KNOWLEDGE-DEVELOPMENT IN APPLIED SCIENCE: THE CASE OF RANGE MANAGEMENT

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(ABSTRACT)

This study traces the evolution of the applied ecological discipline of range management in terms of the goals, methods, concepts, and criteria developed by range management researchers for their science between 1897 and 1920. It argues, in contrast to the traditional view that describes the knowledge-development process in applied science as just science applied to social problems, that wider social goals, values, concepts, and criteria play a definite role in shaping the applied science knowledge-development process.

The first generation of range management researchers allowed the primary users of the knowledge in the wider society, the stockmen in the West and Southwest, to have a direct influence on the knowledge-development process. The next generation of scientists eliminated the stockmen's direct influence on the knowledge-development process, yet the stockmen still influenced that process indirectly in various ways.

This study concludes that an orientation towards the wider society that actually applies the knowledge is characteristic of range management and may be illustrative of illustrative of many applied sciences. Due to that orientation towards the wider society and to the wider society's influence on the scientist's choice of methods, concepts, and criteria, another characteristic of range management and possibly of other applied sciences is a tension in the knowledge-

development process between that orientation and the individual goals of scientists in their research.

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INTRODUCTION

STATEMENT OF THE PROBLEM

The traditional view of the knowledge-development process characteristic of applied science within the USDA and the state agricultural experiment stations argues that applied science is just science applied to social problems.[1] Such a view presumes a sharp separation between science and society, while implicitly reifying both science and society. Society provides the problem, and science provides the solutions. Yet neither the focus on the problem nor on society has any direct or indirect influence on the knowledge-production process or on the knowledge produced, other than providing an impetus for research by indicating a problem. Knowledge-development in applied science is

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[1] In the second half of the nineteenth century, the U.S government created several institutions to foster the development and diffusion of knowledge useful to farmers and ranchers. In 1862, Congress created the U.S. Department of Agriculture (USDA). Later that same year, Congress passed the Morrill Land-Grant Act, creating the land-grant colleges. In 1887, President Cleveland signed the Hatch Act, and the federal government began to provide financial support to state agricultural experiment stations. Congress completed the system in 1914 by institutionalizing an extension service to ensure that the knowledge reached the agriculturists. For a history of science in the USDA, see T. Swann Harding, 1947, Two Blades of Grass: A History of Scientific Development in the U.S. Department of Agriculture (Norman, OK: University of Oklahoma Press), and A. Hunter Dupree, 1957, Science in the Federal Government: A History of Policies and Activities to 1940 (Cambridge, MA: the Belknap Press of Harvard University Press). On the experiment stations, see Norwood A. Kerr, 1987, The Legacy: A Centennial History of the State Agricultural Experiment Stations, 1887-1987 (Columbia, MO: University of Missouri, Missouri Agricultural Experiment Station); and Edward H. Beardsley, 1969, Harry L. Russell and Agricultural Science in Wisconsin (Madison and Milwaukee, WI: The University of Wisconsin Press). On the extension service, see Roy V. Scott, 1970, The Reluctant Farmer: The Rise of Agricultural Extension to 1914 (Urbana & Chicago, IL: University of Illinois Press).

therefore presumed to be the same as in the "pure" sciences.[2] The traditional view furthermore assumes that scientists and society share the same goal. The individual goals of scientists in their research and the more general objective of the applied science are presumed to be the same because the objective provides the impetus for the scientists to generate new research questions. That view then identifies two problems facing applied science: maintaining the autonomy of science and adapting the new knowledge to fit the social and economic world of the ranchers and farmers.[3]

Applied scientists present the same view of knowledge-development in applied science. The chemist W.O. Atwater, the first director of the Office of Experiment Stations in the USDA, gave a good summary of that view when articulating a standard for agricultural research.[4] He argued that after talking to the farmers to determine their problems, the scientists should generate their own questions for research, guided not by those problems but by their disciplines. They should then draw on their disciplines' methods and concepts - and if necessary develop new methods and concepts - to carry out their research. They should also use the criteria of their disciplines to evaluate their research. The goal of the scientists, developing new knowledge, rather than the goal of the farmer, solving the problems facing agriculture, should thus be

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[2] Dupree, 1957, 152-7, therefore describes that knowledge-development process as the "problem approach."

[3] For a discussion of these problems, see Dupree, 1957; Beardsley, 1969; Scott, 1970; Kerr, 1987; and Charles E. Rosenberg, 1961, No Other Gods: On Science and American Social Thought (Baltimore: The Johns Hopkins University Press), 153-72.

[4] This account is based primarily on Kerr, 1987, 38-47.

the guiding principle motivating research. In 1888, Atwater stated that in research "the highest scientific ideal [should] be maintained and every effort [should] be made towards its realization."[5] The scientists should concentrate on conducting the "most abstract and profound research,"[6] without worrying about the potential application of the knowledge. Finally, they should publish the results in such a way that the farmer could understand them.

This study traces the historical development of the applied ecological discipline of range management. In it, I argue that wider social goals, values, concepts, and criteria play a definite role in shaping the applied science knowledge-production process. Specifically, the goals, etc., of the users of the knowledge, in this case the stockmen in the West and Southwest, influenced that process.

Ecologists study the interrelationships between organisms and their environment. Haeckel coined the term "Oekologie" in 1866 to refer to the body of knowledge concerned with the economy of nature. He thought ecologists should study the totality of relationships between animals and their organic and inorganic environment.[7]

I will show that range managers, as applied ecologists, analyze how human actions affect those interrelationships. Sometimes they analyze

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[5] W.O. Atwater, quoted in H.C. Knoblauch, E.M. Law, W.P. Meyer, B.F. Beacher, R.B. Nestler, and B.S. White, Jr., 1962, "State Agricultural Experiment Stations: A History of Research Policy and Procedure," U.S. Department of Agriculture, Miscellaneous Publication 904, 83.

[6] Ibid., 85.

[7] See W.C. Allee, Orlando Park, Alfred E. Emerson, Thomas Park, and Karl P. Schmidt, 1949, <u>Principles of Animal Ecology</u> (Philadelphia and London: W.B. Saunders Company), frontispiece.

how human actions have affected those interrelationships in ways that hurt the organisms and the environment, at other times how humans can manage the organisms and the environment to gain benefits without hurting the environment. They then attempt to construct management plans for the use of the range based on their understanding of the interrelationships between organisms and their environment on the range.

Range management evolved out of attempts by government scientists to tackle the problems facing stockmen in the West and Southwest. By the end of the nineteenth century, the ranges where stockmen grazed their animals had deteriorated tremendously. Although the federal government owned most of the land, sheepmen and cattlemen killed each other in attempts to monopolize access to the "free" range, often under the mistaken belief that sheep and cattle could not graze the same area. Many people opposed grazing, arguing it interfered with other uses and with preservation of natural resources.[8]

Between 1898 and 1902, the foundation for the research program of range management was developed by allowing the users of the knowledge - the stockmen - direct influence on the knowledge production process. Once the first researchers had established that foundation, however, the next generation of scientists gradually eliminated the users' direct influence on research. By 1920, only scientists determined the course of the research program. Yet this study attempts to show that even then

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[8] For a more detailed discussion of the condition on the range at the turn of the century, see M.W. Talbot and F.P. Cronemiller, 1961, "Some of the Beginnings of Range Management," Journal of Range Management 14(2):95-102; and Philip O. Foss, 1960, Politics and Grass: The Administration of Grazing on the Public Domain (Seattle: University of Washington Press), esp. 33-4.

the social objective of the research program influenced the scientists' choice of methods, concepts, and criteria used to produce and evaluate knowledge. Analyzing the historical development of that research program between 1898 and 1920 in terms of changes in the relationship between science and society, and its effect on the knowledge-production process therefore provides us with some insights into both the establishment and evolution of an applied science and the history of range management.

One of the outstanding problems in Science and Technology Studies (STS) concerns the nature and validity of the knowledge produced by the applied sciences and technology. This problem is even more significant because any analysis of applied sciences appears to require the integration of social and philosophical perspectives. The naive model that technology and the applied sciences are nothing more than the application of knowledge developed by the "pure" sciences has recently been challenged by many historians, sociologists, and philosophers of science and technology. But there is currently no agreement on how to analyze the nature and validity of the knowledge produced by technologists and applied scientists. In the conclusion, this study attempts to show that due to the influence of the relationship between science and society on the knowledge-production process a tension emerges between the general objectives of the research program and the goals of particular bits of research.[9] That tension influences the

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[9] To keep the analysis manageable, this case study focusses on one element of the relationship between science and society, that between science and the stockmen in the West and Southwest who had the most immediate interest in the new knowledge because their use of the range could potentially be affected by the scientists' knowledge.

scientists' use and development of methods, concepts, and criteria, and is therefore illustrative of the applied science knowledge-production process.

LITERATURE REVIEW

Only one historical analysis deals exclusively with the development of knowledge in range management, and that account is based on the assumption that society only provides the problem, but has no influence on the actual knowledge-development process. The author, Thomas Alexander, a historian of the American West, gives an interesting account of research conducted in the Intermountain Region of the Forest Service between 1905 and 1930.[10] He recognizes that applied science does not simply consist of finding ways to apply knowledge developed by the "pure" sciences. The scientists whose research he describes developed new knowledge. Nevertheless, because he accepts that assumption about applied science, he does not analyze whether there are any characteristics illustrative of the knowledge-development process in applied science. Instead, he presents us with a picture in which applied science knowledge-development is just like that in "pure" science, basing his view of science on the philosophy of Thomas S. Kuhn. His emphasis on research in the Intermountain Region after 1905 also shapes his analysis. He does not analyze contributions made by researchers in other regions and he does not recognize that steps had already been undertaken to replace rule of thumb management with

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10 Thomas G. Alexander, 1987, "From Rule of Thumb to Scientific Range Management: The Case of the Intermountain Region of the Forest Service," Western Historical Quarterly, 18(4):409-28.

management based on knowledge generated by research.

Other historians briefly discuss the development of knowledge in range management to provide a background for their analyses of public policy. William Rowley, Philip Foss, and Lawrence Rakestraw describe aspects of the science of range management in their analyses of natural resource policy on the ranges.[11] The development of the knowledge is not analyzed. Instead, the knowledge is taken as given. Those studies tend to describe the results of knowledge production, and concentrate on analyzing the impact of that knowledge on public policy.

Their analyses are based on the same model of knowledge-development in applied sciences. They tend to assume that science, including applied science, develops knowledge according to its own inner logic, autonomous from society. Society may point out some problems the scientists can study, but that is the extent of society's influence on science. Lawrence Rakestraw, for example, in his article on the controversy over sheep grazing in the Cascade Mountains in Oregon, recognizes that the researcher was influenced by his contacts with the stockmen, most notably John Minto. In his analysis, however, he then skips a step. He moves from discussing the researcher's contacts with Minto directly into an analysis of the results of the study to set the stage for the analysis of the public debate over government policy with respect to grazing. He does not clearly analyze the exact nature of Minto's influence on the development of the knowledge because of the

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[11] William D. Rowley, 1985, U.S Forest Service Grazing and Rangelands, a History (College Station, TX: Texas A&M University Press), esp. chapter 4, "The Science of Management;" Philip O. Foss, 1960; and Lawrence Rakestraw, 1958, "Sheep Grazing in the Cascade Range: John Minto vs. John Muir," Pacific Historical Review, 27(4):371-82.

presumption that the knowledge-development process proceeds autonomous from society.

Furthermore, they do not analyze whether range managers changed concepts they adopted from ecology. In part, that is due to the assumption that the applied sciences find ways to apply knowledge developed by the pure sciences. It is also due to the view that science is autonomous. The methods and concepts used by applied scientists are therefore presumed to be similar to those used by scientists, and nature should therefore give the same answer. On the basis of those assumptions it may not make sense to wonder whether applied scientists change concepts and methods they adopt from the pure sciences. On the contrary, it may make more sense to think they do not change those methods and concepts, but just utilize what is available. Rowley, for example, briefly describes the concepts and methods range managers adopted from plant ecology. But he does not analyze whether they changed those concepts, as I will show they did. He only describes the way ecologists used those concepts and methods, and then describes how range managers applied them to provide the foundation for scientific management of the range. The heart of his analysis is then concerned with the question how that scientific foundation was incorporated in public policy.

The historian Ronald Tobey briefly discusses some aspects of the emergence of range management in his account of the history of ecology. His analysis concentrates on the historical development of the first school of plant ecology in the United States. He touches on range management because many scientists trained in pure ecology moved into range management beginning in the 1930's. According to Tobey, some of those ecologists moved into range management because they thought their discipline could help solve the problems facing the range during the Dust Bowl. Others saw an opportunity for a career in bringing knowledge from pure to applied ecology.[12]

Unfortunately, Tobey reinforces the idea that pure science provides the foundation for applied science. Initially, according to Tobey, Frederick E. Clements, the developer of the dominant theory in plant ecology between 1900 and 1930, provided that foundation. Throughout his career, Clements attempted to show the practical value of ecology. Later, his students provided that foundation when they moved into range management. Yet Tobey does not analyze the history of range management prior to the migration of the ecologists into range management. Nor does he mention that there is a history. He therefore leaves the impression that range management did not become a knowledge-producer until "pure" ecologists moved into range management.[13]

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[12] See Ronald C. Tobey, 1981, Saving the Prairies: The Life Cycle of the Founding School of American Plant Ecology, 1895-1955 (Berkeley, CA: University of California Press), esp. 220.

[13] Including an analysis of some of the early range management would have forced Tobey to ask some different questions about the emergence of the "pure" science of plant ecology. Some of Clements' ideas and methods bear a striking resemblance to ones developed by range management researchers. Clements acknowledged the similarities, but never discussed to what extent they had influenced him. Range managers also adopted some of his concepts and methods, but never wholesale. They offered significant criticisms of Clements' use of those ideas, and changed them before adopting them in range management. Clements cited the articles in which range managers critiqued his ideas, but never acknowledged or discussed the criticisms. He did, however, attempt to refute the criticisms made by his peers in "pure" ecology. See Frederick E. Clements, 1928, Plant Succession and Indicators: A Definitive Edition of Plant Succession and Plant Indicators (New York: The H.W. Wilson Company), esp. 224-32. The question then arises to what

Frank Egerton, who has written the most comprehensive review of applied ecology, including range management, argues that the potential for ecologically based management of natural resources was not formally recognized until after World War II.[14] He accepts Tobey's view of range management as an applied ecological discipline, and argues there could not be any applied ecological disciplines until applied ecological investigations were linked clearly with the pure science of ecology. His criterion for the emergence of applied ecology is not the nature and validity of the knowledge-production process in applied ecological disciplines. Rather, it is the existence of obvious institutional expressions of linkage with the pure science. Inadvertently, therefore, although he wants to recognize that applied sciences are characterized by unique knowledge-production processes, he draws our attention away from analyzing aspects of those processes by focussing on the link with ecology.

Furthermore, without analyzing the history of range management prior to the period discussed by Tobey, Egerton also thinks there was no formal science of range management before the end of World War II because institutions, journals, and legislation centered primarily around applied ecology did not become institutionalized until then.

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extent an applied science influenced theoretical and methodological developments in the "pure" science of plant ecology. Tobey unfortunately does not address that question - it would complicated his otherwise excellent analysis tremendously. Addressing that question is also beyond the scope of this analysis, but this analysis will help set the stage to address it by analyzing the sources Clements' was aware of but Tobey ignored in his analysis.

[14] Frank N. Egerton, 1985, "The History of Ecology: Achievements and Opportunities," Journal of the History of Biology, 18(1):103-43.

Formal education in range management, however, had begun in the first two decades of the twentieth century. Range managers influenced public land policy through the USDA, where most of them worked, and influenced Congress on legislation pertaining to grazing on the public lands in 1926 and 1936.[15] It also is clear that ecologists and other scientists were aware of the potential to apply ecological knowledge to natural resource management much earlier.[16]

Sociologists and philosophers have not analyzed knowledge-production in range management at all. They have only recently begun to analyze ecology, and restricted their analyses to "pure" ecology.[17] Several reasons can account for that neglect. If we accept the

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15 See U.S Congress, Senate, Committee on Public Lands and Surveys, "Hearing Pursuant to Senate Resolution No. 347," 69th Congress, 1st Session, 1926, part 1, Vol. 246; and U.S. Congress, Senate, "A Report on the Western Range: A Great but Neglected Natural Resource," 74th Congress, 2d Session, 1936, Senate Document 199, Vol. 7.

16 For example, see B.E. Fernow, 1903, "Applied Ecology," Science, 17:605-7; Stephen A. Forbes, 1915, "The Ecological Foundations of Applied Entomology," Annals of the Entomology Society of America, 8:1-19; J.W. Toumey, 1928, Foundations of Silviculture upon an Ecological Basis (New York: John Wiley and Sons); Aldo Leopold, 1933, Game Management (New York: Charles Scribner's Sons); W.P. Taylor, 1936, "What is Ecology and What Good is It?" Ecology, 17:333-46; and H.L. Shantz, 1940, "The Relation of Plant Ecology to Human Welfare," Ecological Monographs, 10:311-42.

17 For some analyses of ecology by sociologists or philosophers see, Dorothy Nelkin, 1977, "Scientists and Professional Responsibility: The Experience of American Ecologists," Social Studies of Science, 7(1):75-95; Daniel Simberloff, 1980, "A Succession of Paradigms in Ecology: Essentialism to Materialism and Probabilism," Synthese, 43:3-39; Marjorie Grene, 1980, "A note on Simberloff's 'Succession of Paradigms in Ecology,'" Synthese, 43:41-45; Richard Levins and Richard Lewontin, 1980, "Dialectics and Reductionism in Ecology," Synthese, 43:47-48; Robert P. McIntosh, 1985, The Background of Ecology: Concept and Theory (Cambridge: Cambridge University Press). Although these works do not analyze applied ecology, they may provide an integral part of future analyses of applied ecology when we extend our analysis to include the transmission of knowledge from pure to applied ecology.

assumption that knowledge-development in the applied sciences is similar to that in the "pure" sciences, as the historians mentioned above do, there is really no reason to focus on an applied science like range management. We may then legitimately restrict our analyses to the "pure" science, where we do not have to worry about distinguishing knowledge-development from application of knowledge.

Furthermore, if there are indeed ways that society plays a role in shaping the knowledge-production process in applied science, as I will attempt to show, then that introduces at least two complications for the sociologist and philosopher. First, analysis of the influence of that role in shaping knowledge-production would require the skills of both the sociologist and philosopher. In fact, even those historians who assume range management research is just like knowledge-production in "pure" science drew on models from both sociology and philosophy in their analyses. Alexander, for example, drew from the works of Max Weber, Anthony Giddens, Robert L. Heilbroner, and Alfred D. Chandler, Jr., as well as Kuhn; and Tobey relied extensively on both Kuhn and Diane Crane's notion of "invisible colleges." Second, if society indeed has that influence, then we may not be able to generalize from particular studies. We need to consider that in each case a different social context provides the background, and that the influence of the role of society on shaping knowledge-production may therefore vary in each context. Yet sociologists and philosophers have, until recently, been concerned primarily with developing generalized sociological and philosophical models to account for the knowledge-development processes in the sciences. Even if we can develop ways to generalize about knowledge-development in the applied sciences, we may still not be able to construct models on the basis of those generalizations. It is not surprising therefore that sociologists and philosophers have not paid much attention to applied sciences like range management.

One work, however, deserves mention because it claims to be an STS-type analysis and because it presents an argument that societal influences can direct the course of knowledge-development. In Mission-Orientation in Ecology, Jacqueline Cramer argues that societal goals can be integrated into research strategies.[18] Those societal goals can therefore influence the development of concepts and methods within a particular discipline, or influence the extension of concepts and methods in particular directions.[19] She argues that Dutch ecologists saw social orientation - or mission-orientation, as she calls it - as a

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[18] Jacqueline Cramer, 1987, Mission-Orientation in Ecology: The Case of Dutch Fresh-Water Ecology (Amsterdam, the Netherlands: Rodopi).

[19] Her argument was influenced tremendously by the finalization movement in science studies. The finalists argued that factors external to science can at times influence theoretical developments within science (see Gernot Bohme, Wolfgang van den Daele, Rainer Hohlfeld, Wolfgang Krohn, Wolf Schafer, 1983, Finalization in Science: The Social Orientation of Scientific Progress, ed. by Wolf Schafer, translated by Pete Burgess (Dordrecht, Boston: D. Reidel Publishing Company). The movement contained a strong normative and Marxist component in that the finalists argued that governments thus could and should steer developments in science by means of science policy to meet social needs. The movement immediately attacked by both scientists and science studies analysts, and the debate was picked up by the mass media in Germany because of its implications for science policy. The attacks centered primarily on the finalists' view of science. Some were troubled by the finalists' attack on the autonomy of science. Others questioned the finalists division of science into stages. For a review of the criticisms and the debate in the mass media, see Frank R. Pfetsch, 1979, "The 'Finalization' Debate in Germany: Some Comments and Explanations," Social Studies of Science 9:115-24.

means to develop theory.[20]

Unfortunately, she never establishes her case clearly. She shows how society's interest in ecology increased the amount of funding available for ecological research. She also showed how a group of scientists subscribing to a minority view - the ecosystem concept, and the study of ecosystem dynamics and productivity - received the bulk of the funding. While it is true that the government as a condition for funding the research stipulated that the ecologists concentrate on ecosystem productivity, she never establishes that this had any effect on either the knowledge-production process or on the nature of the knowledge produced. Those scientists received a competitive advantage over their colleagues by having their research funded, and could therefore develop new insights into ecosystem dynamics. But that does not imply that the social direction actually influenced the kinds of concepts and methods that the ecologists developed. She thus overstates her claim that societal goals can be integrated into knowledge production.

Range managers and scientists within the USDA at times also reviewed the history of their discipline.[21] Most of those accounts

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20 Cramer, 1987, 215.

21 See William R. Chapline, Robert S. Campbell, Raymond Price, and George Stewart, 1944, "The History of Western Range Research," Agricultural History, 18(3):127-43; Robert S. Campbell, 1948, "Milestones in Range Management," Journal of Range Management, 1(10):4-8; Laurence A. Stoddart, 1950, "Range Management," in Fifty Years of Forestry in the U.S.A., ed. by Robert K. Winters (Washington D.C.: Society of American Foresters), 113-135; William R. Chapline, 1951, "Range Management History and Philosophy," Journal of Range Management, 4(9):634-8; Arthur W. Sampson, 1955, "Where Have We Been and Where are We Going in Range Management?" Journal of Range Management, 8(6): 241-6; Talbot and Cronemiller, 1961; and William R. Chapline, 1980, "First Ten

simply review the accomplishments and are not very analytical. Although they respect the work of the early researchers in establishing the research program, they fail to analyze in any detail how that knowledge was developed. They describe the period between 1898 and 1908 as characterized by "observational type studies,"[22] or as the "exploratory period."[23] The value of those early studies lay in the fact that they "indicated the real necessity of range research."[24] Those scientists also acknowledged the value of the recommendations on range management articulated by the first researchers. Nevertheless, because they fail to analyze the knowledge-production process, they do not recognize that those early studies contributed more to the knowledge-base of range management than merely the recognition of the need for further study.

Furthermore, when those scientists discuss the relationship between range management and other disciplines, they tend not to analyze the exact nature of that relationship. They concentrate instead on outlining what those other disciplines have contributed. Those reviews, moreover, are sometimes merely a glorification of past accomplishments and the value of "pure" science. The purpose then is to justify continued funding and to argue that scientists should be free of any kind of social interference in their research.[25] Those reviews are

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Years of the Office of Grazing Studies," Rangelands, 2(6):223-7.

22 Talbot and Cronemiller, 1961, 99.

23 Chapline et al, 1944, 127.

24 Ibid., 128.

25 Although most of these reviews written by scientists suffer from that malady somewhat, one is really built on nothing but that kind of argument. See Harding, 1947.

nevertheless, important historical documents because they mention most of the major works that have shaped the development of range management.

METHOD AND MATERIALS

I will attempt to show in this study of the development of range management as an example of an applied science that society - in this case the stockmen - played a continuous role in shaping the knowledge-development process. To show the influence of te stockmen on the knowledge-development process of range management researchers, I will analyze the goals, concepts, methods, and criteria outlined in primary sources as appropriate for research in range management.

If society indeed influenced knowledge-development, we would expect to see it reflected at least in the objectives of range management's research program. We would expect that the objective of the research plan was not just to develop new knowledge, but to develop new knowledge that could provide the foundation for a management plan usable by society. In fact, that was why the U.S. government sponsored research in such areas as range management: to help the farmers and ranchers. The traditional view of government-sponsored applied science in agriculture also showed that influence as providing the research programs' objectives, thereby providing the impetus for research. Yet this thesis will challenge their assumption that society did not influence other elements of the knowledge-production process.

I include methods and concepts as part of the analysis because they constitute the means used to achieve the research program's objective. They are the cornerstones of the practice of empirically-oriented applied sciences like range management because they provide the foundation for the formulation, execution and interpretation of experiments. This study attempts to show that the stockmen influenced the scientists' use of concepts and methods, and also influenced the development of new concepts and methods. Finally, I will focus on the criteria scientists used to evaluate new knowledge they had produced. Those criteria determine the validity of the knowledge. I will therefore analyze whether the stockmen played a role shaping the scientists' interpretation of each experiment, and whether the stockmen's interpretation influenced the scientists' judgment whether the new knowledge contributed to meeting the objective of the research program.

To accumulate the data necessary for this thesis, I collected a group of articles and books, most of which were written by range managers; the rest of which were written by ecologists. I analyzed those articles and books in terms of the goals, methods, concepts and criteria articulated by their authors as appropriate for applied ecology. I also examined the general ecology texts referred in those books and papers. The purpose, however, was not to compare "pure" and "applied" ecology. Applied ecologists undoubtedly integrated concepts and methods from pure ecology - they do not operate in isolation. When they integrated methods or concepts from pure ecology into their discipline, I pointed that out, but limited the analysis to the way applied ecologists used and defined those concepts and methods. The objective is to analyze the role these concepts and methods played in applied ecological knowledge-development.

I used several strategies to locate those articles and books. First, I worked backwards through bibliographic citations in order to identify the books and articles that are considered crucial by range managers. Second, I used review articles and symposia concerned with the development of range management and with the question of the application of ecological knowledge. Such articles appeared, for example, in the Journal of Range Management (first published in 1948), in Agricultural History, in Ecology, and in Ecological Monographs. These allowed me to identify traditions, thereby outlining at least a part of the conceptual framework of the field, and also pointed to important books and articles in each field. Third, each year, the Secretary of the Department of Agriculture publishes a review of that year's work in the Yearbook of the Department of Agriculture. I examined the "Report of the Secretary" it contains to gain some insights into the important work in range management for each year. Fourth, I used secondary sources, especially Egerton, Van Dyne, and Rowley.[26] Although each of these sources contains its own bias, they are useful resources if used carefully. In these particular texts, I was as interested in the primary sources cited by their authors as in their arguments.

Fifth, I used articles that appeared in journals targeted at a wider audience, including Science and stockmen's magazines and journals.

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[26] Egerton, 1985; Rowley, 1985; and George M. Van Dyne, ed., 1969, The Ecosystem Concept in Natural Resource Management (New York: Academic Press). Van Dyne's book contains a collection of articles reviewing various fields in applied ecology that argue that applied ecology has "philosophical validity of its own" (ecologists and forester Stephen Spurr, in Van Dyne, 3).

These may articulate elements of the conceptual framework that applied ecologists wanted to project to a larger audience. Sixth, I analyzed books used as textbooks in universities, both to analyze the conceptual framework they outlined for the students as appropriate for applied ecology, and also as a resource pointing me to texts their authors considered significant.[27] Last, three general works on range ecology and research have also been helpful in identifying crucial concepts and methods in range management.[28]

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27 Arthur W. Sampson, 1923, Range and Pasture Management (New York: John Wiley and Sons, Inc); Arthur W. Sampson, 1952, Range Management: Principles and Practices (New York: John Wiley and Sons, Inc.); and Laurence A. Stoddart and Arthur D. Smith, 1955, Range Management, second edition (New York: McGraw-Hill Book Company).

28 Robert R. Humphrey, 1962, Range Ecology (New York: The Ronald Press Company); National Research Council, Committee on Range and Pasture Problems, 1962, Basic Problems and Techniques in Range Research (Washington D.C.: National Academy of Sciences - National Research Council, Publication No. 890); John F. Vallentine and Phillip L. Sims, 1980, Range Science: A Guide to Information Sources (Detroit: Gale Research Company).

CHAPTER ONE
ESTABLISHMENT OF AN APPLIED SCIENCE RESEARCH PROGRAM: SOCIETY'S CRITERION OF "IMMEDIATE UTILITY."

In 1896, the Secretary of the Interior asked the National Academy of Sciences to investigate range conditions. The Academy appointed a committee consisting primarily of Harvard and Yale professors and curators of museums, and only one forester, Gifford Pinchot.[1] The preservationist John Muir also accompanied the committee, and his ideas influenced its conclusion that grazing should be excluded from all Forest Reserves. That recommendation caused an immediate uproar among the stockmen in the West.[2] The Secretary then asked the USDA to look into the problem. Beginning in 1897, USDA researchers performed several studies to examine the background of those problems. Between 1897 and 1902, as a result of those studies, many basic principles and methods for range management were developed and put into practice.[3]

ESTABLISHMENT OF THE RESEARCH PROGRAM: 1897-1902.

The first range management researchers allowed the primary users of the range, the stockmen whose livelihood depended on it, influence on the knowledge-production process. Their studies provided the foundation for a research program. They pointed to specific problems, and some

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[1] Those may have been the only qualified scientists available.

[2] For a more detailed analysis of this committee's activities and the effect of its report in the West, see Rowley, 1985, 25-31; Rakestraw, 1958, 375-6, 380-2; and Dupree, 1957, 242-44.

[3] Talbot and Cronemiller, 1961, 99, argue that these studies "yielded many clues to basic principles in management and improved systems of grazing," that "were incorporated into the grazing policy pertaining to public lands, and to a large extent are in effect today."

possible solutions subsequent researchers could test. They also developed some of the first concepts and methods to study the range, and developed methods to test and revise knowledge. Finally, they established the objective of range management. The stockmen played that direct role in the development of knowledge only for a short time - however, their influence would permeate the foundation of range management's research program.

In 1897, the USDA commissioned two studies to examine conditions on the Western range. In one, Frederick V. Coville examined the effects of sheep grazing on the range in the Cascade Mountains of Oregon. In the other, H.L. Bentley, and Jared G. Smith examined the condition of the range in the Southwest.

Coville was born in New Jersey in 1867 and received his A.B. from Cornell in 1887. In 1888, the USDA hired him as an assistant botanist; five years later, he was promoted to chief botanist. In 1890, before the USDA sent him to Oregon, he conducted an ecological study of Death Valley.[4] At the time they wrote their articles, Smith was an assistant chief of the USDA's Division of Agrostology, and Bentley was a special agent of the USDA.[5] All indications are that neither had any formal

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[4] See Eugene Cittadino, 1980, "Ecology and the Professionalization of Botany in America," Studies in the History of Biology 4:171-98. He indicates it was a "pure" ecological study analyzing the distribution of species and the adaptations of plants to the desert climate.

[5] Smith had written an earlier paper on range conditions in the West. See Jared G. Smith, 1895, "Forage Conditions of the Prairie Region," Yearbook of the Department of Agriculture, 309-24. However, in that paper he did not indicate how he arrived at those observations and recommendations, he just stated them. I therefore concentrate on his later study.

higher education beyond a bachelor's degree.[6]

Although these two studies differed in some details, they employed nearly the same methods and drew similar conclusions. The USDA's sponsorship of those studies could account for part of that similarity. In 1862, the USDA was created under an Act stipulating it should develop and diffuse knowledge concerned with agriculture.[7] Within that general framework, the USDA gave its researchers considerable latitude in the questions they wanted to pursue, while encouraging research useful to the users of the knowledge.[8] An additional factor, however, was also important.

Faced with the problem of developing some insights into current conditions of the range, all three researchers chose the same starting point: the experiences and knowledge of stockmen.[9] They sought the

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[6] Unfortunately, not much more is known about these researchers.

[7] Harding, 1947, 328.

[8] See, for example, Everett M. Rogers, 1988, "The Intellectual Foundation and History of the Agricultural Extension Model," Knowledge: Creation,Diffusion, Utilization, 9(4):492-510. He argues that the reward system in the USDA and experiment stations encourages developing useful knowledge and publishing it in a way that the farmers and ranchers can understand.

[9] According to L.A. Stoddart, 1950, all three researchers were born in farming areas. It is likely that the researchers were inclined to begin their studies by incorporating the stockmen because they were raised in farming areas. That possibility will need to be examined in the future. I do not think, however, that it poses a problem for my argument. If these scientists were raised on farms, it would only add another element to my explanation of their reasons for developing certain conclusions, and thereby strengthen my argument. My argument, in that respect, is similar to Tobey's. He argues that Frederick E. Clements, one of the founders of the "pure" science of plant ecology, was inspired by the recognition of the impact agriculturists can have on nature. (Ronald Tobey, 1976, "Theoretical Science and Technology in American Ecology," Technology and Culture, 17:718-28).

stockmen's opinions on the current condition of the range, causes of range deterioration, and suggestions on range improvement. Smith mailed out a letter to stockmen "asking for estimates as to the percentage of increase or decrease"[10] of the range's condition. He further asked them "what, ..., were the chief forage problems ..., and advice was asked as to methods of restoring, renewing, and improving the ranges where they had been overgrazed."[11] Coville, before setting out on his investigation, talked to John Minto. Minto was a stockman in Oregon and an outspoken opponent of both Muir and the Academy's report.[12] Coville explicitly acknowledged that he incorporated the stockmen in his research through his contact with Minto. In his introduction he stated that "Mr. John Minto, ... gave a general letter of introduction to the sheepmen of eastern Oregon which made it possible to secure a large amount of information through channels that ordinarily would have been closed to a Government officer investigating this subject."[13]

The stockmen reported that human activity was a major cause responsible for the depletion of the ranges. Unfortunately, Coville,

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10 Jared G. Smith, 1899, "Grazing Problems in the Southwest and How to Meet Them," U.S. Department of Agriculture, Division of Agrostology, Bulletin 16, 10.

11 Ibid., 10-11.

12 On Minto's views on sheep grazing in Oregon, see Rakestraw, 1958. For a more general overview of Minto's response to government policies pertaining to the Western ranges and public lands, see Thomas R. Cox, 1983, "The Conservationist as Reactionary: John Minto and American Forest Policy," Pacific Northwest Quarterly, 74(4): 146-53.

13 Coville, 1898, "Forest Growth and Sheep Grazing in the Cascade Mountains of Oregon," U.S. Department of Agriculture, Division of Forestry, Bulletin 15, 7. Rakestraw also argues that Coville's report reflected the opinions of Minto and the stockmen rather than those of Muir (Rakestraw, 1958, 377).

Smith, and Bentley only summarized those responses, without actually quoting any of the stockmen.[14] In 1898, Bentley, after talking to local stockmen, stated that "there is hardly one cowman out of ten in central Texas but knows that he has more cows on his range already, or is prepared to put more on, than his best judgment suggests."[15] Four years later, he stated that according to Mr. Middleton, a local stockman, "the large difference between its present and its former condition was due to the fact that, in common with all the other range lands of the section, it had for years been overstocked."[16] According to Smith, stockmen had recognized years earlier that their actions could have a dramatic impact on the range. "Cow men thought that they could not put enough cattle on the ranges to eat all the free grass, and it was a very great surprise to most of them when in 1884 they began to discover the fallacy of this idea."[17] Economic realities and the land-holding pattern prevented them

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[14] In 1904, the USDA sent out another questionnaire to stockmen in the West and Southwest, and again asked them for their opinion on the causes of range depletion. By far the largest proportion of the 1,400 respondents attributed the depletion to human causes such as bad management and overstocking. However, this study also only summarizes the responses. See Albert F. Potter and Frederick V. Coville, 1905, "Grazing on the Public Lands: Extracts from the Report of the Public Lands Commission," U.S. Department of Agriculture, Forest Service, Bulletin 62, esp. p.13.

[15] H.L. Bentley, 1898, "Cattle Ranges of the Southwest: A History of the Exhaustion of the Pasturage and Suggestions for its Restoration," U.S. Department of Agriculture, Farmer's Bulletin 72, 11. Their reason for overstocking the range is discussed in a subsequent section.

[16] H.L. Bentley, 1902, "Experiments in Range Improvement in Central Texas," U.S. Department of Agriculture, Bureau of Plant Industry, Bulletin 13, 16.

[17] Smith, 1899, 10; see also pp. 11-12, where he summarizes the stockmen's responses to the questionnaire in a table.

from acting on that realization at the time.[18]

In 1900, David Griffiths, another range researcher, also mailed out a questionnaire to stockmen, and quoted their responses. One stockman, C.H. Bayless argued that the vegetation should grow as well then as it had fifteen years previously. That it did not he attributed solely to overstocking. "Vegetation does not thrive as it once did, not because of drought, but because the seed is gone, the roots are gone, the soil is gone. This is all the direct result of overstocking."[19] H.C. Hooker, another stockman, attributed the depletion "principally to overstocking."[20]

The researchers adopted the stockmen's view that nature is dynamic, and that humans are the most powerful actors affecting the course of that dynamism. Smith argued that overgrazing and bad management are the most important causes of range destruction. For example, he mentioned that "overstocking not only causes loss of cattle and sheep from

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18 I will discuss the land-holding pattern in the next section. The way Coville summarized and utilized the stockmen's perceptions is the most difficult to retrieve from his report because of the way he wrote. He described two different management practices current among stockmen in Oregon, one where the herders herded the animals together in bands, and another where the herders allowed the sheep much more freedom to roam. He then stated that stockmen who used the second method thought that it caused less damage to the range because sheep "trample the feed less." (Coville, 1898, 14). He continued by stating that "each system doubtless has its advantages."(Ibid.) Yet later, when he discussed overgrazing, he implicitly adopted the argument of the stockmen who gave their sheep the freedom to roam, arguing that "the principal bad effects of overgrazing are to be attributed rather to trampling than to close cropping." (Ibid., 27)

19 Quoted in David Griffiths, 1901, "Range Improvement in Arizona," U.S. Department of Agriculture, Bureau of Plant Industry, Bulletin 4, 13. The details of Griffiths' research will be analyzed in the next chapter.

20 Ibid., 12.

starvation in time of drought, but it causes the rapid extermination of the most valuable of the native grasses and forage plants."[21] Coville argued that human actions accounted for most of the damage to timber production and to the range in the forest reserve. Many herders would herd the sheep together in one large band to protect them from predators, and to make their own job easier. They would also bring the band of sheep to the same grounds every night to bed them down. That stock-management system caused trampling of forage before the sheep had a chance to utilize it, and the sheep tended to eat all the plant-life in the one small area where they were confined by the herder.[22]

The recognition of human impact on the course of nature led these researchers to argue that human actions needed to be controlled in order to preserve the range. For example, Coville examined the economic importance of the sheep industry in Oregon. He concluded that grazing should not be banned from the National Forests but should be controlled so "the forage crop does not decrease from year to year."[23] All three also pointed out that conservation of the range would provide economic benefits to the stockmen. Smith outlined two important benefits. First, the economic return on individual animals would increase because their condition is directly related to the condition of the range. Second, the value of the land would increase because more animals could graze on it.[24]

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[21] Smith, 1899, 13.

[22] Frederick V. Coville, 1898, 26-7.

[23] Ibid., 26.

[24] Smith, 1899, 10.

Coville, Bentley, and Smith sought to establish a research program that could provide the knowledge-base for continued use of the range consistent with conservation. Therefore, the objective of range management was to devise a plan for the management of the range. That plan should be usable by others, and use of the range should lead to maximum returns by ensuring it is consistent with conservation. Use consistent with conservation would remain the objective of range management.[25] That objective did not imply use only for grazing, but meant multiple-use, including timber production, recreation, wildlife management, watershed protection, etc.. In practice, however, they concentrated primarily on the effects of grazing, and on ensuring that grazing would be consistent with conservation. Use consistent with conservation also did not imply preservation. Muir and other preservationists thought use would inevitably damage the natural environment, and therefore opposed use. These researchers assumed that the natural resources could be used without damaging the environment.

They established the foundation of that research program by allowing the stockmen to shape the knowledge-development process, and that process therefore reflected the interests of the stockmen. The stockmen used the range to gain economic benefits. As such, the stockmen were interested not only in knowledge that was somehow useful, but primarily in knowledge that was immediately utilizable. Both the concern with economic returns and with immediate use influenced the knowledge-development process.

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25 See, for example, the two major textbooks in range management in the 1950's: A.W. Sampson, 1952, ; and L.A. Stoddart and A.D. Smith, 1955.

At times, the stockmen clearly influenced knowledge-development directly. At other times, that influence was apparently only indirect: the researchers internalized the values and concerns of the stockmen, and then went their own way without dealing directly with the stockmen. Yet the way they internalized those concerns still meant that the stockmen's values, concerns, and interests shaped knowledge-development. The researchers made the stockmen's values and concerns their primary focus rather than relegating them to secondary status in favor of, for example, their own status as scientists or the demands of their discipline.

The researchers allowed the stockmen to shape the knowledge-production process in four ways beyond problem definition. First, to develop the initial concepts for range management, they adopted the stockmen's language. Second, they adopted the stockmen's recommendation on the best way to deal with the source of the problems facing the range. There were two problems regarding the state of the range that needed to be solved: determine the source of the problem to prevent further damage and analyze how to restore the range to a more productive condition. The researchers adopted the stockmen's view of the source of overgrazing and their proposed remedy, thereby allowing the stockmen to determine the first plan for the management on the range. Third, they gave the stockmen the authority to help design and to evaluate the experiments carried out by the researchers to analyze how to restore the range. Finally, they adopted the stockmen's criterion for the evaluation of new knowledge, thus ensuring that the knowledge-development process reflected the interests of the stockmen.

LANGUAGE

Due to their contacts with the stockmen, Coville, Bentley, and Smith adopted part of the stockmen's language to provide the first concepts for range management, thereby allowing the stockmen's values and interests to shape knowledge-production. Adopting the stockmen's language ensured that their economic concerns played the central role in knowledge-production. It also ensured that the stockmen could use the knowledge because it would be expressed in language they could understand.

Coville superimposed the language of the stockmen onto the language of the taxonomist. To distinguish among the different kinds of forage, he used the definitions of the herders. He adopted the herders' distinctions among forage plants as grass, weeds and browse. He also accepted the herders', rather than the taxonomists', definitions of grass, weeds, and browse. Following the herders, Coville applied the term grass "not only to true grasses, but to all plants resembling grass in appearance."[26] Furthermore, he defined weeds as "all herbaceous plants that do not have the general appearance of grasses, a difference due to chiefly to their broader leaves," and browse as "shrubs and young trees, the leaves and twigs of which are eaten by sheep".[27] The herders made one other distinction among forage plants that Coville also adopted. The herders distinguished between "light" and "strong" feed.[28] "Light" feed fattened quickly, but that fat was lost quickly when the

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[26] Coville, 1898, 18.

[27] Ibid..

[28] Ibid., 23.

sheep had to move. "Strong" feed, in contrast, did not fatten as quickly, but the sheep retained the gain in weight even if forced to travel long distances.

When he analyzed the management practices best suited to those ranges, he did so in terms of the herders' language. He discussed the vegetation on the ranges in Oregon, naming the plants on the basis of the taxonomists' vocabulary, but then analyzing their characteristics on the basis of the stockmen's language, e.g., a plant's qualities as light or strong feed. As a result, he ensured that his analysis focussed on the effect the vegetation had on the production of sheep. That focus reflected the economic interests of the stockmen. Thus, because he adopted the stockmen's language, their language influenced his interpretations of the vegetation. The stockmen's language thereby also became embedded in subsequent analyses of a vegetation's characteristics. Finally, the herders based their management practices on their definitions of the characteristics of the feed, feeding the sheep "light" feed early in the summer and "strong" feed later, just before the sheep had to travel out of the mountains to their winter pasture. Coville recognized the usefulness of that distinction between the different qualities of feed and of the management practice based on it.[29]

Both Bentley and Smith adopted the concept "grazing capacity" from to the stockmen. They used the concept to express how many animals

29 His recommendation, discussed in the next section, that sheepmen should have access to the ranges in the Cascade Mountains was based in part on his acceptance of that distinction and the management practice based on it.

could graze on a certain range of a certain size, and benefit from that grazing - e.g. gain weight. Note that the definition was in terms of animal production, a measure the users of the knowledge could understand, and which for them struck to the economic heart of the matter. Neither one formally defined the term at this time, but just adopted it from the stockmen.

Initially, range management researchers used the terms "grazing capacity" and "carrying capacity" interchangeably. The term "carrying capacity," however, gradually disappeared from the range management literature.[30]

The term "grazing capacity" would become central in range management, but other range researchers such as range ecologists argued that range management researchers erroneously attributed the same meaning to that term and "carrying capacity." They distinguished between the two concepts. Agreeing with the range management researchers, they defined "grazing capacity" as the maximum number of stock that could graze a range without causing deterioration. They thus accepted the economic concerns at the heart of that definition. However, they did not define "carrying capacity" in economic terms. Instead, they defined "carrying capacity" in terms of the number of animals a certain area could consistently support under the greatest possible amount of stress.[31]

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[30] I will therefore use the term "grazing capacity".

[31] They defined "grazing capacity" as "the maximum stocking rate possible which a range can support without deterioration." They did not use the same definition for carrying capacity: "In its true sense, the maximum number of individual animals that can survive the greatest period of stress each year on a given land area. It does not refer to

Nevertheless, because range managers, due to the influence of the stockmen, defined both concepts in terms of the economic concerns of the stockmen, those concerns penetrated to the core of range management research. Future research would be shaped by that definition, containing the stockmen's concerns. Scientists would design research attempting to measure, conserve, and improve range condition guided by a view of range condition based on the stockmen's economic concerns.

That view of range condition was inconsistent with the general objective of the research program. That objective was to ensure use consistent with conservation, where use implied multiple use. Yet due to the particular economic concerns at the core of their view of range condition, range management researchers generally would not focus their research on analyzing the effects of multiple use, or on what combinations of use were most consistent with conservation.[32] Instead,

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sustained production. In range management, the term has become erroneously synonymous with grazing capacity." For these definitions, see Kamal Ibrahim, 1975, Glossary of Terms Used in Pasture and Range Survey Research, Ecology, and Management (Rome, Italy: Food and Agricultural Organization of the United Nations), 21 and 56.

[32] See, for example, the research by the next generation of scientists, discussed in the next chapter. This would remain a problem in range management for a long time. For example, in 1955, Donald Huss developed a formula to calculate a base lease price that would encourage conservation. Yet all the terms in his formula were related to either stock animals or their economic value (Donald L. Huss, 1955, "A Basis for a Conservation Lease of Rangeland on the Edwards Plateau of Texas," Journal of Range Management, 8(5):208-10). Elsewhere, Robert Williams analyzes how to achieve uniform use of ranges, yet limits his analysis solely to stock management practices and other practices related to the production of stock (Robert E. Williams, 1954, "Modern Methods of Getting Uniform Use of Ranges," Journal of Range Management, 7(2):77-81. Rhetorically, scientists would argue that they should consider many other factors as well. In 1956, David Costello argued that scientists "must include measurement or evaluation of factors which effect stream flow, siltation, water yield, wildlife production, and recreation values"(David F. Costello, 1956, "Factors to Consider in the Evaluation

they would focus their research on the effects of grazing and on how to increase the grazing capacity in order to increase production of stock. Furthermore, methods to manage the range would be judged not with regard to their effect on the carrying capacity but with regard to their effect on the grazing capacity.[33]

Allowing the economic concerns of the stockmen to shape the research process in such a way was also inconsistent with the view in the USDA and the experiment stations on what the nature of that process should be. The objective of the USDA and the experiment stations was and remained producing knowledge of use to the farmers and ranchers, and the objective was therefore oriented towards their economic concerns. According to the standard set by Atwater, however, that objective was not supposed to have any influence, either direct or indirect, on the knowledge-production process. Achieving that objective should be accomplished by focussing the research along strictly scientific lines. Nevertheless, by adopting the stockmen's language, Bentley and Smith clearly allowed the stockmen's economic interests to have a direct

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of Vegetation Condition," Journal of Range Management, 9(2):74). Yet he does not indicate how or when scientists should include these factors, focusing instead on factors that have much more immediate relation to grazing.

[33] As a result, they were conservationist in only a very limited sense. They were only concerned with measuring, conserving, and if possible improving the grazing capacity. Other factors of the range were studied only marginally when they decreased the grazing capacity. Rodents and predators, for example, decreased the grazing capacity; rodents because they consumed some of the forage and predators because they killed stock. Range management researchers' therefore focussed on eliminating them as factors affecting grazing capacity. Since they already knew the effect each of these kinds of animals had on the grazing capacity, they did not study them. Instead, they focussed on finding ways to eliminate them, which usually meant killing them.

influence on the course of knowledge-production.

IDENTIFYING THE SOURCE OF THE PROBLEM

In order to deal with the problems on the range, the researchers needed to determine the source of those problems and to propose a plan to remove that source. They allowed the stockmen direct influence on the knowledge-development process when they attempted to address those questions. The problems facing the range were partly caused by the land-holding pattern in the West and Southwest. That land-holding pattern made management difficult. The government owned most of the range land, and the stockmen therefore tended to regard the range as "free" land. They were therefore motivated to get all they could off the land. After all, they thought, why insure that the land will produce a good forage crop next year if next year another stockman beats you to that land and benefits from your prudence. And to ensure they would get to the land first, the stockmen would race there and begin grazing before the plants had a chance to mature, thereby destroying the forage before it had a chance to reproduce.

The stockmen identified the land-holding pattern as a source of the problem, pointed to the need to reform that land-holding pattern, and all three researchers accepted their opinion.[34] The stockman C.H. Bayless argued for changes in the land-holding pattern. Bayless thought that depletion "can not be prevented on our open range where the land is

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[34] On this problem of the land-holding pattern in the West and Southwest, in addition to the papers by Coville, Bentley, and Smith, see also Rowley, 1985, esp. 15-21; and Foss, 1960.

not subject to private control."[35] He argued that "no practical plan can well be advanced for increasing plant growth on any open range while free for the use of everybody."[36] In 1905, a report of the Public Lands Commission further corroborated the stockmen's and researchers' opinion.[37]

Following the stockmen's suggestion, Coville and Smith recommended that stockmen be granted long-term leases guaranteeing them access to particular sections of the range.[38] Coville worked out a detailed proposal under which a stockman would purchase a five-year permit to a particular section of the range at a minimal fee, and that stockman would have first right of renewal. The stockmen would assume the responsibility to attempt to prevent fires on the range and to prevent overgrazing. In return, he received guaranteed access to the same range for a number of years, and therefore did not have to race to the range to beat out his competitors. In addition, it might motivate him to

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[35] Griffiths, 1901, 13.

[36] Ibid., 12-13.

[37] Potter and Coville, 1905. The majority of stockmen in the West and Southwest indicated that some form of government action was necessary, with "only 64 out of 1,400" wanting to be left alone.(24)

[38] One of the standard criticisms in the historical literature on science in the USDA and the experiment stations is that they helped the rich farmers and ranchers. The usual argument is that innovation and application of new knowledge required money and education, which in turn required money (see, for example, Rosenberg, 1961, 170; and Margaret W. Rossiter, 1979, "The Organization of the Agricultural Sciences," in The Organization of Knowledge in Modern America, ed. by Alexandra Oleson and John Voss (Baltimore and London: The Johns Hopkins University Press), 241). In this case, it is certainly possible that these three researchers built that bias towards the rich into their recommendation. Unfortunately, neither Coville, Bentley nor Smith provided information on the social and economic background of the stockmen they talked to.

ensure that he did not damage the reproductive capabilities of the range, and might even motivate him to improve his part of the range in order to increase its grazing capacity.[39]

By recommending that range permits be granted for five years, the researchers had incorporated the stockmen's concerns into their recommendation. C.H. Bayless, for example, argued that "I must respectfully urge upon you [the researcher] the importance of impressing the government officials with the fact that no general improvement of range country can be expected until the land is placed under individual control by lease or otherwise."[40] Furthermore, rather than alienating the stockmen, for example, by recommending exclusion of livestock, the researchers' recommendation actually improved the stockmen's business by guaranteeing them access to the land. Not surprisingly, many stockmen approved of the recommendation. "In discussing the proposed plan with stockmen it was found ...that all those to whom there was opportunity of explaining it fully, without exception approved it ...."[41]

These researchers, in cooperation with the stockmen, reached a conclusion different from the National Academy's recommendation. In part, that may have been due to the orientation within the USDA. Recommending that stockmen should not be allowed to graze the range would not be useful to those stockmen. That would therefore be inconsistent with the mission of the USDA. But Gifford Pinchot, one of

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[39] See Coville, 1899, 46-54; and also Smith, 1899, 44.

[40] Griffiths, 1901, 13.

[41] Coville, 1899, 51. Rakestraw also states that the stockmen accepted the recommendation; see Rakestraw, 1958, 378-81.

the major players in the government with respect to grazing, especially on the National Forests, initially opposed grazing. He had agreed with the National Academy's committee, of which he was a member. He did not change his opinion until 1899, possibly influenced by Coville's report.[42] Furthermore, these three researchers explicitly credited the direct influence of the stockmen on their thinking. Bentley and Smith acknowledged they incorporated the stockmen's opinions through their discussions with them and the stockmen's responses to the questionnaire. Smith, for example, had asked them for advice on "... methods of restoring, renewing and improving the ranges."[43] Coville had accumulated much of his information through his contacts with Minto and other local stockmen.[44] He also stated that "it is found, ..., on conversation with a large number of them that they are opposed to the present method [of control over public lands], and would welcome a change in government policy which would give them a financial interest in the maintenance of good pasturage."[45]

The researchers' recommendation met the objective they had set for their research program. Use consistent with conservation would be enhanced by dealing with the problem created by the land-holding pattern. Allowing the stockmen to influence knowledge-production by adopting their view of the source of the problem and their plan to deal

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[42] See Rakestraw, 1958, 382. Rakestraw thinks Coville's report influenced Pinchot's change of opinion.

[43] Smith, 1899, op. cit..

[44] Coville, 1898, op. cit..

[45] Ibid, 51.

with that problem therefore helped the researchers to satisfy the objective they had set for their research.

METHODS

The inclusion of the stockmen in the researchers' experiments was the third way they allowed the stockmen to influence the knowledge-development process. Recognizing the badly damaged condition of the range, an important question became how to improve it. To get a starting-point to attack that problem, the researchers again turned to the stockmen, and allowed them to participate in both the planning and evaluation of the experiments. The researchers included the stockmen in testing some of the stockmen's recommendations, utilized some of the farm technology available to stockmen and tested their effect, and also devised a method for the management of the range superior to the one advocated by the stockmen. Coville did not perform any experiments, but Bentley and Smith explicitly reached out to the stockmen through their experiments.

Bentley and Smith adopted the stockmen's method for determining grazing capacity. Stockmen determined the grazing capacity by comparing an unknown range with a range of known grazing capacity. That method was very subjective because it depended on the stockman's ability and experience. Its subjectivity, however, did not deter the researchers.

Bentley's 1898 paper included a section addressing the problem of range renewal. He opens that section with the statement that "in considering the question of how the ranges may be renewed, the ideas and opinions of the leading stockmen have been solicited."[46] In their discussions with Bentley, stockmen recommended that finding ways to improve the stock's access to water on the range and reseeding the range were the best ways to improve the range.[47] That raised several questions. Did the land need to be cultivated before it was reseeded? A local stockman suggested that "all the bare spots in the pasture should be harrowed or the crust otherwise broken and seed from the grasses known to be valuable should be systematically gathered and sown there before rains."[48]

The researchers would test that suggestion, but it also raised another question: Was it better to reseed with native plants, or are there some species of plants elsewhere that may be better suited to the climate, and hence should be introduced onto the range? Another local stockman suggested that "seed from the best grasses native to the section should be gathered and scattered about over the pastures, either when the ground is wet or when rain is anticipated."[49] Bentley followed that suggestion with an analysis of the results achieved by someone who planted non-native seeds. Those plants were not well-adapted to the local environment, and died when conditions became harsh. He argued that "here was an object lesson which emphasized the suggestion that native grasses are by far the best for home use; they are suited for the

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[46] H.L. Bentley, 1898, 15.

[47] See ibid., 16-21.

[48] Ibid., 20.

[49] Ibid..

climate and the climate is suited to them."[50] He concluded his 1898 paper with a discussion of those native grass and forage plants that appeared to offer a solution to the reseeding problem, utilizing both the stockmen's and taxonomists' classification system, merging them in the same way Coville had when he adopted the stockmen's language.

Bentley's discussion set the stage for a series of experiments, carried out by Bentley and Smith over the course of three years. They took the stockmen's suggestions on range improvement, and submitted them to experimental testing. The stockmen, therefore, directly influenced the experimental process because they determined the questions that would be examined experimentally. In that way, the researchers hoped to determine those suggestions that had the most merit. In 1899, Smith reported on the first year of those experiments' progress.

The USDA leased land in Texas on which to carry out the experiments, and the researchers divided that land into sections. Each section was then treated in a different way to analyze the results of the stockmen's recommendations. Three sections were treated either with a disk, a disk harrow, or a straight-toothed iron harrow, pieces of farm machinery familiar to ranchers. On other sections, the stock were allowed either to graze until the first of June, and then moved to another range, or they were excluded from a section until June first. The purpose was to examine the effect of resting the range early in the season versus resting it later in the season. Stock were excluded from another section of the range, and the last section was used to

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[50] Ibid., 21.

experiment with artificial reseeding.[51]

A committee of stockmen, not the researchers, evaluated the results of the experiment.

Extension practices current within the USDA could offer some explanation for that move on the researchers' part. The USDA had for several years studied the farm and ranch management practices of the most successful farmers and stockmen. USDA researchers would observe those practices, try to determine why they were successful, and then spread that knowledge to others farmers and ranchers through demonstrations. Yet those farmers and ranchers were never given the power to evaluate the results of experiments, or even to evaluate the practices of their peers. The USDA reserved that task to its scientists. The farmers' and ranchers' role was limited to providing land and stock for the demonstrations.[52]

Practices current within the state experiment stations could also offer some explanation for that move. In 1877, the Connecticut station began utilizing what they called "cooperative experiments."[53] Those experiments were initially designed to test the performance of fertilizers and new crops in different habitats. Many experiment stations adopted this technique, and by 1900 most colleges and stations

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[51] For a more complete description of the experiments, see Smith, 1899, 19-20.

[52] USDA publications (e.g., the Farmers' Bulletin and the bulletins published by the Bureau of Plant Industry) contain may descriptions of the practices of successful farmers and ranchers. The Secretary of Agriculture also often reviewed this branch of the research carried out by the Department; see e.g. the "Report of the Secretary," Yearbook of the Department of Agriculture, 1905, 1909, 1911.

[53] On these cooperative experiments, see Scott, 1970, 148-53.

used it. Yet the terminology was somewhat misleading. First, the scientists had already developed the new knowledge, and the ultimate users of that knowledge had not cooperated in its production. Second, even in these so-called "cooperative experiments," the scientists designed and evaluated the tests, and gave the farmers explicit instructions on how to carry them out. The farmers cooperated merely by providing the land, tools, and labor necessary to carry out the experiment. As payment for their efforts, they were allowed to keep the crops produced during the course of the tests. Finally, from 1895 on, these "cooperative experiments" were used as part of the extension program. The scientists would ask some farmers or ranchers to implement some new knowledge, developed by the scientists, on their farms so others could see the value of the new knowledge.

Bentley and Smith clearly went beyond those traditions. Although those traditions may have inspired them, they went beyond them by giving the stockmen the authority to propose and validate the experiments. They consciously chose to incorporate the stockmen's expertise and judgments into the research process in order to develop the knowledge-base that could serve as the foundation for a research program in range management.

The stockmen inspected each section of the range before the experiment began to determine its grazing capacity. The stockmen returned periodically during the course of the experiment, evaluated the results at the end of each year, and also determined the final result. Smith explicitly stated that role for the stockmen: "These experiments will be carried on for three years, at the end of which time sufficiently definite results ought to be secured to enable the stockmen to decide what is the most practical method of bringing back the grasses."[54]

The committee was only the initial validating group of people. "Stockmen" here referred not only to the committee that evaluated the experiment, but also to all interested stockmen. The researchers allowed all stockmen a say in determining the validity of the knowledge. Application of the methods signalled validation of the knowledge, and non-application signalled that the result of the experiment was not acceptable to the community.

At the end of the first year of experimentation, Smith reported that in the stockmen's judgment, disking of the land led to the most rapid improvement. On the basis of his own observations, he concluded that loosening the soil early in the spring stimulated the roots of plants and caused them to grow more vigorously. He also discussed briefly other aids to range improvement, such as the procurement of sufficient feed for winter so that the stock do not have to be grazed on the range too early.

Smith also refuted one of the methods advocated by the majority of stockmen to improve the range. Many stockmen at the time thought that resting the range would be the cheapest and most effective way to improve the condition of the range. Smith disagreed with those stockmen: "It is, however, not the most rapid method, nor can it be considered the cheapest when one takes into consideration the fact that the land to become fully re-grassed must be rested sometimes three or

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[54] Smith, 1899, 21.

four years."[55]

He argued that stockmen should divide their land into a number of sections. Grazing would begin each year on a different section, on which the stock then grazed for a number of weeks or months. They would then rotate onto another section of the range. That way, grazing would be deferred on some of the sections until after the forage has had a chance to mature and reproduce. Considering that different species of plants, occupying different sections of the range, mature at different times, an understanding of the growth pattern of the plant species might even make it possible by carefully managing the rotation pattern to allow most species to mature before being grazed. This concept would later come to be called "deferred-rotation" grazing, although Smith did not use that term. The advantage over the stockmen's idea was that the range could be used throughout the period of revegetation.

Smith did not test the effect of deferred-rotation grazing. Instead he drew on his understanding of the life-histories of plant species to explain why the method would prove beneficial. He could have acquired that understanding of plants possibly through education and through his contacts with USDA scientists who possessed master's degrees and doctorates. He also offered a hypothetical example pointing to its benefits.

Finally, following his own argument, he referred to the experience of stockmen who had tried the method, again reaching out to them to validate his idea. Here he clearly reached out to the extension practices current in the USDA. He said that stockmen who had used this

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[55] Ibid., 21.

method for a number of years claimed that "their ranges are continually improving, in marked contrast to the deterioration that had occurred through bad treatment of neighboring properties where the old methods were practiced."[56] Those stockmen also stated that "Pasture land thus treated will carry more head of cattle through the year and bring them out in better condition than where the herd has access at all seasons of the year to all portions of the range."[57]

His proposal would meet the objective the researchers had set for themselves, i.e., use consistent with conservation. He used his understanding of the life-histories of plants to explain his reasons for advocating deferred-rotation grazing. Then he drew upon the experience of stockmen who had tested the method to clinch his argument. He had effectively combined the scientist's understanding of the life-histories of plants with the experience of the stockmen to advocate a better way of managing the range.

Last, Smith went beyond the experience of the stockmen and drew upon his own knowledge of plants to advocate a different way of determining range condition. He based his method on the realization that different communities of plants, characterized by different dominant species, will succeed each other. The species most dominant in a particular community can then be taken as an indicator of the condition of the range if one knows what kind of conditions those species prefer.

Smith recognized that both natural causes like the climate and

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56 Ibid., 22.

57 Ibid..

human actions could have an effect on what species would be dominant. "There is often a succession of dominant grasses in nature through natural causes, but never to so marked an extent as on the cattle ranges during the process of deterioration from overgrazing."[58] Keeping track of the dominant species could therefore serve as an indicator whether the range is overgrazed, and thus tell the stockmen and researchers something about the condition of the range. Unfortunately, his method was not adopted by other researchers or stockmen.

Smith's method was ignored because he did not derive it from a source or method usually used by stockmen. Instead, the method was based on the scientist's study and understanding of the natural world. To the stockmen, the validity and usefulness of the method may not have been immediately obvious, and they would have to acquire the botanists' understanding of plants to use it. Furthermore, because the researchers had based the knowledge-development on the methods, concepts, and concerns of the stockmen, even they failed to recognize the method's usefulness.

Smith subsequently moved to Washington, D.C., but Bentley continued the experiments. The design of the experiment remained basically the same, and the authority to validate the results of the experiment still rested with a committee of stockmen. The only major difference was that Bentley asked the committee of stockmen to choose the poorest sections of the range for his experiments. Bentley chose those sections in order to convince the stockmen. If he could achieve good results there, they could see the benefits, and that might convince them to apply the

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[58] Ibid., 28.

methods. In 1902, Bentley published the results of those experiments. He reported that he and the committee of stockmen thought the results of the experiment were satisfactory, and that stockmen could profitably apply those methods to improve the condition of the range.[59]

In his 1902 paper, Bentley articulated what he called a theory that provided the foundation for the experiments on the harrowing of the soil. He thought that harrowing would encourage plant growth because the soil would be looser and softer, because the water would penetrate the soil, and because it would create beds in the soil into which the seeds could fall and thus be better protected from the wind. Bentley just stated this idea, without saying where it came from. He could therefore have developed it on his own, the stockmen could have proposed it, or the researcher and the stockmen could have cooperated in its articulation. Whatever the source of the idea, the stockmen again validated the experiment designed to test it and declared the test a success.[60]

THE "IMMEDIATE UTILITY" CRITERION

The researchers adopted the stockmen's criterion for the evaluation of the knowledge produced. Due to the stockmen's economic self-interest, they were interested not only in knowledge that was somehow useful, but primarily in knowledge that was immediately utilizable. The general objective of the knowledge-development process, therefore, included not only the creation of knowledge to provide a foundation for

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[59] H.L. Bentley, 1902, 11.

[60] Ibid., 18-19.

use consistent with conservation, but also contained the imperative that the knowledge be immediately utilizable. That imperative of immediate utility assumed the function of a criterion. These early researchers thus evaluated their methods, concepts and plans for future management of the range against two criteria. One criterion followed logically from their goal. Any methods, concepts, or recommendations inconsistent with use and conservation would have to be rejected. The other criterion was due to the influence of the stockmen: immediate utility to the users of the range.

Therefore, because the stockmen's economic values, concerns, and interests determined a crucial component of the criteria used for knowledge-evaluation, they had direct influence on the knowledge-development process. Any new knowledge that did not satisfy the immediate utility criterion, and therefore did not satisfy their economic values and concerns, had to be rejected. The scientists did more than merely get to know the farmers and ranchers so they would both gain insights into the problems facing the users of the knowledge and facilitate diffusion of the knowledge. Those stockmen's values and concerns provided a crucial touchstone against which to evaluate new knowledge.

The researchers' recommendation on dealing with the source of the problems on the range met the immediate utility criterion and they had adopted from the stockmen. Under Coville's proposal, the stockmen could continue to use the range, they were guaranteed access to a certain segment of the range, and the recommendation solved a situation they considered a problem. The stockmen could thus immediately use that knowledge to change their management practices.[61]

The results of Smith and Bentley's experiments also met the immediate utility criterion. By utilizing common farm technologies, the researchers insured that the stockmen could apply the knowledge immediately. They also insured they would meet the immediate utility criterion by testing the stockmen's recommendations. The stockmen could see the results of their ideas, and adjust their management practices immediately.

Finally, only one of Smith's proposed methods satisfied the immediate utility criterion; the other was therefore rejected. His method based on the recognition that different communities of plants succeed each other, was ignored because it did not meet the immediate utility criterion. Stockmen would have to acquire a botanist's understanding of nature before they could use it, and therefore rejected it. Smith's deferred-rotation grazing method, on the other hand, met the immediate utility criterion, in addition to encouraging use consistent with conservation. Stockmen could begin to apply that method immediately. Smith's argument that deferred-rotation grazing was also cheaper than the alternative method for range improvement advocated by the majority of the stockmen was based on the immediate utility criterion. Stockmen potentially could apply both Smith's method and their method, but their method was more expensive, and therefore did not satisfy the immediate utility criterion to the extent that Smith's did.

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[61] The question remained, of course, how the government would implement Coville's proposal. On problems associated with implementation, see Rakestraw, 1958; Cox, 1983; Rowley, 1985.

APPLICATION OF THE KNOWLEDGE IN THE MANAGEMENT OF THE RANGE

As a result of the inclusion of the stockmen into the knowledge-development process, the researchers also facilitated the diffusion of the knowledge they created. Given the social circumstances on the range, that was not an easy matter. The researchers, because they worked for the USDA, were perceived by the stockmen as agents of the government. The stockmen generally did not like the government and its agents. They were perceived as interfering with the stockmen's freedom.[62]

By including the stockmen in the design and execution of the knowledge-development process, the researchers had overcome some of that hostility and built a mechanism for the diffusion of knowledge into their research. In 1898, Bentley reported a lack of interest in scientific studies on range management among stockmen.[63] In 1902, after four years of cooperative research, he quoted a stockman to the effect that most stockmen recognized the importance of the research:

> Don't think that because every stockman in these parts hasn't taken up the methods adopted here [i.e. on the experimental range] they are blind or indifferent. Many of them have been watching and taking notes, and are quietly making experiments on their own places, and I predict that the others will do likewise.[64]

The researchers, although employed by the government and working on government-owned land, were having an impact on the stockmen.

Bentley also indicated that the method of incorporating the

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[62] Talbot and Cronemiller, 1961, 97.

[63] Bentley, 1898, 12-13.

[64] Bentley, 1902, 12.

stockmen into the research aided in the diffusion of the knowledge. "These and other results secured had satisfied many stockmen and farmers that, at comparatively small expense, they could greatly improve their ranges."[65] By including the stockmen into the research, and because all new knowledge had to satisfy the immediate utility criterion, the researchers had ensured that the new knowledge fit into the social and economic world of the stockmen.

Coville's argument that the then current system of bedding sheep damaged the range was also accepted by the sheepmen. In 1901, Filibert Roth, another USDA researcher, analyzed grazing practices on the Forest Reserves to determine whether they were consistent with the objectives for which the Reserves had been created. He did not talk to the stockmen directly, but only observed their management practices. He reported that "the best sheepmen have given up the old method of bedding for long periods in the same place and are adopting the proper way, bedding only one or at least only a few nights in a place."[66] He also supported Coville's recommendation that stockmen be given five-year leases to range lands in the Reserves.

Considering the role of the stockmen in the knowledge-development

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[65] Ibid, 26. There is a question how much validity to ascribe to Bentley's statement and to the stockman he quotes. They are rather self-serving, and would justify continued research along the same lines. Unfortunately, I have not been able to locate any sources against which to compare his assessment. However, since these scientists were very goal-oriented, i.e., produce knowledge on the basis of which a management plan can be developed that is consistent with conservation, I think his statement can be taken at face-value. To falsely report that stockmen were testing and adopting some of the results would defeat that goal.

[66] Filibert Roth, 1901, "Grazing in the Forest Reserves, <u>Yearbook of the Department of Agriculture</u>, 345.

process, they had validated the knowledge that provided the foundation for the research program in range management by adopting it into their range management practices. Furthermore, these early researchers had successfully established an applied science research program. Two results made their efforts a success. First, stockmen could begin to deal with the problems facing the range. Second, subsequent researchers could, and would, build on the foundation they had constructed. There was, however, a negative element to the success they had achieved by including the stockmen in knowledge-development: no one recognized the value of Smith's method.

The knowledge-development practices of these researchers nevertheless shared many similarities with those of other USDA and experiment station scientists. All talked with the farmers and ranchers, trying to identify and define their goals, and to determine what they really needed. At the same time, while they were getting to know the farmers and ranchers, they tried to convince the often skeptical farmers and ranchers of science's value. William A. Henry at the University of Wisconsin, Eugene Davenport at the University of Illinois, and Eugene W. Hilgard at Berkeley all established successful experiment stations because they understood the needs and goals of the farmers and ranchers and were able to convince the farmers and ranchers of science's potential to contribute.[67]

Other USDA and experiment station scientists also attempted to present science to the farmers and ranchers in such a way that they could, if they desired, do the science themselves, or at least

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[67] See Rosenberg, 1961, 160-5.

understand the science. Harry L. Russell, for example, recognized the value of that approach during the campaign to eradicate bovine tuberculosis in Wisconsin.[68] No cure for tuberculosis had been developed. The traditional way to deal with infected animals, killing them, was unacceptable to the stockmen. Russell became aware of a new technique to deal with infected animals, developed by scientists in Denmark: quarantining the infected animals. To convince Wisconsin cattlemen, Russell found a sympathetic farmer, and applied the method to his herd. He thereby showed farmers, in a way they could duplicate, that they could build up a new herd of healthy animals from a herd containing both healthy and infected animals.

He then convinced farmers to use tuberculin, the diagnostic scientists had developed, by performing postmortem autopsies on cattle around the state, often at local fairs, showing them that even apparently healthy animals often suffered from advanced cases of tuberculosis. Russell next developed a service linking the farmer and the scientists. The farmer would inject the cattle, record the animals' responses, send the data to the University, where the scientists would analyze the data and tell the farmer what animals had tuberculosis. In return, the farmer had to quarantine all infected animals. The campaign proved a huge financial success for Wisconsin farmers because their cattle became known around the country for being free from tuberculosis.

Coville, Bentley and Smith shared many similarities with scientists in the USDA and experiment stations. By talking to the stockmen, they had gotten to know their goals and needs. By using the farm implements

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[68] This analysis is based on Beardsley, 1969, 25-34.

familiar to the stockmen to experimentally analyze the problems facing the stockmen, they had ensured that all stockmen could perform the experiments, see the results, and determine the validity of the new knowledge for themselves. In the process, they had shown the stockmen the value of science.

Yet some crucial differences also distinguished these range management researchers from other government scientists. They differed most significantly in that they allowed the stockmen to propose and evaluate experiments, thereby giving the stockmen an important direct role in the knowledge-development process. The researchers did not just do the science so that the stockmen could do it, they allowed the stockmen to participate in doing the science. That certainly contrasted sharply with Atwater's view of government-sponsored agricultural science. It also distinguished them from Russell, who turned to the stockmen with knowledge the scientists had already developed. Russell merely showed them the economic advantages of the new method. Furthermore, these three researchers adopted one of their criteria to evaluate the knowledge-production process from the stockmen. Other government scientists relied only on the criteria of their disciplines to evaluate the knowledge - the criteria of the farmers and ranchers never played a role in evaluating the actual knowledge itself. They were only interested in the criteria of the farmers and ranchers when they tried to figure out how the new knowledge could be applied by the farmers and ranchers. These researchers also adopted the "grazing capacity" concept from the stockmen, and thereby ensured that the economic concerns of the stockmen would have a direct influence on the future course of knowledge-development. Last, they adopted a recommendation to deal with the problems on the range from the stockmen, whereas usually the scientists would make the recommendations, and then attempted to show why the farmers and ranchers should apply the recommendation.

CHAPTER TWO
STOCKMEN: FROM DIRECT TO INDIRECT PARTICIPATION, AND THE SCIENTIFICATION OF RESEARCH

Scientists in the early part of the twentieth century generated new questions from the work of Coville, Bentley, and Smith, and adopted many of their conclusions and recommendations.[1] For example, in 1904, USDA agrostologist W.J. Spillman argued that scientists faced two major problems: 1) determining grazing capacity; and 2) developing management methods "to secure the largest amount of feed from it [the range] without permanent injury to the food plants ...."[2] He also outlined other problems: proper number of stock and season of grazing, conservation of native plants, and introduction of other suitable plants. Four years later, Gifford Pinchot argued that scientists should concentrate on analyzing artificial and natural reseeding and changes in stock-handling methods.[3]

Building on that foundation, the new generation tried to be more "scientific," and adhered much closer to Atwater's standard for research.[4] Atwater's standard created a social definition of research

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[1] For example, in 1904, Professor R.H. Forbes, the director of the Arizona Agricultural Experiment Station, repeated their argument about the land-holding pattern. R.H. Forbes, 1904, "The Range Problem," Forestry and Irrigation, 477.

[2] W.J. Spillman, in the preface to Griffiths, 1904, "Range Investigations in Arizona," U.S. Department of Agriculture, Bureau of Plant Industry, Bulletin 67, 5.

[3] Gifford Pinchot, in the preface to A.W. Sampson, 1908, "The Revegetation of Overgrazed Range Areas," U.S. Department of Agriculture, Circular 158, 2.

[4] For an account of the influence of Atwater's standard on the knowledge-development practices in the USDA and the agricultural experiment stations, see Kerr, 1987, op. cit.. Although none of the people cited in the discussion that follows cite Atwater to substantiate

among USDA and experiment station scientists explicitly excluding the farmers or ranchers from the knowledge-development process. As a result, when examining questions generated from that foundation, the scientists alone desired to control the research. Yet the stockmen still influenced the knowledge-development process in several ways, although indirectly. Nevertheless, as a result of the change in research strategy, the immediate utility criterion disappeared from range management research.

Attempting to be more scientific, those scientists adopted a view of science prevalent in much of biology. During the last quarter of the nineteenth century, many American biologists desired to make biology more experimental and quantifiable. They believed they could then develop more certain knowledge and generate causal explanations, hoping thereby to make biology more of a predictive science. That view motivated Frederick E. Clements, one of plant ecology's founders.[5] It also influenced American scientists studying evolution and animal breeding.[6]

Administrators and scientists in the USDA and the experiment

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their views, those views about the knowledge-development process appropriate for science in the USDA and the experiment stations are, as Kerr shows, based on Atwater's standard.

[5] See Tobey, 1981, and Cittadino, 1980.

[6] American evolutionary thinkers, for example, were attracted to the mutation theory as an alternative to Darwinism because it was more rigorously quantifiable and more open to experimentation. See William B. Provine, 1971, The Origins of Theoretical Population Genetics (Chicago and London: The University of Chicago Press). For a more general overview of the attraction of quantification and the experimentation, see William Coleman, 1977, Biology in the Nineteenth Century: Problems of Form, Function, and Transformation (Cambridge: Cambridge University Press), esp. chapter vii, "The Experimental Ideal."

stations expressed their adherence to Atwater's view of research in several ways. They began to exhort the values of "pure" science, and to distinguish between research on the one hand and application on the other hand, arguing that the search for knowledge should proceed without concern with its application. They also expressed that view through the establishment of applied-science disciplines, often in the College of Agriculture in many of the land-grant schools.[7] With that move they hoped both to increase their autonomy and to gain the respect of their peers in the "pure" sciences, who often regarded them as farmers.[8] Range management became established in the colleges as an applied science between 1914 and 1920.[9]

In 1901, Secretary of Agriculture James Wilson stated that scientists should not have to worry about application of knowledge, but "must be able to devote their time and energy quite fully to their investigations."[10] Two years later, he argued that concern with application interfered in knowledge development.

> "There is a danger that the attention which the work of the Bureau in promoting the actual management of forests naturally

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[7] See, for example, Beardsley, 1969, 64-82.

[8] For example, Rossiter, 1979, 224, shows that economic entomologists thought other scientists saw them as farmers.

[9] In 1914, Utah State College offered the first range management course. In 1916, Montana State University established the first range management curriculum, followed by the University of Idaho (1917) and Washington State College (1923). By 1953, fifteen schools offered an undergraduate degree, twelve a M.S. degree, and three a doctorate. These figures are compiled from information in Arthur W. Sampson, 1954, "The Education of Range Managers," Journal of Range Management 7(5):207-12, esp. 210.

[10] "Report of the Secretary," Yearbook of the Department of Agriculture 1901, 68.

> receives may obscure the importance of the investigations which it is conducting along other lines. These investigations are largely scientific in scope and method, but always entirely practical in purpose and outcome."[11]

Other USDA scientists and administrators also drew that distinction between science and its application. In 1905, E.W. Allen, a USDA scientist, stated that making knowledge utilizable had a serious drawback, arguing it took too much of the scientists' time and was "a serious inconvenience and interruption."[12] Allen also thought that politicians and the general public placed too much emphasis on extension work, failing to recognize the contributions of the scientists.[13] Ten years later, D.F. Houston, Wilson's successor, clearly indicated that scientists used different criteria to determine the validity of knowledge and when it could be applied. "Many investigations, while more or less successful from the standpoint of the scientist, have not progressed far enough to yield results which can be applied safely to improve agricultural practice."[14] He did not say how the scientists determined when the research had progressed far enough, or what criteria they used to make the decision. Nevertheless, he assumed they knew the farmers well enough to determine how and when the knowledge could be used.

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[11] "Report of the Secretary" Yearbook of the Department of Agriculture, 1903, 47.

[12] E.W. Allen, 1905, "Some Ways in which the Department of Agriculture and the Experiment Stations Supplement Each Other," Yearbook of the Department of Agriculture, 181.

[13] Ibid..

[14] "Report of the Secretary, Yearbook of the Department of Agriculture, 1915, 15.

Harry L. Russell's career at Wisconsin in the early part of the twentieth century reflected these attitudes towards agricultural science. He exhorted the value of pure research both in his own work (for example, the research he did with Stephen M. Babcock on the role of enzymes in the curing of cheese), and as dean of the College of Agriculture.[15] His objective remained producing knowledge farmers could use. But he thought he could best solve the dairymen's problems through research designed to discover new facts and develop new theories, without concern for use.[16] Russell, and Babcock, carried that view to an extreme: determining the meaning of the knowledge they had developed with respect to current scientific knowledge interested them more than applying the knowledge - they actually hesitated to apply it.[17]

Due to the emphasis on pure science and because scientists concentrated on research, the USDA created new means to bring knowledge to the users. The new knowledge often did not fit the social and economic concerns of the farmers and ranchers. Since the new knowledge consisted of "pure" science, someone needed to explain it and show the farmers and ranchers how to use it. In 1903, Wilson appointed a Farmer's Institute Expert in the USDA's Office of Experiment Stations to coordinate extension activities, but the extension system did not become

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[15] See Beardsley, 1969, 49-82.

[16] The dairymen did not always appreciate that approach; while Russell and Babcock were carrying out their research they complained bitterly that such theoretical research could never lead to practical knowledge, i.e. knowledge of use in their business. In response, when Russell and Babcock announced their results to the dairymen, which turned out to be very useful, Babcock lectured them on the value of pure science (ibid., 58).

[17] Ibid., 55-56.

fully institutionalized until 1914, when Congress passed the Smith-Lever Act.

At the heart of the extension service lay a system developed by Seaman A. Knapp during the first decade of the twentieth century.[18] To convince farmers reluctant to apply new knowledge because they thought they lacked the resources, Knapp developed a system where government experts regularly visited "demonstration farms," operated by local farmers concerned with earning a profit, to provide guidance and technical advice on how to apply the new methods. Under an alternative system, the farmers would only be provided with instructions, and were then left on their own.

Knapp hoped to accomplish two things with this system. First, since the demonstrations used only means available to every farmer, he hoped they would convince the farmers of the usefulness of the new management practices. Second, he saw the demonstrations as a means to test new knowledge in local conditions. In Knapp's system, the users of the new knowledge, the farmers and ranchers, provided only the materials, land, and stock necessary to carry out the demonstration. They were not involved in developing the new knowledge, certainly not in the way that Coville, Bentley, and Smith had involved them.

That view of science and extension provided the background to the "scientification" of range management in the twentieth century as scientists attempted to deal with the problems outlined by Spillman and Pinchot. Many range management scientists, including David Griffiths, James T. Jardine, Arthur W. Sampson, J.S. Cotton, J.J. Thornber, and

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[18] This account is based on Scott, 1970, 216-54.

E.O. Wooton, played a more or less prominent role in that process. Griffiths, Jardine, and Sampson, however, made crucial contributions.[19]

AN ATTEMPT TO QUANTIFY "GRAZING CAPACITY": DAVID GRIFFITHS

As part of their effort to base range management on a more scientific foundation, scientists attempted to remove some of the subjectivity from the work of Coville, Bentley and Smith. For Spillman, the subjectivity in the method to determine grazing capacity that Bentley and Smith adopted from the stockmen became problematic. David Griffiths developed a quantitative method he thought could serve as an indicator of grazing capacity. With quantification, he thought he gained insights into nature, thus reflecting the social definition of a scientists' identity. He was exposed to that view of the identity of a scientist while earning his doctorate. Yet he was never sure what his method actually meant, and was unable to eliminate the stockmen's influence.

David Griffiths received his B.S. in 1892 and his M.S. the next year from South Dakota Agricultural College; in 1900, he earned his

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[19] However, see also J.S. Cotton, 1905, "Range Management in the State of Washington," U.S. Department of Agriculture, Bureau of Plant Industry, Bulletin 75; 1906, "Range Management," Yearbook of the Department of Agriculture, 225-38; and 1908, "The Improvement of Mountain Meadows," U.S. Department of Agriculture, Bureau of Plant Industry, Bulletin 127; J.J. Thornber, 1910, "The Grazing Ranges of Arizona," Arizona Agricultural Experiment Station, Bulletin 65; and 1915, and "The Eleven Year Experiment," American Sheep Breeder and Wool Grower, Dec.:572-73; and E.O. Wooton, 1915. "Factors Affecting Range Management in New Mexico," U.S. Department of Agriculture, Bulletin 211; 1916, "Carrying Capacity of Grazing Ranges in Southern Arizona," U.S. Department of Agriculture, Bulletin 367; and 1918, "Certain Desert Plants as Emergency Stock Feed," U.S. Department of Agriculture, Bulletin 728.

Ph.D. from Columbia and the University of Arizona hired him as a professor of botany. In 1901, the USDA hired him as a botanist at the Arizona Experiment Station to carry out research at Arizona's Santa Anita Range Reserve, where the federal government had created an experimental range to examine range restoration.

Like the three researchers described in the last chapter, Griffiths started with the stockmen when he tackled Arizona's range problem. He talked to stockmen throughout Arizona, and mailed out a questionnaire to another group of stockmen.[20] Griffiths accepted their perception that due to overgrazing the range had deteriorated to less than half of its former condition. In his experiments, Griffiths used tools familiar to the farmer, such as the disk and the harrow, to examine the effect of cultivating the ground on revegetation and on the soil's ability to retain water.

Nevertheless, Griffiths virtually excluded the stockmen from the actual research, implicitly assuming that the expertise they had developed as a result of managing the range for years was invalid when compared to experimentally-produced knowledge. He did not seek their input when planning and interpreting experiments, or when developing ways to apply the knowledge. Even when experimentally examining the value of the stockmen's methods for range cultivation, he alone evaluated the results. However, he included the stockmen in an experiment to determine the plant species best adapted to local habitats, but, like other scientists at the time, only asked them to

[20] He stated that he mailed the questionnaire "to a selected list of correspondents,"(Griffiths, 1901, 10) but did not specify how or why those particular stockmen were chosen.

provide land and labor.[21] He also excluded them when determining the meaning of the experimental results. He stated explicitly that the scientists would determine that meaning: "Such an experiment would enable us to determine what forage plants are best adapted to your locality."[22] Thus, the user of the knowledge no longer had any say in determining how to use the range. Griffiths, the expert-scientist, would tell them what to do.

To remove some of the subjectivity from determining grazing capacity, Griffiths used a new method. He pulled the plants out of the ground, clipped their roots, then dried and weighed them. He performed that procedure on eighteen tracts of range, each measuring fifteen by three feet. In 1903, he used the method again, but performed it on plots three by seven feet. He did not indicate his reasons for reducing the size of the area. In any case, it showed clearly that scientists had yet to develop standardized experimental procedures.[23]

Griffiths' method shared many similarities with a method developed by the plant ecologists Frederick E. Clements and Roscoe Pound. Between 1892 and 1898, desiring to develop quantitative methods for the emerging discipline of plant ecology, they created the quadrat method.[24] They would count individual plants in a few small areas typical of the larger

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[21] See Griffiths, 1901, 11.

[22] Ibid..

[23] For a complete description of this method, see Griffiths, 1901, 15-6; and David Griffiths, 1904, "Range Investigations in Arizona," U.S. Department of Agriculture, Bureau of Plant Industry, Bulletin 67, 24-32.

[24] This discussion of the quadrat method is based on Tobey, 1981, 48-75.

area they wanted to study. The smaller areas they called "quadrats." Initially they decided on a quadrat of five square meters, but Clements later changed it to a quadrat of one square meter. By applying statistical analyses to the results gained by counting the quadrats, they argued they knew the composition of the larger area.

For Clements, the numbers generated with the quadrat method became more real than nature. By means of the data, Clements thought he gained insight into nature that could not be seen otherwise.[25] With that new insight, Clements argued he could understand the causal processes responsible for changes in vegetation.

Clearly, Griffiths' dry-weight method shared many similarities with Clements' quadrat method. Griffiths thought the weight of the vegetation could serve as an indicator, whereas Clements concentrated on numbers. Clements recognized the similarities. In 1928, he argued that in Griffiths' studies, "the quadrat method was employed more or less."[26] Clements, however, developed the idea for his method before 1900, and Griffiths gave no indication that he knew of Clements' work. He may, however, have adopted the method form other USDA agrostologists, who used it to analyze the botany of grasses.[27]

Griffiths definitely intended this dry-weight measure to serve as an indicator of grazing capacity. The dry-weight served as an indication of the amount of forage on the range. Since the amount of

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[25] Ibid., 68 and 73.

[26] Frederick E. Clements, 1928, 727.

[27] However, he did not give any references. These USDA publications were at best only sparsely referenced.

forage available determined "the amount of stock that can be carried upon these lands,"[28] Griffiths thought he had developed a method to express grazing capacity quantitatively. He also thought later scientists could use it to calculate the effect of protection or controlled grazing on the grazing capacity.

Although he hoped to remove subjectivity through quantification, he had to re-introduce the stockmen's subjective opinion to solve one of his problems. The dry-weight measure did not give a good indication of the amount of feed on a range because the scientists did not know exactly what plants the various kinds of stock consumed, and therefore did not know what plants to weigh. That posed a serious problem: whether (and how) to deduct the amount of non-forage plants contained in the weight. Griffiths here gave the stockmen a minor role in solving the scientists' dilemma, thus re-introducing subjectivity. He estimated "the non-forage plants by deducting from the totals thus obtained such a percentage as seems justifiable, based upon personal observations as well as the testimony of stockmen."[29] He then summarized the dry-weights in tables.[30]

Furthermore, although he thought he had quantified grazing capacity, he recognized so many methodological limitations shaped by the stockmen's concerns that the question remained whether he had really

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[28] Griffiths, 1904, 24. Note that he was concerned with measuring grazing capacity in terms of production. The stockmen's economic concerns thus shaped future research, even among scientists who thought the development of basic facts and new theories should be the focus of their research.

[29] Griffiths, 1904, 25.

[30] Ibid., 26-30.

measured grazing capacity. The stockmen, thus, continued to influence knowledge-development, albeit indirectly. First, stock would consume all of some plant species whenever they had a chance, but would utilize others only if forced to. Second, the stock could never graze the plants as closely as the scientists had clipped them. Therefore, "it would be impossible for cattle to secure the same amount of feed as is indicated [in the tables]."[31] Third, the scientists had removed all plant-life from the experimental area, not leaving any for reproduction of the various species. They had, in effect, seriously overgrazed the area.[32]

To compensate for these problems, Griffiths recognized he needed to subtract a certain amount of forage from the dry-weight measure in order to get an indication of grazing capacity. He analyzed briefly the deductions he needed to make to the measure more realistic, without, however, appealing to the knowledge of stockmen. He relied exclusively on his own knowledge concerning plants, his own opinion as to how much of the plants could safely be grazed without damaging the range, and his opinion on the amount of each species that cattle actually consumed. Yet he never clearly indicated the basis for those opinions. After making those deductions, he arrived at the conclusion that each cow needed fifty acres per year to live off, or it would damage the range

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[31] Ibid., 31.

[32] Spillman also recognized that scientists still needed to determine the validity of the measurement, proposing "to determine by actual trial the amount of stock this fenced area will carry without deteriorating." Spillman, in the introduction to Griffiths, 1904, 6.

and face a food shortage.[33]

He then appealed to the stockmen's opinion on the current grazing capacity of Arizona's ranges, which he had held to be invalid because subjective, to corroborate his method and prescribe range management practices.[34] He pointed out that his figure came very close to the fifty-acre estimate most farmers and ranchers used.[35] He noted with approval those ranchers that stocked only one steer per fifty acres. He also criticized one who thought each steer required only thirty-four acres because "the land could not maintain stock at the above ratio."[36]

Griffiths, probably because he recognized the problems with his method, did not use it to generate data in his experiments on reseeding the range or in the experiments on the value of the various methods stockmen used to cultivate the range. He interpreted those experiments in terms of the condition of the stand, which could range from very thin to good. Nevertheless, he never articulated the criteria to determine whether a stand was good or thin. From all appearances, he introduced another very subjective criterion: his own opinion.[37] Since he tried to remove the stockmen's subjective opinions from the science of range management, he implicitly assumed that his subjective opinion was

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[33] Ibid., 31-32.

[34] That may, however, have been the only available base for comparison.

[35] He derived that fifty-acre estimate from the census of 1900. He thus used the actual management practices of the stockmen in 1900 as a baseline against which to compare his experimental results in 1904.

[36] Ibid., 34.

[37] For example, see Griffiths, 1904, 12-13.

better. Yet he never stated why he thought his subjective opinion was better.

Subsequent scientists could eliminate the problems inherent in Griffiths' method. They could analyze the kinds of forage stock consumed, and how close they cropped the forage. Scientists could then clip the forage at that height and various other heights to determine how much feed the stock were really getting from the range and how close they could crop it without damaging the range. The question then remained whether the method would be useful to stockmen, or only to scientists.[38] Furthermore, it took a long time before scientists could use his method. In 1909, James T. Jardine analyzed the difficulties involved in comparing the grazing capacities of different ranges, but did not appeal to Griffiths' method to solve the problems.[39]

Nevertheless, with Griffiths, range management had clearly taken a different direction. He had removed the immediate utility criterion from range management because his method required numerous calculations and went beyond the stockmen's knowledge. The stockmen could even legitimately wonder whether his method contributed anything they did not already know, especially since he appealed to their opinions to solve a problem and to corroborate his method. He had, furthermore, implicitly replaced that criterion with one rooted in the values of a scientist:

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[38] Subsequent researchers would indeed study exactly what sort of forage the various kinds of stock consumed, and recurring themes in range management studies would be what percentage of the stand had to be left at the end of the grazing season and what percentage of the height of the plant the stock could safely graze without damaging the plant. On this point, see also Alexander, 1987.

[39] See James T. Jardine, 1909, "Coyote-Proof Pasture Experiment, 1908," U.S. Department of Agriculture, Forest Service, Circular 160, 30.

that quantification was good because it led to insights into nature. Even though he recognized problems in his method, he continued to develop it because he assumed quantification would lead to better insights that could provide the foundation for continued use of the range consistent with conservation.

The stockmen, however, still exerted an indirect influence on the knowledge-development process. Griffiths measured grazing capacity in terms of productivity because that was how grazing capacity had been defined due to the stockmen's influence on Bentley and Smith. Due to the stockmen's indirect influence, he recognized so many problems with his method that he did not even use it to interpret his other experiments. The stockmen's time-frame may also have been inconsistent with his time-frame: facing a serious crisis on the range, they wanted it solved. That may have motivated him to publish his method, even though he recognized problems. Their time-frame therefore may have limited his research time, preventing him from addressing the problems he recognized and thus from showing the potential of his method. That scientists required a much longer time-frame was made clear in the research of J.J. Thornber, who continued Griffiths' research on range restoration. He published his results in 1910, meaning that the total experiment took ten years.[40]

PRESCRIBING MANAGEMENT PRACTICES - JAMES T. JARDINE

Whereas Griffiths attempted to remove subjectivity through quantification, another researcher, James T. Jardine, tried to do so

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[40] See Thornber, 1910.

through controlled experimentation and by developing two new methods for determining the condition of the range. Furthermore, whereas Griffiths did not prescribe management practices on the basis of knowledge developed with his method, Jardine relied exclusively on his results gained through experimentation and with his new methods to prescribe how stockmen should manage the range. Only in one instance did he turn to a stockman to corroborate his results, and then only to prove the economic feasibility of those results. For Jardine, however, in contrast to Griffiths, the concerns of the stockmen provided explicit criteria against which to evaluate new knowledge. Yet that did not mean that Jardine allowed the stockmen direct influence on the knowledge-production process. He internalized their criteria, presuming he knew the stockmen's concerns, values and interests well-enough to judge for them.

James T. Jardine was born on a ranch in Idaho in 1881. In 1905, he received his bachelor's from Utah Agricultural College. During the summers of 1905 and 1906 he studied at Chicago, never earning an advanced degree; but three schools later awarded him honorary degrees for his work in range management. From 1905 to 1907, he was a special agent in the Forest Service. In 1907, Coville hired him and named him inspector of grazing in charge of the National Forest Range, with duties including range investigations and surveys. He left in 1910 to become director at the Oregon Agricultural Experiment Station. In 1920, he moved to Washington D.C., and became an inspector in the office of the secretary of the USDA. He retired in 1931, and died in 1954.

In 1907, Jardine started a three year experiment, designed by

Coville, to determine the value of a range management method familiar to most stockmen, but not applied often enough. His design reflected the tradition in the USDA where known methods were subjected to testing to convince farmers and ranchers to adopt it. The experiment's goal was to examine the possibility of increasing grazing capacity by modifying stock-handling methods, thus addressing one of the problems identified by Pinchot.[41]

Coville designed the experiment to determine the effect on the range when sheep had the freedom to roam and bed down where they wanted in order to show the value of that management method.[42] Stockmen herded sheep to protect them from predators. Jardine argued that herded sheep trample and destroy the forage, and therefore "a large loss of vegetation by trampling is inherent in the herding system itself."[43] He created a predator-free environment by building a fence around 2,560 acres of range land to study the behavior of sheep when not herded. The stockmen contributed the sheep for the experiment and their management practices provided the baseline against which to evaluate its results.

To convince the stockmen of the usefulness of the method, Jardine utilized a criterion the stockmen could understand to evaluate the

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[41] Jardine published his results in a series of three papers: James T. Jardine, 1908, "Preliminary Report on Grazing Experiments in a Coyote-Proof Pasture," USDA, Forest Service, Circular 156; 1909, op. cit.; and 1910, "The Pasturage System for Handling Range Sheep," USDA, Forest Service, Circular 178.

[42] He thus experimentally examined the validity of an argument proposed by stockmen and the usefulness of the method based on that argument; see Coville, 1898, op. cit., ch. 1, footnote 18. The stockmen clearly influenced the scientists' experimental methods, even when the scientists attempted to be more "scientific," though only indirectly.

[43] Jardine, 1908, 5.

experiment: production. Although Griffith's had designed his dry-weight method thinking that other scientists could use it to measure the effect of protection or controlled grazing on the range, Jardine ignored that method. Furthermore, in 1895, the USDA had established a Division of Agrostology. Scientists in that Division had studied the effects of among other things grazing on various grasses, but Jardine did not utilize any of their methods or criteria in his experiment.

Jardine concluded that the management method he had examined experimentally increased the grazing capacity, calculating that on the open range sheep required from 64 to 123 percent more rangeland during a ninety-day period.[44] He thus clearly defined grazing capacity in terms of production as the number of acres a sheep required to graze ninety days, and used that as his criterion to evaluate the experimental results. Jardine attributed the increase in grazing capacity to two factors related to production. With the pasture method, "the forage was actually consumed, while on the outside range part of it was destroyed by trampling."[45] The new method also effectively implemented a system of rotation grazing, and, as a result, "the continued growth is more vigorous, and a greater amount of forage is produced."[46]

Jardine utilized another criterion directly related to production: the condition of the sheep. That measure struck to the heart of the stockmen's immediate economic interests. Sheep grazed on the enclosure weighed more and produced more and better-quality wool. The new method

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[44] See the table in Jardine, 1909, 31.

[45] Jardine, 1910, 30.

[46] Ibid., 32.

also prevented loss due to poison plants.[47] Finally, Jardine recognized that unless he could show how stockmen could apply the results on the open range, the results would not have any validity for them. Not all stockmen could build fences to enclose the range, especially since they did not own the land. He therefore outlined four implications of the experimental results for herding methods on the open range. Most herders already knew those conclusions, but he argued they should make a more concerted effort to apply them, especially since his experiment had shown their value. "It is believed, ..., that a concentrated effort by sheepmen along these lines will do much toward producing heavier sheep, a better wool crop, ..., all on less range."[48]

In addition to that experiment, Jardine also developed two new methods to determine the condition of the range: the grazing-reconnaissance method and the range inspection method.[49] Those methods were designed, respectively, to develop new management plans and to

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[47] Ibid., 24.

[48] Ibid, 32. To further underscore the value of the new method, he analyzed the results achieved by J.W. Emmons, a private stockmen who built a coyote-proof pasture in 1907. He concluded that Emmons' system proved economically feasible, and stated that according to Emmons "the pasturage system is the only way to handle sheep." Jardine, 1909, 38.

[49] He did not, unfortunately, discuss how or when he developed those methods in any of his writings. He and several other range researchers first applied the range reconnaissance method in 1911, on the Coconino National Forest in Arizona (Talbot and Cronemiller, 1961, 100). Other range researchers and forestry officials quickly adopted his method, even though he had not published it. An informal network must have existed within the USDA that allowed the idea to spread rapidly. In 1919, he presented a summary account of both methods in an article he coauthored with Mark Anderson (James T. Jardine and Mark Anderson, 1919, "Range Management on the National Forests," U.S. Department of Agriculture, Bulletin 790). That article quickly became the "Bible" of range managers because it provided a synoptic overview of all that had been learned about range management up to that time.

adjust existing plans. Even though the results gained with both methods would have an immediate impact on the stockmen's day-to-day management of the range by prescribing what they should do, both methods required the expertise of the scientists. Both therefore clearly did not satisfy an immediate utility criterion, which for Jardine played no role in evaluating their validity. Both, however, did satisfy the objective of range management because both attempted to determine management methods so that use would be consistent with conservation. Jardine, like Griffiths, thus assumed that he could satisfy the stockmen's concerns by satisfying the research program's objective.

Grazing-reconnaissance meant taking inventory of the range to collect information in order to develop management plans best suited to each range. By proceeding very systematically, the reconnaissance party collected information to prepare a map "classifying the area examined into grazing types, showing for each type the location, acreage, topography, amount and character of vegetation, condition of the range, available watering places, and cultural features."[50] Collecting all that information required a lot of time and money. They therefore developed range inspection, a less-intensive method that required less time and money. It was developed to quickly assess current conditions on the range so that management practices could be adjusted immediately. The goal, therefore, was not to design a new plan, but to change existing management practices.[51] Although range inspection was less time-consuming, it was also less accurate, and they did not intend that

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50 Jardine and Anderson, 1919, 75.

51 Ibid..

it should take the place of range reconnaissance.

The group carrying out the reconnaissance did not have to be very experienced as long as it had an expert-leader, while range inspection had to be performed by people with several years experience assessing range conditions. Jardine and his associate, Mark Anderson, focused primarily on range inspection because it cost less and was less time consuming. They outlined seventeen questions the range inspectors should address. By means of those questions they thought they removed some of the subjectivity from determining range condition. Those questions included analyses of stock-type best suited to the range, best season for grazing, current management practices and intensity of grazing, grazing capacity, erosion, principle forage types, present and necessary cultural improvements, and whether a range reconnaissance was needed.[52]

Jardine and Anderson thought the stockmen did not possess enough expertise about the ranges they managed to participate in range inspections or to help develop the questions that guided the inspection. Instead, they formulated the questions that should provide the focus for a range inspection on the basis of knowledge developed by range management scientists and scientists in such specialties as soil science and agrostology. Those studies had utilized controlled experimentation and quantification to develop new knowledge. Jardine and Anderson, like Griffiths, thus implicitly accepted that the socially defined standard for research that excluded stockmen and emphasized quantification and

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52 Jardine and Anderson, 1919, 76-79. They did not stipulate how grazing capacity should be determined.

experimentation led to superior knowledge and therefore should provide the foundation for any plans to manage the range.

They realized, however, that implementation of the results depended on the stockmen's cooperation. To foster that cooperation, they argued that "whenever convenient, during the process of the work, stockmen concerned should be informed that work of this character is being done."[53] The inspectors, as experts, would then present the information to the stockmen and tell them what to do. "The findings and recommendations resulting from the inspection can be presented and explained to the stockmen most effectively through the advisory board of the stockmen's association, where an association has been recognized by the Forest Service."[54]

Jardine also attempted to bridge the gap between the experts and the stockmen through rhetorical devices. In 1918, he made an interesting statement in a leading weekly magazine for sheep breeders. After reviewing the knowledge the scientists had developed, and arguing that the sheep breeders should adopt the management practices developed by the experts, especially deferred-rotation grazing, he concluded with a call for cooperation. "Close cooperation in accumulating further information ... and in adjusting livestock production and grazing in accordance with the new findings is imperative to the most efficient use of the range and to its improvement and maintenance."[55]

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[53] Ibid., 81.

[54] Ibid..

[55] James T. Jardine, 1918, "Improvement and Maintenance of our Far Western Ranges," American Sheep Breeder and Wool Grower, 10:368.

He appeared to argue that the stockmen should be given a role in the development of knowledge, but unfortunately never discussed the nature of the cooperation. However, he questioned the ability of the stockmen to determine the condition of their own ranges and to adjust their management practices.[56] Therefore, he likely meant that they could play only a very limited role in knowledge-development, e.g. tell the researchers problems they faced in applying the knowledge, and provide the resources for demonstrations and for tests to adapt the knowledge to the local habitats. With that strategy he hoped to establish sufficiently strong bonds with the stockmen so that they would adopt his knowledge in their grazing practices.

Jardine believed that scientists would ask better questions and generate better solutions and therefore thought they should assume control over range management. In both the enclosure-study and when discussing his new methods, he assumed that the knowledge of the scientists was superior to the knowledge of the stockmen, and used the scientifically developed knowledge to prescribe range management plans. Although the stockmen's concerns played a role in his evaluations, he presumed to know those concerns well enough to presuppose what they would use as criteria to evaluate the knowledge. He therefore assumed that the superiority of the knowledge developed by the scientists gave them the power to prescribe management of the range.

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[56] Jardine, 1919, op cit. The 1918 article also presupposed that the stockmen needed the scientists to tell them what to do, and when and how to do it. That was the purpose of the article in a leading sheep herders magazine.

ESTABLISHING A LINK WITH PLANT ECOLOGY - ARTHUR W. SAMPSON

Between 1908 and 1919, the range management scientist Arthur W. Sampson established an intellectual connection between range management and plant ecology, primarily as articulated by Frederick E. Clements. Sampson adopted both a concept and a method from Clements' work. He made that concept the theoretical cornerstone of range management science, but did not just accept it as defined by Clements. Due to continued indirect influence from the stockmen, he changed the concept significantly. Sampson also adopted Clements' quadrat method. In adopting that method, he furthered the trend towards quantification begun by Griffiths. He also utilized both the concept and the method to prescribe management practices, furthering the trend Jardine was developing at the same time, basing it on a stronger scientific foundation by providing a theoretical and methodological justification.

Arthur W. Sampson was born in Nebraska in 1884. His father was a farmer and banker. He received his bachelor's in 1906, and his master's in 1907, from the University of Nebraska. While at Nebraska, he studied under Clements, the developer of one of the first theories in plant ecology.[57] In 1917, he earned his doctorate from George Washington University. In 1907, Sampson accepted employment as an assistant plant ecologist in the research branch of Forest Service; and the next year, under the direction of Coville, he began studying the possibility of natural revegetation of overgrazed areas at Wallowa National Forest in Oregon. In 1912, he was appointed the first director of the newly established Great Basin Experiment Station in Utah. In 1922, he assumed

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[57] Rowley, 1985, 103.

the position of associate professor in forestry at Berkeley. In 1940, he was promoted to full professor, retiring in 1951. Sampson wrote four textbooks and published close to 100 articles in federal and state publications and scientific journals. He died in 1967, having played a major role in the development of range management as a science and an academic discipline.

In 1908, Sampson published a preliminary report on the first year of experimentation to examine revegetation of the overgrazed ranges. The final report, published six years later, described the experiments he performed from 1907 through 1910, and the application of the results of those experiments in actual management of the range. The experiment was designed to test the efficacy of deferred-rotation grazing by comparing it to the effects of two other systems of grazing: yearlong grazing, and yearlong or season-long grazing yet alternately letting one section of the range rest for the whole year. He concluded that current regulations on the government-owned lands would not lead to revegetation of the range.[58] He duplicated Smith and Bentley's argument that the range could be reseeded most economically and quickly if the stockmen practiced deferred-rotation grazing, and added a new element to their explanation.[59] He argued that cattle when grazing would do the work of

58 See Sampson, 1908, 6.

59 Without, however, crediting his predecessors. Sampson's argument for deferred-rotation grazing was almost immediately corroborated by a researcher in Wyoming (see L.H. Douglas, 1915, "Deferred and Rotation Grazing," National Wool Grower, 5(10):11-14. However, two other researchers in the Caribou Range in Idaho and Wyoming, Clarence E. Favre and W. Vincent Evans, also experimentally examined Sampson's argument. They argued that deferred grazing was impractical because of the physical environment and because the forage consisted primarily of weeds rather than grasses. They agreed that

the stockmen because "the stock would do the work of a harrow by trampling most of the seeds beneath the surface of the soil, and thereby ensure a higher percentage of germination."[60]

In the preliminary report, Sampson reached out to the developing discipline of plant ecology. Both Coville, in the introduction to the paper, and Sampson implicitly utilized the idea of succession to explain the decrease in grazing capacity on the ranges and to argue for the merits of the experiment. The concept of succession was being developed at that time by Clements, one of Sampson's teachers at Nebraska. In addition, Sampson utilized Clements' quadrat method to generate data for his study.

Clements, the developer of the first theory and school in plant ecology, made "succession" the first paradigm in plant ecology.[61] He developed his concept of succession between 1900 and 1916, when he published Plant Succession, which quickly became one of the most important books in plant ecology. He thought succession was the

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deferred-rotation grazing worked best with grasses and should be used where the range was badly damaged, but otherwise thought the system impractical because it required to much supervision and questioned its applicability to all forage types (see Alexander, 1987, 421).

60 Sampson, 1908, 20; see also Sampson, 1914, 147. Sampson continued the experiments until 1910, and then applied the results for three years on the Wallowa National Forest in Oregon. Although he did not explicitly say so, it appears he asked stockmen to practice deferred-rotation grazing when they grazed their stock on the National Forest, but never gave them a voice in setting up the experiment or evaluating the results. Even when the stockmen practiced deferred-rotation grazing, they were told what to do and their opinion about the results was not solicited. Only the experts were involved in developing the knowledge.

61 Tobey, 1981, chapters 3 and 4.

"universal process of plant formation development."[62]

Clements had a very neolamarckian, organismic, and dynamic view of succession and of nature. He thought of plant formations as organisms that went through a life cycle: growth, maturity, decline, and death, when they were replaced by the next plant formation. Successive plant formations would succeed each other in stages. Furthermore, climate was the major determinant influencing what plant formation would develop in a certain area. Clements' neolamarckism clearly influenced his concept of succession because he thought of the course of development as virtually inevitable and clearly directional, rather than random, and ultimately culminating with that plant formation best suited for the climate in a particular area. That plant formation Clements called the climax stage. "Succession is inherently and inevitably progressive. As a developmental process, it proceeds as certainly from bare area to climax as does the individual from seed to mature plant."[63]

Regressive or retrogressive succession could not occur, and he explicitly ruled out the possibility that human actions, or human-

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[62] Frederick E. Clements, 1928, 3. This book contains two of Clements' most important books in abbreviated form: Plant Succession and Plant Indicators, published in 1916 and 1920 respectively.

[63] Clements, 1928, 147. For a more complete analysis of Clements' neolamarckism, see Tobey, 1981, 85-87. Rival views of plant ecology were articulated by several people, most prominently by Henry C. Cowles at the University of Chicago (see Henry C. Cowles, 1901, "The Physiographic Ecology of Chicago and Vicinity," Botanical Gazette 31(2):73-108). For analyses that differ from Tobey's and discuss these rival views, see Joel B. Hagen, 1988, "Organism and Environment: Frederick Clements' Vision of a Unified Physiological Ecology" in The American Development of Biology ed. by R. Rainger, K.R. Benson, and J. Maienschein (Philadelphia: University of Pennsylvania Press), 257-80; and Eugene Cittadino, 1980. Nevertheless, the Clementsian view was dominant in U.S. plant ecology until at least the 1930's.

controlled actions like grazing, could cause regressive succession. "It is conceivable that lumbering, grazing, and fire might cooperate to produce artificial regression, but there is nowhere evidence that this is the case."[64] Such actions might temporarily destroy the area, but, provided the climate did not change, the original process of succession would resume. He did recognize that if such actions persisted, they might halt the process of succession. The stage that would then be maintained artificially by those actions he called a subclimax. As soon as those constraints on succession were removed, the normal process would resume, and the subclimax would develop into a climax.[65]

Different plant formations could be distinguished because they would contain different indicator species: different ratios of plants or entirely different plant species that could serve as an indicator of the stage of a formation's development. On the basis of the quadrat method and succession, Clements identified climate and the impact of the vegetation on the environment preparing the habitat for the next vegetational stage as the primary causal agents responsible for changes in vegetative formations.[66]

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64 Ibid..

65 For a more detailed discussion of succession, see Clements, 1928; or for an analysis of the philosophical and scientific background to Clements' views, see Tobey, 1981, esp. ch. 4.

66 Clements' concept of succession had many similarities to the idea developed by Smith in 1899. Smith, however, had only articulated an idea without arguing that it could provide a theoretical foundation to understand changes in nature. Although Clements recognized Smith's idea, he never acknowledged it as an influence on his own concept of succession (see Clements, 1928, 255). Clements did not realize, however, that Smith clearly recognized both that human and natural causes could influence succession, and that regressive succession was a distinct possibility."(See Smith, 1899, op. cit.) Therefore, to gain a

In 1908, when Sampson first utilized the idea of succession, he did not credit anybody as a source for the idea. Clements, although working with the idea, had not yet published it. Sampson, however, was likely exposed to the idea at Nebraska while Clements was developing it. In 1913 and 1914, Sampson published two papers describing the final results of his experiments, in neither referring explicitly to the quadrat method or the idea of succession. However, he had built both the concept and the method into his analysis. Clements recognized that when he noted that Sampson's 1914 paper took "into account the need of thoroughgoing and extensive studies of quadrats, ..., and succession."[67]

In 1917, one year after Clements published Plant Succession, Sampson presented a short paper to the Botanical Society explicitly discussing the potential role of the concept of succession in range management.[68] Thus, although he had already utilized succession implicitly, he did not take the lead explicitly. Instead, he chose to wait until his mentor, the "pure" scientist, had published his version, and then followed his lead almost immediately.

In that paper Sampson argued against what he saw as Clements' view, although without mentioning it specifically, that human actions could affect the course of succession. At first glance he appeared to follow a purely Clementsian line: "succession continues until the equilibrium

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deeper understanding of the history of ecology, an interesting question, beyond the scope of this analysis, is to what extent Smith influenced Clements.

67 Clements, 1928, 228.

68 Arthur W. Sampson, 1917, "Succession as a Factor in Range Management," Journal of Forestry 15(5):593-96.

is finally established between the environment and the vegetation it supports."[69] He continued, however, by outlining harmful management practices that can alter the course of succession and cause retrogressive succession. He concluded that due to those management practices, "the stability of the type is interfered with and the invasion and succession of transitory species made possible."[70]

Sampson thought use of the concept of succession and the quadrat method made possible a causal analysis of changes in range condition that could provide the foundation for grazing compatible with the growth requirements of the vegetation - and therefore compatible with conservation.[71] He accepted Clements' argument that the quadrat method combined with the concept of succession gave scientists insights into the causal processes responsible for changes in vegetation. Range managers should pay particular attention to the course of succession because it would enable them to understand the effect of grazing on the life histories and reproductive abilities of the vegetation, and thus to understand the causes behind the invasion of new plants into a region. He also used Clements' quadrats to gather data on the plant formations because he thought them "of invaluable aid in securing reliable data on the changes in the type of vegetation."[72]

Utilizing the quadrat method and the concept of succession in

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[69] Ibid., 593.

[70] Ibid..

[71] That view of the value of the method and the concept was already present in his earlier papers.

[72] Sampson, 1908, 13. After making that argument, Sampson referred his readers to Clements' Research Methods in Ecology.

developing management plans would make range management more scientific, and therefore better. "Thus the management of grazing lands, like that of woodlands, is gradually being placed upon a scientific basis in which applied ecology, and particularly succession, plays a prominent and highly important part."[73] Relying on quadrats, range managers could keep track of both progressive and retrogressive succession, and of changes too minute to influence the course of succession. They could then track when, due to overgrazing, less-valuable forage species lower on the successional scale replaced more valuable ones. He argued they could use that data to adjust management practices: "On the basis of such data it is possible to state authoritatively whether or not a change should be made in a given system of management."[74] Sampson deeply believed that scientific range management equalled better range management. In the concluding paragraph he argued that "knowledge of the successional trend of the vegetation is well nigh indispensable to the working out of a judicious system of management."[75]

He recognized that people needed special skills to utilize ecology, and by recognizing that limitation created work for USDA scientists and additional reasons to reach out to ecologists, who were also studying succession and life histories. "The practical application of successional studies to the establishment and maintenance of a definite vegetative type will be determined chiefly on the basis of the knowledge one has of the successional stages represented by the different species

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73 Sampson, 1917, 596.

74 Ibid., 594.

75 Ibid., 596.

coupled with their growth requirements."[76] The stockmen would not have time to fully develop that knowledge. The scientists, however, did; and they could then publish that information for the consumption of extension agents and stockmen. In the process, the scientists role as experts would be solidified.[77]

In 1919, Sampson published a longer article on the role of succession in range management that became a landmark in the discipline.[78] In it he provided detailed information on the nature of succession, and the means by which the successional stage of the range could be recognized. He outlined the effects of grazing on the course of succession - on its trend.

In that paper Sampson explicitly took issue with Clements, while at the same time strengthening the links with Clements and other prominent ecologists like Henry C. Cowles. Emphasizing a linguistic case rather than one based on knowledge he had developed in his previous experiments, Sampson argued that succession could be both progressive and retrogressive. "Coming as it does from the Latin verb "succedo," meaning literally "I go under," the word "succeed" originally had

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76 Ibid., 595.

77 Provided, of course, that the stockmen accept the experts. However, the scientists played a role in the USDA in shaping grazing policy on the public lands, and in 1926 and 1936, among other occasions, testified before Congress. The question, arises, here how much influence the scientists really had on shaping policy, or whether their advice was sought only to justify the politicians' decisions. Their recommendations, however, clearly flowed from their research. And whether the politicians just used them or really listened to them is, in one sense, immaterial: their research would still be funded.

78 Arthur W. Sampson, 1919, "Plant Succession in Relation to Range Management." U.S. Department of Agriculture, Bulletin 791.

nothing to do with the superiority of one crop over another."[79] He therefore expanded the definition of succession. "Succession is here considered in the sense to "follow," "take the place of," etc., and is applied in a vegetative invasional sense."[80] Sampson argued that his conception of succession did not create a new concept. Furthermore, he admitted that the details of retrogressive succession needed further study.

> "Regardless of whether retrogressive succession occurs in the same specific descending series as it has been recorded to occur in the ascending development toward the climax, the use of the term "retrogression" or "retrogressive succession" is a convenient and self-explanatory term, and its use in no way involves a fundamental principle."[81]

He concluded that analysis of his use of the concept by referring the reader to the works of several ecologists who had studied progressive succession.

Before outlining how succession, as he had re-elaborated it, could be applied in range management, Sampson described the four principal stages of range vegetation: first or early weed, second or late weed, mixed grass and weed stage, and the wheat-grass cover subclimax stage. The subclimax stage was the highest stage useful for grazing because the climax stage was usually timber. For each of the stages, he described the preferred habitat, the conditions for their growth and reproduction, their physiology, factors that could disturb them, how much forage of what kinds they produced, and the palatability of that forage.

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[79] Ibid., 5.

[80] Ibid..

[81] Ibid..

For Sampson, the concept of succession as he had re-elaborated it to include both progressive and retrogressive succession, became the foundation for prescribing range management practices. The subclimax stage was the most desirable stage, and the goal of practicing range management became grazing in such a way that range condition showed progressive succession. For each stage, Sampson listed the dominant species. Tracking changes in those species with the quadrat method could reveal range condition because "the species that are increasing appreciably on the range invariably reveal one of two stories."[82] An increase of species lower in the succession scale meant that "the range is being utilized unwisely in one or more respects,"[83] and vice versa.

Sampson, interpreting his data in light of the concept of succession, then identified two main causes of range deterioration: too-early grazing and overgrazing. Both induced a reverse successional trend because they either interfered with reproduction or removed too much cover from the soil, thereby leaving the area barren and open to invasion by species lower on the successional scale. The use of bedgrounds and stock-driveways caused overgrazing because too many animals were confined to a small section of range for too long.

On the basis of succession, he then prescribed more precisely how the range should be managed: to protect against reverse succession, the stockmen should practice deferred-rotation grazing. That would allow plants to reproduce, and also protect against overgrazing because stock would move to a new section of range before having destroyed the cover.

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82 Ibid., 73.

83 Ibid..

Deferred-rotation grazing also had the advantage that the stock would help with the revegetation by working the seeds into the ground. However, range in the first weed stage should not be grazed at all. "Ranges that have been so destructively used ... can not be grazed without further deterioration."[84]

Application of the concept of succession, as defined by Sampson, required thinking in time. For Clements time moved in only one direction, progressing. With Sampson's re- elaboration of the concept it was necessary to know what the condition of the range in the past had been so changes in the trends of succession could be tracked. To apply that knowledge, range management scientists, stockmen, and range managers would have to keep track of changes in vegetation over time in order to understand the trends of succession. They could then adjust their grazing practices to induce progressive succession or to maintain the subclimax.

Thinking in time also provided an aid in determining grazing capacity. If range managers or stockmen saw reverse succession occurring, they could adjust by reducing the number of stock on the range the next grazing season. Thus, by keeping track of the changes in indicator species over time, and by counting the number of stock they allowed on the range, they could make the adjustments necessary to ensure scientific range management. To keep track of the indicator species, they should, of course, set up several quadrats on the range the stock were grazing in order to generate data that was as accurate as possible.

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[84] Ibid., 74.

Clearly, Sampson's re-elaboration of succession differed strikingly from the way ecologists, especially Clements, used it. Quite likely, however, he did not want to antagonize the ecologists. Instead, he was trying to establish a link between range management and plant ecology because he recognized that their methods and concepts could make valuable contributions to range management, even if range managers needed to elaborate or to expand the application of those concepts and methods. He therefore did not directly criticize their definition of the concept on the basis of the knowledge he possessed.

As Clements used succession, it had only limited meaning for range management, whereas Sampson's re-elaboration of succession gave it meaning for range management. For Sampson to have adopted Clements' strictly progressive view of succession would have meant denial of part of the tradition in range management. It would also have meant reducing the potential impact the range manager or stockmen could have on the trend of succession. Clements' view was therefore inconsistent with the identity of range managers.

The first range management researchers had recognized that grazing could damage the range. Range management research began because grazing had damaged the range until the plant species that occupied large sections of the range had no food value at all - or worse, the range had been denuded. Continued grazing could permanently affect the condition of the range. Therefore, for Sampson to have adopted Clements' definition would have meant denying the knowledge developed by his predecessors in the range management research tradition. Furthermore, his own studies had also shown him the impact grazing can have on plants.

Re-elaborating the concept created questions range management scientists could research, thereby making range management more scientifically respectable as a research program. They needed to analyze the stages and causes of reverse succession. They could thus claim to be respectable scientists in their own right because they were not only solving problems society, the stockmen, gave them; they were also addressing questions that arose from their view of succession and thereby making a contribution to understanding succession. Their view of succession was based on the recognition of a phenomenon the pure scientists had not recognized. Their analysis of that phenomenon would contribute new knowledge, and that knowledge could potentially contribute to the scientists', including the ecologists', understanding of nature. Although that knowledge could also be applied in the actual management of the range, they could claim to make contributions that had meaning not only for society but also for science, and claim to have their own research program. In addition, instead of just finding ways to apply knowledge developed by others, the ecologists, they themselves generated new causal analyses. Thus, by re-elaborating succession, he had reinforced the view of the identity of scientists adopted as a result of Atwater's standard for research.

Just adopting Clements' concept would have made range management less relevant as a research program. If climate and the actions of plants on their habitat were the primary causal agents responsible for changes in vegetation, why study the impact of grazing? Clements, although he had a life-long interest in making his knowledge relevant as potentially applicable to solve problems on the range, had almost ruled range management research out of existence. The only reason to study range management would be to understand how humans could create and maintain a temporary subclimax. If people needed any other knowledge pertaining to the range, they could just turn to ecologists, who, if necessary, could also study the creation and maintenance of subclimaxes by human action.

Range management's objective, developed because Coville, Bentley, and Smith allowed the stockmen to influence their research, also shaped Sampson's re-elaboration of succession. That objective presupposed that human actions can have a significant impact on the vegetation, including the trend of succession. Sampson throughout his life accepted that objective. In 1952, he stated that "range management is the science and the art of procuring maximum sustained use of the forage crop without jeopardy to other resources or uses of the land."[85] Yet if grazing could only have a minor and secondary impact on the trend of succession, then developing a system of range management that both maximized production and conserved the range by means of human manipulation became insignificant with Clements' definition.

Therefore, even though scientists like Sampson did not allow the stockmen direct influence on the research, the stockmen still influenced knowledge-production because they had helped to shape the objective of range management. However, they had only an indirect effect: they were no longer immediately involved in producing the knowledge they were expected to apply.

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[85] Arthur W. Sampson, 1952.

Sampson, thus, developed methods and ideas that removed some of the subjectivity from determining grazing capacity and deciding what kind of range management to practice. In the process, he made range management research more scientifically respectable. Yet he also reinforced the need for the scientists as experts, further excluding the stockmen from developing the knowledge they were expected to apply because only the scientists possessed the prerequisite background knowledge. Like Jardine, he believed in the superiority of the scientists' knowledge and relied on it to prescribe range management practices. That stands in sharp contrast to the practice of the first generation of researchers, who had cooperated with the stockmen to develop management practices.

CONCLUSION

The traditional view of knowledge-development in applied science as just science applied to social problems can not describe the evolution of range management research. Throughout the first two decades of the development of range management's research program, the stockmen exerted either a direct or indirect influence on the knowledge-development process. Coville, Bentley, and Smith allowed them to shape that process directly; and even though the next generation attempted to behave in a way they felt was more "scientific," they could never eliminate the stockmen's influence entirely. The research carried out in the USDA and the state agricultural experiment stations therefore needs to be re-examined to analyze how wider societal goals, values, interests, and concerns influenced the goals, methods, concepts, and criteria of scientists engaged in the applied science knowledge-production process.[1]

There were nevertheless differences between the two groups of researchers: they differed significantly in their research practices. Coville, Bentley, and Smith in their research did what the stockmen did

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[1] A problem for those analyses is to develop a workable and relevant definition of society. Yet in most instances it should be possible to identify relevant groups in the wider society that could potentially have had an impact on the knowledge-development process because they would be affected by the new knowledge or had some other interest in it. In this study, the stockmen provided that relevant group, although that does not imply that no other groups existed in society that had an interest in the knowledge. To keep the analysis manageable I concentrated only on the stockmen, but other groups, such as the foresters who managed the ranges on the Forest Reserves, also were affected by the knowledge range management scientists developed. I chose to concentrate on the stockmen because they had the most immediate interest in it. Future analyses will have to analyze range management knowledge-development in a wider social context that considers whether and how other groups in society influenced the production of knowledge, and also consider how developments in other scientific and applied scientific disciplines affected that process.

or could do in their everyday practices managing the range, "going native" with the users of the knowledge by being "scientific stockmen." They also recognized that the stockmen's expertise on the range had some validity and could be used to develop knowledge to deal with the problems on the range. They set out to determine which of the stockmen's practices had validity, and why? Finally, they adopted the stockmen's immediate utility criterion to evaluate their knowledge. In contrast, Griffiths, Jardine and Sampson focused their research practices on gaining more certain knowledge about nature through controlled experimentation and quantification. They denied implicitly that the stockmen's expertise was valid because it was not based on knowledge-development practices considered scientific. They therefore removed themselves from the stockmen's immediate world, replaced the immediate utility criterion with disciplinary criteria, and pursued their own set of questions in order to establish a knowledge-development process that satisfied the standard for scientific research.

The two groups also differed significantly in their social practices. Coville, Bentley, and Smith established egalitarian relationships with the stockmen and defined themselves almost exclusively in relation to the stockmen. Griffiths, Jardine, and Sampson excluded the stockmen from direct participation in the knowledge-development process and replaced the egalitarian relationship with one where the developers of the knowledge told the users of the knowledge what to do. Also, although they presumed they had internalized the stockmen's goals, values, and interests and thereby defined themselves in relation to those concerns of the stockmen, they defined themselves primarily in relation to the standard of research acceptable in the scientific community.

Even though those significant differences between the two groups of researchers existed, both groups shared another similarity in that the knowledge-development process of both exhibited a tension between the general objective of the research program and the individual goals of the scientists in their research. Implicitly, they assumed congruence between the general objective and their individual goals, which was necessary to argue that their knowledge could achieve the general objective. Yet that congruency was not always present in the historical evolution of the knowledge-development process.

As an applied science research program, its general objective contained two components. First, that objective presupposed that scientists could develop knowledge to manage the impact human beings have on nature.[2] Analyzing the interrelationships between organisms and their environment in order to develop plans for the management of the range to gain benefits without harming the environment is based on that assumption. Second, that objective was oriented as much toward the wider society as toward developing new knowledge about nature. Society applied the knowledge to manage the impact it had on nature. Due to that orientation towards the wider society, society always shaped the knowledge-development process, even if only indirectly. The objective of range management research, as articulated by Coville, Bentley and Smith, and adopted by subsequent generations of scientists, contained

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[2] That presupposition was based on another assumption: that human beings are active agents who can have an impact on nature.

both components: develop new knowledge that others in society, the stockmen, could use to insure use of the range consistent with conservation. Thus, although those components can be distinguished for analytical purposes, they were intimately intertwined in the general objective of range management's research program.

Although individual scientists in their research attempted to satisfy that general objective of the research program, the tension arose when researchers isolated one component from that objective and focused their research on that component too-exclusively. They presupposed that a relationship existed between the means and the end so that they could focus on only one component and still satisfy their general objective. The basis for that assumption was the implicit idea that because the general objective provided the impetus for the questions they examined in their research, their goal - answering those questions - would satisfy both components of the general objective. In other words, because the general objective was the source of the goal, meeting the goal supposedly also satisfied the general objective.

Yet questions for research are relevant only when they are formulated properly, and that introduces a problem: what does it mean for a question to be formulated properly? Considering the nature of the general objective in range management research, they should be formulated properly with respect to both components of that objective.

The research of Coville, Bentley and Smith suffered very little from the effects of that tension. By allowing the stockmen to participate in the knowledge-development process, they insured that most of their research satisfied both components of the general objective they had articulated for range management research. They both developed new knowledge and ensured that others in society could use it.

Nevertheless, by allowing the stockmen as much influence on the knowledge-development process as they did, they introduced the tension by focusing too much on developing useful knowledge to the detriment of the development of new knowledge. Smith's method was ignored because it was based on a scientist's understanding of nature. His method could serve as an indicator of range condition, and thus serve as the basis for the development of a management plan. As Sampson later showed, the insight on which he based his method was also valid. Yet because it was not obvious to the stockmen who participated so directly in the knowledge-development process how they could immediately use his method, no one recognized the validity or usefulness of the method. Even Bentley, who continued the research begun by Smith, did not recognize the usefulness of the method. Scientists could recognize the validity and usefulness of that method only when the social context surrounding knowledge-development had changed.

By attempting to be more scientific, the second generation also introduced a tension into the knowledge-development process, a tension between the general objective and the individual goals of the scientists in their research. When the goal became developing more certain knowledge through quantification and experimentation, the questions they phrased for research were properly formulated with respect to only one component of that objective. Therefore, the tension in their research emerged as a result of the "scientification" of the applied science knowledge-development process.[3]

The general objective presented the end they tried to achieve. As the first means to achieve that end, they focused their research on developing new knowledge that could provide the foundation for more scientific management of the range without worrying about the other component, use of that knowledge by others. To develop new knowledge, they introduced a second set of means: controlled experimentation and quantification. By thus isolating what they should do, they adhered to the standard for research acceptable in the scientific community. Yet in the process, they also introduced a tension between their individual goals in the research they conducted attempting to develop new knowledge and the orientation towards the wider society. They nevertheless generally ignored that tension and pursued their research in accordance with that standard.

Griffiths' dry-weight method provided the clearest example of the tension between the research program's objective and his goals in his research. Griffiths set out to determine how much forage a particular range produced. That question clearly had relevance with respect to the objective of the research program because the amount of forage determined how many animals the stockmen could graze on a range without damaging it. Yet he then addressed that question in isolation, not concerned with the applicability of his method. He focused his research

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[3] For a more general view of the goal-oriented nature of scientific research, see Ron Johnston and Tom Jagtenberg, 1978, "Goal Direction of Scientific Research," in The Dynamics of Science and Technology. Sociology of the Sciences, Volume II, ed. by Wolfgang Krohn, Edwin T. Layton, and Peter Weingart (Dordrecht, Holland: D. Reidel Publishing Company), 29-58.

on answering the question in accordance with the standard for scientific research. As a result, he reformulated the question and was only concerned with gaining a quantitative measure of the amount of forage, without wondering before he set out to generate those numbers what they actually meant. He developed a new method that gave such a quantitative indication in terms of the dry-weight of the forage. Yet that dry-weight did not tell him anything about the number of animals that could graze on the range, and could certainly not be used by the stockmen as the foundation for their management practices on the range.

Therefore, the way he reformulated the question when attempting to exhibit the identity of a scientist was not the proper way to rephrase the question with respect to the objective of the research program. It ignored range management's orientation towards society, particularly the orientation towards the stockmen, and the influence that orientation would have when he had to interpret what his method actually meant. The question was only properly rephrased with respect to the goal he set for his research: gain a quantitative indication of the amount of forage on the range.

Griffiths also had to introduce a two-stage evaluative procedure into the knowledge-development process as a result of his attempt to be more "scientific." He first determined whether his method achieved the goal he had set for himself. The implicit criterion in that evaluation was whether he had satisfied the standard for scientific research, i.e. whether he had developed a method that could provide a quantitative insight into nature. Only then did he attempt to determine what the measure actually meant for the actual management on the range. Thus, as a concomitant to the tension between the general objective and individual research goals, another tension emerged in the validating process because scientists had to use different criteria to evaluate two different kinds of meaning of new knowledge. They assumed congruency between these two validating processes, just as they assumed congruence between goals and objective.[4]

Later scientists would fine-tune Griffiths' method and develop other quantitative methods to determine how much forage a range contained and how much could be used without damaging the range, yet continued to wonder about the meaning of those measurements. In 1949, the range management researcher Harold F. Heady reviewed several of those quantitative methods, including a weight method. He concluded that "the real problem is not the measurement ..., because many of the above methods will give accurate measurements, but the interpretation of those measurements."[5] He also observed that most range management researchers did not use quantitative methods to determine the condition, grazing capacity, and degree of use of the range, relying instead on estimates. "From the standpoint of actual management of range land the methods based on estimation seem to have found the greatest favor."[6] Furthermore, according to Heady, they were justified in relying on

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[4] Smith must also have used two-stage evaluative procedure, one to determine whether the insight on which he based his method was valid, and then another to determine the meaning of that method for the actual management of the range. However, because he, due to the influence of the stockmen, focussed his research on developing knowledge of use to others, that two-stage process did not show up as clearly in his account.

[5] Harold F. Heady, 1949, "Methods of Determining Utilization of Range Forage," Journal of Range Management, 2(2):61.

[6] Ibid..

estimates: "Even though the determinations may not be quite as accurate as those with the measurement methods, they are usually sufficiently accurate for efficient management."[7] Methods based on estimation, however, never figured as prominently in range management research, or in his review. Even though they satisfied both components of the general objective, they probably were not pursued further because they were not congruent with the identity of scientists, i.e., they were not based on quantification and controlled experimentation.

The tension between the general objective and the individual goals of scientists in their research was not as obvious in the methods, concepts, and criteria Jardine and Sampson used in their research. In part, both avoided some of that tension because they focused part of their research on testing various management practices. They thereby oriented their research directly towards the consumers of the knowledge, and that orientation influenced their methods. Both also presumed to know the concerns of the stockmen well-enough to judge for them whether they could and should use the knowledge the scientists had shown to be valid and useful. Those concerns influenced how they formulated their research questions and influenced the criteria they used to determine the results of their experiments. Jardine, when he developed his new methods to determine range condition, also focused directly on evaluating and changing current management practices and developing new management methods. Sampson, when he re-elaborated the concept of succession, was influenced by the general objective of the research program. The wider social concerns that provided a crucial component of

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[7] Ibid..

that general objective thereby shaped his re-elaboration. Their results, therefore, whether in the form of new methods, management practices, or re-elaborated concepts, reflected both components of the general objective to a larger extent than did Griffiths' method.[8]

Still, the research of both contained the seeds of that tension because both accentuated the distinction between users and scientists, arguing that the development of plans for the management of the range required expertise the stockmen did not possess.[9] In and of itself, that distinction does not have to lead to increased tension between the scientists' goals and the objective of the research program. The danger exists, however, that the scientists when they develop their expertise further remove themselves from the concerns of the stockmen whom they think they are helping. The knowledge-development process then no longer reflects the concerns of the stockmen, but reflects the individual goals the scientists try to achieve in their research, thus creating the tension by influencing the applied scientists' choice of methods, concepts, and criteria to use in their knowledge-development process. Although the new knowledge they develop may be valid in disciplinary terms, it may no longer be useful to address the problems in society for which the scientists think they are developing new

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[8] As a result, some of their recommendations were adopted by stockmen, whereas Griffiths' method was not. For example, by 1915 the most successful sheep owners in Texas had adopted the pasture system advocated by Jardine, although only after the wolf had been exterminated (see B.L. Crouch, 1915, "The Herding System Giving Way to the Pasture System," American Sheep Breeder and Wool Grower June:252-3).

[9] See Sampson, 1917, op. cit., and Jardine and Anderson, 1919, op. cit..

knowledge.[10] Thus, even though wider social concerns still influence the knowledge-development process implicitly, the scientification of that process then leads to a tension within that process.[11]

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10 Harold J. Laski, 1931, The Limitations of the Expert (London: The Fabian Society), is also concerned that as a consequence of the distinction between experts and the rest of society, the experts' advice no longer addresses society's problems in ways that are sensitive to society's concerns. I am grateful to Dr. Raphael Sassower for this reference to Laski (Raphael Sassower, "The Social Role of Specialists," unpublished manuscript).

11 That happened as scientists worked out many of the details of Sampson's re-elaborated conception of succession. He published another paper in 1926 with Harry Malmsten in which he worked out details for the use of indicator species, distinguishing among the meanings of various indicator species for actual range management. See Arthur W. Sampson and Harry E. Malmsten, 1926, "Grazing Periods and Forage Production on the National Forests," U.S. Department of Agriculture, Bulletin 1405. Sampson, however, was remarkable because in almost all his research he was able to maintain congruence between both components of the objective because he focussed much of his research on questions that directly addressed wider social concerns. Other researchers were not always as successful. They generated questions on the basis of problems they perceived in the methods and concepts they used. They then developed new or refined methods and concepts to deal with those problems. Although they assumed that answers to those questions would also solve the problems in the wider society, by focussing on problems that emerged internal to their discipline, they actually often increased the distance between themselves and the wider society. In the 1950's, one range management scientist recognized that scientists had concentrated too much on developing new knowledge and too little on whether and how that knowledge could be used, thus pointing to the what I have called the tension between research goals and general objective. See H.R. Hochmuth, 1954, "A Continuing Appraisal of Range Management," Journal of Range Management, 7(4):147-8. Hochmuth, however, was a member of a very small minority. For examples of scientists who were not as successful as Sampson in achieving congruence between both components, see H.H. Biswell and J.E. Weaver, 1933, "Effects of Frequent Clipping on the Development of Roots and Tops of Grasses in Prairie Sod," Ecology, 14(4):368-90; T. Lommasson and C. Jensen, 1938, "Grass Volume Tables for Determining Range Utilization," Science, 87(2263):444; D.F. Costello and G.T. Turner, 1944, "Judging Condition and Utilization of Short-Grass Ranges on the Central Great Plains," U.S Department of Agriculture, Farmer's Bulletin 1949; Lincoln Ellison, 1949, "The Ecological Basis for Judging Condition and Trend on Mountain Range Land," Journal of Forestry, 47:789-95; R.R. Humphrey, 1949, "Field Comments on the Range Condition Method of Forage Survey," Journal of Range Management, 2(1):1-10; E.J. Dyksterhuis, 1949, "Condition and Management of Range Land

In Sampson's research, the tension also appeared in the criteria he utilized to determine the validity and usefulness of the method and concept he used in his research and to prescribe management practices. He adopted the concept of succession, after re-elaborating it, and the quadrat method because, like Clements, he believed they provided him with insights into nature that could not be gained any other way. In his 1917 paper he first outlined the "scientific" merits of both the concept and the method, and did the same in the first part of the 1919 paper. Only then did he attempt to show how those "scientific" insights could provide the foundation for better range management. He, like Griffiths, thus implicitly distinguished between his goals as a scientist and the general objective of the research program, and used a two-stage validating procedure. But, in contrast to Griffiths, Sampson successfully achieved congruence between his goals and the objective and between the two validating processes because his re-elaboration of the concept was influenced by the wider social orientation of the research program.

Although the scientification of the research process caused the scientists to focus too exclusively on one component of their general objective, it also allowed them to develop new knowledge that could not have been produced otherwise. Through controlled experimentation and quantification both Jardine and Sampson also developed insights that allowed scientists to generalize across habitats to develop and adjust

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Based on Quantitative Ecology," Journal of Range Management, 2(3):104-15; and Kenneth W. Parker, 1954, "Application of Ecology in the Determination of Range Condition and Trend," Journal of Range Management, 7(1):14-23.

management practices. They thereby made it possible for other scientists to utilize short cuts when prescribing management practices.[12] Those scientists would not have to duplicate their efforts. Sampson, for example, recognized that the list of indicator plants he outlined suited only the habitat where he had developed it. He also recognized, however, that he could use his understanding of the plant world and the concept of succession to generalize to other habitats. "The plants here listed as indicators of range conditions are not necessarily the same as those of other regions in the West. As a rule, the same genera will be represented, but in many instances the species will be different."[13] It is doubtful that those insights that allowed such generalizations could have been developed without the expertise range management scientists developed as a result of attempting to be more scientific.

Benefits to the stockmen thus emerge due to the introduction of the tension into the knowledge-development process, even though the application of those benefits required the knowledge of experts. The esoteric knowledge scientists develop as a result of pursuing their own goals in their research could be applied. The scientists or extension agents could often explain the more abstract knowledge, or the implications of that abstract knowledge for the actual management of the

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[12] This part of the discussion owes much to two papers analyzing the role of theory in ecology. See D.B. Botkin, B. Maguire, B. Moore III, H.J. Morowitz, and L.B. Slobodkin, 1979, "A Foundation for Ecological Theory, Memorie dell'Istituto Italiano di Idrobiologia Supplement 37:13-31; and L.B. Slobodkin, 1988, "Intellectual Problems in Applied Ecology," BioScience 38(5):337-42.

[13] Sampson, 1919, 74.

range, to the stockmen so they could use it. At times, furthermore, the tension arose due to the economic and social context of the stockmen. But the scientists and extension agents often could adapt the knowledge to fit the social and economic concerns of the stockmen.

Nevertheless, the assumption that only range management based on knowledge developed by the scientists through quantification and experimentation can provide the foundation for use consistent with conservation introduces the tension. There is then no reason to develop knowledge useful to the stockmen, or that a use-orientation should be part of the knowledge-development process. The scientists can go ahead and pursue their own goals in their research, believing they are helping the stockmen, and unaware that wider social concerns are still influencing their knowledge-development process. The only remaining problem for them then concerns extension work to insure that the stockmen actually use the knowledge. Yet although extension work could overcome certain elements of that tension, it could never be eradicated because that tension was ingrained into the knowledge-development process when range management researchers attempted to be more scientific and distinguished between experts and stockmen. As a result, although they developed knowledge they considered valid with respect to the standard for research, even they could not determine how that knowledge actually helped the stockmen.

Furthermore, many stockmen questioned whether the knowledge produced by the scientists could actually provide the foundation for range use. They argued that the scientists had not discovered anything new, and they questioned whether the research pertained to actual range management.[14] Implicitly, therefore, they argued that range management researchers had not met the general objective of range management as an applied science.

The scientists contributed to that attitude among the stockmen by arguing that only "scientifically" developed knowledge could provide the foundation for range management. They unnecessarily set two knowledge-development processes that shared a common objective in opposition to each other: their own, based on quantification and experimentation, versus that of the stockmen, based on experience and common sense. When even the scientists could not articulate why the knowledge they developed had meaning for the actual management of the range, the stockmen, not surprisingly, raised serious questions about the nature of the knowledge-development process of the scientists and continued to rely on their own.[15]

Many range management researchers in the 1950's, when reviewing their accomplishments in further developing the basic framework for range management science laid down by Griffiths, Jardine, and Sampson, also wondered why stockmen did not use more of the knowledge developed by the scientists. They continued to assume congruency between goals

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[14] See Rowley, 1985, 111. The way Rowley describes it, the stockmen already questioned the usefulness of much of the scientists' knowledge by about 1915. However, his account is very sketchy, and further research analyzing the responses of the stockmen to the scientists' knowledge in the twentieth century is certainly necessary.

[15] This possibility that the scientists contributed to that attitude among the stockmen needs to be examined further in future research. Interestingly enough, the stockmen quickly adopted most of the knowledge developed by Coville, Bentley, and Smith, who had not created such an opposition between the two knowledge-development processes.

and objective and between the two validating processes, and that only newly developed scientific insights could provide the foundation for range use consistent with conservation.

In 1953, L.A. Stoddart, a prominent scientist and textbook writer, argued that scientists and stockmen shared the same general objective: "I do not believe that we are basically different in our objectives in range management regardless of affiliation."[16] He thought this should cause scientists to examine whether they had met the needs of the stockmen. "Fearless examination of principles, techniques and policies ... seems fundamental to harmonious solution of the many problems which now face the range user and range administrator."[17] Yet he did not think that the real source of the problem was imbedded in the knowledge-development process. Instead, he located the problem in the way the stockmen perceived the scientists' knowledge as the foundation for the source of power over their personal lives. "Range management should be as welcome to the rancher as rain. Too often he associates it with curbs and controls - he fears it."[18] Thus, although he recognized a problem, he could not accept that it could also have a source in the knowledge-development process. He firmly believed that only a knowledge-development process focused on goals scientists set for themselves and utilizing means recognized as scientific could provide

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[16] L.A. Stoddart, 1953, "Unanimity - Our Key to Progress in Range Management," Journal of Range Management, 6(3):146.

[17] Ibid..

[18] Ibid..

the foundation for better range management.[19]

In sum, when applied scientists like range management researchers focused on either component of the general objective of their research program to the detriment or exclusion of the other, it limited the knowledge-development process. As a result of such a too-exclusive orientation, they either excluded knowledge that possessed validity in disciplinary terms and that could provide the foundation for the development of range management plans, or they limited even their own ability to understand how the knowledge they developed had meaning with respect to the wider society.

That tension between general objective and individual research goals may well be a characteristic of other applied sciences. Applied sciences seek specifically to develop knowledge in order to facilitate the human manipulation of nature and are thereby oriented towards the wider society that uses that knowledge to manipulate nature. That orientation towards the wider society often influences the objectives, methods, concepts, and criteria applied scientists use in their knowledge-development process, though usually only indirectly. That

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[19] Other scientists also saw scientification of research as the only way to develop knowledge to solve the stockmen's problems. See Arthur W. Sampson, 1936, "Research Problems Pertinent to Better Range Management," The Michigan Forester, 17(6):10-15, 39-41; A.W. Sampson, 1955, "Where we have Been and Where we are Going in Range Management," Journal of Range Management, 8(6):241-6; and E.J. Dyksterhuis, 1953, "Some Notes and Quotes on Range Education," Journal of Range Management, 6(5):295-8. However, see Hochmuth, 1954, for an alternative point of view. The Journal of Range Management occasionally published papers by stockmen who argued that scientists should receive more of a practical education and who saw the further scientification of research as a problem; see George E. Weaver, 1953, "Range Education from the Ranchers' Viewpoint," Journal of Range Management, 6(5):307-8; and E.O. Moore, Jr., 1954, "Range Management on the U Ranch near Carlsbad, New Mexico," Journal of Range Management, 7(1):23-4.

orientation also introduces so many variables into the knowledge-development process that the applied scientists are forced to isolate certain elements so they can focus their research. In that sense they are similar to "pure" scientists. Furthermore, like "pure" scientists, they assume that certain background conditions remain unchanged. Jardine and Sampson, when they implicitly assumed they knew the stockmen so that they could prescribe management practices they thought satisfied the stockmen's concerns, also assumed those concerns of the stockmen had not changed during the course of their research. However, because isolating only one component of the general objective may mean that the individual goals of the scientists no longer satisfy the concerns of the wider society, the applied scientists face a problem their peers in the "pure" sciences do not have to worry about.

At least part of the time, applied scientists have to decide how to isolate a question for research in such a manner that they do not lose their orientation towards the wider society. Yet the belief that only a "scientific" knowledge-development process can provide the foundation for the management of nature removes the scientists from that orientation towards society.[20] At the same time they have to insure that they are far enough removed from the orientation towards the wider society. They have to insure they have the opportunity to pursue their goals utilizing their own means so they can make the contributions only a "scientific" knowledge-development process can make, such as knowledge that allows generalization across habitats. That orientation towards

[20] They generally ignore the tension by attempting to ignore the orientation towards the wider society.

two not always congruent objectives introduces a tension into the knowledge-development process that, in addition to the influences of the wider society on that process, helps distinguish the applied science knowledge-development process from that in the pure sciences.

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