the relationships between plant communities, as well as how individual plants adapted to their physical environment. Hagen (1988) cited botanist Henry Cowles' work at the University of Chicago on the theory of plant succession developed at the turn of the century as a unifying factor within the field of ecology. Cowles believed ecology would uncover causal laws that governed development of individual plants and plant communities. Nevertheless, Cowles described the field in 1903 as one in a state of chaos which lacked "fundamental principles or motives." In response, Frederic Clements wished to unify the field by building upon Cowles' work. He proposed a controversial theory that the plant community was a kind of organism that responded by stimulus-response to the physical environment. He emphasized a mechanistic methodology and experimental laboratory work. Other ecologists believed nature was more complex than Clements acknowledged and found his theory and approach too simplistic. Clements also leveled an attack against natural historians for not investigating process and function, a move that failed to promote unification within the field.

Despite the differences, The Ecological Society of America was established in 1915. Hagen noted that the field had close ties with agriculture and forestry, and that ecological study was centered in America's Midwestern universities in Chicago, Nebraska, Minnesota and Illinois where undeveloped land could still be found. Outside of academia, the Carnegie Institution of Washington supported Clements' ecological research in the Rocky Mountains of Colorado. Cowles' theory of succession was also adopted by ecologists interested in animal ecology. Both Cowles and Clements worked with animal ecologist Victor Shelford at the University of Chicago, and Clements and Shelford later wrote the first book showing connections between plants and animals.[86]

The growth of a biological field in the United States which would study animal behavior was hampered by conflicts between amateur and professional scientists as well as a lack of institutional structure in the 1930s and 1940s. Mitman and Burkhardt (1991) noted that the "laboratory became the professional biologists' trademark" early in the century. Researchers Ward Allee and Gladwyn Noble at the University of Chicago Department of Zoology both centered investigation on animal behavior within the laboratory, Allee explaining behavior in ecological terms, and Noble using evolutionary terms. Allee studied the behavior of fishes early in the century with Victor Shelford. The two studied breeding behavior and distributional patterns and suggested that "a certain species of fish would be found in a particular location depending on the carbon dioxide content of the water." By the 1920s, ecological research at Chicago involved individual ecology, the relationship between the individual and the environment usually studied in the laboratory, and community ecology, usually studied in the field. Studying animal behavior in the natural environment became the province of amateur naturalists like Margaret Nice who studied behavioral ecology and ethology in the mid-1930s, as well as by academicians in physiology, psychology and zoology. Because study of animal behavior was split between different disciplines, like psychology, physiology and zoology, the differences in disciplinary training caused friction between professionals. In addition, differences in methodology between amateurs and professionals also inhibited growth of the field. Studies in animal behavior were not unified under the umbrella of a professional organization in the United States until formation of the Animal Behavior Society in 1963.[87]

The Conservation Movement

The conservation movement had many definitions and factors pushing its development. Leopold's vision of ecological conservation contrasted sharply with the conservation program Progressive Era specialists evoked when advising U. S. Presidents Theodore and Franklin Roosevelt. The conservation movement began at the turn of the century as a reaction to the monopolization, exploitation and destruction of the nation's natural resources by private industry. U. S. President Theodore Roosevelt and Gifford Pinchot, Chief Forester of the United States, coined the term "conservation" to promote the full development of natural resources in an efficient and scientific manner which would guarantee maximum productivity as well as perpetuation of the resource. Led by a group of Progressive Era specialists, this movement would enlist scientific practices to manipulate nature and harness the productivity of the nation's natural resources.[88] This explanation has its critics, however. For example, Federal reclamation efforts were bolstered in 1902 by passage of the Newlands Act which would use federal dollars to construct irrigation projects in the west. This measure would encourage settlement by improving and expanding on farmable lands.

While passage of the Newlands Act is often cited as evidence of the strength of Progressive Era conservation, Worster (1985) argued otherwise. He acknowledged that the conservation movement initially was described as a grassroots, democratic movement that worked to defend natural resources. However, Samuel Hays' work in 1959 argued that the conservation movement was peopled by elite professionals who wished to imposed a centralized power structure over natural resource development. Worster indicated that the Newlands Act could not simply be considered a major victory for Progressive Era conservationists but argued that the act provided an escape valve for the nation's under classes in the east, thereby lessening the threat of revolt against centers of wealth. Agricultural expansion in the west would "cure the monopoly of concentrated wealth," and would also perpetuate the value of an agrarian America.[89] Brian Balogh's book *Chain Reaction* (1991) further argues that in the early twentieth century the Progressive Era specialists did not wield as much power in the federal government as previously thought, and scientific expertise was in its infancy within the federal bureaucracy.

One exception to Balogh's argument was the Department of Agriculture that had produced scientific experts within the system of land grant colleges and experiment stations before the Progressive Era.[90] The U.S. Department of Agriculture's (USDA) scientific experts played an important role in disseminating Mendelism to land grant colleges in the early part of the century. The scientific expertise that the USDA had already created through agricultural experiments stations and extension services then disseminated techniques that would make the farm more efficient and more productive, a more scientific system of production based on Mendel's genetics. Paul and Kimmelman (1988) indicated that the breeding techniques Mendelism involved was an applied science and "certainly was science." Researchers at the USDA and agricultural colleges enthusiastically responded to the new procedures that included the "development of genetics within an agricultural context, where breeding, selection techniques, hybridization, and even evolutionary issues...endowed Mendelism in the United States with a strongly practical and popular aspect."[91]

Conclusion

Because of the emphasis of the federal conservation movement and because of the emphasis of the Department of Agriculture, when fisheries science developed within the United States the emphasis fell on artificial propagation based on Mendelian genetics. Implications of the theory of evolution and natural selection were neither developed enough to challenge Mendel, nor were they embraced by ecologists. As the next chapter explains, a history of fisheries science shows that while some basic research was completed, most of the emphasis within fisheries science in the U.S. fell on improving and expanding the fish hatchery system. In keeping with the supply and demand themes of agricultural breeding science, when OSC developed a wildlife management program it was not placed within the College of Science where basic research would uncover life histories and the characteristics of behavior of local fishes. Rather the Fish and Game Management program was placed in the College of Agriculture within the division of animal industries to facilitate the mass production of fish to appease the commercial and sports fishers who were demanding greater numbers of fish in Oregon's lakes and streams.

Chapter 1 Endnotes

[1] Garland Allen, *Thomas Hunt Morgan: The Man and His Science* (Princeton, NJ: Princeton University Press, 1978), pp. 70-80.

[2] Fasten (1935), pp. 17-20.

[3] Bowler (1983), pp. 58-61.

[4] Charles Darwin, *The Origin of Species by Means of Natural Selection* (New York, NY: Penguin Books 1982/1859), pp. 130-172.

[5] Provine (1971), pp. 11-14.

[6] Ibid., pp. 14-24; also Kevles (1985), pp. 3-19.

[7] Ridley, Mark, *The Darwin Reader* (New York, NY: W. W. Norton & Company, 1987), p. 115.

[8] Provine (1971), pp. 66-67; also Bowler (1992), pp. 360-361.

[9] Provine (1971), pp. 92-96; see also Allen (1979), pp. 41-72.

[10] Ibid., pp. 100-105.

[11] Ibid., p. 114.

[12] Ibid., pp. 120-126.

[13] Ibid., pp. 115-120.

[14] Ibid., pp. 122-123.

[15] Ibid., p. 122.

[16] Allen (1978), pp. 97-318.

[17] Bowler (1992), p. 361.

[18] While many of the researchers cited in Provine's book *The Origin of Theoretical Population Genetics* were involved with eugenics, their role and how it affected the development of genetics is not discussed at all.

[19] This is the theme of Daniel Kevles' book *In the Name of Eugenics* (1985), that facets of eugenics, primarily the theme of improvement over nature, can be found in modern genetics research programs. See also Bowler (1992), p. 364, in which he writes that "the experimental approach to the study of heredity led to the creation of the new science of genetics."

[20] Bowler (1992), pp. 360-361.

[21] Barbara Kimmelman, "Organisms and Interests in Scientific Research: R. A. Emerson's Claim for the Unique Contribution of Agricultural Genetics" in Clarke and Fujimura (1992), pp. 192-232, on pp. 204-207.

[22] Ibid., p. 206.

[23] See Provine (1971), pp. 14-24 and Kevles (1985), pp. 3-19.

[24] Peter J. Bowler, *Biology and Social Thought: 1850-1914* (Berkeley, CA: Office of History and Science of Technology, 1993); Paul (1995).

[25] See Bowler (1993), pp. 77-83 and Paul (1995), pp. 40-45.

[26] Kevles (1985), pp. 66-67.

[27] David Starr Jordan and Vernon Lyman Kellogg, *Evolution and Animal Life* (New York, NY: D. Appleton & Co.,1916), p. 68.

[28] Ibid., p. 56.

[29] Ibid., p. 210.

[30] Ibid., p. 202.

[31] Ibid., p. 203.

[32] Castle (1930), p. 44.

[33] Ibid., pp. 330-331.

[34] Ibid., p. 87.

[35] Ibid., pp. 402-403.

[36] Nathan Fasten is not listed in the *American Men of Science* biographical directory.

[37] MCFASTEN, Box I.

[38] CATALOGS, 1935-1936.

[39] Ibid.

[40] MCFASTEN.

[41] Fasten (1935) and Nathan Fasten, *Introduction to Zoology* (Boston, MA: Ginn and Company, 1941). In the 1941 book Fasten's views are abbreviated but essentially unchanged. With respect to variation he does include other forms. However, he still maintained that the only heritable form of variation was a mutation. He did not overtly chose mutation theory over natural selection but expositorily explained both. The effect of the environment was discussed briefly, and he continued to maintain that the environment played a role in evolution. Most interesting though with the 1941 zoology book, in the section discussing types of animals, the first heading for each phylum is "general characteristics", and the second heading for each phylum is "economic importance."

[42] Ida Stamhuis, Onno Meijer and Erik Zevenhuizen, "Hugo de Vries on Heredity, 1889-1903: Statistics, Mendelian Laws, Pangenes, Mutations," *The Journal of the History of Science*, 1999, 6: 238-267. The article shows that de Vries overlooked genetics and clung to mutation theory throughout his career. Like Nathan Fasten, de Vries developed professional reasons for ignoring science that conflicted with closely held theories.

[43] Fasten (1935), p. 252-253.

[44] Ibid., p. 252.

[45] Ibid. Additionally, when Fasten's book was reviewed by the *Journal of Heredity*, the reviewer noted that Fasten had erroneously presented Castle's work; Castle was a member of the editorial board of the journal.

[46] Allen, Garland, "The Transformation of a Science: T. H. Morgan and the Emergence of a New American Biology," in Alexandra Oleson and John Voss, eds., *The Organization of Knowledge in Modern America, 1860-1920* (Baltimore, MD: The Johns Hopkins Univ. Press, 1979), pp.265-288, on p. 276.

[47] Fasten (1935), p. 245.

[48] Ibid., p. 257.

[49] Ibid.

[50] Ibid., p. 25.

[51] Ibid., p. 259.

[52] Ibid., p. 57.

[53] Ibid., pp. 28-31.

[54] Ibid., p. 267.

[55] Ibid., p. 298.

[56] Kimmelman (1992), pp. 204-207.

[57] Ibid., p. 203.

[58] American Breeders Association. *American Breeders Magazine*, 1910, 1:III-IV, 6-11. On pages III and IV, a list of 39 committees meant to address breeding problems for plants and animals was presented and David Starr Jordan headed the Committee on Eugenics; short biographies on Charles Darwin, Gregor Mendel and Amos Cruikshank were included on pages 6-11; and Charles Davenport, leader of the eugenics movement in the United States, wrote two short introductory articles, as well as an article, "The Imperfection of Dominance." Other articles addressed subjects like "Value of Wild Animals," and "New Methods in Plant Breeding."

[59] Kimmelman (1992), p. 206.

[60] Allen (1978), pp. 227-229.

[61] *Genetics*, 1916, 1:pp. i-ii.

[62] *Journal of Heredity,* 1933, 3:2-6.

[63] Ibid., 1934, 11:65-67.

[64] Ibid., 1937, 6:122.

[65] Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago, IL: The Univ. of Chicago Press, 1996/1962).

[66] Allen (1979), p. 72.

[67] Kuhn (1996/1962), p. 176.

[68] Ibid., p. 10.

[69] Ibid., p. 11.

[70] Theodosius Dobzhansky, "N. I. Vavilov, A Martyr of Genetics," *The Journal of Heredity*, 1947, 8: 227-232; see also Julian Huxley, *Soviet Genetics and World Science: Lysenko and the Meaning of Heredity* (London, England: Chatto and Windus, 1949); and Bowler (1993) writes on pp. 513-514 that Lysenko "destroyed Soviet genetics in the name of a Lamarckian programme designed to improve nature."

[71] Unsigned article, "Men and Mice at Edinburgh: Reports from the Genetics Congress," *Journal of Heredity*, 1939, 9:371-374.

[72] Deichmann (1996), p. 83.

[73] Ibid., pp. 80-84.

[74] Ibid., pp. 150-157; coincidentally, in almost every issue of *The Journal of Heredity* in the 1930s an article on twin research appears.

[75] Ibid., pp. 251-258.

[76] Ibid., pp. 321-322.

[77] Leopold (1949), pp. 161-162.

[78] Ibid., pp. 230-231, see also Bowler, pg. 515.

[79] Ibid., pp. 258-259.

[80] Ibid., pp. 218-219.

[81] Ibid., pp. 250-251.

[82] Ibid., p. 274.

[83] Ibid., p. 260.

[84] Bowler (1993), p. 363.

[85] Bowler (1993), pp. 361-369.

[86] Hagen (1988), p. 260; see also Bowler, pp. 521-527.

[87] Mitman and Burkhardt (1991), pp. 164-187see also Bowler, pp. 527-534.

[88] Robbins (1997), p. 7.

[89] Donald Worster, *Rivers of Empire: Water, Aridity, and the Growth of the American West,* (New York, NY: Oxford University Press, 1985), pp. 161-169.

[90] Brian Balogh, *Chain Reaction: Expert debate and public participation in American commercial nuclear power, 1945-1975,* (New York, NY: Cambridge Univ. Press, 1991), pp. 1-15.

[91] Paul and Kimmelman (1988), pp. 281-302.

Chapter 2 - Fisheries Science and Fish Culture

Nature offered unexplained cyclical fluctuations of fish runs while fisheries managers preferred uniform, predictable runs which provide a controlled harvest for commercial and sports fishers. Fish defy those who wish to study them because they reside in a medium in which the researchers do not. Water inhibits classification and understanding aquatic life histories as fish are difficult to observe, and water makes them even more difficult to count. These problems are exacerbated by anadromous fish, like salmon, because they migrate. Until the early part of the twentieth century the entire process of migration was a mystery. Salmon leave freshwater in one form, as a newly hatched smolt or fingerling, and return as an adult ready to spawn. Where they went and how they got there were unknown. While some researchers were beginning to investigate and theorize about fluctuations in catch from the mid-nineteenth century, description and classification of the world's fishes was the predominant function for those naturalists and fisheries biologists interested in aquatic life throughout the nineteenth and the beginning of the twentieth century. This chapter will discuss salmon life histories, the basic research of fisheries science, and the importance and unquestioned reliance on fish culture.

Salmon Life Histories

Netboy (1980) wrote that Pacific salmon were believed to be descendant from Atlantic salmon that crossed the Bering Strait when it was underwater. However, Lichatowich (1999) wrote that the Salmonidae may have split from teleost fishes as early as 100 million years ago.[1] Five species of Pacific salmon reside in the Northwest, chinook, coho, sockeye, chum, pink, including two trout groups, steelhead and cutthroat trout. Each species includes many subspecies that have adapted to different aquatic environments and have different biological characteristics. Without understanding a salmon's life history as well as the various subspecies, classification would have been difficult as many of the factors assigned to a species can be contradicted by a subspecies.[2]

Most salmon are anadromous, which means that they are born in freshwater, migrate to the ocean and return to freshwater to spawn. During spawning, the female gouges out a nest in a bed of gravel, lays hundred to thousands of eggs which the male then fertilizes. The eggs develop and fry emerge and live in freshwater from two weeks to two or more years depending on the species. Sockeye usually spend their freshwater period in lakes, but in Alaska and Canada, they may migrate to sea shortly after hatching. Juvenile coho spend their freshwater life history in small streams and tributaries, except in southwestern Oregon, where coho spend their juvenile period in lakes. Pink and chum juveniles migrate to sea after a brief period in freshwater. The life history variation within species guarantees that the entire group could not be exterminated by one ecological event.[3]

Migration enables salmon to take advantage of the ocean's greater forage areas for food, mostly small fish and zooplankton. Salmon may spend one to five years in the ocean where they may travel thousands of miles or they may stay close to shore. For example, some groups of Pacific coastal cutthroat only migrate to estuaries or to the river mainstem and some don't migrate at all. Both Atlantic and Pacific salmon generally return to their freshwater natal streams to reproduce and after spawning; most Pacific salmon die afterwards whereas Atlantic salmon do not. However, some groups of Pacific steelhead and cutthroat do not die after spawning either. After leaving the Columbia River or coastal streams, most salmon head towards British Columbia and Alaska while some migrate to waters off Oregon and Northern California.[4]

Each species has a different life cycle broken into the amount of time spent in freshwater, and the amount of time spent in the ocean. A Chinook spends a few days to two years in freshwater after birth and one to five years in the ocean, whereas the sockeye will spend a few days to three years in freshwater after birth, and spend a half year to four years in the ocean. Chinook have an average mature weight of twenty two pounds and length of thirty six inches, whereas the sockeye has an average mature weight of six to eight pounds and a length of twenty three inches. Pink, chum and sockeye are mostly found in the waters of British Columbia and Alaska, while Chinook, coho and steelhead and sea-run cutthroat are mostly found in the waters of Washington, Oregon, Idaho and northern California.[5]

Chinook has historically been the most valuable commercial fish due to its size and fat content, whereas steelhead and cutthroat are the most valuable sports fish. In older literature, each species may be referred to by different names. For example, Chinook are also called king salmon, Quinnat and Tyee, whereas sockeye may also be called red or blueback. Names may seem contradictory, but often refer to physical characteristics at different times in a species' life cycle. Once salmon start their return trip to their natal stream to spawn their physical appearance changes. Therefore, salmon caught heading upriver to spawn may look markedly different than the same species caught in the ocean.[6]

Fisheries Science

Intensive fishing often led by a technological innovation caused declines in fisheries and led to competition between economic interests internationally as well in Oregon. However, the boom and bust cycles of fisheries were also caused by natural disasters and natural cycles that were not understood by fisheries managers.[7] The fluctuations inherent in fisheries pushed many countries to fund fisheries research in an effort to stop the declines. Most of the investigations were for economic purposes and did not include long range scientific strategies and goals beyond aiding the fishing economies. In *Scaling Fisheries: The Science of Measuring the Effects of Fishing, 1855-1955* (1994), author Tim Smith described the researchers and the affiliated professional organizations in each country, the development of biological and mathematical experimental methods and attempts at theoretical explanations used to study the world's fisheries. The first scientific investigations of fisheries were in Russia between 1850-60, the U. S. Fish Commission was started in 1871, and the Fishery Board of Scotland and the Marine Biological Association in England took form in the 1880s. An international organization for exploration of the oceans in the northwest Atlantic, International Council for the Exploration of the Seas (ICES) was formed in 1890. By the 1930s theories regarding the dynamics of fish populations using techniques from population ecology were developed, though Smith indicated they were incomplete.[8]

Smith (1994) wrote that the development of fishery science was related to the fields of oceanography and ecology, but that fisheries work was different than the other two fields because of its applied emphasis. "One difference between the three areas of study (oceanography, ecology and fisheries) is that while the first two were often addressed as pure science, the last has almost always has been pursued in the context of very strong economic and political interests." Histories on the development of oceanography that touched briefly on fisheries noted that fisheries study was peripheral to understanding the working of the oceans and its production of plankton. Smith acknowledged Sharon Kingsland's work on population ecology, noting that there had been some overlap in the areas of statistics, actuarial science and mathematical modeling, and overlap in the use of some concepts like ecological equilibrium and density-dependent populations, but that the fields of fisheries and population ecology were far from united.[9]

Despite the parallel development in time of the closely related fields of the oceanographic histories, the population ecology history and Smith's book on fisheries, they presented different views of disciplinary development.

> That the views taken within these books are distinct indicates a divergence in methods and concepts between these fields that has not been overcome, a divergence that may have enhanced our understanding in some areas but has undoubtedly impeded progress in understanding the fluctuations in fisheries.[10]

Smith noted that while fishing was a commercially important industry to many countries in the world, the study of the effects of fishing has not had a scientifically coherent theoretical development. While fisheries investigations had been conducted for many years in different academic disciplines, the naming of the field of "fishery science" was created as an administrative tool to fulfill political needs and the terms was picked up in the United States in the 1920s for political reasons.[11] The U. S. Commissioner of Fisheries created a new branch of study called fishery science which would address the study of commercially valuable marine animals, adopting appropriate scientific methods. However, overall, Smith indicated that the field did not have a set of shared questions and methodologies that a scientific discipline normally adopts because its development as a bureaucratic structure was unique. In 1934, Elmer Higgins, a member of the Commission scientific staff defined fishery biology as a field that included artificial propagation, "that body of organized knowledge regarding the natural supply of fishes commercially exploited, the variations in supply and their causes, and the ways and means of husbanding the fishery resources."[12]

Fluctuations in catches were problems addressed by countries all over the world in the late 1800s and early 1900s and was attributed to migration, predation, pollution, over fishing and ocean conditions. Norwegians studied variations in catches for cod and herring and believed migration patterns affected fish numbers. French fishermen believed that predation by dolphins and porpoises affected availability of sardines. And a Canadian and American study of Fraser River sockeye salmon cited overfishing and a river blockage in 1917 as the reason for varying numbers noting, however, that salmon catches varied but with a predictable pattern.[13]

The Canadian and American study is significant because it described the beginning of a scientific tradition in fisheries science influenced by faculty and students from Stanford University starting with David Starr Jordan, Charles Gilbert, Willis Rich, and Paul Needham. While Jordan was recognized as the most influential fisheries expert until well after his death, the direct Oregon connection to this tradition begins with Willis Rich who took a position with the Oregon Fish Commission as Director of Research in 1937, followed by Paul Needham who became Director of Fisheries for the Oregon Game Commission in 1945.

Charles Gilbert and the Fraser River Study

David Starr Jordan, one of the preeminent ichthyologists in the United States in the late nineteenth and early twentieth centuries, wrote many books and articles on the classification of fishes of Asia and the Americas. His 1892 book, *Salmon and Trout of the Pacific Coast* was a often cited work that included life history information as well as classification. While Jordan's views on salmon migration and homing instincts were considered the definitive word by fisheries managers, they were challenged by his students and colleagues. Before Jordan became president of Stanford College in the late 1880s, he had already established a connection with Charles Gilbert, a student and later a colleague interested in salmon research. When Gilbert followed Jordan to Stanford, Smith wrote that they would produce many of the fisheries biologists prominent in the first half of the twentieth century.

In 1911 the Department of Fisheries in British Columbia began to finance a long term study of Gilbert's on Fraser River sockeye salmon. Gilbert and his students worked to substantiate fishermen's observation on two points: whether or not salmon returned to their natal stream to spawn, known as the home stream theory which Smith dated to 1903[14], and whether or not subspecies could be distinguished by eye from each other. Gilbert's Fraser River sockeye study applied techniques that Smith indicated had been developed during earlier international fisheries research programs.

Many of Gilbert's findings served to support the home stream theory. He used fish scales to determine the age of fish and noted that fish with specific types of scales returned to specific sites. Observation of streams showed that fish returned at specific times during the spawning season each year thereby defining specific runs, like a spring chinook run as opposed to a fall chinook run. He also showed that sockeye lived four years, explaining the fluctuation in catches each four years. In addition, he showed that each yearly run within the four year cycle was a genetically-separate breeding stock that returned to different tributaries of the Fraser. Finally, Gilbert's study showed that the blockage of the Fraser by debris from the construction of the Canadian Northern Railway prevented salmon from migrating and caused the collapse of the fishery in 1917. From this incident Gilbert emphasized the importance of escapement: those fish that escape from either the fishermen's net or from blockages in the river, will be the salmon that will spawn in their freshwater natal streams. Without sufficient escapement salmon runs collapse.[15]

At the time of the study, Jordan did not believe in the home stream theory as he did not believe that salmon had a homing instinct; he also did not believe that salmon ranged widely when in the ocean. In discussing evolution, instincts had been one of the hereditary factors which Jordan knew existed, but for which he could not provide a satisfactory materialist explanation. Lamarckians believed instincts were proof of the inheritance of acquired characters, whereas selectionists believed instincts were the result of natural selection working on endless variations. In either case, Jordan thought proof for instincts should be based upon demonstration, not logic. While Jordan was not aware of salmon's wide-ranging migration patterns, he had noted, however, the prevalence of subspecies among fishes. In his 1916 book *Evolution and Animal Life,* Jordan described subspecies as groups within a species that have superficial dissimilarities, such as size or color. "For within any species there may be several subspecies or varieties, the individuals of all of which are capable of fertile mating with each other."[16] He noted there may be subspecies among brook trout indicating that the group adapted easily. "In no group of vertebrates are the life characters more plastic than among trout."[17] Jordan believed that when salmon migrated to the ocean, they did not travel far but stayed near the mouth of their natal stream. When spawning season commenced, the salmon then moved into the stream closest to them which happened to be their natal stream. Smith indicated that Jordan referred to this as the *modern theory* of migration and held to that theory as late as 1925, even though, by that point, many other fisheries experts had opinions that differed with Jordan.[18]

Fish migration was the topic of a 1924 article in the *British Journal of Experimental Biology* titled "A Critical Examination of the Evidence for Physical and Chemical Influences on Fish Migration," by F. E. Chidester. Chidester was a Professor of Zoology at University of West Virginia, and also was affiliated with the Woods Hole Laboratory of the U. S. Bureau of Fisheries. Chidester surveyed the literature on the topic and cited numerous studies from around the world indicating theories regarding fish migration; while anadromous fish are featured, freshwater species are discussed as well. References date between 1866-1923. Gilbert's sockeye study was preeminent on the question of parent streams, noting that Jordan disputed Gilbert's home stream theory. Other physical factors theorized by different investigators to influence migration included the character of the river bottom, water pressure of a stream, water temperature and salinity, the amount of oxygen in the water, and the polluted state of water. Internal influences specific to the fish included sight, smell, tactile and kinesthetic senses and hearing, size of the body of the fish, fat content and chemical senses. Chidester's conclusions then were not definitive but included the many different influential factors that may affect fish migration. Within the discussion were vague references to the role of natural selection, the fish's ability to adapt, the apparent races and subspecies of fish especially within anadromous groups.[19]

Similar tagging studies on salmon migration in the Northwest were conducted throughout the 1920s and the early 1930s by Gilbert and his student and colleague Willis Rich, and later by Rich and E. D. Ball, and Rich and Harlan Holmes.[20] These studies showed that once salmon were in the ocean they migrated as far as Alaska before returning to spawn in their natal stream. The data from these studies supported Rich's paper, "Local Populations and Migration in Relation to the Conservation of Pacific Salmon in the Western States and Alaska," which he presented at the 1939 American Association for the Advancement of Science meeting. By 1939 Willis Rich had received education and practical experience in fisheries. He worked for the California Fish and Game Commission as a fish culturist from 1910-1913, received a Ph.D. in Zoology from Stanford University in 1918 and started working with the U. S. Bureau of Fisheries the same year. He served with the Bureau as the Chief Investigator of Salmon Fisheries from 1926-1930. In 1930 he became a Professor of Biology at Stanford University, achieving emeritus status in 1950. While with Stanford he served concurrently as Director of Research for the Oregon Fish Commission from 1937-1943. He also worked with the U. S. Department of the Interior in 1943 and in 1944 worked with the U. S. Fish and Wildlife Service in charge of North Pacific fisheries investigations. Rich gained international fisheries experience in Japan, India, Indonesia and Taiwan as a technical adviser, and consulted with the U. S. Bureau of Reclamation from 1938-1940, and with the Army Corp of Engineers in 1953.[21]

At the 1939 meeting Rich argued that to conserve salmon, wildlife managers must understand salmon migration routes and accept the home stream theory, that salmon return to their natal streams to spawn after migration. Rich wrote that the five species of Pacific Northwest salmon could be broken into local subpopulations. He indicated that salmon conservation measures must afford protection at the subpopulation level to protect salmon at the species level. The study has been cited as showing that maintaining genetic differentiation of salmon stocks was essential to maintain salmon species.[22] In this paper he left hints in his definition of a subpopulation that indicated questions, like Fasten's, about the source of variation and species formation.

> By population I mean an effectively isolated, self-perpetuating group of organisms of the same species regardless of whether they may or may not display distinguishing characters, and if present, be genetic or environmental in origin . . . These population groups have commonly been termed by biologists on the Pacific coast "races," entirely without any implication that the groups show demonstrable and heritable differences.[23]

In this paper, Rich made an argument for salmon's homing instincts. The first quoted phrase was a phenotype/genotype argument. Rich attempted to show that like does not always produce like. An individual of a subspecies may look like another individual of another subspecies but may be different genetically. In the second phrase Rich indicated that regardless of the scientific process that had produced the "races" of the salmon, five have been identified on the Pacific Coast. Therefore, salmon purposefully return to their natal streams to mate with other individuals from that stream. However, most of the research on salmon migration routes and homing instincts were supported by tagging studies executed since 1911. These tagging studies depended on the voluntary efforts of commercial and sports fishers to report the numbers of fish they had caught that were tagged. The thoroughness of the voluntary efforts was questioned and these studies were criticized as not adhering to rigorous scientific standards.[24]

Tagging Studies Questioned

Similar questions were raised at another conference in 1939 that Rich attended, "Dams and the Problems of Migratory Fishes," held at Stanford University. These comments referred to tagging studies of downstream salmon migrants through Bonneville Dam. In addition, introductory remarks by Francis B. Sumner of the Scripps Institution of Oceanography, indicated that the theoretical meaning of Rich's home stream theory were not understood.

> Their return as sexually mature adults to spawn and die at the scene of their nativity-in the same river, and usually in the same tributary in which they were hatched, or were planted as fingerlings-now appears to be an established fact, though the theoretical implications of all this are still rather bewildering.[25]

Comments from the presenters at this symposium indicate that participants, including Willis Rich, did not understand the theoretical implications as they included artificial propagation as a means of ameliorating the detrimental effects of dam construction. If they believed that fish hatcheries were not a viable solution based on genetics, then inclusion of the practice would not have been so wholly accepted. The symposium included five presentations followed by a resolution regarding water resource development projects. Of the five presentations, four discussed fish hatcheries as a part of the fish conservation program to mitigate the effects of dams on fish habitat. Harlan Holmes, of the U. S. Bureau of Fisheries, presented a paper on the development of fishways at Bonneville, "The Passage of Fish at Bonneville Dam," and did not touch on fish hatcheries, even though fish hatcheries had been a part of the package. Rich discussed problems caused by various water development projects but focused on the Bonneville dam in his speech, "Fisheries Problems Raised by the Development of Water Resources." As Director of Research for the Oregon Fish Commission, a position he held concurrently with a position as a professor of Biology at Stanford, managing commercial fisheries on the Columbia River were his charge. He indicated that because problems seemed to be solved at Bonneville, one should not assume water development projects were without dangers. He referred to a series of dams proposed for the Willamette River, the Willamette Valley Project, which would further inhibit salmon migrations. He described tagging experiments on downstream migrants, indicating that some of the marking techniques used in 1938 had not proven to be successful and the experiment was being repeated in 1939.

Other tagging experiments were planned by the U. S. Bureau of Fisheries for 1940 and 1941 which would test the planting of fish above and below Bonneville. Measures Rich recommended to ameliorate the effects of the dam on salmon habitat included fishways, transferring fish to suitable streams or hauling fish around dams when fishways are not an option, as they were not for Grand Coulee, coupled with artificial propagation.[26] Wilbert McLeod Chapman, of the Washington Department of Fisheries, discussed the problems associated with Grand Coulee in his presentation, "Fish Problems Connected with Grand Coulee." Because the dam rose to 350 above the mean normal river level, the construction of fish ladders and lifts, estimated to cost $2,000,000, were deemed too expensive and projected unworkable regardless. With the completion of the dam, 1,140 miles of salmon habitat on the Columbia River were completely blocked.

> In this 1,140 miles of main stream and tributary, the spring chinook, the Columbia River sockeye (or blueback) salmon, and the steelhead trout have ascended yearly since time immemorial. In this vast area, the young salmon and trout have hatched and fed during their period of freshwater growth. This they will never do again by their own initiative. Today the Grand Coulee Dam has been completed to a height insurmountable to fish.[27]

To counteract the blockage, runs would be transplanted and supplemented by increased hatchery facilities. "This plan has been adopted after a thorough investigation of several alternatives, not because it is an ideal method, but because with limited information available it appeared to have the most likelihood of success."[28] Despite Chapman's reservations, he hoped that the increased hatch rate of artificially propagated salmon and the protection afforded the fish reared for three months would compensate for other adverse factors caused by the dam. "It is hoped that the lessened mortality during this period of the fishes' lives over what would take place in the natural state will counteract the possible increased mortality rates at other points in the plan."[29]

Harry Hanson from the U. S. Bureau of Reclamation presented the paper, "Preliminary Report on an Investigation to Determine Possible Methods of Salvaging the Sacramento River Salmon and Steelhead Trout at Shasta Dam." This dam was the part of the Central Valley Project for Northern California and would provide water for navigation, irrigation, flood control and power generation. The dam would be built to the height of 560 feet and would cut off salmon spawning areas to the Sacramento, Pit and McCloud rivers and their tributaries. Fishways were considered unworkable and too expensive and even artificial propagation looked doubtful after fish counts at the dam site determined the number of eggs each female salmon would produce. "If about 100,000,000 eggs may be expected from the run which now goes past the site of Shasta Dam, and if salvage operations are to depend upon artificial propagation alone, the number of eggs to be handled would be greater than that which is now produced by all the federal salmon hatcheries."[30] He discussed transferring runs, natural propagation efforts and artificial propagation concluding that studies had not been completed which would permit final decision making. "In summary, any plan proposed must be in accord with the habits and instincts of the fish concerned, and be practicable on a long-range economic, as well as a fish cultural basis, in order that the present run of migratory fishes in the upper Sacramento Basin be preserved, or even enlarged, for the present as well as future generations."[31]

A Balance of Water Development and Fisheries

In most of the presentations at the 1939 conference, the politics of water resource development was as much a discussion topic as was biological science. For the final presenter, political considerations constituted the major emphasis. A. C. Taft, Chief of the Bureau Of Fish Conservation for the California State Division of Fish and Game gave the presentation, "A Summary of the Status of Dams versus Migratory Fishes on the Pacific Coast with Especial Reference to Problems in California." He reviewed problems that had been faced, solutions that had been accepted, the present state of laws that protect wildlife, and ways to affect better protection for fisheries. "From the foregoing, it can be seen that the fisheries biologists have not shirked their responsibilities, nor have they been over-awed by the magnitude of the problems presented. However, they have been handicapped in various ways in attempting to make the necessary adjustments for the protection of the fishes."[32]

He addressed the issue of pitting the economic value of fish against the economic value of water development projects, a lack of consultation between planners and fisheries managers, the inadequacy of present laws to enforce fishway regulations, and a lack of funding for fishway projects. He reviewed the process followed by California Fish and Game when evaluating an application for a dam in the state. If the Division found that a fishway was needed, construction could not commence without fishways plans. If a fishway was not appropriate, then a fish hatchery would substitute. "If a fishway is not practicable, the Division may require the construction of a hatchery with its attendant facilities, which is thereafter operated at the expense of the division."[33] Taft charged that the Wildlife Conservation Act of 1934 which charged that the Federal government or a private agency constructing a dam must consult with the Bureau of Fisheries before construction and "when economically practicable" adequate provision for fish migration must be made, needed revision.[34]

After the presentations, a resolution which would give power to fisheries interests when water power development was planned was proposed by Dr. G. S. Myers and seconded by Dr. Paul R. Needham. During discussion of the resolution, varying opinions on fisheries decline were presented. Dr. L. E. Griffin remarked that over fishing may be more detrimental to fish runs than dams. "I have little sympathy with the construction of the dams across the Columbia River; nevertheless, I feel that we may ascribe more blame to Bonneville Dam for the reduction of future salmon runs than may be due it." Further, he questioned the statistical value of Rich's tagging studies in determining the dam's effect on downstream migrants. Rich argued that the experiments had been specifically designed to test for downstream migrants, and the experiment's success was a function of probability, not numbers of salmon returned.[35] Other comments supported Rich's tagging statistics and also denounced over fishing. Dr. W. M. Chapman noted that different runs of fish experienced different numbers of escapement, and that for the spring chinook run, eight out of ten fish were removed by the commercial fishery.[36] Rich supported Chapman's statement, and Chapman continued to show that fishermen recognized that spring and summer chinook were more valuable than fall runs. Therefore, the spring and summer runs were in jeopardy, but the fall runs were not.[37]

Discussion then turned to regulation of water development. Chapman noted that private utilities and private users of water could be regulated locally, but that municipalities and divisions of the federal government were not covered by state laws.[38] Based on the experience of the Bonneville and Grand Coulee Dams on the Columbia River in Oregon and Washington, the Shasta Dam on the upper Sacramento River in California, and other federal and state water development projects, the group unanimously passed a resolution asking that fisheries interests be granted the same consideration as other users of water.

> BE IT RESOLVED: that fisheries resources be given consideration equivalent to that given all other water uses by federal and state planning or construction agencies and that before starting construction of any dam or other type of structure proposed in any basin containing salmon or steelhead trout, surveys of the fisheries resources be made by qualified experts, to parallel engineering surveys, over a minimum period of five years, or sufficient to cover the life cycles of all economically important fishes concerned.[39]

The lack of information about fisheries permeated all discussions about dams and their relationship to fisheries.

Fish Culture

A fisheries science that focused on fish culture did serve to develop research that promoted fisheries as an agricultural product and fish hatchery innovations were the result of that scientific inquiry. However, the emphasis on the agricultural intentions impeded the development of other aspects of fisheries research, exacerbated by the fact that agricultural breeding methods have produced fish no longer fit for the wild. Like Smith, other recent historical accounts of fisheries also stress the emphasis that fish culture as an applied science enjoyed in the United States.

Netboy (1980) traced the breeding of fish from ancient to modern times. Evidence of artificial propagation of many fish species dated back to 2000 B.C. in China and also to the Romans. In the fourteenth century an Italian monk who described the practice as "fertilizing fish eggs and hatching them by burying them in sand or wooden boxes."[40] An eighteenth century Italian landowner Stephan Ludwig Jacobi's description patterned after the monk's experiment was more explicit. "He spread the fertilized eggs over a bed of gravel at the bottom of oak boxes, permitted water to flow over them, and in due time saw them incubate and emerge as fry which he released in a stream...Jacobi's method is essentially the basis of artificial salmonid propagation to this day."[41] Jacobi published his experiment in 1765 and sent it to French naturalist Buffon who had it published in France. British attempts to replenish Atlantic salmon stocks began in 1838 with salmon hatcheries and by the end of the century Scotland had eighteen salmon hatcheries. The first salmon hatchery in North America was built in Canada 1866, and in 1871 the hatchery on the Penobscot River in Orland, Maine was built and was still in use in 1980.

Taylor (1996) wrote that the U.S. Fish Commission was established in 1872 to propagate food fishes, and Spencer Fullerton Baird was chosen to direct the effort. Rather than instituting unpopular regulations on commercial fishers, he chose to instead to supplement over fished runs with artificial propagation. He enlisted the help of Livingston Stone to travel to California to start a salmon hatchery on the McCloud River in Northern California. Enthusiasm for artificial propagation was passed along with little objective study to substantiate the claims of success beyond anecdotal evidence.[42]

For example, Seth Green, reputed to be the father of fish culture in the United States,[43] was Superintendent of Fisheries in New York State when he co-authored the book *Fish Hatching and Fish Catching* in 1879 with R. Barnwell Roosevelt, Commissioner of Fisheries in New York. In this book Green and Roosevelt enthusiastically promoted the benefits of fish culture indicating that the process was unbelievably successful.[44] The most often read claim is that nature wastes most of the eggs salmon produce and that artificial propagation was much more efficient. In the wild, one percent of salmon eggs survive to hatch fry, whereas artificial propagation hatches ninety eight percent. For this reason, the authors declared that fish culture could succeed where nature was often defeated. Fish culture could be practiced on any species of fish, it would be more profitable than any form of agriculture or ranching, it would furnish an abundant source of protein-rich yet inexpensive food, and every section of the country could successfully practice fish culture with a minimum investment and a minimum of governmental regulation.[45] R. D. Hume took up the cause of salmon propagation in Oregon as an important step in saving over fished runs in his 1893 pamphlet *Salmon of the Pacific Coast*. Hume described salmon hatchery installations and methods of successful propagation and admonished canners who had not become involved in artificial propagation of salmon as causing their demise in Oregon.[46]

Bottom (1997) wrote of fish culture as an extension of agriculture goals indicating that threatened fishery resources in New England during the nineteenth century coupled with an expanding populace led to the development of fish hatchery technology that would provide income for farmers and also increase the food supply. He noted, like Smith, that the fields of ecology and fisheries science were separate disciplines with little overlap.[47] For fish culture to be a successful agricultural operation, Bottom wrote that those practicing artificial propagation believed that wild fish had to be tamed.[48] Fish culturists bred for desirable qualities and attempted hybridization experiments to improve species. "Whereas many fisheries scientists today ask questions about the effects of hatchery selection on the gene pools of native fish populations, for most of its history, the primary purpose of fish culture was selection. Domestication meant tame and improved."[49] The best improvement was the hatch rate of hatchery fish over wild. Some fish culturists preferred eggs that had been naturally fertilized as they assumed that fish would choose the mate best adapted to them. However, more efficient methods soon eclipsed natural propagation. A new method of fertilization called dry fecundation first used in the 1850s by a Swiss researcher involved taking the eggs from the female and the milt from the male and effecting fertilization. This method proved to be a major advance for fish culture as it greatly increased the number of eggs fertilized, and also gave complete control of the process to the fish culturist.[50]

This process was best utilized at a modern hatchery where scientific management principles were employed. In 1909 Oregon's Central Hatchery at Bonneville on the Columbia River was constructed, on the assumption that local hatcheries which would have been too expensive to operate.

> With a rearing capacity of 60 million eggs, the new hatchery sought to maximize production efficiency through economies of scale and centralization of the rearing process...(the) Central Hatchery epitomized the 20th-century shift toward an industrial model of fish propagation based on the mass production of a factory product.[51]

From the Central Hatchery, salmon eggs were shipped all over the state, across the nation and throughout the world. Bottom called this process of shipping fish eggs to any geographic location an experiment in acclimatization. The purpose of Livingston Stone's trip to California to open a salmon hatchery was predicated on the belief that west coast Chinook eggs would replenish the dwindling salmon run of the east coast. "Fish culture enabled the easy transport of large numbers of fish for introduction into new waters throughout the world. The term acclimatization was applied to fish introductions in the same way it was applied to describe the transfer of new varieties of domesticated plants from distant lands."[52] As Oregon's population increased and fisheries habitat declined reliance on fish culture grew. Bottom indicated that fisheries managers were following the laws of supply and demand in that increased hatchery production followed the demands of Oregon's anglers.

Hatchery Detractors Ignored

Not all fisheries managers were sanguine about increased reliance on hatchery production by a few fisheries managers. In 1903 Oregon Fish Warden H. G. Van Dusen cautioned that artificial propagation could not completely substitute for natural propagation, Elmer Higgins of the U. S. Bureau of Fisheries noted in 1927 that the popularity of fish culture had been accompanied by a decline in United States fisheries, including the Pacific salmon, and J. N. Cobb of the U. S. Bureau of Fisheries in 1930 indicated that the unquestioned faith in fish culture was not supported by scientific evidence. Attendant science to facilitate hatchery expansion included increased data collection, knowledge of salmon life histories and ocean and spawning migrations, tagging and scale studies, as well as increased regulations (managing on a maximum sustained yield basis did not begin until 1950).

New information about salmon began to change the ideas in fishery conservation. By 1917 the Oregon Fish and Game Commission accepted the "parent stream theory" which then led to the Oregon Fish and Game Commission questioning the wisdom of central hatcheries as opposed to hatcheries located on individual streams. Acceptance of the parent stream theory did not lead to the rejection of fish hatcheries completely, but to a rejection of central hatcheries in lieu of hatcheries on local streams which would more closely imitate nature in perpetuating subspecies within a species. Willis Rich's and H. B. Holmes' papers on tagging experiments on Columbia River salmon from 1916-1927, and Rich's 1939 paper provided further proof of the importance of maintaining subspecies to protect salmon at the species level.[53]

Taylor (1996) and Lichatowich (1999) acknowledged that biologists began discounting the efforts of fish culturists in the early 1900s. Various studies indicated that artificially hatched salmon eggs had a low survival rate. The results of a systematic study of the efficacy of fish hatcheries in Canada completed in 1934 by R. E. Foerster found that while artificial reproduction had a higher hatch rate than natural reproduction, few hatchery fish actually returned to their natal streams to reproduce. The survival rate of the hatchery fish was not significantly different than the survival rate of wild fish, therefore, the growing cost of fisheries supplementation by artificial propagation was debated. The Biological Board of Canada could not defend fish hatchery expenditures and began terminating Canada's hatchery program in 1934, and by 1936 all fish hatcheries in Canada were closed.[54]

To understand why fish culture was abandoned in Canada, yet not in Oregon and Washington, Taylor pointed to bureaucratic and hierarchical differences in Canada where control of fisheries was centralized, as opposed to the many faceted and fractured state control of fisheries in Oregon and Washington. He also referred to a schism between biologists and fish culturists, begun when biologists began to find fault with fish hatcheries and fish culturists countered that laboratory biologists lacked the practical experience needed to evaluate the hatchery system. Finally, Taylor wrote that Oregon and Washington lacked the financial backing to support long term research programs. Lichatowich explained that Foerster's study had focused on sockeye, while the Columbia River was populated by chinook salmon. Since the life histories of each was very different their performance in hatcheries may be different. A study by Rich (1922) on Columbia River chinook also indicated that hatcheries return rate was not significantly better than natural propagation. Both Lichatowich (1999) and Taylor wrote that despite the warnings against reliance on fish culture, despite acknowledgment of subspecies as important units of conservation, despite the closing of fish culture facilities in Canada, the state of Oregon continued to pursue and expand state and federal fish culture facilities as a means to improve fish production.[55] One reason for this, with which we conclude this chapter, is the Lamarckian-influenced science of wildlife and fisheries scientists trained at Oregon State College.

The OSC Department of Fish and Game Management

Research problems regarding fish hatcheries in Oregon were often directed to wildlife experts at Oregon State College from the 1920s-1940s. In 1935 the Oregon Game Commission ran sixteen fish hatcheries. When the Game Commission had fisheries problems they could not solve, they sent samples to labs on the East coast, as no federally funded labs were located on the West coast. Before the Game Commission opted for East coast labs, they first consulted wildlife experts at Oregon State College, the state's land grant college located in Corvallis. Since 1868 the institution contributed to the production and dissemination of scientific knowledge throughout the state from various institutional forums: the College of Science, professional schools specializing in academic disciplines like agriculture and engineering, research efforts of the Agricultural Experiment Station, and the outreach efforts of the institution's Extension Service.[56] Because state and federal wildlife agencies expressed unquestioned support for fish hatchery programs, and because state and federal agencies were involved in the development process, when the college developed a fisheries and wildlife program it also emphasized fish culture. The department's development was also affected by its placement within the School of Agriculture, signaling an emphasis on production science as opposed to basic science.

Not surprisingly, since the 1920s the Oregon State Game Commission, in addition to other public and private groups, pushed to develop a college-based program that would train wildlife workers, especially fish hatchery technicians.[57] In 1935 the Department of Fish, Game and Fur Management (the name changed in 1936 to the Department of Fish and Game Management) was opened within the OSC School of Agriculture under the Division of Animal Industries. The department was funded by federal dollars earmarked for agricultural recovery, and a joint funding scheme was proposed for a wildlife research unit to complement the department. This combination of a wildlife department with a jointly funded research unit was one of eight such alliances similarly situated and funded at land grant institutions throughout the United States.[58] The research unit at OSC, called the Oregon Cooperative Wildlife Research Unit (OCWRU), was jointly funded by the College, the Oregon Game Commission, the U. S. Biological Survey, and the American Wildlife Federation; it was directed by Arthur Einarsen of the U. S. Biological Survey. The combination of jointly funded research between OSC, the Game Commission and the U. S. Biological Survey had a long history, but previously each project had been individually negotiated and pursued as need arose.[59] In 1935 the institutionalizing of joint funding for research served to solidify a standing tradition between the groups in which institutional research was directed by the needs of outside organizations. In a 1945 evaluation of the OCWRU, Einarsen listed twenty nine research projects conducted between 1935-1945. Of these twenty nine projects, six dealt with fish and shellfish, and three of those six represented research related to fish hatchery problems.[60] (See Appendix A)

Roland Dimick, previously with the OSC Agricultural Experiment Station, was appointed head of the Department of Fish and Game Management in 1935 and remained in that post until 1963. Dimick had expertise in fisheries and had previously consulted with the Game Commission.[61] The faculty for the department consisted of Dimick and one other full time instructor supplemented by a floating instructor. Dimick also served as assistant director of the OCWRU, but due to a heavy teaching load, he had little time for research.[62] A survey of classes from the 1935-1936 and 1948-1949 OSC catalogs[63] showed that the original offering of classes changed little in the intervening years. The 1935-1936 catalog showed three undergraduate classes (one class had three sections) and nine classes (representing a total of 17 sections) for graduates, excluding research, reading and conference and seminar hours. Of those 12 classes, only one class, Management of Game Fish, specifically related to fish. The catalog explanation indicated that fish hatchery methods were discussed and that field and lab work were conducted at the Alsea Fish Hatchery, which was owned and operated by the Oregon Game Commission. In the 1948-1949 catalog, two other classes relating to fish were added. Economic Ichthyology (3 sections) discussed classification and distribution of fishes and their economic and recreational importance, while Commercial Fisheries (3 sections) emphasized economic facets of fisheries with some discussion of fish biologies.[64] (See Appendix B)

The makeup of classes was a reflection of the position of FGM within the School of Agricultural, as well as the size of the department's faculty. In the 1945 evaluation of the department Einarsen wrote, "The placing of the department within the School of Agriculture was a fortunate and wise decision, for instruction and research were immediately viewed as essentials in producing wildlife crops in harmony with the major uses of land and water."[65] The FGM classes stressed economic aspects of wildlife resources defining them as agricultural commodities, with minimal discussion of their biological life histories. Classes relating to fish often addressed fish hatchery methods as well as natural propagation. Catalogs cannot give an accurate picture of the department's activities; however they do give a good indication of the department's overall emphasis. The department's focus seemed to dovetail closely with that of the Game Commission, to provide domestically raised fish and wildlife for sportsmen. The department reflected an applied approach to research and instruction, with a great deal of emphasis on fish hatcheries. That emphasis was not balanced by a basic biological investigation of fish either in research or instruction.

In 1942 the FGM Department was suggested for elimination as the School of Agriculture attempted to absorb proposed budget cuts as a result of World War II belt-tightening.[66] Dimick argued that the FGM classes were well attended, the program was highly rated, and it was the only wildlife management program on the West coast.[67] In addition, he showed that the FGM graduates were highly sought after and most were employed in their fields upon graduation. He produced a list of 129 FGM graduates since 1935. Of those 129, 30 graduates were employed by the Oregon Game Commission, and 24 were employed by the U. S. Fish and Wildlife Service (formerly the U. S. Biological Service).[68] This process demonstrated a recycling and stagnation of knowledge that would inhibit the introduction of new ideas. Students entered a program heavily influenced by the goals of the Game Commission and the U.S. Fish and Wildlife Department, and then became employed by the entities that helped to direct their education. Because of the emphasis on fish hatcheries among all these groups the practice was not seriously questioned or evaluated. The agricultural model created at the federal level by the USDA and U. S. Fish Commission was replicated by state fish and game commissions in Oregon and passed on to the Oregon State College Fish and Game Management Program. The "normal science" that was perpetuated at this institution served to define research that addressed questions regarding fish diseases, proper nutrition, and management methodologies that focused on hatchery technology. This scientific tradition rarely addressed questions that the stock concept evoked in research or in class offerings.

Conclusion

When general scientific theories about species formation and genetics were in flux, ecological fundamentals were just beginning to be understood as well. The Modern Synthesis, the unification of all life sciences which was proposed between 1937-1943, would not take effect until the 1950s. Because the new ideas were in process, how fish culture would affect the quality of wild salmon populations while fish hatcheries produced massive quantities of homogenized fish was an unknown during the 1930s and 1940s. Few facts had been gathered that confirmed behavior of salmon with respect to migration patterns and homing instinct. The historical dogma dictated one general story with minor variations for each of the five Pacific species: salmon were born in freshwater streams, foraged up and down that stream and at some point went into the ocean for a given amount of time. It was believed that salmon did not, however, travel far into the ocean but stayed near the mouth of their natal stream. When it came time to spawn, the salmon then moved into the stream closest to them. Therefore, the salmon's return to the same stream year after year was explained as coincidence, not a result of homing instincts.

This theory was challenged by tagging studies but the studies came with inherent faults and often were discounted. Many fisheries managers treated salmon like other domesticated plant and animal species. They believed that fish could be manipulated through breeding techniques and by controlling the local environmental factors in the local streams and lakes. By the late 1990s genetic diversity was based on DNA analysis; however, DNA's structure was unknown until 1953. In the 1930s-1940s, genetic diversity was based on fish scale analysis and tagging studies, again questionable methodologies. Salmon life histories were just beginning to be understood, and a recap of the factors of the home stream theory show this evolution

The 1903 Home Stream Theory: based on fishermen's observations that salmon return to their natal stream. Evolution is based on mutation theory with a neo-Lamarckian source of variation.

The 1917 Parent Stream Theory: included possible knowledge about wide-ranging migration patterns as tagging studies had just begun in 1911, a suspected homing sense, and fishermen's observations supported by Charles Gilbert's observations that salmon returned to their natal stream, which Jordan denied. Evolution was based on either mutation theory or natural selection without a known source of variation. Therefore, neo-Lamarckian environmental factors may be considered to be the source of variation. Fish hatcheries are very popular.

The 1939 Home Stream Theory: tagging evidence supported wide-ranging migration patterns and the theory that salmon returned to their natal streams, facts many fisheries biologists accepted; a homing sense is suspected. Mutation theory and natural selection are in conflict; the source of variation is still unknown. Therefore, neo-Lamarckian environmental factors may be considered to be the source of variation. Fish hatcheries are still very popular though the economics of fish hatchery production has been challenged.

The 1972 Stock Concept: included wide-ranging migration patterns, a defined homing instinct, and the fact that salmon returned to their natal streams. According to Bottom (1997), actual studies to correlate hatchery production to survival did not surface until the 1960s when the hatch rate of hatchery fish and the adult return rate increased. However, later work indicated that the hatchery success in the 1960s was not necessarily the result of a successful hatchery program but the result of beneficial ocean conditions. In the 1970s, studies showed a drop in adult returns.[69] Bottom cited papers from 1972 and 1991 indicating that the identification of subspecies as the units that must be protected to protect species had been named the "stock concept." The stock concept was further developed and accepted, "as biologists identified unique, heritable characteristics of individual populations...that were associated with increased fitness in local environments." Bottom then indicated that the stock concept was based on the principles of natural selection indicating that their native ecosystems were "inseparable parts of their co-evolutionary process." The stock concept suggested that "ecological goals based on fish life history, adaptation and reproduction" were the keys to salmon survival, as opposed to the domestication of fish as agricultural products. The stock concept notes the importance of diversity of subspecies as a source of variation in the process of evolution by natural selection. Fish hatcheries are suspected to be causing genetic problems in salmon.[70]

A Kuhnian explanation of the above events again is instructive. The normal science of breeding and production of salmon through artificial propagation was expected to satisfy the demands of commercial and sports fishers. However, anomalies surfaced when the hatch rate of hatchery fish did not produce a sufficient return rate of adult fish, and commercial and sports fishers decried the disastrous declines in fisheries. The scientific community of agricultural breeders defined the problem within the paradigm of their normal science, production of fish by artificial propagation. This scientific tradition was based upon the principles of Mendel's hereditary units combined with the mutation theory that incorporated the neo-Lamarckian tradition that the environment directly affected species fitness. They believed that the answer to declining fish runs could be solved by controlling the environmental conditions of their product like nutrition, water temperature, pollution.

Other fisheries biologists who embraced different paradigms looked to their normal science to explain the anomalies. Willis Rich applied a version of natural selection that proposed that genetic diversity within fish, the equivalent of Darwin's varieties, must be maintained to protect salmon species. To influence this diversity, he proposed that the centralized hatchery system be replaced with hatcheries on local streams. Like Nathan Fasten, Rich was unsure about the source of variation and he stuck with a neo-Lamarckian belief that the environment directly affected species formation.

If one accepts that a neo-Lamarckian source of variation was the dominant paradigm then it is easier to understand why fish hatcheries were acceptable among professional fisheries biologists as well as fish culturists. A neo-Lamarckian source of variation supported the idea that environmental factors directly affected heritable change in a species, or that the good environmental conditions produced fitness within species that was heritable. Why neo-Lamarckian attitudes persisted in fisheries is evident from the character of the normal science research and instructional focus of FGM. The recycling of ideas affected when FGM students were hired by the Game Commission and the U. S. Bureau of Fisheries further strengthened the paradigms of production science. In addition, Nathan Fasten taught introductory zoology and advanced genetics classes for twenty four years with a paradigm that environmental factors directly affected species fitness. Neo-Lamarckism was not rejected until the 1950s, and the unification of biological sciences based on genetics did not take hold until the 1950s. It is not surprising then that, as Bottom pointed out, genetics was not applied to fish culture until the 1970s. Therefore, it is unlikely that in the 1930s fish hatcheries were challenged by a paradigm based on modern genetics in which a distinction is made between a direct role of the environment, and an indirect role of the environment in species fitness. The genetics that Rich referred to in his 1939 paper most likely was a version that included a neo-Lamarckian element.

Fisheries have always been entwined with economics and politics which adds importance to the need for fish hatcheries. As salmon habitat disappeared with population growth and water resource development expansion, artificial propagation became an adjunct to dam construction in the Pacific Northwest. Therefore, anyone who questioned fish hatcheries threatened economic development and put in jeopardy lucrative pork barrel spending attached to water development projects.

The following case studies show that opposition to either the dams or fish hatchery technology may cost fisheries managers their positions within wildlife management. The fate of the 1934 Game commission came into jeopardy when they opposed the construction of Bonneville Dam. Their discussions show reliance on fish hatcheries to compensate for loss of habitat and as a failsafe mechanism should fishways over Bonneville fail. However, Bonneville was built before Foerster's studies led to the closure of fish hatcheries in Canada. The fate of the 1949 Oregon Game Commission came into jeopardy when they opposed construction of dams on tributaries of the Willamette, and E. E. Wilson questioned the efficacy of fish hatcheries based on Foerster's study.

Chapter 2 Endnotes

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[4] Netboy (1980), p. 29.

[5] Ibid.

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[7] McEvoy (1986), p. 35.

[8] Smith (1994), p. 37.

[9] Ibid., p. 5.

[10] Ibid.

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[12] Ibid., p. 4.

[13] Ibid., p. 21.

[14] Ibid., p. 28.

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[45] Ibid., pp. 9-10.

[46] Hume (1893), p. 36.

[47] Bottom (1997), p. 569.

[48] Ibid., p. 576.

[49] Ibid., p. 577.

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Chapter 3 - The Bonneville Fishway Debate

That fisheries managers may be dismissed if they did not support the governor's plans was not unknown to William Finley. He served as the Game Warden for the Oregon Game Commission in 1911. After his service he made many recommendations to policy makers about how to best protect Oregon's wildlife. Regarding service on the Oregon Game Commission he recommended that the group should not serve at the discretion of the governor. From his experience people who served at the behest of the governor often made decisions to please the boss at the expense of the best interests of wildlife. Further, if bureaucratic entities, they would be dismissed.[1] Finley's concerns were realized in 1935 and in 1949. Paradoxically, the 1934 group supported the use of fish hatcheries to mitigate the effects of such projects and the 1949 group did not. The change in opinion about fish hatcheries reflected the evolution of a scientific paradigm. After presenting background on the Oregon Game Commission and the Columbia River Basin development plans, this essay will address the Bonneville fishway debate as it occurred between 1933-1934, during the initial construction of the Bonneville Dam. The chapter will consider fishway plans proposed by the Interstate Fish Conservation Committee (IFCC) and other interested parties opinions. The chapter concludes with the group dismissal of the 1934 commission by newly elected Governor Charles Martin in January 1935.

History of the Oregon Game Commission

Oregon Fish Commission reports in 1887, 1888, and 1891 showed that regulations were not protecting the fisheries. Reports by Oregon's Fish and Game Protector Hollister McGuire in 1894 and 1896 indicated serious declines in fisheries and made numerous recommendations to conserve the fishery on the Columbia River.[2]

Since regulation was not helping, policy makers gradually turned to supplementation. Concern over dwindling fish runs as early as 1872 led to the establishment of the first federal fish hatchery in the state by 1887. In 1909 the first angling license law provided funds for a fish commission charged with managing the operation of ten fish hatcheries throughout the state. By 1911 interest in the disappearance of all forms of wildlife prompted Oregon Governor Oswald West to create the State Board of Fish and Game Commissioners, replacing the former Board of Fish Commissioners and the State Game Commissioner. West served as chair of the Board, and Finley was appointed the State Game Warden. The group's attention was divided between fish, birds and big game.

In addition to fish propagation the commission's first work involved importing elk from Wyoming to supplement native herds and propagating Chinese pheasants at a game farm in Corvallis. In 1921 the Oregon Legislature wished to separate the management of commercial fisheries from those of fish and wildlife sports interests. It created the State Fish Commission to manage commercial fishery problems and a separate State Game Commission to manage wildlife for Oregon's sports hunters and fishers.[3] Acrimony between fisheries commercial interests and sports interests precipitated the split and remained the source of the infamous legislative fish wars enacted every biennium. The once strong commercial fishery on the Columbia River felt its power base challenged by the growing number of sports fishers throughout the state. Sports anglers charged that commercial groups overfished runs on the Columbia and the coast to the brink of extinction and asked that regulations be adopted to close rivers and streams to commercial activity. Commercial fishers charged that anglers introduced exotic fish species in Oregon's lakes and streams that displaced native fish. Both argued over the status of steelhead, and both were affected by the encroachments posed by Oregon's population growth. Turf wars between the two groups often made establishment of a consistent and coherent fisheries management policy difficult if not impossible.[4]

Game Commission members were appointed and served at the discretion of the governor and were charged with protecting the state's game resources. Revenue to fund commission activities accrued primarily through the sale of game hunting and fishing licenses, supplemented by the accumulation of fines collected from people who broke game laws. The five commissioners met once a month for two days to develop regulations for hunting and fishing, and to develop policy for producing fish and game for the increasing numbers of hunters and fishers. Commission members were not professional wildlife managers but directed the actions of the growing professional staff who oversaw the management of commission game farms and fish hatcheries. To compensate for encroachments on fisheries habitat by farming and irrigation, the commission managed projects that screened drainage ditches, provided for and ensured fishways over public and private dams, and rescued fish diverted into irrigation ditches. The majority of the commission's effort was devoted to the artificial propagation and liberation of game fish meant to supplement natural runs. This process involved gaining a supply of fish eggs, fertilizing the eggs and rearing the fry, and finally, liberating the fish into lakes and streams.[5] A report of expenditures for the 1931-1932 biennium shows that 36 percent of revenue was spent on fisheries activities, 35 percent on game law enforcement, 13 percent on game bird production and liberation, and the remaining 16 percent was divided between administrative and educational costs.[6]

The commissioners appointed in 1930 were given a new charge: to formulate game policy within the confines of a 10-Year Wildlife Program, a program based on "business and scientific management of game affairs."[7] This program had been approved by Oregon voters and became operative on October 1, 1931. The program included fourteen points which emphasized improvements in administration, budgeting, law enforcement, and predatory animal control. It further asked that scientific studies of all factors affecting wildlife be conducted to determine production, stocking and the development of regulations governing game birds and fish. Finally it asked for annual increases in hatchery production of game fish and game birds, the stocking of lands and waters with game fish and birds based on practical fact finding as opposed to requests for wildlife products from local sports groups, and fair allocation of game and fish products throughout the state.[8] Because part of the Game Commission's mandate was to police dams and fishways in an effort to protect sports fisheries, Commission members were included in an advisory group, the Interstate Fish Conservation Committee (IFCC), expected to approve a plan for fish passage over Bonneville Dam. The plan the group submitted proposed fishway plans and also contained a great deal of skepticism about the value of those plans.

The Columbia River Basin Development Plans

Building of federal dams on the river may not have begun in the early 1930s without the lingering economic devastation caused by the Depression coupled with the determined efforts of Oregon's representatives to bring federal work project funds to the Pacific Northwest. The River and Harbor Act of 1925 asked that the U. S. Engineers prepare cost estimates to determine which navigable rivers in the nation would also support hydropower development. This request resulted in the House of Representatives Document 308 in which ten rivers were identified, including the Columbia River and its tributaries. The River and Harbor Act of 1927 then directed the Corps of Engineers to conduct the surveys recommended in Document 308, and to propose water development projects that included analysis based on four criteria: navigation, power, irrigation and flood control. Field work by the Engineers amassed data on foundation, stream flow, topographic and hydrographic surveys, and areas that could be reclaimed for farmland. The information was combined with studies from the Bureau of Reclamation, the U. S. Geological Survey and other consultants into a final report submitted in 1931. The 308 report proposed that ten dams be built on the Columbia.

Projects had to be justified according to their economic feasibility and their engineering feasibility. Inclusion of the potential of power development projects coupled with navigation improvements on the Columbia made development more economically attractive, but not attractive enough to justify the federal expenditures. Upon review of district and division reports, the Board of Engineers suggested that the federal government should be responsible for navigation improvements, and the local governments and private developers should be responsible for power development projects. But the 1932 presidential campaign brought Democratic candidate Franklin Delano Roosevelt to the Pacific Northwest who promised federal power development projects on the Columbia River as part of New Deal work relief projects.

Bonneville Dam was the first project proposed for the Columbia, but it competed with projects from many other states vying for federal work relief dollars. Funding was finally secured in 1933 after extensive persuasive efforts by Oregon Senator Charles L. McNary and Oregon Representative Charles H. Martin.[9] Funding for the project would come from the Works Progress Relief Administration (WPA), and the project was expedited in order to provide jobs as soon as possible for the state. Bonneville Dam would include channel improvements and the installation of locks to allow ships to pass the dam, and would be high enough to create a large pool of slackwater behind the dam so that ships could pass forty eight miles upriver. Power generation resulted from water passing a tall spillway with enough vertical fall to operate turbine generators.

The Debate

Despite earlier charges that the original plans for Bonneville did not include fish passage facilities, historical accounts of the U. S. Engineers indicate that plans for fish passage had been considered since inception but due to time constraints, exact plans and cost estimates were not finalized until after construction began.[10] The 1934 Fish and Wildlife Conservation Act required that federal agencies take into account the loss of wildlife caused by development projects. Federal agencies constructing dams were required to consult with the U. S. Bureau of Fisheries regarding adequate fish passage facilities for upstream and downstream migrants. The provision of devices like fish ladders or lifts were expected to be balanced against the economic feasibility of such installation.[11] Economics governed the Bonneville fishway debate and depending on their perspective, advocacy groups argued either positively or negatively for the economic strength or weaknesses of the proposed fishways. The following perspectives presented in this section represent the viewpoints of government, business and conservationists on the dam and its proposed fish passage facilities. The arguments made for saving salmon runs lay within their economic value to local commercial and sports interests when balanced against either the value of energy produced by the dam or balanced against the cost of the fishways.

Using numbers to rationalize development projects was a trend attached to Progressive Era politics which deemed that experts who hailed from outside the local area could more effectively evaluate projects. Often these experts were credentialed, with college degrees and bureaucratic titles that propped up their expertise. In the arena of public policy formulation, the quantification of subjective factors allowed experts to appear distanced from the subject matter, thereby giving them the appearance of absolute objectivity. Using numbers to represent values like availability of fish runs allowed reasoning to become more uniform and seemingly more rigorously scientific and objective. Because they were objective, expert decisions accompanied by a mathematical formula supporting their contentions were deemed more closely able to represent reality. Critics of this trend charged that this dependence allowed experts to ignore important local values.[12]

The 1934 Wildlife Conservation Act required that local opinion be balanced against outside experts. After construction of Bonneville Dam began, the War Department's U. S. Engineers sought advice from local fisheries experts regarding fish passage facilities at the dam by. To accomplish this, the U. S. Bureau of Fisheries organized and served as consultants to an informal group called the Interstate Fish Conservation Committee (IFCC). This group included representatives from the fish and game commissions of the states of Oregon, Washington, and Idaho, as well representatives of Columbia River fishers and canners. Lichatowich (1999) indicated that this committee was the first time such a comprehensive group had been assembled to consider salmon protection and restoration.[13]

The Interstate Fish Conservation Committee (IFCC)

During the initial deliberations of the IFCC, contentions arose between different groups about the best means for insuring fish passage upstream and downstream. The state of Washington supported fish lifts while the state of Oregon supported fish ladders to carry salmon over the dam. After listening to opinions from all groups, U. S. Engineer Thomas Robins of the War Department sent in a proposal for Bonneville fishways on October 6, 1934 that included both devices, in an attempt to appease all parties; this proposal was made, however, without first consulting the IFCC. Minutes from the October 8, 1934 and the November 19, 1934 Oregon Game Commission meetings indicate that a considerable amount of dissension arose between the Oregon Fish Commission and the Oregon Game Commission over Robins' proposal. The Fish Commission had made agreements with Robins that Game Commission staff engineer Ralph Cowgill refused to approve as they violated the terms of a previous agreement between the Fish Commission and the Game Commission. At issue was the elimination of a fish ladder, a change in the width of fish ladders from forty to thirty feet, and the reduction of the water budget for a fish hatchery from 500 second feet of water to 75 second feet.[14] Cowgill further emphasized the urgency of the situation at the November 19 meeting. In addition, Cowgill charged that the Fish Commission was refusing to pay its share of the salary for Milo Bell, an engineer hired specifically to consult about the Bonneville fishways.[15] The original estimate for fishways for Bonneville dam was only $800,000. Robins' October 6 plan included ladders and lifts and was estimated to cost $3,000,000. The three-fold increase surprised Interior Secretary Ickes who refused to approve the plan without justification.[16]

The $800,000 figure was reached by Army Engineers who were in a hurry to submit plan estimates to the WPA. Instead of analyzing what would have been required by Bonneville, they settled for a doubling of the price of fishways that had been built over Rock Island Dam. The Army engineers knew Bonneville fishways would cost more but the WPA did not. They took the engineers $800,000 estimate at face value. From that point, the cost of Bonneville fishways was a major issue ameliorated only by the intervention of highly placed officials like Senator McNary. McNary would secure $2,500,000 for Bonneville fishways in February 1935, but fisheries experts by that time were hoping for $4,200,000. Fisheries experts argued that the Columbia River fishery represented a valuable industry for the Pacific Northwest and was worth the expense.[17]

The December 10, 1934 Meeting and Proposals

After the initial meeting of the IFCC, a different group of twenty two members met in Portland on December 10, 1934 to discuss plans for carrying fish over Bonneville Dam in an effort to provide Ickes with justification and to develop a unified plan to submit to the War Department. The group included six representatives from the Washington Game Commission, two from the Washington State Department of Fisheries, three from the Oregon Fish Commission, eight from the Oregon Game Commission, and three representatives from the U. S. Bureau of Fisheries who functioned as consultants. At issue were the types of mechanical systems used to transport the fish over and through the dam, collection systems meant to attract fish to the ladder or lifts, the amount of funds available to complete the fishways, and whether it was too late to affect change to the Army Engineers' plans. Minutes from this meeting show that the group did produce a plan on Dec. 10, but this plan contained more than recommendations about fish ladders, lifts and collection systems. The document submitted questioned the ability of any technological innovation that would be able to protect upstream and downstream salmon migrants, and demanded that the government provide funds for further fishway solutions should the attempts made with the submitted plan fail.

Since construction had already begun on the dam, many members questioned the group's ability to effect change at that point. Some members argued that the meeting was a waste of time. John Veatch, chair of the Oregon Fish Commission, believed that the charge of the group was to suggest a type of fishway and to leave the details to the Army engineers.[18] Cowgill attempted to mediate sentiments like Veatch's. He stated that in order to control the type of devices used and the amount of money to be spent for fish passage, this group had to produce an economical plan behind which all members could unite. Cowgill emphasized that the group's charge was to save the fishing industry. Should they fail to produce a unified plan, the fishway question would be resolved by Army engineers without the group's input.[19]

Bell explained Robins' October 6 plan. After discussion of the plan's deficiencies Harlan Holmes, of the U. S. Bureau of Fisheries presented an alternate plan that would cost $3,910,000, representing an increase of $91,000 over Robins' plan. Holmes' plan included a section that expanded and added fishway facilities for upstream migrants that was estimated to cost $3,660,000, as well as a section that tentatively provided structures to protect downstream migrants expected to add an additional $250,000. The downstream migrant plan included six spillway passes, but did not include screening of the powerhouse turbine, which Cowgill insisted was necessary. After spawning in freshwater streams, salmon fingerlings travel downstream to the ocean. To get to the ocean, the fish would have to go through the dams. To get through the dams, they would have to pass through the turbines that produced electrical energy, or pass over spillways that allowed water to flow over the dam. Bonneville engineers argued that there was plenty of room for fingerlings to pass through the turbines safely. Cowgill argued that the fingerlings would be chewed up and repeatedly advised that the power house turbines be screened. While Holmes included the downstream provisions in his plan, he expressed little support for screening. "Any means of preventing the downstream migrants from entering the power wheels would represent additions, and there would be very little saving in the by-passes as a result of the additions."[20]

The group debated the efficacy of either ladders, lifts and locks, where they should go and what size they should be, the water budget for the fishway, and screening of turbines to protect downstream migrants. In addition to these discussions, opposition sentiments to the construction of Bonneville Dam and the detrimental effects it would have on Columbia River fisheries were also well-discussed topics. Thomas Lally, chair of the Washington State Game Commission, initially voiced opposition to the building of the dam. He repeatedly remarked in no uncertain terms that any high dams on the Columbia would ruin the fishery, regardless of which mechanical device was adopted by the group to get fish beyond the dam.[21] As a state official, Lally resented not being consulted regarding the building of the dam, and then being forced to appear to approve a fish passage system that he believed would fail. He feared that any support from their respective groups would indicate approval for the proposed technologies and that they would be credited with the technology's adoption. He feared being complicit in what he expected to be the end of Columbia River salmon runs.

> ...he hesitated about the advisability of sending in a definite recommendation (to) the war department for if the fishing industry is ruined, the war department could very plausibly say that the fish and game departments of these two states had made recommendations for the fishways.[22]

He did not want to put faith in systems that hadn't been adequately tested and believed that whatever the group did the salmon industry would be injured.[23] However, Lally was swayed by Cowgill's arguments knowing that construction on Bonneville couldn't be stopped. In lieu of stopping construction, he made demands of the federal government regarding protection of fish runs. Lally wanted a fund set up to pay for new fishway solutions should the present ones fail, and he wanted compensation for destroyed hatcheries caused by construction as well as an agreement by the federal government to pay for new hatcheries needed to mitigate the loss of habitat caused by the dam. B. M. Brennan, Washington State Director of Fisheries, indicated that "his department had already approached the war department in the matter of establishing hatcheries to propagate fish to make up for loss due to the presence of the dam, but this suggestion had not met with favor."[24] Lally strongly suggested that the governors in Oregon and Washington, and the state planning councils be appraised of the situations and asked to use their authority to "see that everything possible was done" to get adequate funding to rectify the situation whatever the outcome.[25]

Once Lally began his opposition, others followed. Dr. Irving Vining of the Oregon Game Commission said that the Game Commission tried to cooperate with every industry in the state and that "no fish and game commission could stop the building of the dam; that the only thing to be done was to work out some feasible program which would offer some measure of protection to the fishing industry." He also suggested that the groups go on record "stating that it was their belief that the fishing interests were jeopardized by the construction of the dam and that the recommendations presented were offered as the nearest solution for the protection of fish life in the Columbia River."[26] Dr. Vandevert of the Oregon Game Commission and Virgil Bennington of the Washington Game Commission agreed. Brennan added that "some action must be taken for if the matter was left entirely up to the war department, the fishing industry without a doubt would be ruined."[27] H. D. Hinckley of the Washington State Game Commission "thought a statement should be made to the effect that the construction of the dam was a serious menace to the fishing industry."[28] Dr. Hibbard of the Oregon Game Commission disagreed with opposition sentiment stating that, "progress as a whole should be considered and therefore the dam should not be condemned; but that the recommendations should be made and then confidence be expressed in the people building the dam."[29]

After much discussion, Holmes' revised report was compared to Robins' report and a compromise recommendation developed which would be more expensive than Robins' plan. Holmes advised that other new additions may make the expense of the proposed plan prohibitive.[30] Nevertheless, the final report was unanimously adopted and signed by all representatives of the Washington and Oregon fish and game commissions. The report had two sections. The introductory section made general directions, and the resolution section made specific recommendations to correct Robins' October 6 report. The general directions first defined that the group's charge was not just to advise regarding ladders or lifts, but to "consider the effect of the erection of the Bonneville Dam on the commercial and game fish life of the Columbia River."[31] The proposal stated that the salmon industry of the Columbia River totaled $10,000,000 on the river alone, and that the Columbia River furnished salmon for Puget Sound, British Columbia and Alaska fisheries as well and therefore was an industry worth preserving. The continuance of those industries depended on fish migration beyond Bonneville Dam. In the final proposal, Lally's previous statements regarding the negative effects of the dams were softened.[32] This introductory section concluded noting that the previous suggestions by the War Department as well as this proposal may not provide a guaranteed solution to fish migration, and that some fish hatcheries will be destroyed by the dam. Screening of powerhouse turbines was not suggested. The final two statements made demands regarding conservation of fish life and the replacement of destroyed fish hatcheries.

> BE IT FURTHER RESOLVED THAT Congress and the United States Government shall make an appropriation in a sufficient sum so that additions and changes can be made to the dam and appliances which may hereafter be found necessary to conserve the fish life in the Columbia River; BE IT FURTHER RESOLVED that we demand of the said Government that it compensate the states involved for its destruction of certain hatcheries and that it make necessary compensation to the departments for building other hatcheries to take their place.[33]

When the results of this meeting were announced in the Gazette-Times on December 11, 1934, it was a page one story titled, "Game Commissions After Fishways Agreement Pact." However, the story took only two sentences, and made no mention of any opposition to the dam, the inclusion of fish hatcheries to mitigate lost salmon habitat, or the screening of turbines to protect downstream migrants.

> The game commission(s) of Oregon and Washington meeting here in joint session Monday afternoon adopted recommendations for fishways over Bonneville dam. The recommendations included gravity ladders, hydraulic lifts and a canal on the Oregon side, in addition to use of shiplocks for passing migratory fish upstream.[34]

The Public Debate

The fishway debate moved beyond the governmental fishway experts and entered the realm of public comment. David Eccles owned a property management and investment firm in Portland and edited a newsletter, *Commonwealth, Inc. Business Survey of the Portland Area*. He evaluated the fishway debate in the March 15, 1935 issue in the article, "Is King Chinook Doomed?" and then later presented his findings to the Portland Chamber of Commerce. Research for this piece included consultations with U. S. Army Engineers, the Oregon State Fish Commission, the U. S. Bureau of Fisheries, Dr. Lawrence E. Griffen and Mr. I. N. Stresland, but not with members of the Oregon Game Commission. He acknowledged that the Columbia River was the largest producer of fish in the world, and that there were differences of opinion on how to best protect the fisheries resource. He also acknowledged that the source of these differences lay in a lack of research about fishways as well as ignorance of the habits of fish.[35]

Eccles described the fishway system as a combination of fish ladders and fish locks with a gathering channel collection system, and cited two problems connected with this system. First, because salmon were attracted to turbulence, he feared that salmon would be attracted more by the turbulence emitted from the powerhouse turbine outlets than the collection system and may fight the rush of water until exhausted and never reach their spawning sites. "King Chinook instinctively seeks large volumes of cold, rushing water. He almost never swims with current, goes into still water only to rest after a tough battle...While the power dam gathering channel will have available two thousand second feet of water, turbine outlets will pour forth sixty thousand second feet."[36] Other experts argued that migrating salmon would battle the turbine outlet until tired and then take the collection channel. The second problem involved salmon missing the fish ladder entrance and swimming into the one thousand foot tall spillways and "being dashed to bits on the concrete base of the tailrace." He noted that the fisheries experts were attempting to solve this problem by constructing higher walls to guide the fish away from the spillways and towards the ladders.

Eccles then discussed finances. By this point, the amount of money that had been allotted to the fishways was $3,200,000, but the plan presented to the Army Engineers would cost $4,200,000, so cuts had to be made and again he noted that a lack of information exacerbated the situation.[37] Eccles then considered the downstream passage of fingerlings through turbines and declared they were expected to travel safely. "They will be diverted so far as possible into ladders or a fingerling by-pass. Should they go through the turbines it is unlikely they will be damaged." The effects on migration of the "slow-moving lake" created behind a dam as opposed to a rushing riverine environment was discussed indicating that some experts thought it would throw the fish's timing off while others believed it would not affect spawning success. He concluded that it was possible to meet the challenges the dam presented by conducting more research and by relying on artificial propagation as a failsafe option.[38] Eccles then compared the cost of the fishways to the expected income generated by the commercial fisheries, a figure he set at ten million dollars; he indicated that revenue from sportfishing was impossible to know, but estimated it between twenty five to thirty million dollars. Based on these figures he concluded that

King Chinook was a valuable enough commodity to warrant additional appropriations.[39]

The Board of Directors for the Portland Chamber of Commerce held a meeting on September 18, 1935 to discuss a report by the Bonneville Dam Fishways Committee, a Chamber advisory group. Two speakers made presentations: David Eccles of Commonwealth, Inc., gave a brief description on the history of the salmon industry, and William Finley presented a paper on the life and habits of the Columbia River salmon. In addition to Finley's stated topic, he commented on water resource development. Finley argued that the Columbia River was a public commodity and could supply the public with water for many uses: domestic use, irrigation, navigation, recreation and as a source of food. He believed that its value as a producer of the world's finest salmon surpassed its value as a producer of electrical energy, which could have been produced by other means. Finley evaluated the fisheries economic impact acknowledging that fish propagation was part of fisheries upkeep.[40] Finley stated that the effects of overfishing, farming and pollution seriously challenged salmon survival, but that the high dams were the greatest threat to salmon runs. "Now comes the greatest of all dangers in the perpetuation of the salmon runs of the Columbia River, namely high dams for producing electrical energy."[41]

He lamented that the public had never been given a chance to choose which use the public preferred. Had there been such a report, Finley thought it would have indicated that: fishways for salmon over a dam the size of Bonneville were experimental; experts were divided over the passage of fingerlings through turbines, yet Bonneville engineers claimed to be unable to screen the turbines; and the lakes created behind the dam could further disturb salmon migration as spawning habitat would be destroyed, natural food sources would be diminished, and the new "lakes" would encourage the growth of fish that would prey on salmon fingerlings migrating downstream. However, he acknowledged that the construction of Bonneville and the Grand Coulee dams was not up for discussion.[42] Because the dams were a forgone conclusion, he then indicated that the best course to pursue was determining the best type of fishway and ensuring that it was adequately funded. He preferred fish ladders over lifts and argued for expending whatever was necessary to guarantee that the fishways a success. He concluded indicating that trying to save "a million or two dollars is not sound judgment when experimenting with a $200,000,000 industry."[43]

In addition to the presentations, the Chamber Fishways Committee had obtained both written and verbal information from the Oregon State Fish Commission, the Oregon Game Commission, the U. S. Bureau of Fisheries, the U. S. Army Engineers, the Astoria Chamber of Commerce, the State of Washington Department of Fisheries and the State of Washington Department of Game. The report also specifically thanked "authorities on the subject of fishways" with whom the group consulted in a series of special meetings: two people from the War Department; John Veatch, M. T. Hoy and Hugh Mitchell of the Oregon Fish Commission; Harlan Holmes and Henry Blood of the U. S. Bureau of Fisheries; and Milo Bell of the Washington Fish Commission. Members from the game commissions of Oregon and Washington were not included in this panel of fishway experts.[44] The report indicated that the Fishways Committee was considering recommendations on three proposals for Bonneville: additional fish gathering chambers below the spillway dam, the need for a set of locks at the south end of the spillway, and extended entrances to the fingerling bypasses. In the final analysis, the Fishways Committee wrote that the group could not justify the additions at the present time and that it was "...reasonable to expect that the great majority of both upstream and downstream migrants will safely pass around the dam."[45]

Richard White (1995) wrote that those representing fisheries interests, like the fish and game commissions, U.S. Fish and Wildlife, and the Bureau of Indian Affairs, were not allied with politically powerful interests, while those representing the interests of power, irrigation, and navigation had "bureaucratic bruisers" in the Army Corp of Engineers, the Bureau of Reclamation and the Bonneville Power Administration.[46] As a result, dismissing a commission for opposing Bonneville Dam would not have been a politically expensive proposal.

Dismissing Opinions by Dismissing People

The Oregon Game Commission had been excluded from providing expert testimony after January 1935 because the previous group that had been involved in the fishway debate no longer worked for the Oregon Game Commission; they had been dismissed. The new group of commissioners either hadn't developed the fisheries expertise or were politically astute enough to be wary of getting involved. Former House Representative Charles Martin, who had so adroitly negotiated funding for Bonneville Dam, was elected governor of Oregon in 1935 on a platform which espoused the progressive resource development policies of President Franklin Roosevelt. At Martin's inauguration on January 14, 1935, he vigorously supported the development of the state's natural resources. In his address, Martin praised Roosevelt and the Bonneville project stressing the importance of improving the Snake and Willamette rivers.[47] One of Martin's first acts was to clean his house of any opposition to plans for natural resource development in the Columbia River Basin. Any group that appeared to oppose construction of the Bonneville Dam certainly would not support the Willamette Valley Project (WVP), a WPA project that would bring millions in federal dollars to the state during the Depression. As part of the Columbia River Basin development, a section of the House 308 Report included the construction of seven dams on the Willamette River and its tributaries, a project that was not approved until 1938 and not authorized until 1939. In an effort to quell any opposition to Martin's plans, the game commission which had voiced resistance to the Bonneville Dam was dismissed less than two months after the December 10, 1934 meeting. Martin, however, did not acknowledge that the group was dismissed for their efforts with regard to Bonneville Dam. Instead, he charged that the commission had misused game commission funds and they were dismissed for fiscal irresponsibility. The Izaak Walton League, a sportsmen's advocacy group, thought otherwise and charged that Martin's move was a political ploy that threatened Oregon's wildlife.

Budgetary information garnered from Game Commission biennial reports lends credence to the Izaak Walton League's charges. Prior to the dismissal, the Commission acknowledged a $56,000 deficit in the 1933-1934 Game Commission Biennial Report. This red ink had accrued because the Commission was attempting to meet the mandate of the 10-Year Program which required the building of more pheasant farms and fish hatcheries to supply increased numbers of game. The situation had been exacerbated by the onset of the Depression. Not only were revenues from license sales down, the Game Commission was expected to match funds from federal grant programs which led to a deficit of $56,252 but for this deficit the department had a capital investment of about $140,000.[48] The report indicated that due to the building improvements, the "department stands at least five years ahead in its program for increased production facilities," and the production rates at game farms and fish hatcheries were at record levels. Because the Game Commission had to adhere to the mandate of the 10-Year Program, chose to accept federal dollars which required matching funds for capital improvements to meet the 10-Year Program mandate, and because revenue for hunting and fishing licenses during the height of the Depression were down, a deficit had accumulated. Therefore, income did not immediately meet expenses, but the report indicated that with careful budgeting and an expected increase in license sales the deficit would not remain through the next biennium.[49]

Despite the explanations, the entire Game Commission was dismissed for being greedy politicians who misused Commission funds. Martin announced the new members of the Game Commission on January 29: E. E. Wilson from Corvallis, George Aiken from Ontario, Dexter Rice from Roseburg, Lew Wallace from Portland, and Charles Riley from Klamath Falls. In his announcement, Martin made an oblique reference to problems with previous members, indicating to the new members how game funds should be spent. "I am interested in the fees collected only in seeing that they are paid to feed our fish and game - not hungry politicians."[50] In a statement released to the press on January 30, the Izaak Walton League of America accused Martin of playing politics by replacing the Game Commission. The League charged that Martin dismissed the group because some members had taken contrary positions to one of Martin's favorite projects, development of water resources in the Pacific Northwest.

> The league is surprised, indeed, that the executive of the state of Oregon should stoop to the methods he has used in venting his personal spleen against the opponents of some of his pet measures, which mean, we believe, the retardation of the game program in Oregon if enacted into legislation.[51]

A final housecleaning came in March. The minutes from the March 8, 1935 Game Commission meeting indicate that staff Engineer Ralph Cowgill would finish his projects before he left. It is not clear whether he left voluntarily or whether he was fired.[52] However, from the 1931-1932 biennium to the 1933-1934 biennium, Cowgill was listed as the Staff Engineer. In the 1935-1936 Biennial Report, Cowgill's name was no longer listed.[53] After Cowgill's departure, those who spoke out most vigorously for the value of fish as opposed to the value of the Bonneville Dam were gone from the Game Commission.

Fleeting Success and Unwanted Credit

The development of Oregon through FDR's New Deal program was described by Richard Neuberger in *Our Promised Land* (1938). A former journalist, Oregon Legislator and new convert to New Deal liberal activism, Neuberger was one of FDR's most ardent supporters.[54] However, when Neuberger described the promises wrought by the PWA dams, his enthusiasm waned. He described Charles Gilbert's study on the Fraser River that showed that blockages to a river stop anadromous fish runs, significantly lowering salmon canning prospects. During the construction of Bonneville and the planning for Grand Coulee many groups lodged complaints fearing that the salmon runs on the Columbia would become extinct, including representatives from the American Nature Association, the Association of Pacific Fisheries, the Izaak Walton League and the American Indian Federation. By 1938 the cost of fishways at Bonneville had climbed to $7,077,200 and Neuberger and others skeptically wondered if the expensive, experimental lifts and ladders would work. "The food of the Indians, the future of an industry, the sport of thousands of anglers, and the usefulness of the most elaborate and costly fish equipment ever built will be at stake when the salmon runs of the near future reach the massive barrier at Bonneville." Even the U. S. Commissioner of Fisheries Frank Bell admitted that the fishways were an experiment, one he hoped "will meet the crisis."[55]

Construction of Bonneville Dam was completed in 1938, and once salmon began returning the fishways were proclaimed a success. An article in the *Corvallis-Gazette Times* on May 18, 1938 characterized the event. "Secretary of Commerce, Daniel Roper announced that the first Chinook salmon arrived at Bonneville this week and successfully ascended the government's $6,500,00 fishways."[56] The 1938 spring run was further characterized as a success by indicating that the manmade improvements were superior to the salmon's natural environment. A *Gazette-Times* article dated June 18, 1938 indicated that the fishways at Bonneville were credited for making life easier for salmon as they provided effortless access to the headwaters of the river.[57] The initial Bonneville fishway success led fishery managers to believe that dams and salmon could coexist, and the Bonneville fishways became the model for further dam building.

This sentiment was expressed by fisheries biologist Willis Rich at a 1939 conference, "Dams and the Problems of Migratory Fishes," held at Stanford University, as discussed in Chapter 2. He presented the paper, "Fishery Problems Raised by the Development of Water Resources." In the presentation, Rich acknowledged that he previously had been pessimistic about the effects of dams on migratory fishes, but the success at Bonneville proved him wrong.[58] Rich denied that all of the problems had been solved and listed the proliferation of dams planned in Washington, Oregon, Idaho and California, specifically listing the ten dams planned for the Columbia and the series of dams planned for the Willamette River and its tributaries, as potential sources of difficulties.[59]

The fishway bypass system that was finally adopted at Bonneville was described by historian William Willingham in *Army Engineers and the Development of Oregon*, a 1983 history of the Army Corp of Engineers. The upstream bypass system included a combination of three fish ladders, two fish lifts and a two separate fish collection systems located in powerhouse and spillway structures. After the first spring run of salmon in 1938 the upstream fish passage system was labeled a success.[60] The downstream migration systems, however, did not prove to be a success even to the Corps of Engineers who "greatly underestimated the problems of downstream migrants."[61] This account failed to indicate that screening of powerhouse turbines had been proposed to protect downstream migration, and that monetary considerations played an important part in discouraging its adoption. A 1994 version of the Army Corp of Engineers efforts by Lisa Mighetto and Wesley Abel in *Saving the Salmon* acknowledged that downstream migration had been a concern for a few fisheries managers. However, this account downplays the sentiments expressed by members and staff of the Washington and Oregon game commissions regarding downstream passage. Thus, the early dams featured no juvenile bypass facilities, or very limited facilities.[62]

The result of neglecting downstream migration through turbines and spillways has been described by *Seattle Times* newspaper reporter William Dietrich in his 1995 book, *Northwest Passage: The Great Columbia River.* Dietrich wrote from the salmon's perspective and described an unnatural environment for a fictional character, Suzy Salmon, created to give the reader a first hand account of a chinook salmon's downstream migration from Idaho to the Pacific Ocean. The rendition included a proliferation of man-made environmental changes that challenged the salmon's natural instincts. In addition to the physical obstacles, Dietrich wrote of predatory fish who profited from the slackwater lake environment behind the dams, and who have learned to take advantage of the fingerlings disorientation at the dams during their journey downstream. At every point, predators feed on young salmon that have been knocked unconscious by turbine blades, or are woozy from a spillway fall.

By 1995, downstream migrating salmon would encounter not just one or two dams but numerous blockades with a significant loss of each group at each dam. Upon the approach to a dam like Bonneville, Suzy was exposed to a slackwater lake behind each dam, as opposed to strong river currents that would otherwise carry Suzy to the ocean. As she approached the first dam she was pulled eighty feet below the surface into a huge, dark penstock where salmon are expelled dizzy and disoriented.[63] Up to fifteen percent of juvenile salmon are lost to turbines during downstream migration while traveling through one dam, and a similar estimate holds for the survival rate of downstream migrants over spillways where, if the fall doesn't kill salmon, nitrogen saturation will.[64]

Few historical accounts acknowledge that facilities for turbine screening had been strongly suggested; other accounts do what Thomas Lally had feared. They place the credit and the blame for the Bonneville bypass system on the cooperative efforts of the Oregon and Washington fish and game commissions. During the June 29, 1939 symposium "Dams and the Problems of Migratory Fishes," U. S. Fisheries Bureau biologist Harlan B. Holmes, presented the paper, "The Passage of Fish at Bonneville Dam." In the 1939 document, he did what Lally had feared at the December 10, 1934 IFCC meeting, one which Holmes also had attended. Holmes attributed credit for the fish passage devices to the fish and game commissions of Oregon and Washington and did not note the objections that Lally, Cowgill and others had raised. "As a result of the cooperative procedure that was adopted, the credit or blame, as the case may be, belongs jointly to this rather composite group of workers."[65] A similar statement may be found in a 1995 book, *Design of Fishways and Other Fish Facilities*. In a discussion on the history of the Bonneville Dam, author Clay Charles wrote about the innovations included in construction and described the use of fish locks, fish ladders, and the collection system indicating that they were very advanced for the time.

> (The fish bypass systems) were accepted as a standard on many installations in a wide range of locations. The U. S. Fish and Wildlife Service and the Departments of Fisheries from the State of Washington and Oregon were the agencies responsible for the biological details of these fishways, and to their biologists and engineers goes credit for this tremendous advance.[66]

Conclusion

Several recurring themes may be noted from the fishway debate. First, all parties acknowledged that there was a lack of information regarding fishways and fish habits to adequately inform the construction of fish passage facilities over Bonneville. That the people involved with the fishway planning and construction, primarily engineers, could be referred to as "fishway experts" was a serious misnomer since all groups acknowledged that the construction of fishways on a dam as high as Bonneville was an unprecedented experiment. The expertise gained from involvement with Bonneville made participants experts, not experience gained prior to Bonneville. Fisheries managers were willing to chance the great experiment because they believed they could rely on artificial propagation should the new fishways technology fail.

Unquestioned acceptance of artificial propagation is the second recurring theme: each group in the fishway debate suggested that fish hatcheries were an integral part of any fisheries management plan. Conservationist William Finley included propagation as part of the fisheries upkeep, fish and game commissions wanted hatcheries to mitigate the effects of dam construction, and businessman David Eccles suggested immediate construction of fish hatcheries to take over should the fish passage system fail. No one questioned the efficacy of fish propagation, they only argued over who should pay for fish hatchery facilities. Historically, fish hatcheries had been used to supplement Oregon's salmon runs, not replace them. The possible loss of fish populations due to dam construction was countered with a reliance on the mass production techniques of fish hatcheries. If the fish ladders did not work, fish hatcheries could replace lost runs. Rather than augmenting nature, artificial propagation became a replacement for nature.

The economic framework in which arguments were made to evaluate fish passage facilities pitted the value of salmon runs against the value and cost of technology. With respect to Bonneville, the cost of fish passage facilities may be ascertained, the benefits of hydropower may be estimated, but the actual monetary value of the salmon runs was incomprehensible. When forced to compete with economics, nature almost always loses because the monetary values placed on any factor is extremely subjective. Winning such an argument then depends on who is making the argument, the values he or she may hold, and the criteria used to support their beliefs. Finley valued the Columbia fishery at $200,000,000, the IFCC put the commercial value of the Columbia fishery at $10,000,000, while Eccles placed the value of the sports and commercial fishery at $35,000,000. The 1934 Fish and Wildlife Conservation Act required that federal agencies balance provision of devices like fish ladders or lifts against the economic feasibility of such installation. In 1946 Congress amended the Act to require that federal agencies consult with fish and wildlife agencies during the planning of water resource development projects, and in 1958 the Act was further amended to mandate that equal consideration be given to fish and wildlife activities in conjunction with other uses of a river.[67] That evolution came too late to affect Bonneville before Bonneville became the standard. The success of the fishladders showed that dams and anadromous fish could coexist and led to the proliferation of federal dams in the Columbia River Basin before long term scientific research could prove otherwise.

Finally, the dismissal of the 1934 Commission for refusing to approve what they believed were inadequate plans for Bonneville fishways represents a theme that will be repeated. Most historical accounts make the Oregon and Washington Game Commissions look like willing participants in the planning and implementation of the Bonneville fishways. The 1934 Commission did not acquiesce like Oregon Fish Commission Chair John Veatch who stated that their charge was advisory at best. The group expanded its charge as they did their fishway plans and it made demands of the federal government. While many of their demands achieved fruition eventually, the people who made the demands did not. For their efforts, the Oregon Game Commissioners were fired. The loss of personnel who opposed water development projects was a theme that was repeated when E. E. Wilson served on the Game Commission.

Chapter 3 Endnotes

[1] Finley's report (no title) to the State Planning Board and Advisory Research Council, September 20, 1935, Oregon State Planning Board, RG 158, Series IV, folder Minutes, committees and subcommittees, OSU Archives; see also Taylor (1996) when competition between commercial fishers and sports fisher led Governor West in 1914 and Governor Withycombe in 1915 to purge the ranks of the Fish and Game Commission, pp. 283-284.

[2] Joseph Cone and Sandy Ridlington, eds., *The Northwest Salmon Crisis: A Documentary History* (Corvallis, OR: Oregon State Univ. Press, 1996), pp. 75-85.

[3] Frank B. Wire, "A Brief History of the Oregon State Game Commission," *The Oregon Democrat*, 1938, 12:11; see also Willingham (1983), pp. 190-195; Mighetto and Ebel (1994), pp. 35-36.

[4] Taylor (1996), pp. 256-289.

[5] Biennial Report of the Oregon Game Commission, 1935-36, (Salem, OR: State Printing Department, 1936), pp. 1-6.

[6] Biennial Report of the Oregon Game Commission, 1931-1932, (Salem, OR: State Printing Department, 1932), p. 6.

[7] Ibid., p. 3.

[8] Ibid.

[9] Willingham (1983), pp. 93-95; Mighetto and Ebel (1994), pp. 49-53; see also Robbins (1997), pp. 271-283; Cone and Ridlington (1996), pp. 95-99; for an in-depth account of Martin's and McNary's lobbying efforts, see Dietrich (1995), pp. 268-269.

[10] Willingham (1983), pp. 95-99; see also Mighetto and Ebel (1994), pp. 53-58.

[11] Mighetto and Ebel (1994), p. 103; see also Cone and Ridlington (1996), pp. 271-272.

[12] Porter (1995), pp. 3-8.

[13] Lichatowich (1999), p. 183.

[14] MINUTES/October 8, 1934, p. 1124.

[15] MINUTES/November 19, 1934, p. 1133.

[16] Ibid.

[17] Eccles, David, "Is King Chinook Doomed?" *Commonwealth, Inc., The Business Survey of the Portland Area* 2(7) 1935: 2-4, FINLEY, Box 7, folder: Salmon & Bonneville.

[18] MINUTES/December 10, 1934, p. 1142

[19] Ibid., p. 1138.

[20] Ibid., p. 1139.

[21] Ibid., pp. 1139, 1141.

[22] Ibid., p. 1143.

[23] Ibid.

[24] Ibid.

[25] Ibid., p. 1143.

[26] Ibid., p. 1141.

[27] Ibid., p. 1142.

[28] Ibid.

[29] Ibid., p. 1142.

[30] Ibid., p. 1144.

[31] Ibid.

[32] Ibid.

[33] Ibid., 1145.

[34] *Corvallis Gazette-Times*, "Game Commissions After Fishways Agreement Pact," Dec. 11, 1934, p. 1, col. 5.

[35] Eccles, p. 2. Please note: the frequent omission of articles like "the" and "a" are the author's choice.

[36] Ibid., p. 3.

[37] Ibid., p. 4.

[38] Ibid.

[39] Ibid.

[40] William Finley, "Salmon of the Columbia River," p. 2, FINLEY, Box 7, folder: Salmon and Bonneville.

[41] Ibid., pp. 2-3.

[42] Ibid., pp. 3-4.

[43] Ibid., p. 4.

[44] Portland Chamber of Commerce, "Report of the Bonneville Dam Fishways Committee," September 18, 1935, pp. 1-2, FINLEY, Box 7, folder: Salmon and Bonneville.

[45] Ibid., p. 2.

[46] White (1995), pp. 92-95.

[47] *Corvallis Gazette-Times*, "Governor Martin is for Progress" Jan. 14, 1935, p. 1, col. 1.

[48] Biennial Report of the Oregon Game Commission, 1933-34, (Salem, OR: State Printing Department, 1934), p. 5.

[49] Ibid.

[50] *The Oregonian*, February 2, 1935, p. 1, col. 2.

[51] *Corvallis Gazette-Times*, "Fishing League Attacks Martin," January 30, 1935, p. 1, col. 1.

[52] WILSON/March 8, 1935, Series I, folder: Minutes, Game Commission, 1935-1949.

[53] Biennial Report of the Oregon Game Commission, 1931-1932 (p. 2), 1933-1934 (p. 2), 1935-1936 (p. 2.

[54] Robbins (1997), p. 294.

[55] Richard, Neuberger, *Our Promised Land* (Moscow, ID: Univ. of Idaho Press, 1989/1938), pp. 125-139.

[56]*Corvallis-Gazette Times*, "Chinook Salmon Descend Dam Fishways," May 18, 1938, p. 1, col. 1.

[57] *Corvallis Gazette-Times*, "Bonneville Dam Help to Fish," June 19, 1938, p. 1, col. 1.

[58] Rich (1940), pp. 176-181.

[59] Ibid.

[60] Willingham (1983), pp. 196-197.

[61] Ibid., pp. 196-197.

[62] Mighetto and Ebel (1994), pp. 103-104.

[63] Dietrich (1995), pp. 342-343.

[64] Ibid.

[65] Harlan B. Holmes, "The Passage of Fish at Bonneville Dam," *Stanford Ichthyological Bulletin*, 1940, 6:188-193.

[66] Charles H. Clay, *Design of Fishways and Other Fish Facilities*, (Boca Raton, FL: Lewis Publishers, 1995), p. 6.

[67] Mighetto and Ebel (1994), p. 103; see also Cone and Ridlington (1996), pp. 271-272.

Chapter 4 - E. E. Wilson Challenges Artificial Propagation

Fish hatchery concerns linked three events in 1935: Oregon State College opened the Department of Fish and Game Management as discussed in Chapter 2, Eddy Elbridge Wilson was appointed to the Oregon Game Commission as mentioned in Chapter 3, and the Oregon State Planning Commission announced the Willamette Valley Project, a water resource development project for the Willamette River. At a special meeting in his office on February 2, 1935 Governor Martin welcomed the new appointees of the Oregon Game Commission. The Commission was charged with overseeing and protecting recreational fisheries and game for the benefit of the Oregon sportsman. To the new members the Governor said, "You have an important mission to perform in the conservation of the game and fish resources of the State. I feel that our game and fish are among our most important natural resources."[1]

New member E. E. Wilson became a central figure on the Commission as only he, of the five new members, was retained through the administrations of three other Oregon governors. Wilson served on the Commission for fourteen years from 1935-1949, functioning as chair for ten of those years. A lifetime resident of Corvallis and an 1889 graduate of Oregon Agricultural College, the previous name of Oregon State College, Wilson's activities served to form important links between the Commission and the College. Although Wilson was an avid sportsman, professionally he was a lawyer and a banker. The demands of the positions he held on the commission required that he become educated in wildlife processes. To do this he often consulted with many OSC professors, eventually hiring staff and graduates for work on the

Commission. Because he lived in the same city in which the College was located, and because of his insistence on professionalizing the workings of the Commission, Wilson became the conduit between the two groups. The professional associations which Wilson experienced as a member of the commission led him to support natural propagation rather than artificial propagation as the best means for saving salmon runs. Once he came to understand that the WVP would bring some high dams and an increased reliance on fish hatcheries to the Willamette Valley, Wilson attempted to block the project. This section will discuss E. E. Wilson's work on the Oregon Game Commission, his opposition to the Willamette Valley Project and his separation from the Commission due to that opposition.

The Willamette Valley Project

Oregon Governor Charles Martin, a Progressive Era conservationist, made appointments to two different state groups on February 2, 1935: the Oregon State Planning Board and the Oregon Game Commission. Martin named nine members for the Oregon State Planning Board, a group he referred to as his "unofficial brain trust," people who embodied specialized knowledge of the state coupled with the ability to plan for its progress. One of Martin's appointees was William Schoenfeld, Dean of the School of Agriculture at OSC.[2] Three months later the Planning Board presented the 1935 Willamette Valley Project Report to the Governor, a project that would eventually be opposed by the Game Commission. On May 8, 1935, the Governor accepted *The Willamette Valley Project Report* from the Oregon State Planning Board, a preliminary plan that proposed the building of reservoirs on the Willamette River and its tributaries for the improvement of irrigation, flood control, navigation, power development and stream purification.[3] Two reports on the Willamette Valley Project were produced within a year, both primarily emphasizing agriculture and a coordinated water plan. The 1936 report was issued to present a more balanced approach to regional development, and the emphasis on agriculture was more evenly distributed with forestry, manufacturing and transportation. The 1935 report is a 128 page document that addressed many issues that would serve to build an expanding economy in the Valley: agriculture, irrigation, flood control, forestry, mining, navigation, power production, transportation, and recreation. Of those 128 pages, thirty were devoted to analyzing agriculture; no other subject was addressed at such length or in such detail. The need for water resource development could be traced to the area's precipitation patterns.

Unlike most of the arid West, the Willamette Valley offered a mild climate with adequate rainfall, about 40 inches of rain per year. However, the rainfall was not evenly distributed. The majority of precipitation fell in the winter months, and the summer months suffered from too little rain. According to the 1935 Report, the average rainfall for the three months of fall were 11.72 inches; for the three winter months 18.84 inches; for the three months of spring were 9.12; and for the three months of summer rainfall averaged only 2.40 inches.[4] The exceptional rain in fall and winter, often coupled with snow melt, led to flooding on the Valley floor; lack of precipitation in the summer further narrowed agricultural opportunities. Capturing the flood waters in reservoirs and not allowing them to flow onto bottom lands would make that land available for farming. Additionally, the stored flood waters could then be used to irrigate farmland during the dry summer months. While many issues were addressed in the report, the primary emphasis was to improve upon nature's precipitation miscalculation. The reservoirs would guarantee what nature did not: more than adequate water to promote agricultural expansion.

When the 1935 Report was written, only John Veatch, chair of the Fish Commission, contributed to the section on fishing, emphasizing the commercial value of Oregon's fisheries. As compared to the thirty page entry allotted for analyzing agricultural interests, analysis of fisheries interests took only one and a half pages. This analysis acknowledged the importance of the Willamette River as a recreational resource and as a seed stream for the Columbia River. Veatch noted that because the Willamette was located directly down river from the location of the Bonneville Dam construction site located on the Columbia River, the importance of the Willamette as a spawning stream was expected to increase after the construction was completed. The report also recognized that the Fish Commission operated five fish hatcheries and egg-taking stations to augment natural runs, and argued that every effort should be made to maintain those hatcheries. In addition, the report indicated that further hatcheries may be needed to mitigate lost habitat caused by dam construction.[5]

Veatch and Harlan B. Holmes, Aquatic Biologist with the U. S. Bureau of Fisheries in Portland co-authored the two-page fisheries section of the 1936 Report. This entry more clearly defined the interstate role the Willamette River played as a seed stream for the Columbia River. In addition, it recognized recreation for fishermen as a growing source of revenue and mentioned the Game Commission's management role. Fish hatcheries figured prominently as key components of the state's plan to maintain fish populations for both commercial and recreational use. This revision differed from the 1935 version as it stated benefits as well as harmful effects for fisheries. Regulated stream flow provided by the reservoir was expected to add more water for fish runs in the summer. This additional water was expected to flush out pollutants from the river. However, again the report acknowledged that the assessment of harmful effects produced by the dams upon fisheries were mostly unknown and steps must be taken to deal with problems when they surface. "Obviously, this phase of the Coordinated Water Plan has not been given sufficient attention. Further study should be made by all agencies interested in the conservation of fish in the valley. But such investigation must be carried out in a spirit of willingness to face the problems which undoubtedly will arise."[6]

The WVP would be funded by federal monies made available to states wishing to improve land and water resources for the benefit of the present residents, and to provide a proper reception to refugees from the Depression and the Dust Bowl. The report described the Willamette Valley as defined by the Coast Range to the west, the Cascade mountain range to the east, and the Calapooia mountains to the south, and the Columbia River to the north, into which the Willamette River flows. This area represented 13,000 square miles with a 1935 population of 600,000 people. Of the 8,500,000 acres, approximately 2,858,011 acres were classed as agricultural. Soil surveys indicated that 873,722 acres needed drainage and 740,000 acres were well-suited to irrigation to open up more farmland for settlement. "...adoption of this federal project will provide a means whereby approximately 2,500 families may be located in the near future, and 25,000 or more families may be gradually resettled as water conservation, flood control, supplemental irrigation and drainage work and checking soil erosion, with forest, mineral resources and industrial development are accomplished."[7]

Robbins (1978) wrote that the original plans for the WVP would bring in an estimated $62,000,000 of federal funds for the seven dam system throughout the valley, though by the end of Wilson's tenure, that figure would soar as the project expanded. Initially, the majority of the funding was earmarked for flood control and irrigation development through the construction of seven dams on the Willamette River and its tributaries. While the project was proposed in 1935, it was not approved until 1939 because it had to run the gauntlet of bureaucratic and Congressional approval. Porter (1995) wrote that after 1902 Congress certified navigation projects the Army engineers proposed based on their benefit to the community. Because any project would bring construction money into a community the engineers accumulated a backlog of requests, many of which had the support of influential backers in the Capitol. The Army engineers adopted cost-benefit analysis as early as 1920 to evaluate projects outside of the arena of power politics. The engineers needed an acceptable process to justify acceptance or denial of water development projects to Congress and cost-benefit analysis provided procedural regularity and gave evidence of fairness during selection.

Congress adopted the 1936 Flood Control Act with the provision that the cost of a project must be exceeded by its benefits. The cost-benefit hurdle could be overcome however, "by itemizing benefits until they exceeded the cost, or by fixing potential benefits as a cap on expenditures." After the Corps had studied the nation's major river basins for development potential and produced the 308 reports, they were flooded with requests that were evaluated by cost-benefit analysis. However, the evaluation process involved examination at various levels of the Corp bureaucracy which could take years to complete, only to then be challenged Congress. In addition, all federal agencies had not adopted a single cost-benefit formula, and other bureaucracies, like the Department of Agriculture, the Bureau of Reclamation and the Department of the Interior, challenged the Corps number when projects they preferred were denied.[8] From inception to final approval, Robbins documented how business and political factions around the state worked unceasingly to get the PWV project through the many avenues of state and federal approval.

U. S. Army Engineers work on water resource development of the Willamette River began in 1932; by 1937 the Engineers issued House Document 544 regarding the development of the Willamette basin, which Congress approved in 1939 as part of the omnibus flood control bill. Appropriation for the first three dams was authorized in 1939 and construction began that summer. Economic expansion and development did not initially threaten the commercial fishing industry and no opposition to the Project surfaced from that group. Questions regarding the effect of high dams blocking anadromous fish runs, and the significant loss of fish habitat caused by reservoirs did not surface until construction of the first two dams began.

However, some informed sportsmen and wildlife conservationists questioned the project from the beginning. According to Robbins, William L. Finley, a nationally recognized naturalist and a Corvallis resident, provided major opposition to the project and the effect of dams on wildlife. Finley, however, was quickly dismissed as a fringe element. Robbins wrote that maintaining the semblance of public approval for the project was essential to gain congressional approval. When project boosters sensed opposition, they circled their wagons in defense. Efforts to quash opposition to the project was mounted by the substantial clout wielded by the project's backers.[9] Support from business and commercial organizations in the valley coupled with the political support of individuals such as Governor Martin, State Senator Douglas McKay, and Senator Charles McNary made opposition to the project almost impossible. However, much of the project was stalled due to events leading to World War II, after which "strong opposition to the high dams" was voiced.[10]

Alleged Numbers Falsification at Fish Hatcheries

The Game Commission dealt with its share of controversies, but confidence in the hatchery system as supplemental to natural runs rarely wavered. A review of Game Commission minutes from 1935-40 shows that at every monthly meeting hatchery construction and expansion were discussed and implemented.[11] One of the major controversies that surfaced at this time concerned allegations of falsifying of Commission statistics relating to fish produced in hatcheries and fish planted in streams. In much the same way that the efficacy of hatcheries was questioned in Canada, they were being questioned in Oregon. Finally, at the end of 1939 the Commission sent formal objections to the WVP to the Oregon Congressional delegation.

At the April 8, 1939 Commission meeting, chair Dexter Rice resigned, indicating that he did not have time to adequately address Commission and the commission elected E. E. Wilson as the new chair. At the same meeting, the new chair was required to handle one of the more controversial issues the Commission had to face. Members from the Oregon Wildlife Federation and the Izaak Walton League presented testimony from a former Game Commission employee from the Union hatchery, Robert Moore, alleging that the numbers of fish produced in the hatcheries, and the numbers of fish planted in Oregon's lakes and streams had been falsified. Further, Moore charged that the Commission was lax in its fish planting process resulting in the deaths of thousands of fish. Finally, Moore wrote that hatchery employees were allowed to use hatchery supplies for their own use. Rice replied to the allegations saying that Moore was a disgruntled employee and the charges were under investigation, but Rice found little evidence to back up Moore's claims.[12] Again on June 17, 1939 representatives from the Oregon Wildlife Federation and the Izaak Walton League addressed the Commission meeting. Since their presentation of allegations about the falsifying of Commission records in April, the representatives wanted to know what action had been taken to rectify the matter. In addition, they indicated that they had further accounts of inaccurate fish counts from a Wallowa county release. Wilson responded that the Commission was investigating all suggestions and thanked them for their interest.[13]

The controversy was further inflamed when in August, articles began to appear in *The Oregon Journal* newspaper asking where were the fish in Oregon's lakes and streams. An August 18 article headline exclaimed, "Expensive Trout Widely Planted; Where Are They?" The article stated that according to Game Commission records, 45,000,000 fish were planted last year, yet fishermen were hard pressed to account for any of them.[14] An August 21 article, "The Trout That Can't Be Found In the Streams" told of unprofessional fish planting processes that resulted in dead fish.[15] In addition, an August 23 article, "What Becomes of Trout Planted in Oregon Streams?" indicated that hatchery fish were subject to numerous diseases, were often lost to predators after planting, and that Commission statistics were padded.[16]

The reoccurrence of this controversy prompted another former hatchery employee, K. E. Morton, to write a response to Commission member George Aiken on November 2, 1939 making allegations similar to those made by Robert Moore in April. Merton charged that the fish in the McKenzie River Hatchery were diseased and underfed prior to liberation; Merton also charged that the numbers of fish raised and planted were falsified to make up for the losses caused by disease. Merton wrote that he worked for the Commission for six years and resigned because he was dissatisfied with the conditions in which he had to raise fish, and didn't wish to reenter the service "...as long as it is run by its present standards." [17] In a following letter to Aiken on November 6, 1939, Merton volunteered to bring his information to the Commission if he could be guaranteed a closed meeting.[18]

When Wilson was approached with Merton's offer, he wrote to Aiken on November 11, 1939 that he had been made aware of similar incidents, and he believed they were the exception rather than the rule. Because of his suspicions regarding hatchery processes, Wilson wrote that two years ago he began a hatcheryman's school that meet for a week once a year at the college. During that time all hatchery personnel were exposed to professional standards that would help them avoid spreading disease and weakness among the hatchery fish. Regarding the presentation of Merton's material to the Commission, Wilson declined.

> I have no desire to withhold anything from my fellow Commissioners, but I have some misgivings about the making public of this letter as being the most efficacious method of attaining the desired results. Generally speaking, if reformed procedure is desirable, I believe in the case of the Commission's work, that result can be best obtained from within rather than from without.[19]

In addition, an undated, anonymous letter accompanied the previous letters in Wilson's files which repeated the charges made by Moore and Merton in informed detail. The writer asked why the two men had not been called before the Commission to make their charges even though individual Commissioners had suggested they appear.[20] However, commission minutes indicate that the subject was not discussed during regularly scheduled meetings, and the people making the allegations did not appear before the group. At the January 1940 meeting, the Commission inconspicuously addressed the problems of professional planting of fish. The Commission created a new Department of Fish Liberation that was separate and distinct from other departments which would have charge of the planting of all fish produced in the Commission fish hatcheries.[21] While this action appeared to address fish planting problems, it also added a considerable expense to the production of hatchery fish.

Opposition to the Willamette Valley Project

Before congressional approval of the WVP in 1939, the Oregon Game Commission was approached twice by WVP representatives who explained the project and its effects. A representative of the State Planning Board briefed the commission on Feb. 19, 1937 regarding the Willamette Valley Flood Control project that may bring several high dams to tributaries of the Willamette Rivers. He recommended they attend an upcoming meeting but no action was taken.[22] On June 10, 1938 Harlan Holmes of U.S. Bureau of Fisheries attended the monthly meeting to brief the group regarding the WVP. He urged the group to cooperate in determining the best means of conserving fish in affected streams in the event the WVP should be approved by congress and Wilson and Ryckman form a committee to meet with the Army engineers. At the same meeting Wilson announced to the group that William Finley had advised him that the WVP was likely to pass congress unless immediate action was taken.[23] However, the Game Commission failed to voice opposition to the project until after funding had been approved. Wilson was a team player experienced in local and state politics. Rarely did he voice opinions not supported by the group. It was not until he took leadership of the group that opposition to the WVP surfaced. Additionally, minutes from commission meetings often told a partial story. Wilson's letters show that commission business was often conducted outside regularly scheduled meetings.

At the December 1939 meeting, a letter from William Finley announced that the Willamette Valley flood control bill had passed Congress. Finley wrote that the project planned to erect seven dams on the Santiam, McKenzie and Willamette Rivers which would be detrimental to fish life and would affect salmon runs and sport fishing. The WVP offered both the Game Commission and the Fish Commission additional hatchery facilities as a compensation for lost fish habitat caused by dam construction. Finley invited both the Fish Commission and the Game Commission to take action in the matter. Hugh Mitchell, Director of Fish Hatcheries for the Oregon Fish Commission announced that he was assembling information to be presented to Congress regarding the effects the dams would have on fish runs. Under the direction of Wilson who was acutely aware that fish hatcheries were not without their inherent problems, the commission finally voiced opposition to the project. The Game Commission unanimously instructed its staff to prepare like reports "...showing the disastrous effect said dams would have on sport fishing..." and to cooperate with the Fish Commission in the report. Further, the Game Commission directed that the joint report be sent to all Oregon senators and congressmen, representing the first formal objection to the project by the commission.[24]

However, from this point, work on many projects began to slow as the nation prepared itself for entry into World War II. At the September 14, 1940 Commission meeting, the group resolved that any permanent employee called to duty be granted a leave without pay and be guaranteed reinstatement upon return from service. In the ensuing years the Commission found that not only were personnel taking leave, the supplies necessary to work on standing projects were no longer available, and much of the Commission's work stopped. Opposition to the project, however, did not stop.

On August 24, 1940 Wilson chaired a tri-state meeting with other fish and game commissioners from Washington and Idaho. All three states were joined by the Columbia River and wished to forge a coordinated effort on fisheries. Many issues were discussed without coming to resolution except when Wilson introduced the subject of the Willamette Valley Project. The group adopted a resolution that opposed the Project as the construction of high dams would be detrimental to fisheries not only in Oregon, but also in Washington and Idaho. One section of the resolution stated

> ...a very large portion of the spring run of Chinook salmon in the Columbia River is produced in said tributaries of the Willamette River, and it will not be possible by the construction of hatcheries and artificial propagation to compensate the loss resulting from the construction of said dams...[25]

Further, the resolution cited a report of the Army engineers which indicated that flood control on the Willamette could be handled by levees and bank revetments at $33,000,000, half the cost of the present project, and recommended the use of levees and bank revetments rather than high dams.[26] On a national level another stronger statement regarding reclamation efforts was made by the Western Association of State and Game Commissioners at the 1941 annual meeting on June 5-7 which Wilson attended. Resolution 9 stated that "...it is not sound conservation to destroy existing broad public values in order to create a new value for the benefit of some special interest or local minority" and denounced high dams which sacrifice aquatic values for the purpose of water conservation. The resolution denounced reclamation projects in which the surveys and opinion of engineers were the only expert opinions required.[27]

> THEREFORE BE IT RESOLVED by the Western Association of State Game and Fish Commissioners, that before any more high dams be authorized or built the advice of scientists and economists, to be selected by the or states involved, be included in the surveys; that their reports be made public; and that unless reports clearly show that the new values replacing existing ones exceed the latter from the broad public standpoint, the construction of said high dams be disapproved...we protest as being both erroneous and misleading the present designation of such water exploitation projects as "public water conservation."[28]

Copies of the resolution were sent to the U. S. President, the Secretary of War, the Secretary of the Interior, the Federal Power Commission, the Bureau of Reclamation, Army Engineers, and the River and Harbors Committee of the House of Representatives.

Habitat Study Preparation Inadequate

Surveys to determine the costs to wildlife caused by dam construction were considered essential in the original 1935 WVP report and reiterated in the 1936 WVP report. In 1937 the Research Committee of the Oregon State Planning Board issued a position paper, *Problems and Recommendations for Conservation of Fish and Fisheries on Oregon Streams*. The paper acknowledged the need for surveys as outlined in the 1936 WVP report. "A study should be made of the effects which the proposed construction of storage reservoirs, of irrigation and drainage systems, river pollution and other factors have on the permanency of the fish resources in the valley."[29] Agencies requested to undertake these studies were the U.S. Bureau of Fisheries, the Oregon Fish Commission and the Oregon Game Commission. The paper described the biology and behavior of fresh water fish as well as anadromous fish. With respect to the anadromous runs the study indicated that the effects of dams were an unknown, therefore studies were imperative.[30] The committee recommended "...biological studies of all streams important for supporting fish life...Until these data are available it is difficult if not impossible to prepare sound plans which will provide for full use of the water for various purposes."[31]

While many groups believed that studies were important, Wilson's letters indicated that the U. S. Army Engineers were not as interested in what these studies had to say, only that they were completed so that the engineers could begin construction of the third and fourth dams in the WVP. In 1946, Game Commission minutes indicate that in the interest of complying with recommendations, the Commission began its own survey of the Willamette and its tributaries. "The Chairman suggested that the Game Commission should obtain definite information on which to base recommendations to the army engineers regarding whatever remedial action is necessary for the preservation of game fishery resources in the upper Willamette and the North Santiam rivers."[32] However, in mid-March of 1947 Wilson discovered by accident that the Army engineers were proceeding with construction plans on two dams and expected survey results to be presented to them on April 1, 1947. In a March 14, 1947 letter to Game Commissioner Theodore Conn, Wilson wrote of his surprise regarding the expected survey report indicating that the work was not completed. "You will recall that I did not regard our survey report on the North Santiam as sufficiently full upon which to base a satisfactory presentation for full restitution. But there seems to be nothing else to do but to go ahead on what we have and what we can filch from other sources, prepare a statement and file it by April first."[33]

In further investigations, Wilson discovered that not only was the Commission's report not ready, neither was the report by the U. S. Fish and Wildlife Service. In addition, Wilson also discovered that the plans by the U. S. Army Engineers had not as yet determined the locations of some of the proposed dams. "How can they consistently ask us to prepare our report and recommendations on a project until they are in a position to inform us definitely what those plans are, as for example where the dam is to be located?"[34] Wilson continued that he suspected underlying causes to the urgency of the engineer's requests. "...Is it not possible that there is some power higher up that has issued an ultimatum to the engineers demanding conclusion of their work and they are endeavoring to comply regardless of the state of the plans?"[35]

Wilson attended the previously mentioned meeting on April 1 prepared to make his report but was able to stall until April 9 because other participants were not well prepared either.[36] At the April 9 meeting, Wilson again resisted approving any plans by indicating that the engineer's plans were not complete. He deferred his position until he could gain the approval of the Commission at their monthly meeting on April 12. In an April 14 letter to U. S. Engineers District Manager O. C. Walsh, Wilson related that the Commission denied approval of construction plans and deferred the Commission report until the engineers had completed their plans.[37] When Wilson wanted to stall for time, he appealed to the Oregon attorney general for opinions on the law that governs water resource development projects. When it was clear that the Commission had no recourse but to accept fish hatcheries as compensation for lost fish habitat, Wilson worked to devise a plan which would gain as much money and resources for the Commission as possible. While Wilson was winning small battles in the name of fishery conservation, ultimately his disagreements with the powers that be would cause him to lose the war.

Ryckman, Needham, Dams and Fish Hatcheries

The staff Wilson hired affected his opinion about dams and fish. From the beginning of the Oregon Game Commission in 1920 until 1943, Matt Ryckman, a fish culturist, directed the fisheries staff. After Ryckman's death Wilson hired a professional biologist to take Ryckman's place. Wilson chose Paul Needham who had previously consulted with the Game Commission and also had spoken at hatchery short course presentations. A comparison between Matt Ryckman and Paul Needham gives a striking example of the different fisheries management styles afforded by the normal science of a fish culturist as opposed to the normal science of a fisheries biologist.

Ryckman was born in 1868 and died in 1943, after forty one years of service to the State of Oregon. After his death a testimonial to him was placed in the Game Commission minutes. Ryckman started work with the state as a fish culturist in 1902 at the McKenzie Hatchery, the only fish hatchery then run by the state. He was credited with setting up the state hatchery system that by 1943 included sixteen hatcheries that produced over twenty five million game fish to stock Oregon's lakes and streams. Ryckman's testimonial stated that, "At the time he first became connected with the work, the subject of the artificial propagation of fish was little understood and highly experimental. Much of the development of that subject and many of the practices in use today are the result of his study and experimentation."[38] Ryckman was considered a pioneer in trout hatching and one of his accomplishments was an innovation that was important to acclimatization experiments with fish. "Outstanding among these was the discovery that after fish eggs had been eyed (fertilized) they could be safely shipped considerable distances, even to foreign countries, without loss of vitality."[39] He also spent a period of time on salmon investigations for the U. S. Bureau of Fisheries investigating the fishing situation in Alaska.[40]

Despite Ryckman's expertise he did not publish professional papers. Ryckman's legacy appeared to have been passed on orally to many of his colleagues who expressed appreciation for the knowledge he shared with them.[41] From 1929 to 1934 he wrote reports for the Game Commission's biennial report where he outlined his department's activities. He supervised hatchery construction and maintenance, provided statistics confirming hatchery production, and initiated some scientific research. In the 1929-1930 report he wrote of centralizing state hatchery facilities as a way to save money. He also regretted having to use imported cutthroat egg for planting in coastal streams as he found that native eggs worked better.[42] Ryckman lamented a lack of information regarding fish life histories that inhibited his work. "Scientific knowledge as to habits of game fish, methods of handling eggs and fry, comparative value of certain foods at different stages of development, means of combating disease and parasites, would do much toward solving the problems of successful fish culture."[43] He also described tagging studies begun in 1929 in cooperation with the U. S. Bureau of Fisheries to determine the time of migration of steelhead in the Rogue River, and recommended setting aside funds to encourage other fisheries research.[44]

Ryckman noted that the Depression had curtailed work for his department in the 1931-1932 biennial report, but he indicated that his department had conformed to the 10-Year Program in expanding hatchery facilities. Because of increased numbers of fishers, Ryckman believed that the expansion was justified. "Better angling has been enjoyed in the waters of the state of Oregon during the past year than for many years. This must be the result of consistent and increasing liberations of game fish in all Oregon's streams and lakes, for with more than 75,000 anglers fishing in all parts of Oregon, natural supply would soon be exhausted."[45] Improvements at hatcheries included continuation of consolidation by expanding of present facilities, as well as improvements in egg-taking methodologies, game fish liberations and fish salvaging. He indicated that the new Game Commission policy of releasing legal-sized trout caused problems with the fish food supply when in the care of the hatchery, and the natural food supply after liberation.[46]

Ryckman wrote in the 1933-1934 report that the Depression had reduced the department's income inhibiting hatchery department work. "Little work could be done except general operation and we were forced to liberate much smaller fish on an average than usual because we were unable to handle the expense of caring for them at the hatcheries until they reached the desired size of six inches or more."[47] Nevertheless, he described extensive construction work at each hatchery completed with labor provided through the use of federal work project monies. "Oregon hatcheries can now be compared favorably with those of any state."[48] He also described research projects begun with the U. S. Bureau of Fisheries to examine fish diseases and proper fish foods and the problems of fish predators, suggesting that the "...regular hatcheryman with proper instructions can do much of this work without additional cost to the department."[49]

While Ryckman seemed interested in scientific research, the Game Commission was not committed to long term scientific research projects, as Taylor (1996) wrote previously. After the 1934 Biennial Report, Ryckman did not personally sign the section from the fisheries department. Instead, the text of the reports were submitted and signed by Game Commission members, not the staff. After Ryckman's death in 1943, his position was not filled until 1945 when Paul Needham became Director of Fisheries. Needham received his education at Cornell University, earning a Ph.D. in Limnology in 1928. He taught until 1929 when he began working for the U. S. Bureau of Fisheries where he served as an aquatic biologist in charge of California Trout Investigations from 1938-1945. He stayed with the Bureau, which became the U. S. Fish and Wildlife Service in 1940, until he came to Oregon as Director of Fisheries for the Oregon Game Commission. After leaving the Game Commission in 1948, Needham became professor in the Department of Zoology at University of California, Berkeley.[50] He published numerous articles and in 1938 he published a book, *Trout Streams: Conditions that Determine Their Productivity and Suggestions for Stream and Lake Management.* In addition to fish descriptions, physical and chemical descriptions of lakes and streams, distribution of food sources for trout, the book included a chapter on trout propagation. Here Needham wrote of Foerster's work in Canada that led to the closure of sockeye hatcheries but noted, as did Lichatowich (1999), that different species may react differently and he recommended further studies. He recommended that better planting systems were needed but was not opposed to the fish hatchery practices.

Through his work in California, Needham became familiar with the Central Valley Project (CVP) and the evaluation of fisheries with regard to Shasta Dam, a part of the CVP. In a report "Plans For Protection of Salmon Runs That Will be Blocked by Shasta Dam," Needham wrote that the dam would completely block fish runs and the salmon industry was in danger. Plans for the dam had been published since 1932, yet affected parties had not been consulted until after construction plans had been approved. A salmon salvage plan had been devised but its success was questionable as it involved transplanting runs and relying on fish hatcheries to compensate for large runs of fish. Needham was familiar with the WVP noting that that project was similar to the CVP. "Maybe you can have both salmon and high dams but I personally doubt it." He ended the report saying that to keep salmon runs, fisheries management must stop blocking streams to spawning beds.[51]

By 1947, Needham more stridently detailed his stand on dams and salmon in the article "Dams Threaten West Coast Fisheries Industry." Needham's strong opinions in this document regarding the ability to reengineer nature lie not within a disregard for fish hatcheries per se, but within the ability of fish hatcheries to meet the capacity of reproduction demanded by the expansion of water development projects. His statements about fish hatcheries made arguments about quantity, not quality. His opinions about fish hatcheries, which appear to be stronger than those in his 1938 book, were caused not because dams had been proposed, but because of the number of dams proposed had grown since the book's publication. The growth of dam construction projects demanded increased reliance on fish hatchery production. The WVP alone grew from seven dams to thirteen after the end of World War II.

Needham wrote that the proliferation of dams planned for the western states would "completely ruin" the area's fisheries because he believed fish hatcheries could supplement salmon runs, but not replace natural reproduction. Salmon replacement plans for Grand Coulee had not been assessed as successful eight years after operation and he gave the same evaluation for Shasta Dam. In Oregon, he cited the dams on the Columbia River, plans for four dams on the Snake River, dams planned for the Rogue and Deschutes rivers, and the dams planned for the WVP, indicating that money allocated for fish ladders and hatcheries would not solve depleted runs because enough hatcheries could not be built to meet the capacity of natural production. Contrary to the traditional belief that the production rate in hatcheries offered greater yields than fish in the wild, recent studies had shown that the rate of production was similar to natural production. He wrote that hatcheries may supplement fish runs, but could never replace them if more of the natural salmon spawning habitat was lost to dam construction.[52]

Needham was more politically involved than Ryckman, which was his strength and his downfall. When Needham spoke to reporters about dams on the McKenzie, the Game Commission formally rebuked him.[53] Eight months later Needham resigned.[54] The fish culturist lasted for twenty years on the Game Commission and the professional biologist only lasted for three years. It may have been a matter of temperament, but it also may have been a matter of emphasis. Had Needham been more supportive of dams and fish culture, he may have enjoyed more support from the commission. Needham was successful in convincing Wilson of the need for studies that would measure the success of fish hatcheries. Needham first started consulting with the Game Commission in 1940, and by 1941 Wilson had initiated the stream and lake survey. It was stopped in 1942 due to the war, but continued again in 1946. However, the studies were not completed before the Oregon Legislature began an evaluation of the effectiveness of the Game Commission in 1947 for failure to provide fish and game for Oregon's hunters.

Reorganization and Dismissal

Because the Game Commission had come increasingly under fire from local groups for its controversial stands on stream closures, hatchery locations, the screening of irrigation ditches on private lands and other problems, Commission members were often called upon to speak to local groups to explain its work. On June 4, 1948 Wilson made a speech to the Eugene Chamber of Commerce in which he addressed available fish and game habitat compared to the demand for fish and game. In this speech he explained the limitations of fish hatcheries and also referred to the Willamette Valley Project as one of the Commission's biggest problems.

First Wilson established the facts: habitat for fish had decreased, yet demand for fish had increased. He counted 600 fishing lakes and 16,000 miles of fishing waters in Oregon. "These lakes and streams constitute the sum total of the area in which fish exist. Streams and lakes are to fish what grazing lands are to livestock...When a stockman wishes to increase his herd, he simply secures more pasture, but it is not possible to secure more lakes and streams."[55] This argument sounded like an ecological argument made for the carrying capacity, though that terminology was not used.[56] In addition, Wilson cited figures for the number of licensed anglers in the state as having increased two and one half times in 10 years. "There is two and one half times the demand for fish and game this year that [sic] there was ten years ago and yet the habitat for fish and game remains the same."[57] While many people believed that the solution to the problem was artificial propagation, Wilson disputed its efficacy.

> In the mind of the average sportsman the solution is simple enough. All that is necessary to be done is raise more fish and plant them in the lakes and streams. Raise more pheasants and turn them loose in the fields. All without regard to the capacity of the streams or the fields to sustain them...The experience of the Oregon Game Commission as well as the experience of the Commissions of all states is that artificial propagation of fish and of pheasants and their release generally in the streams and fields is not the complete answer."[58]

Wilson did not wholly discount the use of artificial propagation, but indicated that its use was still in the experimental stage and should not be considered a bankable solution based on the study of Russell Foerster that had led to closure of hatcheries in Canada. He also indicated that artificial propagation continued to become increasingly expensive, and the costs may soon outweigh the value of the product. From the experts Wilson had consulted, they indicated that natural propagation was coming to be viewed as the preferred method of supplying game fish. He then said that the Commission was turning towards improving the conditions of available lakes and streams as a way of promoting natural habitat as opposed to fish hatcheries and documented the processes the Commission had pursued. Wilson then addressed the Willamette Valley Project and the loss of habitat the dams would cause. He predicted that there would be no spawning and then no natural propagation in the streams above the dams.

> This simply means that after the dams are constructed, the main reliance for fish will be transferred to stocking with hatchery fish, and I venture the assertion it will never be practical to raise and plant enough fish to anywhere like (to) restore present fishing conditions. The Government officials are going on the false assumption that enough fish can be produced in the hatcheries they propose to provide to adequately meet the situation. The trouble is that when it is demonstrated that this cannot be done, it will just be too late.[59]

He concluded that the Commission was looking into every possible solution, and that it was a very big problem. This was not the first time that Wilson had spoken out against fish hatcheries and on the Willamette Valley Project, but it was one of his last speeches as a member of the Game Commission.

Another problem that had been dogging the Commission was the charge that the members of the group made wildlife policy but were not wildlife professionals and therefore lacked the expertise required. In July 1949 the Oregon Legislature revised by statute the makeup of the Game Commission. The new Commission would function as a policy-making group while a full time, professional wildlife manager would implement the policy. Game commissioners were responsible for administrating policy under the old law. Oregon Governor Douglas McKay interpreted the new law to ask for not just a reorganization of the Game Commission but a replacement of the old members. In keeping with the legislature's wishes McKay replaced the entire Commission with five new members saying that the old members did a remarkable job.[60]

However, the majority report of the legislative committee investigating the commission did ask for reorganization but did not ask for their resignation. In fact, they praised the work the commission was able accomplish given the amount of resources allocated to the group, and given the increasing politicization of the commission's work. Neither a separate report from Representative John Ebinger asked for resignation, nor did the report of Dr. Ira Gabrielson.[61] The decision to dismiss all of the members of the Game Commission fell to newly elected governor Douglas McKay.

Editorials from other newspapers indicated that the governor did not have to replace the Commission, but chose to take that route. From a July 21, 1949 letter of consolation from former Oregon Governor Charles Sprague, at that time publisher of the *Oregon Statesman* newspaper, he included a clipping about the reshuffling. "Now a new set of villains will step onto the stage to get the jibes and the overripe tomatoes and the aged eggs from those who don't like the act."[62] The editorial indicated that sportsmen had complained about the Commission for years regarding no fish in the streams and no doe in the mountains, and the regulation of fishing and hunting seasons. "This is one commission where it is impossible to make everyone happy."[63] A July 27 editorial in the Corvallis *Gazette-Times* also indicated that because of the dwindling numbers of fish and game coupled with an increasing number of sportsmen, the Commission was handed an impossible job. This editorial addressed a charge that Wilson and the Commission did not pursue scientific avenues to solve problems with fish and game.

When Wilson began on the Commission in 1935 no professional biologists were employed but by 1948 fifty professional biologists were employed by the Game Commission. "The penalty of industrial growth is the loss of natural resources, yet the sportsman cannot be reconciled to the loss of fish and game."[64] The Gazette-Times July 27 editorial acknowledged that "...the preservation of fish and game was a complex one" and that fish hatcheries were not the answer. "The best approved methods of artificial propagation of game and fish are open to severe criticism...You (Wilson) have struggled against selfish interest, prejudice, stubborn antagonism and total ignorance of the problems at hand."[65] Wilson repeatedly expressed after his replacement that he would miss the people with whom he worked, but had thought for a while it was time for him to take leave of the position. He wrote in an August 1, 1949 letter to Mrs. Frank Wire that he was not totally surprised with the Commission's replacement, since he had gained his spot the same way 14 years earlier. "Of all the members of the Commission I should have fewer misgivings as to the future of the new Commission, because I was unceremoniously ushered into office and the whole thing did not blow up as a result. History is disposed to repeat itself."[66]

Conclusion

Governor Mc Kay's interpretation of the new law was very convenient. The installation of a completely new Commission served his purposes. The utility of a Game Commission that posed increasingly strident opposition to a plan which would bring many millions of dollars into the state coffers was a problem. The problem was solved by bringing in a new group of commissioners unschooled about the fine points of water resource policy. History would repeat itself in that the new Commission members would have to educate themselves before they could adequately assess the impact of the Willamette Valley Project. In the intervening time, with fewer obstacles to overcome the project's backers could guarantee its completion. Douglas McKay served as one of the original promoters of the project, and now as Governor, hoped to see the fruits of his earlier efforts. Like Charles Martin who wished to protect federal dollars slated for water resource development projects, McKay wished to protect the construction of the dams of the WVP.

Despite charges to the contrary, Wilson pursued his work on the Commission in the best scientific manner possible. He insisted on a wildlife management policy based on fact-finding. He pushed to get a professionally trained staff that would give him the scientific data on which to base a sound wildlife management policy. If the lake and stream surveys had been completed, he would have had the ammunition to attack fish hatcheries in a way that the people of Oregon would appreciate, on an economic basis, not with the science of genetics that did not have a firm foundation from which to argue. Wilson's attitudes towards fish hatcheries were ahead of his time. Wilson was not dismissed because he did not make use of scientific methods; in my opinion, he was fired because his ecologically inspired scientific methods clashed with the methods of agricultural breeding science.

Chapter 4 Endnotes

[1] *The Oregonian*, February 2, 1935, p. 1, col. 1.

[2] Ibid.

[3] Oregon State Planning Board, *The Willamette Valley Project: On the Development of the Willamette River Watershed* (1935), the State Planning Board and Advisory Research Council, Oregon State Planning Board, RG 158, Series IV, folder: Minutes, committees and subcommittees, OSU Archives; see also Robbins (1997), pp. 287-295.

[4] *The Willamette Valley Project Report* (1935), p. 58.

[5] Ibid., pp. 100-101.

[6] Oregon State Planning Board, *The Willamette Valley Project: A Regional Plan* (1936), State Planning Board and Advisory Research Council, Oregon State Planning Board, RG 158, Series IV, folder: Minutes, committees and subcommittees, OSU Archives, pp. 122-123.

[7] *Willamette Valley Project Report* (1935), pp. 1-26.

[8] Porter (1995), pp. 148-175.

[9] William G. Robbins, "The Willamette Valley Project: A Study in the Political Economy of Resource Development," *The Pacific Historical Review*, 1978, 4:585-606.

[10] Ibid. For more on the Willamette Valley Project, see also Robbins (1997), pp. 283-93.

[11] WILSON/minutes from 1935-1940, SG IV, Series I, folder: Minutes, Game Commission, 1935-1949.

[12] WILSON/April 8, 1939, SG IV, Series I, folder: Minutes, Game Commission, 1935-1949.

[13] WILSON/ June 17, 1939, SG IV, Series I, folder: Minutes, Game Commission, 1935-1949.

[14] Oregon Journal (1939), DIMICK.

[15] Ibid., August 21, 1939.

[16] Ibid., August 23, 1939.

[17] Letter from Merton to Aiken, November 2, 1939, WILSON/FISH, folder: Allegations.

[18] Letter from Merton to Aiken, November 6, 1939, WILSON/FISH, folder: Allegations.

[19] Letter from Wilson to Aiken, November 11, 1939, WILSON/FISH, folder: Allegations.

[20] Anonymous, undated letter, not addressed to a specific person, WILSON/FISH, folder: Allegations.

[21] WILSON/minutes from 1935-40, SG IV, Series I, folder: Minutes, Game Commission, 1935-1949.

[22] WILSON/February 19, 1937, SG IV, Series I, folder: Minutes, Game Commission, 1935-1949.

[23] WILSON/June 10, 1938, SG IV, Series I, folder: Minutes, Game Commission, 1935-1949.

[24] WILSON/Dec. 15, 16, 1939, SG IV, Series I, folder: Minutes, Game Commission, 1935-1949.

[25] WILSON/August 24, 1940, SG IV, Series I, folder: Minutes, Game Commission, 1935-1949.

[26] Ibid.

[27] *Resolutions Adopted by the Western Association of State and Game Commissioners at its Twenty-First Annual Conference, Salt Lake City, Utah, June 5, 6, 7, 1941*, WILSON, SG IV, Series VIII, folder: Conferences.

[28] Ibid.

[29] Oregon State Planning Board, *Statement of Problems and Recommendations for Conservation of Fish and Fisheries in Oregon* (1937), State Planning Board and Advisory Research Council, Oregon State Planning Board, RG 158, Series IV, folder: Minutes, committees and subcommittees, OSU Archives.

[30] Ibid.

[31] Ibid.

[32] WILSON/May 11, 1946, SG IV, Series I, folder: Minutes, Game Commission, 1935-1949.

[33] Letter to Conn from Wilson, March 14, 1947, WILSON/FISH, folder: Correspondence; by mid-March of 1947 dams had already been constructed at Fern Ridge and Dorena.

[34] Letter to Frank Wire from Wilson, March 28, 1947, WILSON/FISH, folder: Correspondence.

[35] Ibid.

[36] Letter to Conn from Wilson, April 1, 1947, WILSON/FISH, folder: Correspondence.

[37] Letter to Walsh from Wilson, April 14, 1947, WILSON/FISH, folder: Correspondence.

[38] WILSON/September 13, 1943, SG IV, Series I, folder: Minutes, Game Commission, 1935-1949; Matt Ryckman is not listed in the *American Men of Science* biographical directory.

[39] Ibid.

[40] Ibid.

[41] Ibid.

[42] Biennial Report of the Oregon Game Commission, 1929-1930 (Salem, OR: State Printing Department, 1931), p. 19.

[43] Ibid., p. 22.

[44] Ibid., p. 23.

[45] Biennial Report of the Oregon Game Commission, 1931-1932 (1932), p. 13.

[46] Ibid., p. 13-18.

[47] Biennial Report of the Oregon Game Commission, 1933-1934 (1934), p. 6.

[48] Ibid., p. 9.

[49] Ibid.

[50] Cattell (1961), p. 2937.

[51] Paul Needham, "Plan for Protection of Salmon Runs That Will be Blocked by Shasta Dam," WILSON, SG IV, Series X, folder: Fisheries.

[52] Paul Needham, "Dams Threaten West Coast Fisheries Industry" *Oregon Business Review* 1947, 6:1-5.

[53] WILSON/February 13, 1948, SG IV, Series I, folder: Minutes, Game Commission, 1935-1949.

[54] WILSON/October 15, 1948, SG IV, Series I, folder: Minutes, Game Commission, 1935-1949.

[55] Speech to Eugene Chamber of Commerce, June 4, 1948, WILSON, SG IV, Series IV, folder: Speeches, 1937-1948.

[56] For example, see Christian Young, "Defining the Range: The Development of Carrying Capacity in Wildlife Management Practice" *Journal of the History of Biology*, 1998, 31:61-83.

[57] Speech to the Eugene Chamber of Commerce, (1948), WILSON, SG IV, Series VI, folder: Speeches, 1937-1948.

[58] Ibid.

[59] Ibid.

[60] "Five Named To Oregon Game Body," The Oregonian, July 21, 1949, p. 3, col. 2, WILSON, SG IV, Series I, folder: Separation from the Game Commission, 1947-1950.

[61] State of Oregon, "Wildlife Resources Report of Interim Committee Appointed Pursuant to House Joint Resolution No. 14," 44th Legislative Assembly, December 15, 1948, pp. 3-85.

[62] Letter to Wilson from Sprague, July 21, 1949, WILSON, SG IV, Series I, folder: Separation from the Game Commission, 1947-1950.

[63] Ibid.

[64] "A Job Well Done," *Gazette-Times*, July 27, 1949, WILSON, SG IV, Series I, folder: Separation from the Game Commission, 1947-1950.

[65] Ibid.

[66] Letter to Mrs. Frank Wire from Wilson, August 1, 1949, WILSON, SG IV, Series I, folder: Separation from the Game Commission, 1947-1950.

Conclusion

Several concepts emerge from this study that had previously been overlooked or misunderstood. The role of professional societies within the sciences, the growth of biological professions in academia, and the establishment of public and private research centers located primarily in the East and Midwest characterized scientific development in the first half of the twentieth century, covered in Chapter 1. The persistence of scientists and educators like Nathan Fasten, who held onto the mutation theory and neo-Lamarckian beliefs, challenge standard science histories which emphasize the dominance of natural selection by 1925. Different people accepted different theories at different times for personal, professional and geographic reasons. Fasten needed mutation theory. Without it, Mendelism was a static system that did not change and adapt. Fasten held onto mutation theory because it bolstered his professional belief in the efficacy of animal breeding, as well as personal socio-political beliefs in the efficacy of breeding healthier humans. Natural selection transformed Mendelism. Had Fasten chosen to support Darwin's theory of evolution and natural selection coupled with Mendelian genetics, the science of breeding which he had taught for so many years would have lost its scientific legitimacy when work in the discipline moved from the science of heredity to the science of genetics.

The role that eugenics played in biological science and biological instruction must be taken into consideration when evaluating the science of this timeframe as evidenced by Fasten's textbook as well as the text by William Castle. The dismissal of eugenics as a social theory denies that the development of eugenics was based within the mechanical development of evolving biological principles. The influences of mutation theory, eugenics and the persistence of neo-Lamarckian principles during the 1930s and 1940s must be considered when evaluating the policy decisions fisheries managers adopted. Consideration of principles that promoted a direct role of the environment in the production of species fitness helps to explain the pervasive use of fish hatcheries to supplement depleted salmon runs.

The institutional framework of Oregon State College as a land grant college influenced by the U. S. Department of Agriculture and the applied fisheries science it produced contrasted with the fisheries science of Stanford University, a private institution under the direction of ichthyologist David Starr Jordan. The agricultural production model based on Mendelian science and promoted by the Department of Agriculture at the turn of the century persisted at Oregon State College that placed its Department of Fish and Game Management within the College of Agriculture in 1935. The scientific principles OSC espoused directly affected the policy formation of the Oregon Game Commission due to the scientific expertise provided by the OSC experiment station to the commission, the role the Game Commission played in development of the college's wildlife management program, and the joint affiliation between OSC and the Game Commission in support of the Oregon Cooperative Wildlife Research Unit.

As a new science based in the Midwest, ecology hardly challenged agricultural production science that dominated wildlife management at OSC. At the turn of the century, ecology emerged with other biological fields that emphasized process and function as opposed to the emphasis of description, comparison and classification dominate within natural history. However, ecological principles were first adopted by botanists, and only applied to animal behavior in later years. Like other "modern" biological fields, the scientists who practiced ecology worked to include experimentation within its practice. While ecologists were interested in broader biological problems like adaptation, development and distribution, ecologists believed that natural settings provided the best laboratory. Unlike other biological disciplines that adopted laboratory experimentation over field studies, ecology stressed field studies in natural settings as a means of uncovering natural processes.

While ecologists expressed interest in adaptation, they were ambivalent about theoretical constructs like natural selection and mutation theory, as science had not as yet proven the legitimacy of one over the other. The work by Frederic Clements adopted a mechanical methodology based in physiology. Aldo Leopold adopted more romantic forms of the science that explicitly divorced ecology from Darwinian evolution because it was grounded in mechanical science and could not explain the diversity found within nature. Because ecology lacked coherent theoretical structure and methodology and because it evolved within a state of confusion throughout the 1940s, it was not taken seriously as a legitimate science.

The role ecological principles played within the OSC Department of Fish and Game Management was minimal in 1935 and hardly better in 1949. Fish hatchery production took precedence, and when ecological principles were introduced, adoption of those principles were to improve fish hatchery technology as opposed to supplying biological life histories of affected fisheries groups. Historians of fisheries science in Chapter 2 note the absence of ecological principles from the development of that field.

Fisheries science focused on solving declines in economically important fisheries without adequate life history information. Fish hatchery technology became the emphasis of the U. S. Fish Commission whose members viewed fisheries production as part of the country's agricultural goals. That emphasis was passed down to state and local fisheries management groups in the Pacific Northwest when federal water development projects were planned for the Columbia River Basin.

The first case study shows that the development of fishways over the Bonneville Dam in 1934 included fish hatcheries as a failsafe should the experimental fishways fail, and as mitigation for lost salmon habitat. As Chapter 3 shows, the apparent success of the fishways in 1938 allowed policy makers to believe that dams that blocked fish passage and anadromous fish runs could coexist. While the fishways did allow upstream passage for spawning salmon, failure to adequately address the needs of downstream passage by juveniles led to a serious loss of fish with the addition of new dams within the Columbia River Basin. Historical descriptions credit all parties that participated in the development of the fishway system over Bonneville with ingenuity and foresight. However, minutes from the 1934 Oregon Game Commission meetings with Bonneville engineers and planners show that not all parties were willing participants. When members of Oregon's and Washington's fish and game commissions were invited to take part in the planning process, they produced documents that expressed a lack of confidence in the fishway's success. Pork barrel politics played an important role in both case studies cited in this paper. Oregon representative Charles Martin played a major role in securing funding for Bonneville Dam and was then elected governor in 1934. When he took office in 1935 one of his first actions was to clean his house of detractors of water resource development and he dismissed the 1934 Game Commission. While he claimed the dismissal was based on the commission member's fiscal impropriety, the Izaak Walton League charged that the dismissal was grounded in the commission's actions in opposition to Bonneville.

The 1934 dismissal led to E. E. Wilson's appointment to the Game Commission in 1935 as mentioned in Chapter 4. Wilson worked to school himself in the latest developments in wildlife management. Wilson read continuously during his tenure on the commission devouring professional fisheries papers as well as conservation publications from game commissions across the nation. While Wilson was not an expert, he hired many credentialed wildlife experts for the Game Commission and depended on them for direction. However, his early direction in fisheries came from Matt Ryckman, a fish culturist whose advancement of scientific goals challenges the depiction of fish culturists as unsophisticated craftspeople. Wilson worked with members of the commission to provide a yearly hatchery workshop which would standardize procedures for those working in the Game Commission's fish hatcheries from 1937-1941. Wilson attended professional organizations that took stands against dam construction due to the detrimental effects water resource development had on wildlife. In 1939 he led the Oregon Game Commission in opposition to the Willamette Valley Project and throughout his tenure on the Commission, Wilson's efforts to halt dam construction grew. Wilson did not oppose all dams, just those that blocked fish passage. He did not oppose all fish hatcheries, but did object to further dependence on a production process that was scientifically unproven.

In his efforts to understand fisheries declines, he employed procedures that suggested an understanding of ecological science, though ecological terminology was not used. His depiction of the limits of Oregon's lakes and streams to support fish life was an argument for carrying capacity. His preference for natural reproduction, similar to Haven's description (1988), was not based on genetics. Wilson's opinion was based on his observation that natural reproduction seemed to produce a better product. The tagging studies to determine the birth and death ratios of different species of salmon were precursors to maximum sustained yield and population systematics. Some of the ecological emphasis was the result of Matt Ryckman's expertise. It was Ryckman who was reluctant to use cutthroat salmon eggs to supplement coastal streams as he noticed that native eggs worked better, though the supply of native eggs was inconsistent. It was Ryckman who investigated predator-prey relationship among fish populations. But the fisheries expert who probably influenced Wilson the most was Paul Needham who succeeded Ryckman in 1945 as Game Commission Director of Fisheries.

As a credentialed fisheries biologist who had previously worked for the U. S. Bureau of Fisheries, Needham provided Wilson with fisheries expertise as a consultant to the Game Commission that included agricultural production but also dove into the murky science of ecology as early as 1940.[1] As Chapter 4 showed, Needham had been involved with projects similar to the WVP and believed that fish hatcheries would not be able to compensate for the loss of habitat the project would ensue. Wilson began the Game Commission's lake and stream survey in 1941 to determine fish hatchery efficacy, but the advent of World War II interrupted the work until 1946. Needham then took over the development of the study, but it was not completed before a legislative committee challenged Wilson's ability to effectively lead the Game Commission.

Wilson's support of ecological principles regarding fish hatcheries and dams led to his dismissal in 1949. Sports fishers charged he was not scientific because he did not apply the modern science of fish hatchery production to save Oregon's fish runs. Historically, the dismissal of the entire Game Commission in 1949 was predicated on Governor Douglas McKay's interpretation of a legislative mandate to reorganize the group. However, upon inspection of associated documents it is clear that the legislative bodies did ask for reorganization of the commission while also praising the work that the Game Commission had produced. The legislative committees did not ask for the group's dismissal. That decision was McKay's. He had served as director of the Willamette Valley Flood Control Committee for fourteen years prior to his election as governor, a group which promoted the WVP. He worked to bring the federal dollars to Oregon that supported the WVP and elimination of the 1949 Oregon Game Commission served to eliminate opposition to his pet project. Wilson was not dismissed because he was ignorant of science, in my opinion, he was fired because he his ecologically-inspired knowledge base challenged the knowledge base of production science.

The Depression and World War II framed this story. The economic collapse of the 1930s led to the funding of work projects to harness the power of nature's rivers and provide electricity to rural America. However, it was not just the need to put people to work and the need for electricity that fueled the expansion of water development. The U. S. Bureau of Reclamation and the U. S. Army Corps of Engineers competed to build the biggest dams, and they also competed for bureaucratic control over the country's rivers. That need to provide jobs, electricity and the competition between the two groups led to the construction of an unprecedented number of systems of dams throughout the nation: in the Tennessee Valley, on the Missouri River, the Colorado River, rivers in Texas and Oklahoma, the California Valley Project, on the Columbia River, to name a few. The dams not only supplied jobs, but they afforded a sense of accomplishment and mastery to a nation that had economically failed. During World War II, especially in the Northwest, aluminum plants fueled with electricity from Bonneville and Grand Coulee dams provided the necessary electricity to build the airplanes that helped the Allies win.[2]

At the local level, both events had effects on wildlife management rarely considered. As mentioned in Chapter 3, the loss of funds from license sales during the Depression led the 1934 Oregon Game Commission to secure federal funds for capital expansion, a move which gave new Governor Charles Martin the excuse for their dismissal in 1935. As mentioned in Chapter 2, a Depression-era agricultural recovery act funded development of the OSC Department of Fish and Wildlife Management in 1935. The Department was then put within the college's School of Agriculture, a move that affected its scope and content. As Chapter 4 shows, funding for water development projects, like the Willamette Valley Project, stalled with America's involvement in World War II. The loss of personnel and supplies during World War II curtailed many new projects as well as general maintenance by the Oregon Game Commission. The war so greatly affected wildlife management work that wildlife recovery acts enacted during the end of the war funded the acquisition of lands for game preserves.[3] The E. E. Wilson game preserve established in 1954, located north of Corvallis, was previously used for Camp Adair, a U. S. Army training ground. Wilson himself had negotiated the land donation, and he continued those negotiations after his dismissal from the commission. After the land for the game preserve had been turned over to the commission, the new commission members decided to dedicate the preserve to Wilson in recognition of his service for the benefit of Oregon's wildlife.[4]

The causes of disagreement that precede dismissals of staff, especially within a bureaucracy, are rarely documented. If they are, the reasons offered about those dismissals can be deceiving. Staff in the U. S. who do not tow the party line will not suffer the same fate as Russian plant geneticist N. I. Vavilov. As mentioned in Chapter 1, for supporting Darwinism and the work of T. H. Morgan, Vavilov was sent to a labor camp where he died within a few years. Staff who serve in state and local positions and oppose the will of the official in power are not sent to labor camps, but they often disappear from service nevertheless. Documentary evidence to show that Ralph Cowgill and the 1934 Game Commission, or Paul Needham, E. E. Wilson and the 1949 Game Commission were dismissed for opposition to water resource development was not found. Further research may uncover such documents, and it may uncover more dismissals. An unanswered question regarding the fate of the Washington Fish and Game Commission, whose members were the most vocal in opposition to Bonneville dam at the December 10, 1934 meeting discussed in Chapter 3, begs for further investigation.

Opinions about dismissals in Oregon are supported by William Finley's experience from serving as Game Warden for Oregon early in the twentieth century. As mentioned in Chapter 3, members of the fish and game commission who did not support the governor's plans were dismissed in 1914 and 1915. E. E. Wilson served on the Oregon Game Commission for fourteen years, through the terms of three Oregon governors, Republicans and Democrats, indicating he had been doing credible work and that new governors did not routinely replace the commission. That he was dismissed surprised a number of editorial writers in Oregon, as seen in Chapter 4. Those writers had no documentary evidence to support their beliefs that motives other than an organizational change preceded the dismissal. Their conclusions were logical opinions drawn from experience with Oregon's state government. As argued in Chapter 3, the 1934 Game Commission may have overspent their budget, but they did so during the Depression when hunting and fishing license sales were down, sales that provided the commission's major source of revenue. Additionally, the group overspent their budget in an effort to fulfill a mandate placed upon them by the Oregon voters to expand fish hatchery systems. After accumulation of the debt, the commission outlined a program in which they reasonably expected that they would make up the difference in the next biennium. Despite all of above factors that were listed in the Oregon Game Commission biennial reports, the group was dismissed under allegations of fiscal improprieties. The Izaak Walton League did not accept Oregon Governor Charles Martin's allegations as the real reason for the 1934 commission's dismissal. They charged that the dismissal grew from the commission's opposition to one of Martin's pet projects, building of the Bonneville dam. The

League did not cite documentary evidence to uphold the charge. Further investigations may uncover such evidence.

Conclusion Endnotes

[1] WILSON, SG IV, Series X, folder: Fisheries (7 folders) in which Needham's fifteen publications range in date from 1933-1939; see also WILSON/FISH, folder: Hatcherymen's Short Course, where a listing of speakers includes Needham's name in 1940.

[2] Reisner (1993/1986), pp. 145-168.

[3] WILSON, SG IV, Series IX, folder: Pittman-Robertson Wildlife Restoration Projects, 1939-1946; folder: Post War Readjustment Act, 1943-1946; and folder: Summer Lake, 1941-1947.

[4] WILSON, SG I, folder: Correspondence, 1891-1961 (three folders).

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Memorabilia Collection, Nathan Fasten, OSU Archives

MSS William Finley Papers, OSU Archives

MSS E. E. Wilson Papers, OSU Archives

Oregon State College Catalogs, OSU Archives

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APPENDICES

Appendix A

Oregon Cooperative Wildlife Research Unit Research Projects, 1935-45

Research projects published in Agricultural Experiment Station Bulletins:

1. Small Game in the Willamette Valley

2. Thermographic Records of Nesting Pheasants

3. The Study of Upland Game Birds on an Island Under Natural Conditions

4. Big Game Sex Ratio in Oregon

5. Life History and Management of Antelope in Oregon

6. Game Management on the Squaw Butte Range Experiment Area

7. Sage grouse of Oregon

8. Game management on Farm Grounds

9. Assembly of Material on Game Animal Diseases

10. Educational Pursuits

11. Water and Beaver Surveys

12. Life History and Management of Mountain Quail

13. Diagnostic Research on the Columbia Black-tail Deer in Western Oregon

Publications in progress and those already published in professional journals:

14. Life history and management of the prong horned deer

15. Management of the Tillamook burn

16. Suggestions on Management of Small Game in Oregon (1941)

17. Specific results from Ring-necked Pheasant Studies in the Pacific Northwest

18. The Murrelet

19. Pheasant Management

20. Quadrant inventory method for upland birds

21. Wildlife and war

***22. Native Oyster Investigations in Yaquina Bay (1941)**

***23. New Locality records of three Pacific Coast Fishes from Oregon (1944)**

24. Rabbit Production for Meat (1943)

25. Chastek paralysis produced in Oregon Mink and foxes ... (1943)

****26. A review of the bacteriology of fresh marine fishery products (1937)**

***27. Considerations of the introduction and distribution of Exotic fishes in Oregon**

****28. Measurement of Fish for Liberation by Weighing (1940)**

****29. Production of Trout in a Small Artificial Pond in Western Oregon (1943)**

* 6 fisheries related articles; **3 related to hatchery production

Appendix B

Classes Offered by the Department of Fish and Game Management, 1935-49

Lower Division

from the 1935-36 catalog

FG 251	Wildlife Conservation
FG 261	Wildlife Techniques
FG 271, 272, 273	Fur Farming

from the 1948-49 catalog

FG 274, 275, 276 **Economic Ichthyology**-Classification and distribution of fishes; general consideration to orders and families of fishes with special attention to those of economic and recreational importance in North American and adjacent marine areas.

Upper Division

from the 1935-36 catalog

FG 305	Reading and Conference
FG 310, 311, 312	Forest Wildlife Management
FG 341	Fish and Game Law Enforcement
FG 351, 352, 353	Fish and Game Management
FG 360	Applied Fish and Game Ecology
FG 401	Research
FG 405	Reading and Conference
FG 407	Seminar
FG 451, 452	Management of Game Birds

FG 454, 455, 456 **Management of Game Fish** -Studies of game fish with special emphasis to fish-hatchery methods, natural propagation, and methods of fish liberation. Much of the lab and field work is conducted at the Alsea Fish hatchery (owned by the Game Commission).

FG 457, 458 Management of Big Game

FG 460 Management of Fur Bearers

from the 1948-49 catalog

FG 464, 465, 466 **Commercial Fisheries** -Studies of the commercial fisheries of North America with particular reference to the Pacific coast; biologies of important species; economic values; methods of harvesting and regulating fisheries resources.