GRASS-COUNTERS, STOCK-FEEDERS, AND THE DUAL ORIENTATION OF APPLIED SCIENCE: THE HISTORY OF RANGE SCIENCE, 1895-1960

by

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(ABSTRACT)

According to the predominant image, applied science is a linear, sequential process, the application of science. First scientists or applied scientists develop knowledge that satisfies the epistemic criteria of science, and applied scientists then find ways to use this certified knowledge to solve society's problems. There is, therefore, a sharp distinction between epistemic or scientific criteria and social criteria.

The historical development of the applied ecological discipline called range science or range management demonstrates instead that applied science is a simultaneous process. Range science developed at a time when America increasingly looked to science to solve social, political, and economic problems in the hope that science's ability to predict could provide the basis for organization and rational management. The institutionalization of range science industrialized ranching. Ranchers appealed to a variety of traditional American values in response to this industrialization, but in the new context surrounding ranching those values had become illegitimate.

From the outset, range science acquired a dual orientation toward both the epistemic criteria of science and the social criteria of society. That dual orientation introduced a tension into range science because it was not obvious how range scientists should satisfy both sets of criteria simultaneously.

Researchers in different institutional contexts developed distinct resolutions to that tension. The most significant difference between the institutions were their political objectives and a difference in the power relations between range researchers and their audiences. Those institutional contexts defined the social criteria and provided the background to judge the acceptability of particular resolutions of the tension, in the process providing the motivation and justification for range science. Nevertheless, range science was not just politics by another means because range scientists also satisfied the epistemic criteria of science. The distinction between epistemic and social criteria therefore did not exist in the historical development of range science because range scientists simultaneously satisfied the epistemic criteria of science and the social criteria that flowed from different political objectives and different power relations between researchers and ranchers.

ACKNOWLEDGEMENTS

This work is but one small part in an ongoing project to understand the interactions between science and society, especially as they pertain to the relationship between humans and nature. As such, I owe debts to a tremendous number of scholars, and I can only mention a few of them here. To my committee, Drs. Gary Downey, Richard Burian, Thomas Dunlap, David Lux, Joel Hagen, and Robert Paterson, for the freedom they gave me to formulate my own questions and to make my own mistakes (still the best way to learn), and for the advice they gave that enabled me to persue my questions and resolve my mistakes. A special thanks to Gary, friend and advisor for continuously challenging me to raise my work to a new level. To the staff at the Utah State Historical Society, the Intermountain Forest and Range Experiment Station, and to Robert Parson, Brad Cole, and A.J. and Jeannie Simmonds in Special Collections at the Merrill Library at Utah State University, I am grateful for the help they gave me uncovering the documents that are at the heart of this work. To my peers in STS, for the stimulating environment that makes academia fun and challenging. To Bill Lynch, what can I say - our discussions are a source of constant stimulation from which I have drawn heavily in this dissertation. Finally, I dedicate this work to Sharon, wife and best friend, without whose support it would have been impossible, and to Camille.

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Chapter 1
INTRODUCTION

The historical development of the discipline called range science or range management reveals how applied scientists in different institutional settings develop knowledge.[1] Range science is an applied ecological discipline. Range scientists study the interactions between humans and a particular natural environment, the range, in order to manage those interactions for particular human purposes. Those purposes include increasing and maximizing the agricultural production humans can obtain from the ranges, in particular by grazing stock animals like cattle, sheep, and goats on them. Range science is therefore also an agricultural science.

I examine the historical development of range science knowledge producing practices between 1897 and 1960 in two different institutional settings. The U.S. Department of Agriculture in 1897 sponsored the first experimental studies of the interactions between humans and the range. The Forest Service institutionalized those investigations after 1905 because it needed a knowledge base to manage the ranges on the National Forests. In addition, researchers at agricultural colleges also performed range research. I examine range

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[1] To avoid confusion, I will call the applied science "range science" and the actual activity of managing the people, animals, vegetation, etc. on the range "range management." Range science develops the knowledge that provides the foundation for range management.

science as practiced at the Forest Service's Great Basin Experiment Station in Utah, popularly known as the Great Basin Station (GBS), and at Utah Agricultural College, now called Utah State University (UAC, USU).[2] Both became prominent in range research; and the research at the Great Basin Experiment Station provided the basis for the first textbook in range management, while Utah Agricultural College range scientists wrote the textbook that replaced it.[3] My analysis stops in 1960 because by then both traditions were established, GBS and

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[2] The GBS went through a number of name changes; at its founding in 1912 it was called the Utah Experiment Station, then became the Great Basin Experiment Station, Great Basin Branch Station, Great Basin Research Center, and is now called the Great Basin Experimental Range. Throughout its history it was popularly known as the Great Basin Station, and I will use the acronym GBS throughout my dissertation. Similarly, I will use USU for both Utah Agricultural College and Utah State University.

[3] Arthur W. Sampson, 1923, Range and Pasture Management (New York: John Wiley and Sons, Inc.); Laurence A. Stoddart and Arthur D. Smith, 1943, Range Management (New York: McGraw-Hill Book Company, Inc.). On the importance of the Great Basin Experiment Station, see Wendell M. Keck, 1972a, "Great Basin Station - Sixty Years of Progress in Range and Watershed Research," USDA Forest Service, Research Paper Int-118; idem, 1972b, "Great Basin Experiment Station Completes Sixty Years," Journal of Range Management, 25:163-166; Thomas G. Alexander, 1987a, "The Rise of Multiple-Use Management in the Intermountain West: A History of Region 4 of the Forest Service," USDA Forest Service, FS-399; idem, 1987b, "From Rule of Thumb to Scientific Range Management," Western Historical Quarterly, 18:409-428. There are at present no accounts of range research at the agricultural colleges. Important research was also performed at Santa Rita Range Reserve in Arizona and at the Jornada Experimental Range in New Mexico; on the Jornada, see Fred N. Ares, 1974, "The Jornada Experimental Range: An Epoch in the Era of Southwestern Range Management," Range Monograph No.1 (Denver: Society for Range Management).

USU researchers became involved in an applied science controversy, and that controversy In addition, during the 1960's, a number of important changes occurred in the way range science had become institutionalized and in American culture in general. I briefly describe those changes and their effect on range science in the conclusion, but a more in-depth analysis of range scientists' responses to those changes will have to remain for future study.[4]

As an applied ecological and agricultural science, range science consists of a variety of knowledge producing practices. I will not propose a definition of applied science. Such definitions are historically contingent. Although historical actors propose definitions of applied

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[4] Valuable accounts of range science appear in analyses of the political and socio-economic circumstances surrounding range management, or in histories of the USDA or the Forest Service. Those accounts present the results of range science, and hence analysis of knowledge development in range science falls outside their scope. In addition to the sources cited in note 3, see William D. Rowley, 1985, U.S. Forest Service Grazing and Rangelands: A History (College Station: Texas A&M University Press), the best general account on Forest Service range management policy and practice; Lawrence Rakestraw, 1958, "Sheep Grazing in the Cascade Range: John Minto vs. John Muir," Pacific Historical Review, 27:371-382; Philip O. Foss, 1960, Politics and Grass: The Administration of Grazing on the Public Domain (Seattle: University of Washington Press); T. Swann Harding, 1947, Two Blades of Grass: A History of Scientific Development in the U.S. Department of Agriculture (Norman: University of Oklahoma Press); Harold K. Steen, 1976, The U.S. Forest Service (Seattle: University of Washington Press). The same is true of histories of range science written by range scientists, but I will cite those in the text.

science that historians may well want to analyze, not much can be gained by forcing either this or future analyses into the straight jacket of an atemporal definition that focusses attention on one or a few arbitrarily privileged aspects. Furthermore, the recent trend in History of Science and in Science and Technology Studies (STS) is away from monolithic definitions towards recognizing that definitions of science are situational and that science includes a myriad of practices not capturable by one definition.[5] Nevertheless, I want to propose a very general characterization of applied science on the basis of what I found in the case study of the history of range science. The historical case study will show that range scientists themselves recognized this characterization of their activity, although they did not realize the implications I draw from it. Their job, after all, is to develop knowledge about the interactions between humans and the range, not to analyze range science.

Virtually at its conception range science acquired a dual orientation towards at least two audiences, members of society and other researchers. On the one hand, range science attempted to produce knowledge for society in order to solve problems particular segments of society wanted solved.

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[5] Science and Technology Studies (STS) is an emerging discipline that integrates and builds on historical, philosophical, and sociological analyses of science, technology, and their relationships with society.

Concerned with solving those problems, range science was thus oriented towards society and attempted to develop practical knowledge. Range science simultaneously had another orientation: developing knowledge about those ranges. As such, range science was oriented towards science and other scientists, and consequently range scientists both wanted and were required to adhere to the standards of science when developing knowledge. As a result, range researchers attempted to develop general knowledge and practical knowledge simultaneously. Researchers in each of the two institutional settings meant something different with "science" and "society," and each conceptualized the relationship between science and society differently. As a result, significant differences existed between the knowledge producing practices in those two institutions, and comparing those practices should increase our understanding of knowledge development in applied science. I will develop the specific meaning of these very general terms, "science" and "society," in the remainder of the historical analysis.

The dual orientation towards both science and society introduced a tension into range science. The goals and norms of the two orientations, of the two audiences, were not necessarily congruent. Yet the objective of range science required that researchers somehow satisfy the norms and goals of both orientations simultaneously. Therefore, when a tension emerged between those two sets of goals and norms, range scientists had to resolve that tension to be able to practice applied science. The primary questions driving my analysis are: what is the influence of the dual orientation on knowledge development, how did researchers resolve the tension due to the dual orientation, and what impact did particular resolutions have on their practices?

Two constraints on range researchers enabled them to resolve the tension due to the dual orientation: first, the policy objectives of the institutions that instutionalized range science, which were often related to the the relationship between the institution, its researchers, and their audiences; and second, the epistemic tradition within which a researcher worked. Specific policy objectives and relationships between researchers and their audiences in different institutional contexts had a major impact on knowledge development. Those relationships enabled researchers to resolve the tension due to the dual orientation because they were a major determinant of a researcher's choice of goals to pursue, choice of research methods, and the interpretations of results. But this was not the only enabling constraint on researchers. The epistemic tradition also served as a powerful enabling constraint. That tradition of knowledge, methods, goals, and standards provided researchers both with reasons for pursuing particular goals in particular ways and with criteria to determine whether they had achieved those goals. Yet we then still need to ask why those reasons and criteria had meaning, both for individual researchers proposing a knowledge claim and for other researchers responding to that claim. The epistemic tradition itself can give us only a partial answer because the question remains why researchers imbued particular reasons and criteria with meaning at the beginning of the tradition. Furthermore, it is not always obvious when, how, and why a researcher should apply particular reasons and criteria. In and of themselves, the reasons and criteria do not tell the researcher how, when, and why to apply them.[6]

The relationship between researchers and their audiences proved crucial in the early development of range science. It provided an invaluable aid enabling researchers to give meaning to particular aspects of the epistemic tradition in particular situations. The knowledge development practices developed by range scientists therefore reflected both their epistemic tradition and their relationship with society.

I focus on categorization as a starting-point to analyze the impact of the dual orientation because categorizing the

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[6] Arguing that a researcher has a reason to apply a particular reason or criteria does not get around the problem because it leads to an infinite regress: that reason itself does not tell the researcher how, when, and why to apply it, and the question then still remains why that reason has meaning.

subject under study proved to be an especially crucial and difficult task facing range researchers. The problems facing stockmen on the range were caused by the interactions between those stockmen and the range. How should researchers categorize those interactions in such a way that their research contributed both to society and to science? Should they characterize those interactions from the perspective of the needs and values of the stockmen? Or should they categorize them beginning from the perspective of biological knowledge of the range? How could they balance the needs of the stockmen and the range in one category? As applied scientists, they somehow had to construct a category that incorporated the stockmen and the range simultaneously, but to do that they had to define how to approach the stockmen and the range. The nature of the science-society relationship in the Forest Service, and in a different way at Utah Agricultural College, enabled researchers to accomplish this difficult task. That relationship set goals and standards for research, and through them defined both the stockmen and the range. The resulting categorization of the subject under study enabled researchers to develop an epistemic tradition that would solve society's problems by contributing new knowledge; i.e., by resolving the tension due to the dual orientation, categorization provided the parameters for knowledge development.

Focussing on categorization as a starting-point enables me to incorporate several insights from recent studies in history of science and agricultural science, STS, and psychology. Those studies seek to go beyond the distinction between content or cognition and context, between internal and external, that dominated early analyses of science and technology. This distinction rests on the existence of boundaries between science and society, between content and context. Yet those boundaries are historical constructions on the part of both scientists and society. That does not mean that the existence of boundaries is a trivial fact, but that we should examine how, when, why, and by whom boundaries are constructed, and the effects they have on the creation, use, and diffusion of knowledge claims.

A number of historians recently called for and used an approach that integrates the context and content of agricultural research. Charles Rosenberg, for example, shows how socio-economic factors and institutional constraints influenced the discovery of hybrid corn.[7] Yet his analysis centers primarily on the motivation for the hybrid corn research, and this leaves the question whether the context for the practice influenced the research practices beyond merely

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[7] Charles E. Rosenberg, 1961, No Other Gods: On Science and American Social Thought (Baltimore: Johns Hopkins University Press). esp. pp.190-195. This is perhaps the seminal work in this historiographical approach.

providing them with a focus. In an important sense the intellectual content of the research remains separate from the context where researchers produced it. We can not analyze the links between the general goals for scientific research and the specific objectives and resulting research practices in particular experiments.[8] In more recent articles, Rosenberg realizes we need to intertwine content and context more fully. He argues and demonstrates that potential applications of knowledge are constrained by decisions made during the research process, although historical actors are often unaware that they are constraining potential applications.[9]

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[8] Alan Marcus' important recent analysis of the ascendancy of the agricultural scientists' role in American agriculture could also be supplemented in this direction. Marcus attributes their ascendancy to a series of compromises that had a number of consequences. Farmers received advice from scientists, though they had to acknowledge their dependence on scientific methods and knowledge. Agricultural researchers gained an institutional context where they could perform their practices, the agricultural colleges and state experiment stations, which now became centers of research. Marcus focusses on the compromises leading up to the legitimation of that role for scientists with the passage of the Hatch Act, and hence does not analyze whether the compromises that gained the scientists their institutional home also affected their research practices. In addition, he does not analyze whether decisions made in the course of scientific practice prior to the passage of the Hatch Act influenced the nature of the various compromises and hence aided the legitimation of agricultural science. See Alan I. Marcus, 1985, Agricultural Science and the Quest for Legitimacy: Farmers, Agricultural Colleges, and Experiment Stations, 1870-1890 (Ames: Iowa State University Press).

[9] Charles E. Rosenberg, 1979, "Towards an Ecology of Knowledge," in Alexandra Oleson and John Voss, eds., 1979, The Organization of Knowledge in Modern America, 1870-1920 (Baltimore: Johns Hopkins University Press), pp.440-455; idem,

Analyses by Deborah Fitzgerald and by Diane Paul and Barbara Kimmelman largely adopt this general approach.[10] Fitzgerald presents an analysis that closely resembles my framework for the analysis of range science. She argues that the different contributions made to hybrid corn development by University of Illinois experiment station researchers and researchers working for a private seed company can be explained in terms of the different roles each group of researchers was expected to play. These different roles, in turn, emerged out of the different relationship each group of

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1983, "Science in American Society: A Generation of Historical Debate," Isis, 74:356-367; and idem, 1988, "Woods or Trees? Ideas and Actors in the History of Science," Isis, 79: 565-570. One of the best critiques of agricultural research for its social impact, i.e., for re-creating particular social orders, although polemical, is Jim Hightower, 1973, Hard Tomatoes, Hard Times: A Report of the Agribusiness Accountability Project ... (Cambridge: Shenkman).

[10] Deborah Fitzgerald, 1986, "Tradition and Innovation in Agriculture: A Comparison of Public and Private Development of Hybrid Corn," in Lawrence Busch and William B. Lacy, eds., 1986, The Agricultural Scientific Enterprise: A System in Transition (Boulder: Westview Press); and Diane B. Paul and Barbara A. Kimmelman, 1988, "Mendel in America: Theory and Practice, 1900-1919," in Ronald Rainger, Keith Benson, and Jane Maienschein, eds., 1988, The American Development of Biology (Philadelphia: University of Pennsylvania Press), pp. 281-310, esp. 296-301. Analysts of many other agricultural sciences, especially ones that have the (potential) economic impact of hybrid corn, and also have other potential impacts, e.g., biotechnology, have now also begun to adopt this approach, although I think they can be pushed further along this line; see e.g., Sheldon Krimsky, 1982, Genetic Alchemy: The Social History of the Recombinant DNA Controversy (Cambridge: the MIT Press), and Lawrence Busch et al., 1991, Plants, Power, and Profit: Social, Economic, and Ethical Consequences of the New Biotechnologies (Oxford: Basil Blackwell).

researchers had with the audience, Illinois' farmers. As a result of those different relationships, each group had different concerns regarding corn research, and hence each group adopted a different research strategy.[11] In her dissertation on the introduction of Mendel's theory into the U.S., Barbara Kimmelman links the emergence of experimental agricultural science to changes in American society. She argues that experimentation legitimated the expertise of agricultural scientists and that the interventionist techniques of experimentation mirrored government interventions in the social, political, and economic spheres of American life.[12]

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[11] She does not closely analyze whether those different interests also affected research practice beyond merely providing research goals and thereby taking research in a different direction. She assumes that once the goals are set, the practice of science follows a logic of its own. As a result, although Fitzgerald no longer sees scientific knowledge as neutral, the practices whereby the knowledge is developed is somehow seen as neutral, as distinct from the social influences that helped set its objectives. The same critique also applies to Paul and Kimmelman, 1988, who show that the researchers involved in hybrid corn realized the potential impacts of their knowledge once it became introduced into society, but who also maintain the practice as somehow separate.

[12] Barbara A. Kimmelman, unpubl. Ph.D. dissertation, A Progressive Era Discipline: Genetics at American Agricultural Colleges and Experiment Stations, 1900-1920, University of Pennsylvania, 1987, pp. 22-23. I will make a similar argument. However, although she convincingly shows the role agricultural scientists played in the early introduction of Mendel, she does not analyze whether the agricultural science context that made possible the introduction of genetics also affected the way researchers performed genetics research, and hence the knowledge that emerged out of that research.

None of these approaches to the history of the agricultural sciences focusses on categorization of phenomena as a means to intertwine content and context. Instead, they focus on disciplines, institutions, or individual biographies.[13] The typical argument is that the discipline and institutional home of a researcher constitute a field of possibilities that to some extent determines the outcome of choices facing an individual. One of the choices the individual has to make is how to construct categories, and the influence of those categories on subsequent research, but this aspect of research has not yet received a lot of attention.

Recent research in psychology indicates that analyses of categorization might prove an additional fruitful means to intertwine content and context. I do not mean that we should now rush out and psychologize STS and history, but the opportunity exists to incorporate their findings.[14] The psychological research shows that categories are constructed by extending previous practices rather than following rules, that meaning resides in use, and that categories and meaning are graded and unstable, varying with context, goals, and

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[13] On this point, see Rosenberg, 1983, 1988.

[14] For a very strong argument along these lines, see David Faust, 1984, The Limits of Scientific Reasoning (Minneapolis: University of Minnesota Press), who argues we should not construct arguments that violate what psychologists have shown about the limitations and capacities of human cognitive capabilities.

recent experience.[15] In short, context and content are intertwined in the categorization of an object. Furthermore, once an object is categorized, it constitutes one element constraining the field of possibilities confronting an individual. Yet a particular categorization does not solely determine choices because it is unstable, and individuals can decide to adjust the categorization in particular contexts. Hence the relationship between the content and context of categorization is fluid enough to prove fruitful to historians.

The constructed, contextual nature of categorization meshes well with recent research in STS that open investigations of knowledge to social analyses. Those studies

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[15] See Eleanor Rosch, 1978, "Principles of Categorization," in E. Rosch and B.B. Lloyd, eds., 1978, Cognition and Categorization (Hillsdale, N.J.: Erlbaum), pp. 28-48; Eleanor Rosch and Carolyn B. Mervis, 1975, "Family Resemblances: Studies in the Internal Structures of Catagories," Cognitive Psychology, 7:573-605; Carolyn B. Mervis and Eleanor Rosch, 1981, "Categorization of Natural Objects," Annual Review of Psychology, 32: 89-115; Emilie M. Roth and Edward J. Shoben, 1983, "The Effect of Context on the Structure of Catagories, Cognitive Psychology, 15:346-378; Lawrence W. Barsalou, 1987, "The Instability of Graded Structure: Implications for the Nature of Concepts," in Ulric Neisser, ed., 1987, Concepts and Conceptual Development: Ecological and Intellectual Factors in Categorization (Cambridge: Cambridge University Press), pp. 101-140; and Lawrence W. Barsalou, 1984, Constructing Representations of Catagories from Different Points of View," Emory Cognition Project, Report No. 2 (Atlanta: Emory University). See also Willett Kempton, 1981, "Category Grading and Taxonomic Relations: A Mug is a Sort of a Cup," in Ronald W. Casson, ed., 1981, Language, Culture, and Cognition: Anthropological Perspectives (New York: Macmillan Publishing Co.), pp.203-230.

demonstrate the necessary, enabling role of social factors in science and technology, and the extent to which decisions made during the research process shape the application of its results. As a "sociology of truth," they analyze the open-ended processes whereby scientists and technologists develop new knowledge, and demonstrate the utility of social accounts beyond merely explaining the occurrence of error or the application of science. These studies show the negotiations necessary to construct closure, and hence to construct knowledge[16]; the interests and values that enter into constructing knowledge, and their indispensable role enabling practice and rationality[17]; the work scientists in particular put into constructing boundaries between science and society, the effect of those boundaries, and the nonetheless integral interrelationship between science or technology and society[18]; the role of power and politics in science and

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[16] Karin D. Knorr Cetina, 1981, The Manufacture of Knowledge (Oxford: Permagon); and Bruno Latour and Steve Woolgar, 1986, Laboratory Life: The Construction of Scientific Facts (Princeton: Princeton University Press).

[17] David C. Bloor, 1976, Knowledge and Social Imagery (London: Routledge & Kegan Paul); Barry Barnes, 1977, Interests and the Growth of Knowledge (London: Routledge and Kegan Paul); Helen E. Longino, 1990, Science as Social Knowledge: Values and Objectivity in Scientific Inquiry (Princeton: Princeton University Press). These are fairly programmatic statements; perhaps the best case study is Steve Shapin and Simon Schaffer, 1985, Leviathan and the Airpump: Hobbes, Boyle, and the Experimental Life (Princeton: Princeton University Press).

[18] Among many works on these topics, see Thomas F. Gieryn, 1983, "Boundary Work and the Demarcation of Science from Non-

technology[19]; and that meaning resides in use.[20] Many of these claims have not gone unchallenged, but the sheer number of case studies demonstrates the fruitfulness of this approach.[21]

Only two studies within this general approach analyze categories specifically, and none analyze applied science. Helen Longino in her analysis of reasoning as a practice claims that categories, and the values they often embody, are

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Science: Strains and Interests Among Professional Ideologies of Scientists," American Sociological Review 48:781-795; Robert S. Westman, 1980, "The Astronomer's Role in the Sixteenth Century: A Preliminary Study," History of Science 18:103-147; Bruno Latour, 1987, Science in Action (Cambridge: Harvard University Press).

[19] See, e.g., Joseph Rouse, 1987, Knowledge and Power: Toward a Political Philosophy of Science (Ithaca: Cornell University Press); David Dickson, 1984, The New Politics of Science (Chicago: University of Chicago Press); Robert E. Kohler, 1982, From Medical Chemistry to Biochemistry: The Making of a Biomedical Discipline (Cambridge: Cambridge University Press); and Langdon Winner, 1986, The Whale and the Reactor: A Search for Limits in an Age of High Technology (Chicago: University of Chicago Press); Joseph G. Morone and Edward J. Woodhouse, 1989, The Demise of Nuclear Energy? Lessons for Democratic Control of Technology (New Haven: Yale University Press).

[20] Almost all these studies show this too some extent, although some fail to recognize that current use may be an extension of a tradition of use. For nice examples of this point, see Knorr-Cetina, 1981; Rouse, 1987; and for a general, philosophical argument that provides the foundation for the claims of many but not all of these analysts, and for some of the psychologists, see Ludwig Wittgenstein, 1953, Philosophical Investigations (Oxford: Basil Blackwell).

[21] For a recent critique, see Paul Roth and Robert Barrett, 1990, "Deconstructing Quarks," Social Studies of Science 20:579-632; for a review of the case studies, see Steven Shapin, 1982, "History of Science and Its Sociological Reconstructions," History of Science 20:157-211.

necessary for evidential reasoning.[22] Although this underscores the importance of categories, she does not analyze the process whereby categories are constructed in any detail. David Bloor does present such an analysis, and argues that categorizations of nature reproduce social categories.[23] We therefore have to analyze the social interests of the scientists doing the categorizing. Martin Rudwick, who builds on many of these insights but does not attribute the same causal efficacy to them, also analyzes the construction of a category, the geographical time period known as the Devonian. Rudwick argues that although social factors played a crucial role shaping reliable scientific knowledge, the social construction of knowledge was constrained by the "differentiating effect" of empirical evidence.[24]

Nevertheless, from a historian's perspective many of these studies are too committed to the theoretical insights that motivated them. In fact, they are so committed on a priori grounds to sociological explanation that we can now

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[22] Longino, 1990.

[23] David Bloor, 1982, "Durkheim and Mauss Revisited: Classification and the Sociology of Knowledge," Studies in the History and Philosophy of Science 13:267-297.

[24] Martin Rudwick, 1985, The Great Devonian Controversy, The Shaping of Scientific Knowledge among Gentlemanly Specialists (Chicago: University of Chicago Press), esp. pp. 454-456. Longino would probably agree with Rudwick on this point.

speak of a "tyranny of the social" in much of STS.[25] Based on the theory-ladenness of facts and the Duhem-Quine thesis (facts underdetermine theory choice), they conclude that evidence can not determine theory choices and that therefore social factors must determine the outcome of choices facing scientists.[26] Thus reducing explanations of knowledge development to sociological explanations, the primary debate within this community centers on the form of the sociological explanation. They exclude form consideration, for example,

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[25] With one exception, I will not present specific critiques of these approaches; plenty are available in the literature, and I basically agree with Foucault's statement that "For myself, I prefer to utilize [rather than comment on] the writers I like," though for somewhat different reasons; quoted in Patricia O'Brien, 1989, "Michel Foucault's History of Culture," in Lynn Hunt, ed., 1989, <u>The New Cultural History</u> (Berkeley: University of California Press), pp. 25-46, on p.25, the statement between brackets is O'Brien's clarification. The general critique that follows does not apply to Westman, 1980; Dickson, 1984; Winner, 1980; Kohler, 1982; Rudwick, 1985. Longino's, 1990, contextual empiricism is somewhat of an exception, but she locates objectivity in the social practice of criticism, which eliminates the personal values necessary for evidentiary reasoning from science. But, as she acknowledges, that practice will not eliminate social, institutional, or community values, and she does not explain why we should prefer social values over individual values.

[26] This does not necessarily imply anti-realism or that rationality plays no role: "The role of the social ... is to prestructure choice, not to preclude choice"(Shapin, 1982, p.198); and our practices respond "... to the capacities of the things we deal with" but the world and our practices become "... determinate only in relation to the other," (Rouse, 1987, p.154; thus our practices do not determine what exists, but what exists reveals itself only through our practices, and our practices and the world acquire meaning only through use in a context).

epistemic values scientists often appeal to in the course of research, such as explanatory and predictive power, generality, universality, accuracy, testability, etc.. As part of the practice of science, these values may well help a researcher to determine whether (s)he has developed something interesting, or make it possible for other researchers to reach that conclusion.[27] It is furthermore not clear why we should favor one type of sociological explanation over another. The real question at this stage therefore becomes how we can use these insights. Enough case studies exist to indicate the robustness of their conclusions, and their is no a priori reason to expect that applied science will be different from science in this respect.

We need to historicize STS. When historians adopt an insight such as a theory from a neighboring discipline, whether explicitly or implicitly, they do so because they find it useful, but they do not see it as the only or decisive explanatory factor.[28] Yet because historians do not often

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[27] For an argument on the continuing relevance of the role of epistemic values as offering a parallel account to sociological explanations, see David K. Henderson, 1990, "On the Sociology of Science and the Continuing Importance of Epistemologically Couched Accounts," Social Studies of Science, 20:113-148.

[28] Rudwick, 1985, is a good example of that practice. Cultural historians draw heavily from the work of anthropologist Clifford Geertz, and more recently Marshal Sahlins; see e.g., the articles in Hunt, ed., 1989; or Robert Darnton, 1984, The Great Cat Massacre and Other Episodes in French Cultural History (New York: Basic Books). The impact

present their work in the framework of theories or propose theories, many STS scholars tend to ignore the implications of historical analysis for STS. Historians argue that phrasing questions, especially interesting new questions, is perhaps the most difficult problem they face.[29] STS can provide one source of possible questions for historians, and then not the most important source. Other disciplines of course also face this problem; the difference resides in how they resolve it, and whether the focus is on exemplifying theory or on developing new questions or extending existing questions into new domains. Many STS analysts generate new questions from existing theory or in response to perceived problems in existing theory. In case studies, the analyst then often translates a particular theory into one or a few questions, and the answer reflects back on the theory. For many STS scholars history thus provides a way to exemplify particular theories. I do not mean that STS scholars abuse history, they produce interesting and good case studies. The difference is one of orientation.

Those are not the practice or task of historians. They

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of cultural history on the discipline as a whole still remains to be seen, but it has spawned interesting, fruitful, and at times debatable new interpretations.

[29] See, e.g., G.R. Elton, 1967, The Practice of History (New York, Thomas Y. Crowell Company), esp. pp.84-86; or David Hackett Fischer, 1970, Historians Fallacies: Toward a Logic of Historical Thought (New York: Harper's Torchbooks), esp. ch.1.

draw their questions from a much larger domain, especially emphasizing the empirical material as a potential source of questions to a much larger extent than other analysts who study history (from a historian's perspective, many STS analysts put the cart before the horse by giving theory priority over questions and the empirical material). The historian then constructs a thesis, and the historical account serves as an answer to demonstrate the fruitfulness of the questions and exemplify the thesis. To develop an answer, the historian draws on the empirical material, and at times on insights from other disciplines. A particular theory often can not capture the events of the historical record. After all, we use a number of social sciences to analyze social events, and a number of insights from different disciplines, including history, can help answer the questions. Arbitrarily restricting the historian to use certain theories limits the generation of new questions, their fruitfulness to explore and describe history, and the historians ability to exemplify his or her thesis.[30] On the other hand, historians can make

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[30] An example may clarify this point. I found the different responses of a sociologist and a historian to two historical case studies (Shapin and Schaffer, 1985; Rudwick, 1985) that draw on the same sociological insights informative. The sociologist assumes that the shared background indicates both studies are accountable to the criteria of sociological explanation as he sees them, and then criticizes Rudwick for not giving the same explanation as Shapin and Schaffer. See H.M. Collins, 1987, "Pumps, Rocks, and Reality, Sociological Review, 35:818-828. The historian, on the other hand,

better use of other disciplines' insights, in part to generate new questions, and especially to avoid errors due to a naive empiricism emanating from a commitment to "hyper-objectivity."[31]

In addition to changing our question-asking practices in STS, we need to contextualize our use of theories. Drawing their questions from theory, and studying history to exemplify those theories, STS often produces mono-causal histories. The question asking practices of many STS scholars reduce multi-causality to mono-causality. Although they discuss a number of other causes, their analyses ultimately become mono-causal accounts in terms of the analyst's favorite social factor.[32]

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recognizes their similarity, but also recognizes that they have very different goals. See Rosenberg, 1988, p.567, n.1.

[31] I take the term "hyper-objectivity" from Peter Novick, 1988, <u>That Noble Dream: The "Objectivity Question" and the American Historical Profession</u> (Cambridge: Cambridge University Press). On this issue see also David Hollinger, 1985, <u>In the American Province: Studies in the History and Historiography of Ideas</u> (Bloomington: University of Indiana Press), and Thomas Haskell, ed., 1984, <u>The Authority of Experts: Studies in History and Theory</u> (Bloomington: University of Indiana Press), esp. p.xix.

[32] I am caricaturing STS slightly, but Shapin and Schaffer, 1985, arguably the best historical case study in STS, is ultimately a mono-causal account: "Solutions to the problem of knowledge are solutions to the problem of social order."(p.332) Ironically, in the last two sentences of their book, they then make this causal account appear very ambiguous: "Knowledge, as much as the state, is the product of human action. Hobbes was right." (p.344) I have no qualms with the claim that human action produces knowledge; their own analysis, and many other accounts of knowledge development, make that point indisputable. But if Hobbes, and not Boyle, was right on that point, then why was his solution to the problem of social order not accepted as the solution to the

But science is not merely the result of negotiations, interests, power, rhetoric, etc. Scientific practice is directed toward something external to itself; it is not totally self-referential. Nature, as much as human practice, enables and constrains knowledge, and scientists developed standards and methods that enable nature to play that role in science. Scientific knowledge is constructed, contextual, and contingent; and scientists in their practices intertwine many different elements, from a wide variety of sources both within and outside science, often opportunistically so. Our use of theory should mirror this complexity.

The analytic problem is to understand the role social and epistemic factors played in knowledge development. We should allow the empirical material and our questions to guide our use of STS and historian's theories about knowledge development. Hence for some questions, mono-causal accounts are still possible; their conclusions, however, should not be over-generalized but linked to the question they are intended to answer. Nevertheless, since knowledge development is so deeply embedded in both social and epistemic factors, mono-causal accounts can not explain scientists' practices, and our

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problem of knowledge. That is, given their central claim, why did Hobbes lose? That question becomes even more significant because they do not show that Boyle's solution was the solution to the problem of social order outside science, as they claim.

questions therefore should not force us into such analyses. Furthermore, we deal with different phenomena and processes at different points during our analysis of knowledge development, and analyzing each may require its own conceptualization and methods. STS' theories and methods can be useful in those analyses, but rather than attempting to integrate the available theories and methods into one new theory or method, they should be used at the appropriate level and to complement each other. That is, we have to be more conscious of the restricted domain to which particular theories and methods apply, and use them accordingly.[33] From a historical perspective, the debates between philosophers and sociologists whether epistemic or social factors determine what counts as knowledge is vacuous and not interesting. Neither epistemic validity nor social factors are the only factor involved in assessing a knowledge claim. The meaning of the claim resides in its use, either by the researcher who proposed it in his or her future work or by other researchers in their work. As a result, the meaning of the knowledge claim resides in both its epistemic validity and in the context in which it is

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[33] I am not advocating some weak pluralism. We can judge whether certain theories and methods are better to analyze certain phenomena or processes, but we have to become more conscious of their domain and avoid over-generalization. I advocate a pragmatic use of STS theories in terms of our questions and the available empirical material, not mere opportunism or pluralism for its own sake.

introduced. This would pose a particularly difficult problem for range scientists because their claims had to have meaning for two or more audiences simultaneously.

However, power relations between researchers and audience played a significant role in the Forest Service and at Utah Agricultural College. Because the relationships between researchers and their audiences played an important role, the power relations contained in those relationships are an important part of my analysis. At both the Forest Service and USU, specific power relations enabled specific categorizations and thereby constrained knowledge development practices. I therefore need to say something more specific than the preceding general critique about the framework that underlies my analysis of power.

Joseph Rouse recently presented an interesting account of the role of power in science.[34] The general critique contains my answer to his reduction of knowledge to power, but he also has no account of discretionary power. Power for him consists merely in particular practices constraining other people's future "field of possible actions."[35] As such he presents us with a one-dimensional view of power.

On the other hand, members of the Frankfurt School of critical theory and analysts influenced by their arguments

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[34] Rouse, 1987.
[35] Ibid., see esp. 185-211.

claim that power over nature entails power over people.[36] People who want power over nature become dependent on the experts who develop the knowledge that provides the means to acquire power over nature. However, the critical theorists are insufficiently historical. As a result, they overestimate the value of expertise. They do not recognize that extending power over nature into power over people is an achievement and hence contingent on the institutional contexts in which knowledge is developed and on the socio-political context surrounding those institutions. They also present society as much more homogeneous and cohesive than is warranted, and ignore the role of struggle and negotiations over power and the influence of values, traditions, and local factors in such a struggle. Finally, they presuppose that scientific and technical knowledge has the same meaning for everyone and just imposes itself on people. Recent STS research demonstrating, e.g., that meaning is contingent on local factors and resides in use, and that democracies can shape technology, undermine

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[36] See e.g., Max Horkheimer, 1947, Eclipse of Reason, (New York: Oxford University Press); idem, 1974, Critique of Instrumental Reason, transl. Matthew O'Connell et al. (New York: Seabury Press); idem and Theodor W. Adorno, 1972, Dialectic of Enlightenment, transl. John Cumming (New York, Herder and Herder); Andre Gorz, 1980, Ecology as Politics, transl. Patsy Vigderman and Jonathan Cloud (Boston: South End Press); William Leiss, 1972, The Domination of Nature (Boston: Beacon Press); for a good review and analysis of the Frankfurt School, see David Held, 1980, Introduction to Critical Theory (Berkeley: University of California Press).

this account as too simplistic and deterministic.[37]

I prefer Steven Lukes' three-dimensional account of power.[38] It can accommodate Rouse and the critical theorists, includes an account of discretionary power, and is flexible enough to be of interest to historians. The first dimension of power is the ability to make someone else do something they do not want to do, and depends on the use of political power. The second dimension captures the idea that people in power can construct boundaries that prevent others from even participating in decision-making (e.g., through the use of values, rituals, institutionalized procedures). The third dimension captures the idea that through socialization, enculturation, control of education and information, etc., the

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[37] See, e.g., Knorr-Cetina, 1981; Morone and Woodhouse, 1989. The environmental historian Donald Worster has been influenced by the critical theorists; see his 1977, 1985, Nature's Economy: A History of Ecological Ideas (Cambridge: Cambridge University Press); and his, 1985, Rivers of Empire (New York: Pantheon Books). Although he is historically more sophisticated than the critical theorists, he nonetheless places more weight on the structure he adopts from them than it can bear. For an analysis that places scientists' knowledge and the role of that knowledge in shaping political decisions about our relationship with the natural environment in a wider context, see Thomas R. Dunlap, 1988, Saving America's Wildlife (Princeton: Princeton University Press).

[38] Steven Lukes, 1974, Power: A Radical View (London: Macmillan). An excellent case study using Lukes' framework that convinced me of its utility is John Gaventa, 1980, Power and Powerlessness: Quiescence and Rebellion in an Appalachian Valley (Urbana: University of Illinois Press). I do not mean to imply that my analysis of range science is merely an application or a test of Lukes' framework. Rather, when I do talk about power, Lukes' framework provides the background.

powerless never realize that power is exerted over them and that they have the power to change things; i.e., they are not prevented from participating by boundaries but because they never realize there is something to participate in or that things can be different. Rouse's account seems to me to fall under Lukes' third-dimension. The practices he points us to do not erect specific boundaries and do not depend on the political resources of the actors, but they can prevent actors from realizing the possibility of other options and of change.

I use the theories and methods of STS in the historicized sense as background throughout my analysis. My account is not cast explicitly or implicitly in the framework of any of those analyses, and I will cite specific works only if appropriate. Chapter two analyzes the causes of the range's destruction that led to the emergence of range science, and places that emergence in the context of late-nineteenth century America. I argue that range science emerged in the hope that rationalizing range management would solve problems that had political, economic, and social causes, and that we can there see the emergence of the dual orientation towards both science and society. Chapter three discusses the first range science experiments, and I argue that the dual orientation caused a categorization problem.

Chapter four examines the first experiments performed under the auspices of the Forest Service, and I argue that the

Forest Service's goals enabled the resolution of the tension. A knowledge claim emerging out of that resolution was later challenged. I discuss the background to that claim and the subsequent challenge in chapter five, where I also analyze the introduction of a theory to provide a foundation for range science and range management. Chapter six analyzes various renegotiations of Forest Service range researchers' knowledge claims. I briefly analyze subsequent research in the Forest Service and the negotiations between Forest Service administrators and stockmen about range researchers' claims. The bulk of the chapter, however, concentrates on the development of range science at Utah Agricultural College. USU researchers experienced a difficult time establishing a research program, until they finally developed an alternative resolution to the tension.

As a historical analysis of range science, I have had to be selective in my focus and use of empirical material. This is not a chronicle of every significant contribution range researchers have made. Range science is an incredibly complex discipline. Although applied ecology provides the foundation, it requires a strong background in a broad range of subsidiary disciplines such as botany, physiology, genetics, chemistry, mathematics, cartography, soil science, geology, zoology, economics, sociology, and later of course range science and management itself.[39] This incorporation of knowledge from other disciplines occurred gradually, and I will not analyze every time that occurred.[40] I am interested in the dual orientation towards both science and society simultaneously and its effect on knowledge development practices. That interest guides my selection of the empirical material.

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[39] On this point, see Arthur W. Sampson, 1954, "The Education of Range Managers," Journal of Range Management, 7:207-212 (hereafter cited as JRM); and E.J. Dyksterhuis, 1953, "Some Notes and Quotes on Range Education, JRM, 6:295-298.

[40] Chronicles of range science and range management can be found, in addition to the works cited in notes 3 and 4, in Robert S. Campbell, 1948, "Milestones in Range Management," JRM, 1:4-8; W.R. Chapline, R.S. Campbell, R. Price, and G. Stewart, 1944, "The History of Western Range Research," Agricultural History, 18:127-143; W.R. Chapline, 1951, Range Management History and Philosophy," JRM, 4:634-638; Idem, 1980, "First Ten Years of the Office of Grazing Studies," Rangelands, 2:223-227; Arthur W. Sampson, 1955, "Where We Have been and Where We Are Going in Range Management," JRM, 8:241-246; Laurence A. Stoddart, 1950, "Range Management," in Robert K. Winters, ed., 1950, Fifty Years of Forestry in the U.S.A. (Washington D.C.,: Society of American Foresters), pp.113-135; and M.W. Talbot and F.P. Cronemiller, 1961, "Some of the Beginnings of Range Management," JRM, 14:95-102.

Chapter 2
RANGE SCIENCE AS SOCIAL POWER: THE ORIGIN OF THE DUAL ORIENTATION

From the first observational studies during the last decade of the nineteenth century, range research attempted to develop solutions for problems that had political, economic, and social causes.[1] Range science evolved out of attempts by researchers to solve 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.[2] Yet rather than attack the causes of the

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[1] Those social, political, and economic factors determined who would use the range and how, and "Scientific expertise is merely the most recent means ..." to determine how the range should be used; George M. Van Dyne, William Burch, Sally K. Fairfax, and William Huey, 1984, "Forage Allocation on Arid and Semiarid Public Grazing Lands: Summary and Recommendations," in National Research Council, National Academy of Sciences, 1984, Developing Strategies for Rangeland Management (Boulder: Westview Press), p.2.

[2] One researcher claims that the range was really never in a very great condition for grazing, even prior to its occupation by stockmen, and hence it had not deteriorated as much as often claimed; see Thomas R. Vale, 1975, "Presettlement Vegetation in the Sagebrush-Grass Area of the Intermountain West," Journal of Range Management, 28:32-36. Nevertheless, by most accounts the ranges in the west and southwest had abundant vegetation, even in desert states like Utah. In Utah, Mormon Church authorities created a history indicating that the area they settled was barren. Soon after the Mormons arrived in the area adjacent to the Great Salt Lake, Church leaders decided to expand, and the areas it wanted to expand into were not quite so hospitable as the areas they settled first. To make the task of settling those areas less daunting, Church leaders rewrote their own history, making it appear that with the help of God the new settlers could duplicate the achievements of the first settlers. There is however no evidence in the journals, etc., of the first settlers to substantiate even a shred of this myth; see

problems facing stockmen due to the range's deterioration, society turned to science. It was hoped that scientific research could provide the knowledge to rationalize and systematize range management practices, and that scientific range management would solve the problems on the range. The turn to scientific research therefore represented a conservative attempt to deal with the problems.

Range science evolved as part of the general movement in America toward increasing organization and rationalization to deal with social, economic, and political problems. Although the problems facing stockmen were in some sense unique to their activity, in other ways they mirrored American society at the time. America experienced some fundamental changes in the period 1870-1920, which coincided with the time the range deteriorated and researchers undertook the first attempts to solve the resulting problems. Many people began to think that the laissez-faire economic and political system had caused and could not solve the problems facing the nation at this time. To solve those problems, rational management became gradually more and more prominent in all facets of American life. That same laissez-faire system lay at the heart of the problems on the range, and the first observational studies of the problems

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Richard H. Jackson, 1970, Myth and Reality: Environmental Perception of the Mormons, 1840-1865, an Historical Geosophy, unpubl. Ph.D. dissertation, Utah State University.

on the range had to confront economic and political issues in their "scientific" analyses of the impact of grazing on the range's vegetation. In short, the dual orientation towards both science and society was already apparent in these observational studies.

Causes of the Problems on the Range

Government land policy was perhaps the primary cause of the destruction of the ranges. Under the Homestead Act of 1862, a settler acquired 160 acres of the public domain for free if he lived on the land five years. The Act made sense for environmental conditions in the east, where 160 acres provided a farmer enough land to earn a living. However, as John Wesley Powell already argued in 1878, in the arid climate of the west and southwest a settler could not earn a living from 160 acres of land. He argued that ranching was the only viable form of agriculture in those areas, and that a ranch had to contain at least 2,560 acres in order for a rancher to earn a living.[3] Congress ignored Powell's arguments until 1916, when it passed the Stockmen's Homestead Act. This Act allowed stockmen to acquire 640 acres for grazing; still a far cry from Powell's recommendations, and still short of the

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[3] John Wesley Powell, 1878, Report on the Lands of the Arid Region of the United States, with a More Detailed Account of the Lands of Utah, 45th Cong, 2nd sess., H. Exec. Doc. 73, pp. 21-29.

amount of land necessary to earn a living.

In addition, the government refused to lease large sections of the land to stockmen or to allow them to acquire the land for free.[4] In a Jeffersonian vein, it hoped that numerous small, independent farmers would settle the land, use it in the most efficient way, provide the backbone for democracy, feed an urbanizing America, and provide materials for export. The government did not intend to keep those lands forever; it merely served as temporary owner until some settler would claim 160 acres of the public land as his own.

As a consequence of government policy, stockmen could not legally acquire or lease the land they needed and the government owned most of the land in the west. To most westerners government ownership meant that no one owned the land. They thought the public domain provided them with a source of free food for their stock. The severe competition that ensued among stockmen for access to the government-owned "free" range was a major cause of the destruction of the range's resources. Whoever got to the land first had the right to use it. That person also had no incentive to

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[4] Texas was an exception. When it entered the Union, Texas acquired the rights to its own unclaimed lands, and it instituted a leasing policy on those lands in 1883. But the policy was not enforced, most stockmen just ignored the law and used the land without paying the fee. In 1885, the federal government reaffirmed its opposition to leasing because it violated traditional policy. See Rowley, 1985, pp. 11-12.

preserve the range because he had no guarantee that next year someone else would not get there first. Since the official goal of government policy had always been private ownership of land in order to ensure the best use of the land, Jonathan R.T. Hughes summarized its effect on the western ranges as "an anomaly in the American national experience: a suboptimal use based upon uncertain tenure."[5]

Stockmen themselves recognized that their behavior was a major cause of range destruction. In 1898, H.L. Bentley, a special agent of the U.S. Department of Agriculture, studied range conditions in Texas. He talked to local stockmen and

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[5] Jonathan R.T. Hughes, foreword for Gary T. Libecap, 1981, Locking Up the Range: Federal Land Controls and Grazing (Cambridge: Ballinger Publishing Co.) p. xv. On the land-holding pattern in the west and southwest, see also e.g. Rowley, 1985, esp. pp. 15-21, and Foss, 1960. Other historians have also argued that land policy did not suit the climate and did not accomplish the government's stated goals; for the classic statements, see Roy M. Robbins, 1942, Our Landed Heritage: The Public Domain, 1776-1936 (Princeton: Princeton University Press), and Paul W. Gates, 1936, "The Homestead Law in an Incongrues Land System," American Historical Review, 41(July):652-681. Gates subsequently revised his interpretation, arguing that the Homestead Law at least enabled many farmers to acquire ownership of their own farms, although he still found many problems with the legislation; see idem, 1968, History of Public Land Law Development (Washington, D.C.: Government Printing Office). Nevertheless, the arguments by Rowley, Foss, and Robbins demonstrate convincingly that the land-holding pattern was the major cause of the problems on the range. Most of these works focus primarily on the nineteenth century, for an analysis of the public domain in the twentieth century, see E. Louise Peffer, 1951, The Closing of the Public Domain: Disposal and Reservation Policies, 1900-50 (Stanford: Stanford University Press).

stated that "there is hardly one cowman out of ten in central Texas but knows that he has more cows on his range already than his best judgment suggest."[6] According to Jared G. Smith, Bentley's associate in this study, stockmen had recognized years earlier that their actions had caused severe damage. In the 1870's, the abundant vegetation on the prairies had led them to believe that the supply of grass was inexhaustible, but they soon learned differently. "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."[7] Stockmen repeated those claims on a survey conducted by David Griffiths in Arizona and on a questionnaire the USDA mailed to stockmen in the southwest in 1904.[8]

Almost all stockmen identified land-tenure as the problem and they wanted the government to take action, in particular by changing its policy. Albert F. Potter, who raised cattle in Arizona from 1880-1900 and later became prominent in the

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[6] H.L. Bentley, 1898, "Cattle Ranges of the Southwest: A History of the Exhaustion of the Pasturage and Suggestions for its Restoration," USDA, Farmers' Bulletin, 72, p. 11.

[7] Jared G. Smith, 1899, "Grazing Problems in the Southwest and How to Meet Them," USDA, Division of Agrostology, Bulletin 16, p. 10.

[8] David Griffiths, 1901, "Range Improvement in Arizona," USDA, Bureau of Plant Industry, Bulletin 4, p. 13; the results of the USDA questionnaire were published in Frederick V. Coville and Albert F. Potter, 1905, "Grazing on the Public Lands: Extracts from the Report of the Public Lands Commission," USDA, Forest Service, Bulletin 62, esp. p. 13.

Forest Service, claimed that stockmen deplored the situation of the range, "but they were victims of circumstance and governmental inaction with no course open to them other than the one they followed."[9] Most stockmen responding to the 1904 USDA questionnaire indicated some form of government action to control access to the public lands was necessary, "with only 64 out of 1,400" wanting to be left alone."[10] Arizona stockman C.H. Bayless thought that depletion of the ranges "can not be prevented on our open range where the land is not subject to private control," and argued that "no practical plan can well be advanced for increasing plant growth on any open range while free for the use of everybody."[11] In the late 1890's the National Livestock Association, one of the largest organizations of stockmen in the country, repeatedly called for some form of government control of the rangelands.[12] Congress nevertheless refused to establish a leasing system because it thought that such a policy might close the lands to later settlers, and the ranchers felt they had to monopolize the range to ensure their competitors did not get their first.

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[9] Albert F. Potter, quoted in Paul H. Roberts, 1963, Hoof Prints on Forest Ranges: The Early Years of National Forest Range Administration (San Antonio: The Naylor Company), p. 46.

[10] Potter and Coville, 1905, 24.

[11] Quoted in Griffiths, 1901, 12-13.

[12] Paul H. Roberts, 1963, Hoof Prints on Forest Ranges: The Early Years of National Forest Range Administration (San Antonio: The Naylor Company), p. 18.

Economic factors increased the pressure on the range. Western economies were based primarily on the exploitation of natural resources.[13] Most western economies were agrarian economies, and due to the region's dry climate, ranching economies. Farmers and ranchers attempted to extract as much from the earth as they could. Ranchers then shipped the products of their labor to the east, to the few major cities on the Pacific coast, or to Europe for processing and conversion into goods people could use. Stockmen could only shear their sheep close to the range, and most cattle and sheep were shipped from the grazing areas to be slaughtered for their meat and skin. These finished products, as well as most consumer products, machinery, and other finished

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[13] For a review of historical analyses making this argument, see William L. Lang, 1983, "Using and Abusing Abundance: The Western Resource Economy and the Environment," in Michael P. Malone, ed., 1983, Historians and the American West Lincoln: University of Nebraska Press), pp.270-299. There are unfortunately no general histories of agriculture in the West, but see Leonard J. Arrington, 1963, The Changing Economic Structure of the Mountain West, 1850-1950, Monograph Series, Utah State University Press, Logan, Utah, vol.10, no.3; Gene M. Gressley, 1966, Bankers and Cattlemen (New York: Knopf). On agriculture in the West, see also the January, 1975, issue of Agricultural History, subtitled "Agriculture in the Development of the Far West," which was entirely devoted to this topic. The exploitation of natural resources remains a cornerstone of Western economies, and Michael P. Malone has proposed this exploitation, the region's aridity, its reliance on the federal government, and the legacy of its frontier experience as the four themes around which to organize Western history; see his 1989, "Beyond the Last Frontier: Toward a New Approach to Western American History," Western Historical Quarterly, 20(4):409-427.

products, had to be imported into the agricultural areas. Yet finished products always cost more than raw materials, thereby encouraging agriculturists to extract more from the land in order to buy those finished products. The nature of the economy therefore reinforced the exploitation of the natural resources.[14]

Social and technological changes in the last two decades of the nineteenth century strained that relationship between the economy and the environment beyond its limits. Changes in demand and distribution due to the urbanization of America and the opportunities for export opened up with the new means of transportation caused rising prices. Between 1878 and 1882 the price per head of cattle increased more than 400%, from

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[14] See Leonard J. Arrington, 1963, pp. 20-21. Some historians have argued that the West was merely a colony, its people and natural resources exploited by capitalists in the East. See Walter Prescott Webb, 1944, Divided We Stand: The Crisis of a Frontier Democracy, rev. ed. (Austin: University of Texas Press); and Bernard DeVoto, 1934, "The West: Plundered Province," Harper's, 169(August):355-364. For an analysis of this interpretation, see William G. Robbins, 1986, "The 'Plundered Province' Thesis and the Recent Historiography of the American West," Pacific Historical Review, 55(November):577-597. Patricia Limerick, 1987, The Legacy of Conquest: The Unbroken Past of the American West (New York: W.W. Norton and Company) proposed a controversial historiographical reorientation for the history of the U.S. West, arguing that Western history is "a study of a place undergoing conquest and never fully escaping its consequences."(p.26) Limerick defined conquest very broadly to cover all types of conquest, including the conquest of nature. Grazing was one from of the conquest of nature, both before and after the federal government became an active manager of that conquest. Conquest therefore might be a general theme for the causes of range deterioration.

about seven dollars per head to thirty-three dollars per head.[15] Rising prices provided an incentive to overstock the range and brought more stockmen to the ranges. In order to cash in on the high prices stockmen placed as many stock on the range as possible and allowed their animals to consume all the forage on the range, leaving no plants to reproduce.[16] As a result, they overgrazed the range and overproduced, causing both the destruction of the range resource and a drop in prices after 1885. The severe blizzards of the winter of 1885-86 caused tremendous losses among the stock, already weakened by the lack of forage on the range, and ruined many stockmen. Prices still continued to drop, but overstocking also continued because the stockmen who had survived attempted to compensate for the falling price per head by increasing the number of cattle they sold.[17]

The sheep industry also grew rapidly in the last two decades of the nineteenth century, increasing the competition

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[15] See Joseph M. Petulla, 1977, American Environmental History: The Exploitation and Conservation of Natural Resources (San Francisco: Boyd and Fraser Publishing Co.), p. 209.

[16] See e.g., Rowley, 1985, 8; Petulla, 1977, 209.

[17] Some economic historians argue the drop in prices was not due to over-production because the return on investments in the livestock industry in the west and southwest continued to grow at a healthy rate, and far exceeded potential returns on alternative investments; see the summary of this research in Susan P. Lee and Peter Passell, 1979, A New Economic View of American History (New York: W.W. Norton & Co.), pp. 318-322.

for access to the range. Sheep had the advantage of producing two salable products, wool and mutton, and by 1890 more sheep than cattle grazed on the ranges. Most cattlemen thought cattle could not graze a range after sheep had used it, and certainly not at the same time. Sheep supposedly poisoned the range with a substance secreted from a gland in their hoofs. The competition got so fierce that sheepmen and cattlemen often killed each other in attempts to monopolize access to the "free" range.[18] The continuing decline in the price of cattle while the price of wool rose, coupled with the realization that sheep survive harsh winters better, finally caused many cattlemen to rethink their arguments and to start raising sheep.

In mountainous states, geographical factors further increased the destruction of range resources due to competition for the "free" resource. Stockmen in those states grazed their stock in the valleys during the winter, and then moved up the mountain as the snow melted and the vegetation became available for grazing. However, the valleys contained much more range land then the mountains. Stockmen overstocked the valleys in order to monopolize access to the resource. That in itself caused the destruction of the valley ranges. Matters got worse when the stock moved up the mountain since

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[18] See e.g., Rowley, 1985, 16-17; Foss, 1960, 33-4; Petulla, 1977, 213.

the mountains produced less forage to begin with. According to Amasa Jay Redd, a brother of Charlie Redd, one of the largest stockmen in Utah and in the nation: "We had a dearth of summer range and an over-supply of winter range. When you get enough cattle and sheep to utilize your winter range, you were overgrazing your summer range."[19] In fact, the mountain ranges were so severely overgrazed and eroded that severe and destructive spring floods caused by melting snow became a fairly regular feature. In Utah, for example, Ephraim Canyon and Manti Canyon flooded numerous times between the late 1880's and 1910, causing severe damage to the farms, ranches, and communities below. In the fall of 1913, the floods still affected the town of Ephraim, and the local newspaper headlined its front-page story "Flood Pays Annual Visit of Destruction."[20]

Finally, religious differences also contributed to the destruction of the ranges, at least in Utah and some of the surrounding states. Mormon stockmen used rangelands outside Utah, and stockmen from other states used those in Utah. Competition between Mormons and Gentiles for the grazing lands

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[19] Amasa Jay Redd, interviewed by Charles S. Peterson on July 27, 1973, Charles Redd Oral History Project sponsored by Charles Redd Center for Western Studies, Brigham Young University, Utah State University, and the Ronald V. Jensen Living Historical Farm; transcript at Utah State University, p. 35.

[20] Cited in Keck, 1972a, p. 2.

caused some of the overgrazing in areas where both groups used the ranges, and thereby reinforced the effects on the range due to the other causes. It is unlikely that something inherent in religion caused such competition. But as the frontier closed, societies like the Mormons that had sought and experienced relative isolation came into contact and competition with other groups. Religion then may have provided one motivating force behind the competition and thereby inadvertently contributed to the destruction of the ranges. Amasa Jay Redd acknowledged that source of the problem in an interview for an oral history project. Charles Peterson, a former employee of the Redd's before becoming a professor of history at Utah State University, asked him the leading question "What if you had a competitive element between the Mormon and the non-Mormon? The tendency is to fill that winter range clear to the hilt, isn't it, so that the other group doesn't take advantage of the vacuum and the mountain is harder hit?" Redd concurred: "That's right. That was always my personal feeling. I have never discussed that angle with anybody but I think it is reasonable."[21] The Mormons won that competition; Peterson ascribes their victory to their lifestyle, which stressed cooperative management of herds and a village society, and consequently the Mormons were

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[21] Amasa Jay Redd, interviewed by Charles S. Peterson on July 27, 1973, p. 35.

better organized than their Gentile competitors and able to keep the Gentiles off the summer ranges.[22]

Among the factors that contributed to destruction of the ranges, those due to the laissez-faire political and economic ideology were most amenable to change, but they also caused most of the damage. Due to this ideology, the government refused to manage the natural resources because it considered such involvement inappropriate, a violation of the country's best interest. Under the influence of that ideology, individual stockmen were motivated to extract all they could from the land, and when prices rose and more people began to raise stock, competition became so severe that stock-raising became a struggle for the survival of the fittest. According to Potter, stockmen thought only of their own economic profit and survival, and "the permanent good of the industry was sacrificed to individual greed."[23] Yet America was changing rapidly during this period. Although some of those changes, such as the pressure to produce for expert, may have

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[22] Charles S. Peterson, 1974, "San Juan in Controversy: American Livestock Frontier vs. Mormon Cattle Pool," in Thomas G. Alexander, ed., 1974, Essays on the American West, 1972-1973, Charles Redd Monographs in Western History, No. 3 (Provo: Brigham Young University Press), pp.45-67. For a more extended treatment of grazing in Utah and the relations between stockmen and government officials, see idem, 1975, Look to the Mountains: Southeastern Utah and the La Sal National Forest (Provo: Brigham Young University Press). No one has as yet examined whether such competition between religious groups also played a role in other states.

[23] Albert F. Potter, quoted in Roberts, 1963, p. 46.

contributed to the destruction of the range in some ways, in significant ways those changes would challenge current practices on the range.

Searching for a New Order

The laissez-faire attitude behind the political and economic causes was being challenged in America as politicians and businessmen assumed a more active role managing economic and political processes. Between 1870 and 1920, government officials and businessmen gradually began to intervene in order to manage more and more processes in society. A number of significant changes contributed to a feeling that the old way of doing things would no longer suffice, including increasing urbanization and its impact on both the cities and the countryside, an influx of immigrants, the impact of industrialization and technology on in particular the production and distribution of goods, the increasing globalization of the economy, and more conscious American participation in world politics.

Many Americans sought a new order. Businessmen began to rationalize their businesses and in the process replaced the invisible hand with a visible hand. The Progressives wanted to manage many aspects of life in America, hoped to accomplish their objectives by developing more rational approaches to society and its problems, and wanted the government to implement those approaches.[24] The federal government became a more active participant in the economy, passing the Interstate Commerce Act in 1887 and the Sherman Anti-Trust Act in 1890. Governments began to regulate the railroads, city governments started to address the problems brought on by increasing urbanization, and eugenics, the management of human reproduction, reached the peak of its popularity.[25] This

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[24] On these changes in America, see Robert H. Wiebe, 1967, The Search for Order, 1877-1920 (New York: Hill and Wang); Sidney Fine, 1956, Laissez Faire and the General Welfare State: A Study of Conflict in American Thought, 1865-1901 (Ann Arbor: University of Michigan Press); Samuel P. Hays, 1957, The Response to Industrialism 1885-1914 (Chicago: University of Chicago Press); James Weinstein, 1968, The Corporate Ideal in the Liberal State 1900-1918 (Boston: Beacon Press); William Appleman Williams, 1973, The Contours of American History (New York: New Viewpoints); Samuel Haber, 1964, Efficiency and Uplift: Scientific Management in the Progressive Era 1890-1920 (Chicago: University of Chicago Press); Richard Hofstadter, 1955, The Age of Reform (New York: Vintage Books); Gabriel Kolko, 1963, The Triumph of Conservatism; A Reinterpretation of American History, 1900-1916 (New York: The Free Press of Glencoe); Lewis L. Gould, 1978, Reform and Regulation: American Politics, 1900-1916 (New York: John Wiley and Sons). For the change in business and economics, see especially Alfred D. Chandler, 1977, The Visible Hand: The Managerial Revolution in American Business (Cambridge: Belknap Press of Harvard University Press); and see Lee and Passell, 1979, esp. chs. 13-15. These changes extended well beyond the Progressive Era, see e.g., Ellis W. Hawley, 1979, The Great War and the Search for a Modern Order: A History of the American People and Their Institutions, 1917-1933 (New York: St. Martin's Press). For a recent interdisciplinary interpretation that may at times be too whiggish and mono-causal for historians' tastes, but is nevertheless fun, interesting, and potentially fruitful, see James R. Beniger, 1986, The Control Revolution: Technological and Economic Origins of the Information Society (Cambridge: Harvard University Press).

[25] On the railroads, see Gabriel Kolko, 1965, Railroads and Regulation, 1877-1916 (Princeton: Princeton University

movement culminated with Taylor's arguments for "scientific management," but Taylor in many ways codified and pushed to an extreme conclusion what had already become standard practice in business and in many government agencies.[26]

Management involved prediction, prediction involved organization, and organization in turn required bureaucracy; it required lines of authority and control over the production, use, and diffusion of information to ensure that every member of the organization performed his assigned task. Alfred Chandler gives us a nice example in his discussion of the railroads. Due to the size of the railroads and the speed of the trains, no single individual could oversee a whole railroad because he lacked the necessary information to predict, which led to inefficiencies, a lack of control over both trains and employees, and sometimes to accidents that the press sensationalized. This forced the railroads to innovate, and they developed the first large scale bureaucracy. In the

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Press); on the management of the cities, see Roy Lubove, 1962, The Progressives and the Slums: Tenement House Reform in New York City, 1890-1917 (Pittsburgh: University of Pittsburgh Press); on eugenics see Donald K. Pickens, 1968, Eugenics and the Progressives (Nashville: Vanderbilt University Press) and Daniel J. Kevles, 1985, In the Name of Eugenics: Genetics and the Uses of Human Heredity (New York: Alfred A. Knopf).

[26] Frederick W. Taylor, 1911, 1967, The Principles of Scientific Management (New York: W.W. Norton & Company). Range researchers and administrators would call for scientific management of the ranges, but there is no evidence of a direct influence from Taylor. However, this idea was certainly not unique to Taylor, although he became in many ways its most recognized spokesman.

1850's at the Erie Railroad, Daniel C. McCallum accomplished that by developing a hierarchical system in which each employee had a specific task, fit in a particular place in the line of authority, and collected, transmitted, and used particular bits of information, depending on his place in the hierarchy. The bureaucracy thereby enabled McCallum to oversee and manage the entire railroad.[27] In an important sense, McCallum transformed the Erie Railroad into a machine, in which every employee performed the function of a particular part and which he controlled the way an engineer managed a steam locomotive. McCallum even drew up one of the first organizational charts to map the flow of information and each employee's place in the hierarchy, the way an engineer might draw a steam engine.[28] McCallum's organizational scheme spread rapidly to other industries and ultimately to the government.

The New Order and Agriculture

The movement toward organization and rationalization spread quickly into agriculture, and Congress established lines of authority by legitimating and institutionalizing the role of scientists as the experts who created new information.

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[27] See Alfred D. Chandler, 1965, "The Railroads: Pioneers in Modern Corporate Management," Business History Review 39: 16-40.

[28] Ibid., p. 30.

Farmers themselves wanted to adapt to a changing world and agriculture had become too important to be left to agriculturalists. Farmers now had to produce for distant, even foreign markets, they had to produce more to feed the cities and to generate a surplus for export, and they were subject to fluctuations in the market in ways they had never experienced. The USDA therefore encouraged farmers to improve the quality of their crops and to diversify.[29] In 1887, the federal government organized agricultural research, and hence agriculture, by institutionalizing it under the Hatch Act, which established and allocated funding for agricultural experiment stations in every state.

The Hatch Act defined the relationship between researchers and agriculturalists. As Alan Marcus shows, the

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[29] This emphasis on diversification may have exacerbated the problems on the range. At least one observer examined conditions on the prairies because he wanted agriculturalists to raise more animals; see Jared G. Smith, 1895, "Forage Conditions of the Prairie Regions," Yearbook of the Department of Agriculture, pp. 309-324. According to Smith, agricultural prophets had long preached that if America wanted to remain competitive in a global economy, its agriculturalists should grow "less corn, wheat, and cotton, and more grass and cattle."(p. 311) Profits would then increase because "if we are to produce a surplus to be sold in foreign markets, it is best to export that surplus in the most condensed and marketable from, as meat and animal products, that people want to buy."(p. 311) I am not aware of any studies that link range destruction to agricultural diversification, and this claim thus remains a hypothesis. On the push toward agricultural diversification, see e.g., A. Hunter Dupree, 1957, Science in the Federal Government: A History of Policies and Activities to 1940 (Cambridge: The Belknap Press of Harvard University Press).

condition of agriculture became a concern to farmers and others after 1870, and the Hatch Act was the outcome of negotiations over solutions to those concerns.[30] Some farmers, whom Marcus calls systematic farmers, saw farmers as businessmen and wanted to systematize farming, which meant modeling farming after business. Others, the scientific agriculturalists, wanted farmers to become scientists on the farm, with recognized status in the scientific community. The Hatch Act favored the systematic farmers' perspective by denying scientific agriculturalists the status of scientists. That status became reserved for agricultural scientists, who emerged as the experts who would draw on their expertise as researchers to tell farmers how to organize and rationalize farming. Congress reinforced the scientists' expertise and authority in 1888 when it created the Office of Experiment Stations in the USDA to oversee the state experiment stations and to facilitate communication of information.[31] The Office of Experiment Stations from its inception defined its role as the manager of agricultural science in order to facilitate the managing and rationalizing of agriculture. In 1914, Congress completed the bureaucratic structure of agricultural research

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[30] Marcus, 1985.

[31] That Office published two bulletins, one designed explicitly for specialists, the <u>Experiment Station Bulletin</u>, and another to communicate the latest information in an abbreviated and hopefully clearer and more practical form to farmers, the <u>Farmers' Bulletin</u>.

when it passed the Smith-Lever Act, which institutionalized the extension system to bring information from the scientists to the farmers.[32]

This legitimation and institutionalization enabled the explosion of knowledge in agricultural science; that is, science's success was not a cause because its successes occurred after institutionalization.[33] Prior to the passage of the Hatch Act, most agricultural scientists, including USDA scientists, examined the viability of foreign seeds in the American environment, tested chemical fertilizers, attempted to control crop diseases, and actively pursued hybridization research. In many ways, agricultural scientists performed the research farmers wanted performed but did not have the time to perform themselves; only institutionalization gave researchers the freedom, and mandate, to perform the scientific research they wanted.[34] To change their status, American researchers often pointed to the successes achieved by European,

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[32] On the development of the extension system, and the reluctance of a number of farmers to accept the expertise of scientists and lines of authority it entailed, see Roy V. Scott, 1970, The Reluctant Farmer: The Rise of Agricultural Extension to 1914 (Urbana: University of Illinois Press)

[33] See e.g. Margaret W. Rossiter, 1979, ""The Organization of the Agricultural Sciences," in Oleson and Voss, eds., 1979, for a summary of six major subfields in agricultural science; and Edward H. Beardsley, 1969, Harry L. Russell and Agricultural Science in Wisconsin (Madison: University of Wisconsin Press) on Harry L. Russell's very successful career in the early part of the twentieth century.

[34] See Rosenberg, 1961, p. 154.

particularly German, agricultural scientists as evidence for the benefits of institutionalized agricultural science. At the time of its institutionalization, however, American researchers could not point to many home grown concrete benefits of scientific research per se. Hence it proved difficult to sell the public on agricultural science, and the successes of early leaders in the field must be attributed to the entrepreneurial and political skills of those men, more so then to something inherent in the science that gave it credibility.[35] Furthermore, although science in general had achieved a certain stature in American society by that time, it is not obvious that this status transferred to agricultural science.[36] In fact, scientists looked down their nose upon agricultural scientists, still regarding them as mere farmers after the Hatch Act freed them to do the research they wanted to do.[37] Finally, even if some of that status transferred, we still need to explain why many farmers and the rest of society willingly invested agricultural science with the

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[35] On this point, see Rosenberg, 1961; and Marcus, 1985. For an interesting variant of my argument, see Peter Dobkin Hall, 1984, "The Social Foundations of Professional Credibility: Linking the Medical Profession to Higher Education in Connecticut and Massachusetts, 1700-1830," in Haskell, ed., 1984, pp.107-141.

[36] On the status of science, see e.g. David A. Hollinger, 1984, "Inquiry and Uplift: Late Nineteenth-Century American Academics and the Moral Efficacy of Scientific Practice," in Haskell, ed., 1984, pp. 142-156.

[37] See Rossiter, 1979, p. 224.

authority to manage agriculture.

Society invested agricultural science with authority because science's ability to predict provided a basis to organize and rationally manage agriculture. Science and society intertwined in the last decades of the nineteenth century because science could provide what the reformers were looking for. Scientists' ability to analyze and then predict could serve as a counter-weight to the problems and worst excesses of a strict laissez-faire ideology without requiring radical changes in politics or the economy. Under a laissez-faire system, every farmer would practice agriculture the way he thought best in order to feed his family and to exploit opportunities in the market. That system functioned relatively well in a society oriented towards local markets, where the farmer could determine for himself what and how much to produce. But agriculture had to adapt to changes in society, the economy, and technology.[38] In the movement toward rational management prevalent in America, many thought information could potentially play the same role in

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[38] Not all farmers wanted to adapt; in fact, the late nineteenth century saw a number of radical agrarian challenges against the American political and economic system, collectively known as Populism. On this history, see Lawrence Goodwyn, 1978, The Populist Moment: A Short History of the Agrarian Revolt in America (New York: Oxford University Press). Paul and Kimmelman, 1988; and Kimmelman, 1987; argue that agricultural science was a way to placate the threat of Populism.

agriculture as in business, where it gave managers the ability to predict.[39] Precedents existed to think agricultural science possessed that potential. The testing of chemical fertilizers, for example, demonstrated science's ability to predict the effect of certain chemicals on the soil and the crops, and that information brought some order to the production, distribution, and use of fertilizers.[40] Extending those precedents by institutionalizing agricultural science would hopefully provide farmers the information to produce more, diversify their crops, and remain competitive in the market.

The New Order and Range Science

Range science evolved as part of this general movement,

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[39] A parallel account may explain the tremendous growth of the social sciences during this same time period. We do not as yet possess many accounts that place this phenomenon in a broad historical perspective, but tantalizing hints can be found in the existing literature indicating that the ability to predict and hence control also played a significant role in their growth. For a good review, see Hamilton Cravens, 1986, "History of the Social Sciences," in Sally Gregory Kohlstedt and Margaret W. Rossiter, eds., Historical Writing on American Science: Perspectives and Prospects (Baltimore: The Johns Hopkins University Press), pp. 183-207.

[40] Marcus, 1985, describes the influence of fertilizer testing on their production and use, but he does not analyze in sufficient depth the role played by this ability to predict in the eventual institutionalization of agricultural science. The Germanic kingdoms, principalities, etc., had been more managed societies and agricultural systems for a longer time, at least partly due to the scarcity of land, and this difference may explain the earlier rise of agricultural science in Germany, and its delay in America.

and its ability to analyze and predict would hopefully provide the information necessary to restore the ranges to a more productive state. No specific precedents existed anywhere to believe a research program such as range science could provide the knowledge that would provide a foundation for the rational management of ranching and thereby prevent the worst problems of a strict laissez-faire system. Range science was an American creation, and it was a product of the dominant movement in turn of the century America. That society placed a high value on the ability to predict, and it extended that value into all realms of life, including ranching, perhaps one of the last bastions of unbridled individualism in America.[41] Range science served as a vehicle that, gradually, brought the movement toward organization and rationalization to the West, in the process transforming both the ranching industry and the range.

The role of range research as a source of information to

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[41] Cowboys, cattlemen, etc., were not as individualistic as they are often portrayed in histories and myths. See Joe B. Frantz and Julian E. Choate, 1955, The American Cowboy: The Myth and the Reality (Norman: University of Oklahoma Press); and Don D. Walker, 1978, "Freedom and Individualism: The Historian's Conception of the Cowboy and the Cattleman," in Thomas G. Alexander, ed., 1978, "Soul-Butter and Hog Wash" and Other Essays on the American West, Charles Redd Monographs in Western History, No. 8 (Provo: Brigham Young University Press), pp.69-91. Numerous works have been written about cowboys and the cattle industry, and a few have been written about the sheep industry; for a review of those works, see Gilbert C. Fite, 1983, "The American West of Farmers and Stockmen," in Malone, 1983, 209-233.

manage the range and thus deal with economic and political issues became apparent in two early observational studies motivated by a controversy over grazing on the newly created Forest Reserves.[42] In 1891, Congress under the General Land Revision Act authorized the President to withdraw land from the unclaimed public domain and set it aside as Forest Reserves. Presidents Harrison and Cleveland used this authority to withdraw thirty-nine million acres. Technically, the Act closed the withdrawn lands to any kind of use. In an important sense, the Act therefore represented a major victory for the burgeoning preservationist movement in America, which wanted the government to take control of, but not manage, land in order to preserve it in its pristine state.[43] However,

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42 Smith, 1895, was a third observational study, but Smith's researches are discussed more fully in the next chapter.

43 Roderick Nash, 1982, Wilderness and the American Mind, 3d ed. (New Haven: Yale University Press), is a good general introduction to environmental history. Environmental historians have shown that the preservation/conservation dichotomy is ambiguous. A historical actor may be a preservationist in one context and a conservationist in another. It has also proven difficult to incorporate scientific ecology and modern environmental movements in this dichotomy. Alternative catagories have been proposed, but none has proven satisfactory. I have chosen to adopt the catagories because the vast majority of applied scientists in this study adopted the utilitarian attitude underlying conservationism, even when they performed ecological research. In fact, the dual orientation of range science is due to the attempt to satisfy the utilitarian concerns of the stockmen and the scientific concerns of ecology and ecologists simultaneously. For a review of the debate over the preservation/conservation dichotomy, see the best available review of environmental history: Richard White, 1985,

many people felt the country needed to increase and manage its wood supply to meet growing demand, and as a result the Reserves were opened almost immediately to forestry.[44] Furthermore, forestry was not the primary use of those Reserves, especially in the West where most were located; grazing was. The question then became whether to allow grazing.[45]

Due to the destruction it caused, many people towards the end of the nineteenth century opposed grazing. They argued it interfered with other uses such as watershed protection and the water supply, and with the preservation of natural resources. In 1893, John Muir, one of the principal spokesmen for the preservationist movement, argued sheep should not be allowed to graze on the newly created Cascade Range Forest Reserves in Oregon. Sheepmen had not seriously opposed the creation of the Reserves, primarily because they believed they would be allowed to use the ranges on the Reserve. Yet when the Reserve was created, the Department of Interior, which governed the Reserves, banned sheep grazing. One Oregon sheepman, John Minto, then began to agitate for a reform in policy that would allow sheep to graze in the Cascades. Minto

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"American Environmental History: The Development of A New Historical Field," Pacific Historical Review, 54:297-335.

[44] See Steen, 1976. Most of the analyses of the management of the West's natural resources focus on forestry; for a review of that literature, see Lang, 1983, pp.272-277.

[45] See Rowley, 1985.

argued that the federal government had no right to control the land, which should instead either be turned over to private citizens or be administered by state or local agencies. By 1896 several stock organizations in Oregon sent petitions to Congress requesting it change the policy.[46]

To acquire the knowledge necessary to determine current and predict future impacts of grazing on the Reserve, the Secretary of Interior in 1896 asked the National Academy of Sciences to investigate range conditions in Oregon. The Academy appointed a committee consisting of the best available scientists, primarily Harvard and Yale professors and curators of museums, and only one forester, Gifford Pinchot. The committee observed range conditions while touring the west. John Muir accompanied the committee when it made observations in Oregon and Washington; it did not, however, consult stockmen, nor determine the economic importance of the sheep industry. Muir's ideas influenced its conclusion that the Reserves should remain closed to sheep grazing. The preservationists had apparently scored a major victory; the government not only withdrew the land, it was also willing to become an active agent in society and control its use. However, the committee's recommendation caused an immediate

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[46] See Rowley, 1985, p. 5; on Minto, see Thomas R. Cox, 1983, "The Conservationist as Reactionary: John Minto and American Forest Policy," Pacific Northwest Quarterly, 74:146-153.

uproar among stockmen in the West, and Minto challenged the committee's conclusion on the basis of his own data.[47]

In response to the stockmen's criticisms, the Secretary of Interior asked the USDA to look into the problem, and in the summer of 1897 USDA botanist Frederick V. Coville went to the Cascades. Coville made extensive observations of the effect of sheep grazing on the Forest and discussed the issue with a number of local stockmen, including Minto. The stockmen had a major influence on the kind of information Coville collected, and they shaped his interpretation of that information. Minto gave him a letter of introduction he could show to Oregon's sheepmen, and Coville stated that this letter "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."[48] Coville argued that the sheep industry was crucial to Oregon's economy, that the Cascade Range provided a crucial supply of forage for the Oregon sheep industry and that sheep could graze the Cascades without causing too much damage, provided stockmen manage them properly. In fact, Coville attributed

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[47] See Cox, 1983. That does not mean that Westerners were uniformly or in principle opposed to conservation; see Lang, 1983, p.276.

[48] Frederick V. Coville, 1898, "Forest Growth and Sheep Grazing in the Cascade mountains of oregon," USDA, Division of Forestry, Bulletin 15, p. 7. Rakestraw, 1958, p. 377, also argues that Coville's report reflected the opinions of Minto and the stockmen rather than those of Muir.

most of the damage in the forest to careless behavior not on the part of sheepmen, but by other users of the forest, including tourists and loggers. The sheepmen, however, were not blameless, and Coville thought some form of regulation was necessary to ensure the protection of the ranges. He proposed a leasing system under which stockmen would receive five-year permits allowing them to graze their sheep.[49] Interior, however, was not very consistent in applying these recommendations. Nevertheless, from that point on, managed, efficient, long-term use of the resources in the Reserves, i.e., conservation rather than preservation, became official government policy.[50]

A dual orientation towards both science and society clearly shaped both these studies. Neither the NAS committee

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[49] See Frederick V. Coville, 1898, pp. 46-54. For a more detailed analysis of the NAS committee's activities, the grazing controversy in the Cascades, and Coville's study, see Rowley, 1985, pp. 25-31; Rakestraw, 1958, pp. 375-6, 380-82; Dupree, 1957, pp. 242-4; and my M.S. thesis, Ch. 1.

[50] On this point, see Samuel P. Hays, 1959, <u>Conservation and the Gospel of Efficiency: The Progressive Conservation Movement, 1890-1920</u> (Cambridge: Harvard University Press); idem, 1987, <u>Beauty, Health, and Permanence: Environmental Politics in the United States, 1955-1985</u> (Cambridge: Cambridge University Press), ch. 10, for an analysis of the politics of conservation science; and J. Leonard Bates, 1957, "Fulfilling American Democracy: The Conservation Movement, 1907-1921,"[check title] <u>Mississippi Valley Historical Review</u> 44(June):29-57. Although Hays argues that scientific management was the driving force behind conservation, while Bates claims that the attempt to instantiate the promise of democracy by improving the administration of the nation's natural resources provided the motivation, both agree that conservation rather than preservation was the goal.

nor Coville could exclude economic and political issues from their observations. Both were asked to provide information that would help determine the kind of control the government should exercise over the Forest Reserves. As such, both studies fit the late-nineteenth century movement to seek information that could serve to organize society, and Coville proposed a change in the political structure to rationalize the use of the Reserves. Furthermore, the way they each oriented themselves toward society had a major impact on the outcome of the studies. They defined their study with respect to various audiences in society, including the Secretary of Interior, and either the preservationists or the stockmen. They allowed those audiences a large influence on their observations, and their conclusions reflected their orientation towards their audiences. As a result, a conflict emerged between the knowledge each produced, and that conflict would not be decided on scientific grounds but on political grounds. These studies also left several gaps. Although Coville argued that sheepmen should manage their sheep in a different way, he did not propose an alternative management system. Researchers would not develop that alternative system until 1907. These researchers also merely observed the conditions on the Reserve and the stockmen's range management practices, not a very secure foundation to build management plans on.

In 1897, the USDA also initiated the first experimental investigations of the range problem. Like the researchers in the observational studies, the researchers performing those experiments were asked to solve political, economic, and social problems. However, in the case of these experimental studies, the dual orientation towards doing science and solving society's problems introduced a tension into the research itself, not just between different researchers.

Chapter 3
DUAL ORIENTATION AND EXPERIMENTATION: THE TENSION BETWEEN SCIENCE AND SOCIETY

In 1897 and 1901, the USDA initiated the first experimental studies of the problems on the ranges, and its researchers faced the problem how to conduct those experiments. No tradition of experimental range research existed. Explorers, missionaries, and military expeditions had collected some range plants. In 1868, Congress created the Division of Botany in the USDA to organize plant collecting. By 1895, conditions on the ranges had become so bad that Congress created the Division of Agrostology in the USDA and appropriated $15,000 to study grasses and other forage plants. In 1897, that Division commissioned Jared G. Smith and H.L. Bentley to conduct a series of experiments in Texas. In 1901, the Bureau of Plant Industry combined the Divisions of Botany and Agrostology into one new Bureau. In the same year, the Bureau of Plant Industry hired botanist Dr. David Griffiths to carry out research in cooperation with the Arizona Agricultural Experiment Station at Arizona's Santa Rita Range Reserve, where the federal government had just created an experimental range. Faced with the difficult task of developing applied scientific experiments without a range research tradition to build on, Bentley and Smith developed one approach to the study of the range, and Griffiths another one.

To reproduce the role of scientist as defined in the USDA, these researchers had to experiment, and to perform experiments, they had to exercise control over their subject matter. Reproducing the social definition of science in the USDA involved experimenting to develop knowledge that could lay the foundation for predictions enabling stockmen to solve and prevent problems on the range. The USDA and stockmen called upon range researchers to develop knowledge that would enable the stockmen to manage the range more rationally, i.e., to exercise more control over their animals and over the vegetation on the ranges. However, to make such control possible, the researchers themselves had to exercise control. To experiment, and thereby satisfy the USDA and the stockmen, the researchers had to manipulate the phenomena on the range. Those manipulations required that the experimenter bound or demarcated and controlled the relevant phenomena in one form or another, although the extent and nature of the control could vary depending on a researcher's goals.[1] This requirement to exercise control over their subject matter caused difficulties for the first experimental investigations

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[1] In a different context, Jane Maienschein has therefore proposed a "continuum of experimentalisms, ranging from utilizing simple manipulative techniques to endorsing fully developed experimental hypothesis testing within a research program." In Jane Maienschein, 1983, "Experimental Biology in Transition: Harrison's Embryology, 1895-1910," <u>Studies in the History of Biology</u>, 6:107-127, on p.108.

into the range.

Range science's dual orientation toward both science and society caused a tension in these early experiments because the necessity to exercise control caused a categorization problem. The interactions between stockmen and the range caused the problems on the range. Bentley, Smith, and Griffiths therefore had to make the difficult determination how they could control those interactions. They somehow had to control nature and society simultaneously to study those interactions. As applied scientists, the problem they faced was how to categorize the range and the stockmen in one category or model, and in such a way that they could develop knowledge the stockmen could use and that satisfied the standards of science. They could not build on a tradition of range research practices because none existed. However, alternative ways to construct catagories existed.[2]

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[2] Categorization is one element of gaining control; through categorization scientists, and humans in general, gain conceptual control over a phenomenon. Control, in general, can be conceptual or material, or both. Conceptual control often precedes material control, but that is not always necessary. Andy Pickering , 1989, "Living in the Material World: On Realism and Experimental Practice," in David Gooding, Trevor Pinch, and Simon Schaffer, eds., 1989, <u>The Uses of Experiment: Studies in the Natural Sciences</u> (Cambridge: Cambridge University Press), pp.275-297, proposed a fruitful framework to analyze experimentation. He argues that in experimental practices three elements become intertwined in the construction of a scientific fact: specific actions upon the world, which he calls a material procedure, an instrumental model that explains how those actions function, and a phenomenal model that comprises a scientist's

They could construct their catagories from either end of a spectrum that had the stockmen and the range as its limits. They could construct a category to capture those interactions between stockmen and the range that had the stockmen's range management practices at its heart. Or they could construct a category that had the range's vegetation at its core. Choosing the first option, they could control the stockmen's practices, and determine their effects on the range to provide a basis for prediction. The problem then became what practices to control and how to control them to develop knowledge about the range and about stockmen's practices the stockmen could use. Under the other option, they could control the range or a section of the range to learn all they could about it, the way a biologist controls a biological organism, and generate predictions from that approach. The problem then became how to control the range to develop knowledge about the range the stockmen could use. Choosing between these options, Bentley, Smith, and Griffiths extended

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conceptual understanding of the phenomenon (s)he is investigating and that attributes meaning to facts. I question whether scientists always require a phenomenal model because perhaps simple manipulations do not require such a model. Goals appear to do, and it is a matter of empirical investigations whether a scientist draws those goals from a phenomenal model. I also prefer category over phenomenal model because it implies a less fully developed image of the phenomenon under investigation, and hence may precede phenomenal models. Finally, Pickering, like many other sociological accounts, also does not consider the role of epistemic values.

research practices in the USDA. However, each extended those practices in such a way that each faced a tension between knowledge about the range and knowledge stockmen could use.

Science in the U.S. Department of Agriculture

Constructing catagories from either end of the spectrum, these researchers reproduced the USDA's definition of research, which attempted to reach two audiences simultaneously, agriculturalists and scientists. The USDA expected its researchers to play a particular role based on a two-dimensional social definition of research. Each of those dimensions contained a distinct set of values. To satisfy one audience, the USDA expected its researchers to develop knowledge that society could use to solve its problems. Researchers would seek local input on the kinds of problems that needed solving, and considered local socio-economic and environmental conditions so the recommendations they made fit the local context. As such, the knowledge reflected the values of society, meaning it should at least be practical, economical, specific to the problem, and almost immediate usable. Yet at the same time, the USDA and its researchers attempted to professionalize agricultural science and hoped to accomplish that by satisfying another audience, other researchers. The USDA therefore required that researchers adhere to the values of science in their research.

Researchers should attempt to develop general, accurate, objective knowledge with explanatory and predictive power.

Ideally, researchers satisfied both audiences at least at some point during their research by integrating the values of both dimensions in their knowledge development practices. Knowledge should be both general and practical. The general knowledge would presumably contain the answer to solve society's problem. Thus, in the ideal situation, the knowledge a researcher developed would simultaneously solve society's problems and contribute to science, thereby reflecting the USDA's dual orientation.[3] The rediscovery of Mendel's laws excited many agricultural researchers, including Liberty Hyde Bailey, S.A Beach, and William Jasper Spillman, because it appeared they could produce both theoretical and practical knowledge by performing genetics research.[4] In addition, the USDA established a central clearinghouse of information in Washington D.C.. Scientists working on a specific problem could draw on that information to benefit from knowledge developed by others.

Bentley, Smith, and Griffiths had to interpret the USDA's definition of research in order to instantiate it in practice. Each interpreted that definition in terms of the context of

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[3] Dupree, 1957, aptly labeled this definition of research the "problem approach," but he does not analyze the impact of this approach on the scientists' practices in any detail.

[4] See Kimmelman, 1987, p.147.

existing research practices in the USDA and in American biology at the turn of the century.[5]

USDA researchers had developed two distinct sets of research practices to instantiate the social definition of research. On the one hand, researchers allowed one audience, agriculturalist, a large role shaping their research practices to ensure their knowledge benefitted society. The agriculturalists specified the problem they needed resolved, the researchers performed the research agriculturalists could not perform themselves, and at times researchers analyzed and compared the various practices of agriculturalists to determine which worked best.[6] Although the Hatch Act in many ways sounded the death bell of this approach, it did not disappear, and it never totally disappeared.[7]

On the other hand, many USDA scientists and

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[5] All definitions, values, and rules must be interpreted to be instantiated, but values can be interpreted in numerous ways that can be justified in terms of further values. Some element of the context and tradition within which a scientist works brings that regress of interpretations to an end, and the values, etc., are applied in the traditional ways in which they have been applied. A dialectical relationship then develops between values and practices: the values enable and constrain practices, and at the same time the practices reflect back on and influence the values. On this point, see Wittgenstein, 1951.

[6] See Rosenberg, 1961; Marcus, 1985.

[7] As agricultural science became institutionalized as a science, this type of research gradually became institutionalized in extension, though agricultural scientists still continued to focus on agricultural problems in their own way (see below).

administrators adhered much closer to the practices of scientists, especially biologists. Many biologists desired to make biology more experimental to make it quantifiable because they believed they could then develop more certain, objective knowledge and generate causal explanations.[8] USDA researchers who adopted this approach talked to farmers, but then generated their own questions for research, guided not by the farmer's problems but by their disciplinary backgrounds. To carry on their research, they drew on their disciplines' methods and concepts, developed new ones if necessary, and used the criteria of their disciplines to evaluate their research. The goal of the scientist, developing new knowledge

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[8] That view motivated Frederick E. Clements, one of plant ecology's founders, and whose theory would later be adopted in range research in a modified form (discussed in ch. 5); for differing accounts of the reason behind that motivation see Ronald C. Tobey, 1981, Saving the Prairies: The Life Cycle of the Founding School of American Plant Ecology (Berkeley: University of California Press); Eugene Cittadino, 1980, "Ecology and the Professionalization of Botany in America, 1890-1905," Studies in the History of Biology, 4:171-198; Joel B. Hagen, 1988, "Organism and Environment: Frederic Clements's Vision of a Unified Physiological Ecology," in Rainger, Benson, and Maienschein, 1988. It also influenced scientists studying evolution and animal breeding; see Kimmelman, 1987; and see William B. Provine, 1971, The Origins of Theoretical Population Genetics (Chicago: University of Chicago Press). The original accounts of this move towards experimentation are found in William Coleman, 1971, Biology in the Nineteenth Century (New York: John Wiley & Sons), and Garland Allen, 1975, Life Science In the Twentieth Century (New York: John Wiley & Sons). Their accounts of the nature and reason of this experimental ideal have recently been challenged in Maienschein, 1983; and in Rainger, Benson, and Maienschein, eds., 1988.

rather than immediately solving the problems facing agriculture, guided this research.[9] Many agricultural scientists hoped thereby both to increase their autonomy and to gain the respect of their peers in the "pure" sciences, who often regarded them as farmers.[10] Harry L. Russell's career at Wisconsin exemplified this attitude. He thought he could best solve the farmers' problems by focussing exclusively on satisfying the audience comprised of other scientists. Russell concentrated on developing new theories without concern for their application (in fact, he hesitated to apply them), and determined the meaning of his contributions with respect to current scientific knowledge rather than with respect to the farmer's problems.[11] He was, nevertheless, very successful. Other researchers and agriculturalists made extensive use of his contributions, demonstrating that his knowledge had meaning at both levels. Yet not all researchers were so fortunate, and the first range researchers struggled

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[9] The chemist W.O. Atwater, the first Director of the Office of Experiment Stations, expressed that view forcefully; see Norwood A. Kerr, 1987, The Legacy: A Centennial History of the State Agricultural Experiment Stations, 1887-1987, (Columbia: University of Missouri, Missouri Agricultural Experiment Station), pp. 38-47. Secretary of Agriculture James Wilson, and his successor D.F. Houston, repeated such arguments on numerous occasions in the Yearbook of the Department of Agriculture; see e.g., their "Reports of the Secretary," in 1901, 1903, 1915.

[10] Rossiter, 1979, p.224, shows that economic entomologists thought other scientists saw them as farmers.

[11] See Beardsley, 1969, pp. 49-82.

with the problem how to reconcile the demands of two audiences within one set of experiments.

Bentley and Smith interpreted that definition of research from the stockmen's end of the spectrum, while Griffiths gave priority to the other values. Bentley and Smith allowed the stockmen a large role in their research. Griffiths, by contrast, adhered almost completely to the scientists' epistemic values in his experiments. As a result, they categorized the problem differently and developed two distinct experimental approaches, each based on a one-dimensional application of the USDA's social definition of research. Each then ran into a problem when they attempted to satisfy the USDA's two-dimensional definition of research in the context of their research.[12]

Range Science as the Stockmen's Problem

Reproducing one type of existing research practices, Bentley and Smith actively solicited the experience and knowledge of the stockmen. Their personal backgrounds may have influenced this orientation towards the experiments they performed. Neither was a professional scientist; all indications are that neither had any formal higher education

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[12] Range scientists reviewing the history of their discipline point to these experiments as foundational; see Talbot and Cronemiller, 1961; Chapline et al, 1944.

beyond the master's level, if that.[13] Smith was an assistant agrostologist and had performed research for the USDA prior to 1897, including an observational study of range conditions.[14] Bentley was a resident of Texas, and had not performed any research for the USDA. In 1897, the USDA commissioned Bentley as special agent to study range conditions in Texas and collect specimens of the local vegetation. On the basis of Bentley's observations, the USDA sent Smith to Texas to set up an experiment in range improvement, and it retained Bentley's commission as special agent, placing him in charge of the experiments. Smith returned to Washington after designing the experiment.

They designed the first experiment in range improvement. According to agrostologist F. Lamson-Scribner in his preface to the paper Smith published on the experiments, "This work in range improvement is the first that has been tried either by

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[13] Unfortunately, not much more is known about these researchers. Both researchers were born in farming areas; see Lawrence A. Stoddart, 1950. It is very possible that they therefore were inclined to begin their studies by incorporating the stockmen. There is also some confusion concerning Smith's status. In 1899, Agrostologist F. Lamson-Scribner identified Smith as Mr. Smith, while identifying another researcher as Prof. C.C. Georgeson (in Smith, 1899, p.4); but in 1902 Bentley identified Smith as both Mr. Smith and Prof. Smith (in Bentley, 1902, p.7 and p.8). It is therefore possible that Smith earned his doctorate by 1902; I decided not to pursue this question because it would have taken me to far from my analysis.

[14] Smith, 1895.

the Government or by any State experiment station."[15] Nevertheless, their research, especially the paper Smith published, contained many elements that would subsequently play a large role in range research. Smith pointed to specific problems, and some possible solutions, that subsequent researchers returned to. He also developed some of the first concepts and methods that would become a part of range science and range management.

Bentley began his 1897 observations on range conditions in Texas by talking to stockmen. He sought the stockmen's opinions on the causes of the damage to the range. The stockmen reported that human activity was the major cause responsible for the depletion of the range.[16] He also solicited suggestions on range improvement: "In considering the question of how the ranges may be renewed, the ideas and opinions of the leading stockmen have been solicited."[17] 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.[18] When Smith arrived in Texas in 1898, he also sought the stockmen's opinions "as to methods of restoring, renewing, and improving the ranges where

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[15] F. Lamson Scribner, in Smith, 1899, p.4.
[16] See H.L. Bentley, 1898, 11; and H.L. Bentley, 1902, 16.
[17] Bentley, 1898, 15.
[18] See ibid., 16-21.

they had been overgrazed."[19]

Bentley's discussions with stockmen raised several questions that would provide the goals for research. 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."[20] The researchers tested the stockmen's suggestions. Those suggestions also raised further 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."[21] Bentley followed that suggestion with an analysis of the results achieved by a local resident who planted non-native seeds. Those plants died when conditions became harsh because they were not well-adapted to the local environment. He concluded that "here was an object lesson which emphasized the suggestion that native grasses are by far the best for home

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[19] Smith, 1899, 10-11.
[20] Bentley, 1898, 20.
[21] Ibid..

use; they are suited for the climate and the climate is suited to them."[22]

As a result of Bentley's contacts with the stockmen, Bentley and Smith categorized range management in terms of a practical problem facing stockmen. That is, they oriented their research specifically towards one audience, the stockmen, to the exclusion of the other audience. Smith made that point explicitly: "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."[23] Underlying this categorization in terms of a practical problem, and the experimental practices that followed from it, lay the idea that nature is basically a machine, and that stockmen could manipulate the machine in more systematic so it would produce what stockmen desired. Most of the experiments therefore focussed on manipulating nature more systematically so it would produce more efficiently. Smith again made that point explicitly: "The most obvious methods to bring about the desired improvement are either resting for several seasons to enable the grasses to retake the land ... or cultivating the surface of the

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[22] Ibid., 21.
[23] Smith, 1899, 21.

pasture in order to accelerate the gradual natural processes."[24] Total rest was impractical because the stockmen needed the land to raise their stock.

That categorization, in turn, enabled and constrained their experimental practices, both Smith's design and Bentley's execution of the experiments. They submitted the stockmen's suggestions on range improvement to experimental testing. The stockmen, therefore, directly influenced the experimental process because they determined the questions that Bentley and Smith examined experimentally. In that way, the researchers hoped to determine those suggestions based on the stockmen's knowledge and experience that had the most merit.

That categorization also determined their choice of experimental "instruments," how they manipulated the "instruments," the data they collected, and how they interpreted that data. The USDA leased land in Texas on which to carry out the experiments, making sure it was among the poorest land around so the results would really convince the stockmen.[25] The researchers divided that land into sections, and treated each section in a different way to analyze the results of the stockmen's recommendations. Three sections were treated either with a disk harrow or a straight-

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[24] Ibid., p.19.
[25] Bentley, 1902, 12.

toothed iron harrow, and hence the "instruments" to manipulate nature were all pieces of farm machinery familiar to ranchers. On other sections, they allowed the stock either to graze until the first of June, and then moved the animals to another range, or they excluded the stock from a section until June first. They thought they could thereby compare the effect of resting the range early in the season versus resting it later in the season. They excluded stock from another section of the range, and used the last section to experiment with artificial reseeding.[26]

In addition, Bentley and Smith adopted the stockmen's method for determining grazing capacity. Stockmen determined grazing capacity in terms of the gains of stock animals on the range and the condition of the vegetation (of the stand). If, for example, fifty cattle grazed a certain section of range for a month, and the cattle made good weight gains while the range appeared to have handled the grazing fairly well (i.e., the stock did not damage the range too severely), then that was its grazing capacity for cattle. To determine the grazing capacity of an unknown section of range, stockmen compared the unknown section to a range of known grazing capacity. The stockmen's method was very subjective because it depended on an individual stockman's ability and experience. Its

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

subjectivity, however, did not deter the researchers.

Furthermore, a committee of stockmen, not the researchers, evaluated the results of the experiment. That committee consisted of three prominent ranchers from Taylor County, Texas: C.W. Middleton, J.W. Parramore, and W.J. Bryan.[27] 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. 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 interested 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 of stockmen.

The stockmen's criteria for success therefore provided

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[27] Bentley, 1902, 15. One of the standard criticisms in the historical literature on science in the USDA and the experiment stations is that it primarily helped the richer farmers and ranchers; see, for example, Rosenberg (1961) or Rossiter (1979). In this case, it is certainly possible that the scientists built that bias towards the rich into their science by giving these particular stockmen such an influential role. Unfortunately, Bentley and Smith do not provide more information about these stockmen.

the background to interpret the results of the experiment and their meaning for managing the range, even for a theory Bentley proposed. Due to their self-interest, the stockmen wanted practical knowledge; that is, immediately utilizable and economically beneficial knowledge. At the end of the first year of experimentation, Smith reported that in the stockmen's judgment treating the land with a disk harrow 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 briefly discussed other aids to range improvement, such as the procurement of sufficient feed for winter so the stock do not have to be grazed on the range too early.[28]

Bentley continued the experiments in the same vein, and in 1902 articulated what he called a theory to provide an explanation for the experiments on the harrowing of the soil. His theory, thus, was not just a theory about plant growth, as it might be in a science like botany. Instead, as an applied scientist he articulated a different kind of theory to explain the ways human manipulations affect plant growth. He thought that harrowing would encourage plant growth because the soil would be looser and softer, because the water would penetrate

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[28] Smith, 1899, 21-27.

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. But whatever the source of the idea, the stockmen again validated the experiment designed to test it and declared the test a success, thereby validating the theory.[29]

As a result of this research strategy the stockmen applied the knowledge immediately; i.e., the community that Bentley and Smith had chosen as their audience validated the knowledge. Bentley reported in 1902 that the experimental results had "satisfied many stockmen and farmers that, at comparatively small expense, they could greatly improve their ranges."[30] That result was really not so surprising because the stockmen had shaped every facet of the researchers' experimental practices. The interactions between scientists and stockmen had determined the categorization of the problem, the questions to be investigated, the "instruments" (i.e., farm tools) used in the experiment, the way those instruments were used to generate data, and the interpretation of that data. However, in 1898 Bentley reported a lack of interest among stockmen in scientific studies on range management.[31]

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[29] Bentley, 1902, p.18.
[30] Ibid., 26.
[31] Bentley, 1898, 12-13.

Stockmen, furthermore, often saw government researchers as interfering with their freedom.[32] The inclusion of the stockmen in the research apparently convinced them of the results because they could see them with their own eyes. All research has to start somewhere, and by testing the stockmen's "rule-of-thumb" knowledge of range management, the researchers provided the stockmen with usable knowledge and overcame some of the hostility to research.

Yet there was also a drawback to this strategy because a method to reseed and improve the ranges was ignored. In 1895, Smith proposed a method to manage stock on the basis of plant life-histories. He realized that due to overgrazing many of the native grasses had been replaced by weeds. He used the term "succession" to characterize that replacement, although not in the theoretical sense it later acquired in plant ecology.[33] He argued succession occurred because those grasses "are neither reseeded by the farmer nor allowed to reseed themselves."[34] Overstocking the ranges caused the animals to consume all the grasses before any had a chance to produce seed, "a condition necessary if the continued existence of any species is to be maintained."[35] He

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[32] See Talbot and Cronemiller, 1961, p.97.

[33] Smith, 1895, p.312. On the "theory of succession" in plant ecology, see Cittadino, 1980; Tobey, 1981; and Hagen, 1988.

[34] Ibid., pp.318-319.

[35] Ibid., p.323.

therefore argued that stockmen should divide their land into a number of sections. Grazing would begin each year on a different section, and the stock should rotate among the various sections during the course of the season. He linked the rotation pattern to the life histories of the various species on the range, arguing stockmen should rotate their stock so they could take advantage of the fact that "there is a constant succession of species that ripen their seed from June until October."[36] That way grazing would be deferred on most of the sections until after the forage had a chance to mature and reproduce. Range scientists and managers later reintroduced this system and called it either "rotation grazing" or "deferred-rotation grazing," depending on the way it was implemented, but Smith did not use those terms.[37]

In 1899, Smith strengthened his arguments for his stock management method by pointing out how it benefitted stockmen, and set up an experiment to test it. He compared his method to the method most stockmen advocated, who thought the range needed complete rest to revegetate itself, probably for a few years. That meant the stock could not use the range, and Smith argued that his method would prove more efficient because the stockmen's method was "not the most rapid method,

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[36] Ibid.
[37] The idea of rotation itself was nothing new in agriculture.

nor can it be considered the cheapest."[38] He therefore concluded that his system was the best way to manage the range: "In this way the succession of grasses which normally occurs in nature can be fostered and improved."[39] He then designed an experiment to test his system, setting aside two of the sections of range two carry it out, and rotating the stock between those sections every two weeks.

Bentley performed the experiment for three years, and the tension became apparent in the evaluation of its results. Smith, in 1899, reported that a few stockmen used the method, and according to their criteria achieved great success with it, but he did not say what the official committee of stockmen thought of the results.[40] In 1902, Bentley reported on the results of each of the three years of experimentation, and concluded every time that the method worked very well. At the beginning of the experiment, 16 acres of range supported one cow, or 40 cattle on the whole section allotted to the experiment. The committee of stockmen had recommended that as the rate of stocking. After three years of experimentation that figure had increased dramatically. "During the third year of station work from 80 to 85 head of mixed cattle were held in the pastures, in the proportion of about 1 head to

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[38] Smith, 1899, 21.
[39] Ibid., 22.
[40] Ibid., p.22.

every 7 1/2 acres."[41] Horses and mules had even shared the pastures with the cattle, thus increasing the actual carrying capacity even more. Bentley attributed the success to rotation grazing. "As one result of this systematic resting, the grasses in each pasture were, to a greater or less extent, permitted to mature seeds, which, falling to the ground, increased the number of grass roots and in that way added materially to the capacity of the range for supporting stock."[42] He expected that in six years a system of rotation grazing would return the range to its original capacity, when 3 or 4 acres could support one cow.

However, whereas stockmen evaluated all the other experiments, he never mentioned that they also evaluated this experiment. He was very careful to say explicitly that the committee of stockmen declared the other experiments a success, probably to enhance the authority of the experiments. What better way, after all, to convince stockmen to use the results of research then by showing that in their own judgment the experiments succeeded, and that rhetorical and experimental strategy apparently worked because stockmen adopted those methods.[43] It is strange, therefore, that he

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[41] Bentley, 1902, p.34; see also pp.19, 26, 35.
[42] Ibid.
[43] For other arguments that experimentation and the reporting of experiments function in part to convince and create authority see Simon Schaffer, 1989, "Glass Works: Newton's Prisms and the Uses of Experiment," in Gooding,

did not adopt the same strategy when he reported on these experiments. Hence either the stockmen did not evaluate these experiments, or Bentley did not report what they thought, in which case they probably did not like this method for if they had he probably would have said so. It is unfortunately impossible to determine from the available records why Bentley did not adopt the same strategy for this experiment, but the impact of the experiment is clear.

Smith's method did not fit the categorization of range management as a practical problem facing stockmen, and most of them ignored it. Bentley and Smith's strategy of including the stockmen in their experiments locked them into one knowledge-development context. They inadvertently erected a barrier against Smith's grazing method by categorizing range improvement as a practical problem facing stockmen. Although Smith said some stockmen adopted it, this method was not widely adopted. That method did not fit the stockmen's existing range management practices. Stockmen would just take their cattle to a section of range and leave them there to fend for themselves. Under rotation grazing, the stockmen would have to alter their practices radically; they would actually have to manage their stock. In addition, rotation

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Pinch, and Schaffer, 1989, pp.67-104; and Geoffrey Cantor, 1989, "The Rhetoric of Experiment," in ibid., pp.159-180. That is not all experiments do, of course.

grazing required a botanist's knowledge of plant life-histories, and the majority of the experiments were based on the stockmen's knowledge. Furthermore, they gave the stockmen discretionary power over the experiments, and the majority of stockmen, by not adopting it, rejected rotation grazing.[44]

For similar reasons, most other researchers also ignored this grazing method and the fact that Smith proposed it and Bentley tested it. They believed that Bentley and Smith's study was not quite scientific, perhaps partly due to the influential role of the stockmen. In their review of the history of range research, W.R. Chapline, R.S. Campbell, R. Price, and G. Stewart, all members of the next generation of researchers, classified Bentley and Smith's studies under a section they called "The Exploratory Period Prior to 1905;" while they labeled the next two categories "Limited Intensive Studies, 1905-1909," and "Organized Experiments Throughout the Mountainous West and Great Plains, 1910-1927."[45] They did not acknowledge that Smith proposed this system, and credited the next generation with the insight that grazing systems should be based on the growth requirements of the vegetation.[46] The subjectivity at the heart of the

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[44] Pointing to the creation of that barrier and the stockmen's discretionary power, I am thinking in terms of Lukes', 1974, first and second dimension of power.

[45] Chapline et al, 1944.

[46] Ibid., pp.129-131. One other researcher, in addition to Smith, in 1902 actually also realized the need to allow the

experiment perhaps also influenced that reaction. Smith and Bentley did not systematically interfere in the life histories of plants to determine the effect of grazing on growth requirements, as a researcher could do by, for example, grazing the plants at various stages of their growth. Instead, they rotated the stock every two weeks, merely observed the resulting range condition, and adopted the stockmen's method to determine the effect on grazing capacity.

Only one researcher cited Smith and Bentley's argument for rotation grazing, and he also performed an observational study. Between 1901 and 1904, J.S. Cotton observed the various grazing systems stockmen used in Washington. His arguments showed clearly that the system had not spread. On the basis of his comparisons, Cotton argued that more stockmen should follow the lead of the few who used this system.[47] Nevertheless, none of the researchers who performed more rigorous and hence presumably more objective experiments build on or credited Smith and Bentley.

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vegetation to reproduce, but did not cite Smith or recognize that Smith had proposed a grazing system to accomplish that goal; see Joseph B. Davy, 1902, "Stock Ranges of Northwestern California: Notes on the Grasses and Forage Plants and Range Conditions," USDA, Bureau of Plant Industry, Bulletin 12.

[47] J.S. Cotton, 1905, "Range management in the State of Washington," USDA, Bureau of Plant Industry, Bulletin 75, esp. pp.14, 23-24. Chapline et al, 1944, also classify this study under "The Exploratory Period Prior to 1905."

In their attempt to satisfy the dual orientation towards science and society simultaneously, Bentley and Smith inadvertently introduced a tension into their research they could not overcome. They successfully developed knowledge for society by including the stockmen. However, their decision to include the stockmen also constrained their practices because their attempt to build on the knowledge of scientists failed. They could perform the experiment, but in that context they could not establish the meaning of the knowledge it produced. The question of the validity of the experiment therefore never arose outside the experiment. Smith and Bentley thought it valid from a scientific point of view, and from an applied scientific point of view. They based their system on the knowledge of a scientist and the experiment corroborated that knowledge, thus satisfying one audience, and they demonstrated that it would enable stockmen to increase their productivity, thus satisfying the other audience of applied science. Nevertheless, the question of validity never arose for the audience they had set up to determine the meaning of the knowledge. It also never emerged within the context of the categorization of range management as a practical problem facing stockmen. Although they argued the system produced economic benefits for the stockmen, they never attempted to determine the practicality of the system for stockmen who grazed their animals on the range.

Range Science as the Scientist's Problem

In another set of experiments, begun in Arizona in 1901, Dr. David Griffiths also introduced that tension, but in a very different way. 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 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 Rita Range Reserve.

As the first researcher with a doctorate to examine the problems on the range, it is not surprising that he gravitated toward the other dimension of the social definition of research in the USDA. That dimension required that scientists develop general, accurate and objective knowledge by means of controlled experimentation. His education undoubtedly inclined him towards accepting those epistemic values. The problems facing researchers also seemed ideally suited to such and approach. In the preface to one of the papers Griffiths published, USDA agrostologist W.J. Spillman argued that range management 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."[48] The subjectivity in the method to determine grazing capacity that Bentley and Smith adopted from the stockmen became problematic. As part of his effort to base range management on a more scientific foundation, Griffiths attempted to develop an objective method to determine grazing capacity. Furthermore, obviously Spillman, nor Griffiths, recognized that with rotation grazing Smith already proposed a method to use the range without damaging it, at least a method worth testing.

Griffiths attempted to develop a quantitative method that could serve as a more objective indicator of grazing capacity. With quantification he thought he gained objective insights into nature. In that respect Griffiths was a typical member of the community and culture of science at the turn of the century. Griffiths nevertheless, like Bentley and Smith, talked with stockmen (and mailed out a questionnaire) to understand Arizona's range problem, but only to understand local problems in order to develop background knowledge for his experiments. Griffiths, however, did not include the stockmen in the actual research. He did not seek their input when planning and performing experiments, or when developing

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[48] W.J. Spillman, in the preface to Griffiths, 1904, 5. He also outlined more minor problems: proper number of stock and season of grazing, conservation of native plants, and introduction of other suitable plants. It appears he assumed that the first two of those would be resolved if researchers solved the major problems.

ways to apply the knowledge.

Griffiths used a new method to determine grazing capacity, hoping thereby to both remove some of the subjectivity and adhere to the USDA standard for research. R.H. Forbes, the Director of the Arizona Experiment Station suggested that researchers "ascertain the precise amount of feed" in the various plants that comprised the vegetation on the range.[49] To accomplish that goal, Griffiths pulled the plants out of the ground, clipped their roots, then dried and weighed them, and in a 1904 paper summarized the dry-weights in tables.[50] In 1901, he performed that procedure on eighteen tracts of range, each measuring fifteen by three feet. As he continued to use the method, he performed used plots three by seven feet. He did not indicate his reasons for reducing the size of the area, but probably changed it to make the work less labor intensive and time consuming. In any case, it showed clearly that range scientists had yet to develop standardized experimental procedures.[51]

Griffiths, however, never clearly categorized the problem of determining grazing capacity from the perspective of humans

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[49] Griffiths, 1901, p.15.

[50] Griffiths, 1904, pp.26-30.

[51] For a complete description of this method, see Griffiths, 1901, 15-6; and Griffiths, 1904, 24-32. This method shared many similarities with the "quadrat method" developed by the plant ecologists Frederic E. Clements and Roscoe Pound between 1892 and 1898, but he did not copy it form them.

interacting with the range. He categorized the range as a biological entity in and of itself, treated it as a machine, and measured its maximum output without considering anything other than the range. Griffiths thus adopted a totally one-dimensional approach to measuring grazing capacity. He categorized range management as a strictly scientific problem; i.e., he assumed that by developing knowledge about the range that satisfied the norms of science he would provide the foundation for range management. He thus clearly worked within the scientific dimension of the USDA's social definition of research that insisted on quantitative knowledge, but he worked only within that dimension; quantitative knowledge became a goal in itself. Furthermore, he never clearly articulated who would use the knowledge he generated with his experiment. In contrast to Bentley and Smith, he did not indicate who the audience for his research was supposed to be.

Consequently, although he introduced a new method, he never developed a clear understanding of the ways in which that method functioned from the perspective of humans interacting with the range.[52] In 1901, he already said the method provided an "estimate," which he "believed to be approximately accurate" because it "was made from actual

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[52] That is, he never developed what Pickering, 1989, calls an instrumental model.

measurements" and he therefore thought his data useful to "compare the relative productivity" of the different areas he had measured.[53] By 1904, he recognized more severe methodological limitations. First, stock consumed all of some plant species whenever they had a chance, but utilized others only if forced to. Griffiths, however, clipped and weighed all species, and included all in his initial calculation. Second, the stock could never graze the plants as closely as the scientists had clipped them and hence the weight did not reflect the amount of available feed. 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.[54] Nevertheless, Griffiths still 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 because the amount of forage available provided "an estimate of the amount of stock that can be carried upon these lands."[55] Yet the methodological limitations he recognized made it doubtful whether he had measured productivity in terms

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[53] Griffiths, 1901, p.15.

[54] Griffiths, 1904, pp. 24, 31. 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, p. 6.

[55] Griffiths, 1904, p. 24.

of humans interacting with the range, a requirement to determine grazing capacity.

Griffiths recognized he needed to subtract a certain amount of forage from the dry-weight measure get an indication of grazing capacity. He briefly analyzed the deductions he needed to make the measure more realistic, basing those deductions on his knowledge about plants, his 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 did not indicate the basis for those opinions. At this point, he also introduced the stockmen's knowledge to help him deduct the amount of non-forage plants contained in the weight.[56] Ironically, the stockmen's subjective knowledge enabled Griffiths to develop quantitative knowledge and thereby enabled him to adhere to the epistemic values of the USDA's definition of research. Following these deductions, he arrived at the conclusion that each cow needed fifty acres per year to live off, or it would damage the range and face a food shortage.[57]

Nevertheless, he still did not have a background within which to interpret the meaning of his results from the perspective of humans interacting with the range. Griffiths was never sure what his method or the quantitative data

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[56] Ibid., 25.
[57] Ibid., pp. 26-32.

derived from it meant for the actual management of the range. Recognizing those problems, Griffiths did not use his experimentally developed quantitative results to prescribe range management practices. Instead, to prescribe range management practices, and to corroborate his method, he appealed to the stockmen's opinion on the current grazing capacity of Arizona's ranges, even though he had held that opinion to be invalid because subjective.[58] He pointed out that his figure came very close to the fifty-acre estimate most farmers and ranchers used.[59] The results of the method after the deductions basically provided a base to compare systematic clipping with the actual management practices of stockmen. 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."[60]

Furthermore, probably because of those problems, in 1904 Griffiths did not use his dry-weight method to generate data in his experiments on reseeding the range or in the

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

[59] He derived that fifty-acre estimate from another source of quantitative information, 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.

[60] Ibid., 34.

experiments on the value of the various methods stockmen used to cultivate the range.[61] He interpreted those experiments in terms of the condition of the stand. That was a measure the stockmen had already used for a long time as a supplement to the condition of their stock, although perhaps as a botanist he possessed a better basis for his judgments.

The problems inherent in Griffiths' method probably occurred every time a new method has been introduced into science or applied science, and subsequent researchers eliminated most of them. By using the method, researchers gained a clearer understanding of the way it functioned, and then made the appropriate changes. They analyzed 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.

Curiously, neither Griffiths nor any of the other Arizona researchers tried to change the method. Griffiths never made the required changes in the way he clipped the vegetation. In 1910, he used basically the same method again to calculate the yield of the vegetation. He made some changes in the method,

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[61] He continued the reseeding experiment, and did not use this method in a paper he published in 1907 either; see David Griffiths, 1907, "The Reseeding of Depleted Range and Native Pastures," USDA, Bureau of Plant Industry, Bulletin 117.

but they were not important, and he therefore did not describe them. "Inasmuch as these are more or less estimates anyway," he said, "it is not important that in this paper a slightly different method of computation is used."[62] He thought the changes were not important because "The comparisons are not altered."[63] Those comparisons involved comparing the relative productivity of different areas and comparing the results of the clipping experiment with grazing. But instead of changing his method to make it more accurate, the subjective elements remained as an appendage. Furthermore, he did not use the method to gain a single, objective indicator of grazing capacity as he initially intended, relying instead on comparisons. That introduced another subjective element, because the researchers had to assume that the two sections under comparison were similar enough for the comparison to be valid. In 1916, E.O. Wooton used this method to calculate the carrying capacity of southern Arizona's ranges, again without making any changes.[64]

Griffiths and Wooton indicated through their actions they accepted the method as producing valid knowledge about the range. But that should not obscure the difficulty Griffiths

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[62] David Griffiths, 1910, "A Protected Stock Range in Arizona," USDA, Bureau of Plant Industry, Bulletin 177, p.19.
[63] Ibid.
[64] E.O. Wooton, 1916, "Carrying Capacity of Grazing Ranges in Southern Arizona," USDA, Bulletin 367.

faced satisfying the dual orientation as a result of categorizing range management as a problem he could solve through strict adherence to the epistemic values of science. Their actions also demonstrated they thought the method had meaning for range management, at least for researchers interested in building a foundation for range management, because they continued to use it in their research. However, J.J. Thornber, who continued some of Griffiths' studies, did not adopt this method. He determined carrying capacity in terms of the condition of the vegetation.[65] In 1909, James T. Jardine, while performing range research in Oregon for the Forest Service, analyzed the difficulties involved in comparing the grazing capacities of different ranges. He did not appeal to Griffiths' method to solve the problems.[66] Apparently, therefore, among an audience of other researchers there was some doubt about either the epistemic validity of this method or the meaning of this method from a practical, range management point of view. Unfortunately neither Thornber nor Jardine said why he did not adopt this method. Nevertheless, the core of Griffiths' method, clipping and weighing the vegetation, remained a part of range science, and over time other researchers gradually worked out the various

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[65] J.J. Thornber, 1910, "The Grazing Ranges of Arizona," Arizona Agricultural Experiment Station, Bulletin 65.

[66] James T. Jardine, 1909, "Coyote-Proof Pasture Experiment, 1908," USDA, Forest Service, Circular 160, p.30.

problems he identified.[67]

The tension thus emerged within individual researcher's knowledge development practices, although it was due to the dual orientation the USDA expected its researchers to satisfy. The USDA did not perceive that its social definition of research could introduce such a tension. Agricultural scientists attempting to professionalize agricultural science also apparently did not perceive the tension. The tension, in short, did not appear at the institutional level. Nevertheless, the tension appeared in practice, when individual researchers attempted to instantiate their institution's ideals. A change in the institutional context of range research would mitigate the impact of the tension on knowledge development practices.

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[67] See, e.g., the description of the clipping method in 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 890), pp.58-59.

Chapter 4
SOCIAL POWER AND RANGE SCIENCE: RESOLUTION OF THE TENSION

In 1907, the Forest Service institutionalized range science and linked research directly with its management of the National Forests. Prior to that time the General Land Office in the Department of Interior managed the Forests. The USDA's Division of Forestry performed some range research, including Frederick V. Coville's observations of grazing in the Cascade Mountains of Oregon. Coville concluded that stock could graze the forests' ranges provided they were managed properly, but never specified what proper management entailed. He did not make many specific range management recommendations, but suggested a number of administrative changes, especially regarding leases of Forest land.[1] In the years between Coville's study and 1905, the tasks of Forestry grew in number and importance, and its status was elevated to a Bureau of the USDA. However, the USDA could only give advice, and implementation of those suggestions depended on Interior.[2] In 1905, Congress passed a bill transferring administration of the Forest Reserves to the USDA, the Bureau

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[1] See Coville, 1898.

[2] For example, in 1898 Interior implemented most of Coville's recommendations, but in 1899 it changed its policy and excluded grazing. A few years later it changed policies and implemented most of Coville's recommendations, but then only on selected forests. On the relations between the USDA and Interior, see Rowley, 1985, ch.2; Steen, 1976, p.56ff.

of Forestry became the USDA Forest Service, responsible for both research and administration.

Range Science as the Forest Service's Problem

Forest Service policy favored use of the Reserves, provided users adhere to the regulations set by its experts. Congress transferred the Forests to the USDA partly as recognition that the resources in the forests are crops. The first Forester, Gifford Pinchot, changed the name from Forest Reserves to National Forests because he believed the term "reserves" implied stockmen, lumbermen, etc., could not use the forest resources.[3] Conservation meant use while improving or maintaining the forest resources, and Pinchot argued grazing should not be eliminated but regulated for the greater benefit of society and to ensure the long-term availability of the resources.[4] On the day of the transfer of the Forests, Secretary of Agriculture Wilson send a letter to Pinchot, probably written by Pinchot himself, outlining the basic management objectives in the Forests. The letter stated that society could use the resources in the Forests "under such restrictions only as will insure the permanence of these resources."[5] As part of those restrictions Pinchot in 1905

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[3] Steen, 1976, p.75.
[4] See Steen, 1976, 66 and 75.
[5] Quoted in Darrell H. Smith, 1930, The Forest Service: Its History, Activities and Organization Brookings Institution

instituted a permit system to exercise strict control over grazing, which he felt was the only way grazing could occur.[6]

As a result of the link between research and management, Forest Service administrators and researchers categorized range management primarily as a practical problem facing experts, the Forest Service. Human interactions with the range caused the depletion of the range, which in turn had severe social consequences. The Forest Service and its researchers saw the range and the stockmen as one machine that was not operating smoothly and efficiently. It set managing that machine as its administrative goal, and its researchers provided the knowledge necessary for management. The range, as one component of that machine, was seen as a producer of resources for human consumption. The stockmen, on the other hand, were seen as economic agents interested in maximizing the range's production so they could increase their production of meat, wool, etc., and thus increase their profits. The Forest Service set as its goal to maximize the range's production in order to maximize the economic returns to stockmen in such a way that the stockmen would not destroy the range in the process.

Monograph No. 58 (Washington D.C.: The Brookings Institution), p.33.

[6] See Michael Frome, 1984, The Forest Service, 2nd ed. (Boulder: Westview Press), p.128.

That categorization enabled range researchers to resolve the tension due to the dual orientation. It specified the relationship between the stockmen and the range, enabling the researchers to determine what they should control in their experiments. They studied the relationship between use of the range and the vegetation's requirements to develop grazing systems that would improve or maintain the range while producing the greatest economic return. The specification of that relationship, in turn, allowed them to adhere to the epistemic values of the USDA's social definition of research because it provided the context to apply those values. It told them what to measure and how to interpret their results, enabling them to develop quantitative, objective knowledge. In fact, they used a method very similar to Griffiths' method but avoided his difficulties.

In addition, this categorization empowered the epistemic values because it specified the relationship between researchers and their audiences. The researchers primarily provided knowledge the Forest Service's other experts, its administrators and forest rangers, used to manage the range. Those experts possessed power over the stockmen because stockmen had to adhere to Forest Service policy, although in practice the Forest Service negotiated with local stock organizations to ensure its rules would not cause undue economic hardship.[7] The researchers, consequently, possessed power vis-a-vis the stockmen and would not experience the problem faced by Bentley and Smith when they proposed their grazing systems. In addition, providing knowledge directed immediately towards the stockmen as an audience was of only secondary concern. Finally, their knowledge legitimated and justified both their own power and that of the administrators and rangers.[8] That justification depended on their adherence to the epistemic values, which made the knowledge objective, etc. This categorization of range management thus provided the foundation to resolve the tension due to the dual orientation by intertwining the stockmen, the range, the USDA's epistemic values, and the Forest Service's power. Researchers would completely intertwine those disparate elements only after more than a decade of research. But in the mean time, this categorization enabled them to resolve the tension and practice range science.

The Role of Science in the Forest Service

Pinchot was a driving force behind the categorization of range management as a practical problem facing the Forest

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[7] See Rowley, 1985; and Herbert Kaufman, 1960, The Forest Ranger: a Study in Administrative Behavior (Baltimore: Johns Hopkins University Press, publ. for Resources for the Future, Inc.).

[8] See Rowley, 1985, pp.95 & 111; Alexander, 1987b, p.413.

Service. In 1895, when he managed the Vanderbilt estate forest in North Carolina, he accepted a proposal by Professor Charles S. Sargent, Director of the Harvard Botanic Garden, that the War Department manage the Forest Reserves. Pinchot supported Sargent's proposal because "... the management of the national forests is the most pressing need and duty of forestry in the United States."[9] Sargent also argued that military officers at West Point should take one course in forestry to prepare them for their job. Pinchot disagreed strongly, arguing that the experts necessary for such a job needed a much more extensive education. Finally, he already defined the role for research in an organization faced with the task of managing the forests. "While the army was doing protective work, the first duty of a forest service, a commission of scientifically trained men would study the reservations on the ground, outline general features of policy, recommend legislation, and do the other preliminary work which must precede the introduction of regular forest management."[10] He changed his mind only about who should manage the forests, arguing the USDA should perform that task.

That categorization also determined Pinchot's view of the role of the Division of Forestry. When he first met Bernard

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[9] Gifford Pinchot, 1895, "A Plan to Save the Forests," Century, 27:626-634, on p.630.

[10] Ibid.

Fernow, his predecessor as chief of the Division of Forestry, he left with the impression that Fernow focussed too much on leading a scientific bureau and too little on the problems of actually managing a forest or range. When he succeeded Fernow, Pinchot criticized him for preferring the technical and theoretical aspects of strictly scientific research while ignoring the practical problems facing anyone who managed a forest or range. For a short period as chief of the Division Pinchot focussed so much on practical problems that he began to fear he lacked a scientific foundation. Nevertheless, he wanted researchers to produce practical knowledge; i.e., he expected researchers to provide knowledge about the natural resources of the forests that would have practical implications. They could, for example, study the effects of fire on forests, a study that could simultaneously yield fundamental knowledge about changes in forests and practical knowledge about the use of fire to accomplish goals a manager of forests wanted to achieve. A number of people claimed fire could be used for beneficial purposes. Reflecting its categorization that management is a practical problem facing the Forest Service, it opposed the use of fire because it did not think most people could distinguish a good from a bad fire.[11]

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[11] Steen, 1976, pp. vii, 48, 131-137.

A number of interrelated factors shaped that categorization, among them the German education of the early foresters, including Pinchot and Fernow. German forestry research and forest management based treatment of the forests as agricultural crops on a number of assumptions these foresters imported into the U.S., sometimes inappropriately. These assumptions included the idea that wood is scarce because it grows on a limited amount of land; that the demand for wood is stable; and that therefore the task of management is obvious, namely "limit the consumption of wood to the growth potential of the Forest."[12] The resources of the forest, in other words, could be used provided they were used wisely, and in Germany and other European countries a cadre of experts specified what constituted wise use. Those assumptions extended easily to all resources of a forest, and Pinchot extended them into range management and research. Already in 1896, he disagreed with the majority of the National Academy of Sciences' Committee when it recommended

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[12] Samuel T. Dana and Sally K. Fairfax, 1980, Forest and Range Policy: Its Development in the United States, 2nd ed. (New York: McGraw-Hill Book Company), p.52; they claim these assumptions were false because timber was not a scarce resource in the U.S. and because demand was not stable due to the changing nature of the social structure in America; on the role of the educational background of these early foresters, see also Nash, 1982, p.134. For a scathing critique of the inappropriate introduction of German ideas into the American context, see also Ashley L. Schiff, 1962, Fire and Water: Scientific Heresy in the Forest Service (Cambridge: Harvard University Press).

the government close the forests to any kind of use, especially sheep grazing. Also in contrast to the majority of members, he saw the Committee's purpose as preparing the way for wise use of the Forest Reserves.[13] The difference between Pinchot and Fernow was apparently a difference in temperament and attitude. Fernow also accepted these assumptions, but believed that painstaking scientific research should precede the attempt to manage. The differences between Pinchot and Fernow emerged clearly in their attitude toward the National Academy of Sciences' study of range conditions in Oregon. Pinchot entered the study with great hopes. Fernow, on the other hand, saw it as a romp through the west that could not possibly produce valid knowledge. He predicted it would therefore only antagonize westerners.[14]

When Theodore Roosevelt became President in 1901, Pinchot had a friend in a powerful political office who shared his views. Under Roosevelt, the nation had a President willing to assume an active role managing economic and political processes, at least at times. As a conservative Progressive,

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[13] Gifford Pinchot, 1947, Breaking New Ground (New York: Harcourt, Brace & Co.; reprint, 1972, Seattle: University of Washington Press) p.94. Pinchot reiterated that argument on several occasions prior to the creation of the Forest Service; see e.g., idem, 1901, "Grazing in the Forest Reserves," The Forester 7:276-280. See also Rowley, 1985, esp. ch.2.

[14] See Rowley, 1985, pp.26, 30. For a succinct statement of Fernow's attitude, see B.E. Fernow, 1903, "Applied Ecology," Science, 17:605-607.

Roosevelt felt he had to curb the extreme implications of Progressivism. He thought, however, that the federal government should take control over certain political and economic processes in order to save capitalism from itself. Unless the government exercised such control, the more extreme implications of progressivism might become reality, especially its socialistic components. Business left alone might cause its own downfall because of the short-sightedness of many business men, and that Roosevelt wanted to avoid. Hence he used his power as president to interfere in the economy when he thought that would serve the public interest by curbing the excesses of unrestrained capitalism. As such Roosevelt reflected the attitudes not only of many Americans in general, but also of many businessmen who recognized that some forms of regulation would favor their own interests. Not surprisingly, therefore, a significant section of the American population idolized Roosevelt as the originator of the new order they were searching for.[15]

Roosevelt, an avid outdoorsman, became worried about the destruction of the nation's natural resources, and adopted the perspective of Progressive conservationists like Pinchot and

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[15] On Roosevelt, see Hofstadter, 1955, ch.6; Kolko, 1963, esp. p.130; Wiebe, 1967, esp. pp.189-195; Weinstein, 1968, ch.3; Gould, 1978, chs.2&3.

W J McGee.[16] He shared some of the preservationists' concerns, but did not agree that he should set those resources aside and prohibit their use. He recognized their importance in the nation's economic life, and thought the President was responsible to satisfy those economic needs. He also thought that preserving those natural resources ran counter to the traditional policy of encouraging prosperity for American families. As Roderick Nash shows, at times his statements were a little ambiguous because he attempted to hold preservationists and conservationists together in one political front, but he generally agreed with the utilitarian outlook of the conservationists.[17] In 1895, he argued federal government experts should manage the Forest Reserves because America "needs a thoroughly scientific and permanent system of forest management."[18] Soon after his inauguration he publicly supported Pinchot's arguments that Congress should transfer management of the Forest Reserves to the Division of Forestry in the USDA. Pinchot's outlook also clearly

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[16] On W J McGee, see Whitney R. Cross, 1953, "W J McGee and the Idea of Conservation," Historian, 15:148-162; McGee clearly articulated the Progressive conservationists attitude in an article where he called for a modified Bill of Rights that would incorporate the conservationists' concerns; see W J McGee, 1909-10, "The Conservation of Natural Resources," Mississippi Valley Historical Association, 3:365-367, 371, 376-379.

[17] Nash, 1982, pp.162-163.

[18] Theodore Roosevelt, 1895, in "A Plan to Save the Forests," Century, 27:626-634, on p.629.

influenced Roosevelt when he moved beyond the rhetoric of public statements and actually set policy. He admitted that he followed Pinchot all the way: "in all forestry matters I have put my conscience in the keeping of Gifford Pinchot."[19]

Not surprisingly, therefore, under Roosevelt the Forest Service acquired the authority to rationally manage the National Forests, and Roosevelt was very proud of his accomplishments in this area. The new social and political climate perhaps benefitted experts more than anyone else. As the situation in America appeared more complex, chaotic, and disordered, many people began to believe that only the specialized skills of the expert could bring order, and the experts sold their skills very effectively. Roosevelt believed strongly in the value of expertise, in particular its ability to predict, as the means to eliminate waste, destructive competition, and other excesses of capitalism through rational planning. As Hays shows, in America conservation emerged primarily out of experts' claims that their knowledge could provide the means to enhance the nation's prosperity by providing the foundation for rational, more efficient use of the nation's natural resources.[20] In

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[19] Quoted in Robert U. Johnson, 1923, Remembered Yesterdays (Boston: Little, Brown), p.307. Johnson was an influential preservationist because he published Century, at that time the leading literary journal in the U.S..

[20] Hays, 1959.

Roosevelt, and among politically or economically powerful segments of Congress and other leaders in America, these experts found sympathetic ears for their claims about the value of their ability to predict and then prescribe.[21] Furthermore, in 1905 stockmen themselves supported "reasonable" regulation of grazing by a five-to-one margin.[22] Finally, two U.S. Supreme Court decisions in 1911 reflected the changed social climate by upholding the Forest Service's right to manage the grazing areas on the National Forests,

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[21] On this interpretation, see also Wiebe, 1967, esp. ch.7; Gould, 1978, p.98. The Ballinger-Pinchot controversy over the management of natural resources outside the National Forests that erupted shortly after Taft became President centered primarily on the power of the experts in the federal government. Taft and Richard Ballinger, his Secretary of Interior, supported conservation but thought some of that power should remain in the private sector, while Pinchot and his supporters argued that such a course of action would cause and encourage natural resource users to continue their wasteful, inefficient, and destructive practices. Although Pinchot lost, Taft fired him, that did not affect the Forest Service because by then it had become firmly institutionalized. Nevertheless, by demonstrating the differences between Roosevelt and his successor, this controversy underscores Roosevelt's faith in experts, and hence why those experts were so successful institutionalizing themselves in the government under his presidency. Roosevelt also thought Taft undermined what he believed was an appropriate use of the power of the presidency to benefit the nation. On this controversy, in addition to the works cited above, see Rowley, 1985, p.88; Elmo R. Richardson, 1962, The Politics of Conservation: Crusades and Controversies, 1897-1913 (Berkeley: University of California Press); and James L. Penick, Jr., 1968, Progressive Politics and Conservation: The Ballinger-Pinchot Affair (Chicago: University of Chicago Press).

[22] Steen 1976, 67, Coville and Potter, 1905.

including the right to levy grazing fees and prosecute violators.[23]

Range Science and the Management of People

Administering the Forests then became experts managing the people who used the Forests in order to connect use with the growth requirements of the resources.[24] Limiting the consumption of wood to the growth requirement of trees, for example, meant managing the primary harvesters of wood, the lumbermen. Pinchot made that point explicitly in his autobiography, Breaking New Ground. Although the Forest Service had only limited powers because its agents "could still say nothing but 'Please' to private forest owners," its power had certainly grown because "on the national Forest Reserves we could say, and we did say, 'Do this,' and 'Don't do that.'"[25] William Rowley, in his excellent review of range management policy, shows that "Range managers from the beginning of government range administration to the present have conceded that the basic problem of range management is handling people, not stock."[26] Pinchot quickly assembled

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[23] Steen, 1976, 89.
[24] Limerick (1987) analyzes that orientation towards managing people in some detail.
[25] Gifford Pinchot, 1947, pp.152, 259.
[26] Rowley, 1985, 83-4

such a group of experts once Congress created the Forest Service.[27]

Finally, Coville's study of sheep grazing in Oregon both influenced and provided the background to apply the categorization of range management as a practical problem facing the Forest Service. It probably reinforced the opinions Pinchot acquired during his German education and thus helped to shape that categorization.[28] Coville also studied the grazing practices used by most sheepherders in Oregon. He observed that many herders herded the sheep together to protect them from predators and make their own job easier, and that they brought the band of sheep to the same grounds every night to bed them down. Coville concluded that these methods caused trampling of the forage before sheep had a chance to

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[27] Unfortunately, no range management experts existed, and the job of managing the range fell to graduates of the forestry schools, not always with satisfactory results. Stockmen at times questioned the ability of forest school graduates to understand the problems facing them. Partly because he recognized that problem, and partly to appease the fears of stockmen, Pinchot hired a former stockman, Albert F. Potter, in 1901 as "grazing expert" in the Bureau of Forestry. In the Forest Service, Potter directed the Grazing Section, and his practical knowledge brought credibility to the Forest Service's management of the ranges. See Steen, 1976; Rowley, 1985.

[28] Rakestraw, 1958, p.382, argues that Coville's report changed Pinchot's mind, from agreeing that the Forests should be closed as a member of the NAS committee to thinking that the forests could be used. However, since Pinchot disagreed with the majority of the NAS committee, it is more probable that Coville reinforced already existing beliefs, underscoring for Pinchot that the German approach to Forestry could work in the U.S.

consume it, and that sheep tended to consume all the plant-life in the small area where they were confined. As a result, they caused soil erosion and killed the vegetation, which caused less valuable forage plants to occupy the range.[29] In 1898, Coville therefore argued sheepmen should change their methods, but did not propose any alternatives. The Forest Service sought and received the cooperation of the Division of Agrostology, Coville's employer, when it initiated range research. This cooperation gave Coville the opportunity to extend the implications of his previous observations.

Coville in 1907 designed two studies, performed by James T. Jardine and Arthur W. Sampson, to develop grazing systems the Forest Service could use to manage the range and its users. His previous research and the deteriorating condition of the range in general led to three questions. First, could the Forest Service improve the range through artificial revegetation with cultivated grasses? Various researchers performed a lot of research along these lines, but the results proved disappointing. In 1919, Jardine published a summary of range research to date with Mark Anderson that became the "bible" of range management. They concluded that "Such plants may be found or may be developed at some time in the distant

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[29] Coville, 1898, pp.26-27.

future; they are not available at present."[30] Second, could the Forest Service improve the ranges by changing the stockmen's methods to remove the cause of trampling? Third, could the Forest Service improve the ranges by changing the stockmen's methods to allow plants to reproduce and thereby naturally revegetate the range? Coville designed those two studies, and Jardine examined the second question and Sampson the third.

Coville thus designed two studies that explicitly attempted to modify the stockmen's practices, clearly reflecting the categorization that range management was a practical problem facing the Forest Service rather than the stockmen. Sampson published a summary of his conclusions in a national sheepmen's magazine, and clearly reproduced that categorization. Explaining the role of the Forest Service to the sheepmen, he acknowledged that stockmen themselves recognized the need for research to restore the ranges, but did not think restoring them was the stockmen's task. Instead, he stated that "much is now being done by the U.S. Forest Service to improve the carrying capacity of the range lands to a high point and as speedily as possible."[31] The

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[30] James T. Jardine and Mark Anderson, 1919, "Range Management on the National Forests," USDA, Bulletin 790, p.57. I therefore do not analyze these studies artificial revegetation studies in any detail.

[31] Arthur W. Sampson, 1913b, "Scientific Range Management," The National Wool Grower, 3(12):7-9, on p.7.

stockmen also were not the primary audience for his study. Sampson stated that he performed the studies "in the interest of the Branch of Grazing of the U.S. Forest Service."[32] Bentley and Smith, in contrast, categorized range management as a practical problem facing the stockmen and performed their research within the context of the stockmen's practices in order to increase the range's production. Although they proposed a grazing system to connect use of the range with the vegetation's growth requirements, Coville ignored their contribution when he articulated his questions about grazing systems. He apparently assumed Forest Service researchers were the first researchers willing to undertake such research.

In his study, Jardine tested a grazing system to connect use of the range with the vegetation's growth requirements by removing the cause of trampling. The changed legal context on the range made Jardine's search for an alternative grazing system possible. Under the old grazing method, herders herded sheep in bands instead of allowing them to roam freely, which caused a lot of trampling of the forage, thereby decreasing carrying capacity by destroying food that otherwise sheep could have consumed. Yet two factors mandated the use of such a system. Herders had to herd sheep because laws prohibited the building of fences on the public lands. In addition, many

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[32] Ibid., p.8.

predators roamed the ranges and sheep had to be kept together for their protection. On the National Forests sheepmen could build fences, removing the legal constraint on grazing practices. That still left the predator problem, and the question became whether it was possible to build a fence that would keep them out, thus negating the need for herding altogether.[33] This alternative system, allowing the sheep to roam freely within a fenced area, was called the pasture system. It was used extensively in areas where agriculturalists had access to much less land than on the ranges, such as in the east, and the Australians had developed a similar system they called a paddock system. Coville apparently modelled his design of the experiment on the way other agriculturalists used the pasture system.[34] In the east, the scarcity of land served as a powerful constraint forcing agriculturalists to adopt the pasture system; in the west, the Forest Service now served as an equally powerful constraint.

The search for a grazing system the Forest Service could require the stockmen to apply on the National Forests enabled and constrained the experiment by structuring the breakdown of the study into questions. In general terms, Jardine broke the

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[33] See Coville's Introduction to Jardine, 1908, esp. p.6.

[34] See Coville's Introduction to Jardine, 1908, p.6; he unfortunately did not cite any studies on the paddock system. Jardine later cited an Australian study that demonstrated that the pasture system increased wool crops, "Special Consular Reports, Australian Sheep and Wool, 1892."

problem of collecting information down into two objectives. First, to establish a baseline for comparison, he carefully observed the current management practices and their effects on the stock and the range. Second, he performed an experiment designed "for the purpose of determining the action of sheep when allowed perfect freedom in an area protected from marauding animals, and the result of such a system upon the forage crop."[35] With that experiment he wanted to answer three questions. First, did the fence indeed keep the predators out, and did the sheep as a result split up into small groups? If the sheep stayed together, then the pasture system made no difference. Second, what was the effect of the pasture system on the range; and third, what was the effect of the system on the sheep? Jardine therefore created a predator-free environment by building a fence around 2,560 acres of range land in Oregon's Wallowa National Forest. Local stockmen contributed the sheep for the experiment and their management practices on neighboring ranges provided the baseline against which to evaluate its results. Jardine used the fence to perform the experiment from 1907 to 1909.

The focus on grazing systems enabled Jardine to instantiate the epistemic value mandating quantitative knowledge. The three questions emerging out of that focus

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[35] Jardine, 1908, 8.

determined the data he collected. Jardine systematically collected data on "the attitude of animals [predators] toward the fence, the actions of the sheep when turned at liberty within the enclosure, and the result of such a grazing system upon the forage crop."[36] He measured the attitude of the predators in terms of their behavior towards the fence. He counted the number of animals of several species that came to the fence by the tracks they left, then counted how many got past the fence, and hence also how many turned away upon encountering the fence. For example, in 1908, 136 coyotes approached the fence, but all turned away.[37] In general, the fence kept out the most troublesome predator, the coyote, although bears just went through it, bobcats and lynxes over it, and badgers under it. He therefore declared the fence a success: "The coyote is the one great menace to the sheep industry, and in excluding him the fence has served the purpose for which it was designed."[38] Jardine also carefully collected data on the behavior of the sheep under the pasturage system to determine whether they split up into smaller groups or still remained in one large band. He counted the number of bands of sheep, approximated the number of sheep in each band, measured the distance each band

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[36] Jardine, 1908, 8.
[37] Jardine, 1909, p.9.
[38] Ibid., 11.

traveled while grazing, and kept track of the bed grounds the sheep used. He collected this data daily, usually several times a day, from September 3 to September 30 in 1907, from June 21 until September 24 in 1908, and from June 22 until September 28 in 1909.[39] The sheep split up into from one to eight bands, moved a much shorter distance when in small bands, and used seventy-seven different bedgrounds.[40]

Jardine then determined the impact of the pasture system on the range. He began by calculating the carrying capacities of the experimental range and neighboring ranges where stockmen used the traditional grazing methods. Although Griffiths had developed a method to determine carrying capacity, Jardine developed his own calculation. He divided the total acreage of each of the ranges by the number of days the bands of sheep had grazed the ranges, and then divided this first total by the number of sheep in each band. To determine how many acres one sheep required for the grazing period, he then multiplied the second total times the number of grazing days in a grazing season.[41] In 1908, Jardine

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[39] For the tables with those data, see Jardine, 1908, 26-28; 1909, 16-21; and 1910, 11-16.

[40] Jardine, 1910, 17-21.

[41] For example, the experimental band of sheep contained 1,575 sheep, and grazed 2,560 acres of range for 99 days; while a neighboring band of 4,202 sheep grazed for 94 days on 9,962 acres. One sheep in the experimental band thus required 1.626 acres to graze for 99 days, while on the unfenced range one sheep required 2.472 acres to graze for 99 days. For these figures, see Jardine, 1910, p.27.

concluded that herded sheep required from 64 to 123 percent more rangeland.[42] He therefore argued the pasture system increased carrying capacity at least 50 percent, and in 1909 he argued it increased carrying capacity between 25 to 50 percent.[43]

He attributed the increased carrying capacity under the pasture system to the behavior of the sheep and the resulting improved condition of the range. First, sheep fully utilized the available forage under this system because "every corner of the range was grazed, while on the outside range certain small areas were utilized only in part or not at all," and because "the forage was actually consumed, while on the outside range part of it was destroyed by trampling."[44] In addition to the increased efficiency of range use, he also stated that the range's condition had improved. Comparing photographs of the range taken in 1908 and 1909 and his memory of range conditions in those years, he said that "The amount of forage ... was at least 100 percent greater in 1909 than in 1908."[45] Finally, he argued the behavior of the sheep balanced their requirements with those of the vegetation. Sheep partly grazed a section and then moved on, returning to

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[42] Jardine, 1909, p.31.
[43] Jardine, 1909, p.32; idem, 1910, p.28. I will explain the difference between 1908 and 1909 later.
[44] Jardine, 1910, p.30.
[45] Ibid., 31.

the first section sometimes days or sometimes weeks later. As a result, their behavior had the effect of implementing a system of rotation grazing, of "alternate grazing and resting," thereby improving the condition of the range. "The vegetation is not so completely robbed of its leafy foliage as it is when each area is close grazed before the sheep are moved off. In consequence the continued growth is more vigorous, and a greater amount of forage is produced."[46]

Since the Forest Service saw the stockmen as economic maximizers, Jardine also carefully calculated the economic benefits of the pasture system. Comparing the sheep grazed on the experimental pasture with sheep grazed on neighboring ranges, he showed that the sheep in the experimental band weighed more and produced more and better-quality wool, suffered fewer losses from predators, and were cheaper to handle than sheep on the open range.[47] The new method also prevented loss due to poison plants, thereby increasing the economic benefits.[48] In 1908, he calculated the cost-benefit ratio of the pasture system. The fence cost $400 per mile to construct, $25 per year to maintain, and assuming 8% interest on the money invested, he calculated it cost $3,481 to build eight miles of fence around the experimental pasture enclosing

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[46] Ibid., 32.
[47] For these conclusions, see Jardine, 1909, 26-29; and 1910, 22-27.
[48] Ibid., 24.

2,560 acres. Calculating the benefits on the basis of increased carrying capacity, increased weight, decreased cost associated with handling the sheep, but not including increases in the wool and lamb crop because those figures were too speculative, he concluded that the pasture system increased the sheepmen's profit $746.50 per year. After six years the fence returned a dividend of $215, at which point dividends would increase because the fence was paid for.

The results of the experiment on the pasture system thus satisfied all the Forest Service's objectives, and became embedded in its policies. This system of range use improved the range's condition and simultaneously increased stockmen's profits. Under the pasture system, the machine functioned smoothly. The final report contained the results of "experimental work to determine the efficiency of the pasturage system of handling sheep, with a view to the best utilization of the grazing lands," and the results were conclusive.[49] Not surprisingly, the Forest Service therefore applied the implications on the National Forests. Herders who bedded the sheep down on the same bedground many nights in a row caused trampling of the forage, thereby reducing the grazing capacity of the range. In the pasture experiment, the sheep used numerous bedgrounds, seldom bedded down in the same

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[49] Jardine, 1910, p.5.

place two nights in a row, and did not trample the forage. The Forest Service therefore regulated the behavior of sheepherders: "Sheep and goats must not be bedded down more than three nights in succession in the same place."[50] Jardine and Anderson proposed an even stricter rule. They argued that "As a general rule sheep should be bedded one night in a place."[51]

Yet the categorization of range management as a practical problem facing the Forest Service rather than the stockmen also constrained Jardine's knowledge development practices. He never examined the pasture system's practicality from the perspective of the stockmen. The pasture system solved a large part of the grazing problem from the perspective of the Forest Service; i.e., it improved the range and increased stockmen's profits. The stockmen, furthermore, undoubtedly needed to change their practices, which had caused the damage to the range. But Jardine never examined what impact his results would have on their practices, or whether they could still use the range on the National Forests if required to adopt something like the pasture system.

Jardine claimed his experiment demonstrated the system's practicality from the perspective of stockmen because it

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[50] U.S. Forest Service, "The National Forest Manual," Grazing Regulation 26; quoted in Jardine and Anderson, 1919, p.51.

[51] Ibid.

mirrored possible range management practices more closely than other forms of experimentation. In the final report on the experiment, he argued that the results were "of wider practical application to National Forest Grazing lands than results obtained upon small, workable areas would be, since actual conditions of handling could not well be secured on a very small experimental inclosure."[52]

However, his cost-benefit calculations did not consider the economic practicality of the pasture system from the perspective of the stockmen. He based those calculations on the size of the experimental area, the only area for which he had data. The experimental area of 2,500 acres, although large, was significantly smaller than the areas of range used by sheepmen, which his data indicated measured from 4,500 to 6,500 acres.[53] The stockmen's ranges therefore required many more miles of fence, at $400 per mile, and it is not obvious how many stockmen possessed the resources to make that kind of investment. By a quirk of fate, sheepmen at this time began to switch to a heavier breed of sheep, the Merino, and discovered the Merino were more difficult to herd. They consequently reduced the size of their bands, and could therefore reduce the size of the range they used per band of sheep. This reduction brought the cost of fence construction

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[52] Jardine, 1910, p.28.
[53] See Jardine, 1909, p.31; 1910, p.27.

more in line with Jardine's figures. The question then remained whether stockmen could afford that kind of investment. The cost was apparently a big enough burden for many stockmen that the Forest Service gave them economic assistance. Within the constraints of its budget, the Forest Service allowed stockmen free use of poles, posts, wire, nails, and staples to build fences. These materials remained the property of the Forest Service, but stockmen could use them as long as they grazed on the Forests.[54] In 1914, Albert Potter told sheepmen assembled in Salt Lake City for the annual convention of the National Wool Growers Association that the Forest Service was willing "to cooperate with them in the construction of such improvements [fences]."[55]

Jardine himself also recognized that the results of his experiment might pose practical problems for stockmen. In his final report, Jardine extended his results into a discussion of the implications of his experiment for sheepmen who herded their sheep. Describing what became known as the "open" herding system, he said herders should allow their sheep "natural, quiet freedom;" bed the sheep wherever night overtook them; and move the sheep from a section of range before all the vegetation was consumed to ensure they left it

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[54] See Roberts, 1963, 103.

[55] Albert F. Potter, 1914, "Improvement in Range Conditions," National Wool Grower, 4(Jan):15.

in good condition, perhaps returning to that section later once it had a chance to recover from the grazing.[56] The recognition of the reduced benefits of the pasture system may have motivated him to prescribe herding practices. He claimed in 1908 that under the pasture system carrying capacity increased 50 percent, but in 1909 he reduced that to from 25 to 50 percent. He based that reduction on the herding practices of good herder because "an excellent herder can, to a considerable extent, allow his sheep freedom and keep them quiet, thereby increasing the carrying capacity of his range.[57] He therefore reduced the difference between the pasture system and good herding, although he never explained or justified why good herding could not achieve the same improvements in carrying capacity as the pasture system. At any rate, his extension of the experimental results into a prescription of herding practices closely resembled the herding practices of good herders. He thought most sheepmen therefore already knew about those herding practices, and also recognized their practical difficulties. Concluding his discussion of these herding practices, he said that "It is not assumed that the suggestions given are new to sheepmen, or that they can be followed easily."[58]

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[56] Jardine, 1910, p.32.
[57] Ibid., p.28.
[58] Ibid., p.32.

The implications of Jardine's experiment required drastic changes in herders' range management practices, and many did not adopt them easily. There were perhaps as many range management methods as there were herders, ranging from the good methods described by Jardine to very poor methods. Jardine also described the methods of poor herders, who often did not own the sheep but herded them for their owners. "A poor herder will sit under a tree or near a fire, and at frequent intervals send the dog around the sheep," and under that method, "forage is destroyed and the sheep do poorly."[59] Herders also bed the sheep down on the same grounds many nights in a row so they could set up their camp near the bedground and would not have to pack it up very often. Under the pasture system, herders would have to walk along the fence every morning to determine whether any predators got into the pasture during the night and to ensure the fence remained in good repair. Only then could they begin to check the sheep, which they would have to locate all over the pasture. The "open" herding method also required much more walking because the sheep would scatter when left to graze naturally and quietly. In addition, a sheep or lamb could more easily get separated from the band, necessitating a search. Without the fence, herders would also have to keep a closer watch for

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[59] Ibid., p.31.

predators. Finally, they would have to pack up and set up their camp more frequently to adhere to the Forest Service's regulations about bedding down sheep. Many therefore objected to the implications of Jardine's study, and some only applied them when they knew the owner was watching. Almost a decade after Jardine's study, only about fifty percent of the herders on the National Forests had accepted those implications and made the required changes, though ultimately almost all swore by the benefits.[60]

The implications of Jardine's research in many ways required the professionalization of sheep herding, and a professional audience existed among the Forest Service's experts. It required the replacement of a wide variety of range management practices with one standardized form of range management. Instead of each herder herding his sheep differently, all sheep would now be herded or managed in roughly the same way. To accomplish that, a new kind of "sheep herder" appeared on the range, the professionals of the Forest Service.

Foresters and forest rangers functioned much like the

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[60] See Roberts, 1963, p.100; Jardine and Anderson, 1919, p.54. Jardine and Anderson at that point issued a veiled threat. Watershed and forest protection was one of the Forest Service's main tasks, and "The manner in which the sheep are handled is an important factor in deciding whether certain watershed and forest areas may be used for sheep grazing without unwarranted interference with watershed protection and forest protection ."

managers and supervisors who had become prominent in bureaucracies in private business and in other governmental agencies. They indirectly herded the sheep because they managed and supervised the owners and herders of the sheep and told them how to manage their animals. The Forest Service allowed its local foresters and rangers considerable latitude in deciding how to manage the forests and ranges, but expected them to work within the framework of the results of research. As a result, range management practices became standardized, and the increasing professionalization of the Forest Service reinforced that trend. The Forest Service gradually build an elaborate bureaucratic structure to facilitate the flow of information from research to local forest rangers. It also required all its employees to satisfy certain minimum requirements of professionals such as educational requirements, passing a civil service exam, and a basic knowledge of range and forest management. Although some rangers initially became captives of the stockmen, they quickly learned that to succeed in the Forest Service they had to act like professionals, and that meant working within the flow of information in the bureaucratic structure. They therefore adopted the standardized method of range management certified by research, in the process reinforcing their own status as professionals in the Forest Service.[61]

In short, the Forest Service's categorization of range management as a problem facing the Forest Service enabled research, and research fed back into the categorization. Research thereby legitimated the Forest Service's power to mandate range management practices. Forest rangers could now point to a body of knowledge developed through careful experimentation to justify their actions and their authority when they told stockmen to modify their practices.[62]

After this study, Jardine moved on and became an administrator. In 1910, he moved into a primarily administrative position in charge of the USDA's range investigations and surveys, although he continued to publish occasionally. In that position he was instrumental in the development of range reconnaissance, a method to determine and rate range conditions, and of range inspection, a method to determine the kinds of changes stockmen needed to make in their range management practices.[63] He left in 1920 to become director at the Oregon Agricultural Experiment Station. In 1931 he moved to Washington D.C., became the USDA's

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[61] On these points see Alexander, 1987a, and esp. idem, 1987b, pp.410-411; and Kaufman, 1960.

[62] See Rowley, 1985, pp.95 and 111.

[63] For a description of these methods see Jardine and Anderson, 1919, pp.74-82; for a summary see Rowley, 1985, pp.99-101.

Director for Research and the chief of the Office of Experiments.[64] He never earned an advanced degree, but three schools awarded him honorary degrees for his work in range management.

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[64] Material for this biography has been drawn from the WPA History of Grazing.

Chapter 5

THE TENSION REAPPEARS: THE PROBLEM OF PRACTICALITY AND THE APPLIED ECOLOGICAL FOUNDATION OF RANGE SCIENCE

In 1907, Arthur W. Sampson initiated the second study Coville designed to develop a grazing system, and that study ultimately led to the applied ecological foundation of range science.[1] Many consider Sampson the "father of range science" because of his many contributions, and possibly the Forest Service's first professional ecologist. While James T. Jardine was identified merely as a "special agent" of the Forest Service on the title page of his first paper, Sampson

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[1] Ecologists distinguish between autecology, the study of individual species or populations, and synecology, the study of communities that consist of various species. Autecology studies the processes of plant growth and reproduction as affected by various factors in the environment, and as such shares many similarities with plant physiology. Sampson switched from applied plant physiology to applied ecology to justify the grazing system. The initial study that led Sampson to propose his grazing system was an autecological study, but he later justified the same system on synecological grounds. However, Sampson did not use those terms, although the distinction between autecology and synecology had been drawn. Instead, he used the term "ecology" and used it to mean synecology. I adopt his usage, and use the term ecology throughout to mean the study of communities. On the distinction between autecology and synecology, see Robert P. McIntosh, 1985, The Background of Ecology: Concept and Theory (Cambridge: Cambridge University Press), esp. pp. 146-147. Later range scientists did adopt the distinction, at least to some extent; see e.g., Kenneth W. Parker, 1954, "Application of Ecology in the Determination of Range Condition and Trend," JRM, 7:14-23. Many plant ecologists, who were primarily synecologists, apparently thought range scientists were strictly autecologists, and as a result ignored their contributions; see J. White, 1985, "The Census of Plants in Vegetation," in J. White, ed., 1985, The Population Structure of Vegetation (The Hague: Dr W. Junk Publishers), pp.33-88.

was identified as an "expert in plant ecology" on the title page of his 1908 paper, and by 1913 he was identified as a "plant ecologist."[2] Sampson attended the University of Nebraska, where he received his bachelor's degree in 1906 and his master's degree in 1907. In 1917, he earned his doctorate from George Washington University. At Nebraska he studied under Frederic E. Clements, who developed one of the first methods and theories in plant ecology, the quadrat method and the theory of succession. In 1907, Coville hired him to perform natural revegetation experiments.[3] In 1912, he became the first director of the Utah Experiment Station, one among several experiment stations the Forest Service established at this time to produce the knowledge base for natural resource management. At the GBS he continued his earlier researches and developed many other lines of research, culminating in his 1919 paper that provided a theoretical basis for range science and his 1923 textbook on range science and range management.[4] He left the GBS in 1922 to become associate professor of Forestry at Berkeley, where he was promoted to full professor

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[2] In 1908, Pinchot called him an "expert in plant ecology," Pinchot, in Sampson, 1908, p.2; Pinchot did not use any such designation in his preface to Jardine, 1908.

[3] That is, allowing the range to revegetate or reproduce itself, rather than for example reseeding by humans, as in artificial reseeding.

[4] Arthur W. Samspon, 1919b, "Plant Succession in Relation to Range Management," USDA Bulletin 791; idem, 1923, Range and Pasture Management (New York: John Wiley and Sons, Inc.).

in 1940. Sampson wrote four textbooks and published close to 100 articles in federal and state publications and scientific journals.

During the course of his research, Sampson completely intertwined the range, the stockmen, the Forest Service's power, and the USDA's epistemic values, thereby extending range scientists' power over nature, forest rangers, and the stockmen. The Forest Service's categorization of range management enabled him to introduce the quadrat method, a method very similar to Griffiths' method, and thereby to go beyond common sense, unaided observation to enhance range scientists' predictive abilities. Only the range scientists could penetrate behind the appearance of nature and gain the access to reality necessary to make predictions. It also enabled him to reintroduce the grazing system Smith proposed, deferred-rotation grazing, which he justified on the basis of predictions he made with data generated using the quadrat method.[5] However, within ten years he faced a challenge to the deferred-rotation grazing system. Two forest rangers claimed Sampson had not successfully resolved the tension due

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[5] Clements recognized the similarities; see Frederic E. Clements, 1928, Plant Succession and Indicators, (New York: The H.W. Wilson Co.), pp.227,228, 390. Sampson never acknowledged the similarities, although thirty years later he did acknowledge that Smith had performed the first range science studies; Arthur W. Sampson, 1939, "Plant Indicators - Concept and Status," The Botanical Review, 5(3):155-206, on p.169.

to range science's dual orientation. They questioned the utility of the ability to predict because they found the knowledge and the grazing system Sampson produced impractical. As part of the response to this critique, Sampson placed range science on an applied ecological foundation.

Sampson's turn to applied ecology further extended range scientists' power and provided forest rangers with a practical tool to manage the range and the stockmen. He introduced the theory of succession into range science, and he derived a tool enabling rangers to manage stockmen from the theory of succession. However, Sampson changed the theory of succession in significant ways to suit range science.

With the turn to applied ecology, Sampson also satisfied ecology and economics simultaneously. In the context of the Forest Service's search for grazing systems, no range science claim could be valid unless it satisfied both epistemic and economic criteria.[6] In effect, therefore, the knowledge embedded in the grazing systems Jardine and Sampson proposed had a dual meaning because those grazing systems satisfied

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[6] It is perhaps more accurate to say that the economic criteria and the epistemic criteria such as explanatory or predictive power, etc., had equal epistemic value in the evaluation of knowledge claims range researchers such as Jardine and Sampson proposed. The economic criteria thus also served as epistemic criteria. For an applied science like range science, the distinction between economic and epistemic criteria is therefore an artificial distinction that did not exist in the research of Jardine and Samspon. However, I will continue to use the distinction to avoid linguistic confusion.

economic and scientific criteria simultaneously. Sampson, furthermore, with the turn to applied ecology equated the ability of the range to produce resources for use, or its economic value, with its ecological condition. Ecological processes and the economic interests of the stockmen became equivalent. Range scientists could, therefore, guarantee the goal of conservation, maximum profits in perpetuity, or grazing without destroying the range resource, provided forest rangers and stockmen adhere strictly to the range scientists' knowledge. Adhering to the researchers' knowledge remained, however, a matter of negotiation between the rangers and the stockmen.

Beyond Common Sense Observation

Like Jardine, Sampson hoped to develop a grazing system, but one that connected grazing to the vegetation's growth requirements in order to revegetate the range naturally. Sampson thought the lack of grazing systems had destroyed the range: "by continued overstocking and the injudicious removal of the forage crop, as the inevitable result of the lack of a system of range management during the free grazing period, the forage supply was rapidly reduced."[7] Such a system would have to balance the requirements of the vegetation with the

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[7] Sampson, 1908, 7.

requirements of the animals grazing the ranges. That was a difficult task because, according to Sampson, "the requirements of the vegetation and the requirements of the stock are to a great extent antagonistic."[8] Nevertheless, in 1907 Sampson began by studying the life histories of range plants. The institutionally mandated search for grazing systems then determined the kind of data he should collect to accurately measure and predict the impact of grazing on plant life histories. On the basis of the life history data he proposed the deferred-rotation grazing system, and the Forest Service mandated that all stockmen use this system when grazing their stock on the National Forests.

To generate data in his research, Sampson introduced the quadrat method from ecology into range science, and thereby denied the validity of common sense observation. In the late 1890's, Frederic Clements and Roscoe Pound developed the quadrat method in ecology, a method that Clements but not Sampson acknowledged shared many similarities with Griffiths' method.[9] In their studies of Nebraska's grasslands, Clements and Pound recognized that unaided observation can lead to errors because it does not reveal reality. In particular, the

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[8] Arthur W. Sampson, 1914, "Natural Revegetation of Range Lands Based Upon Growth Requirements and Life History of the Vegetation," Journal of Agricultural Research, 3:93-147, on p.93.

[9] Clements, 1928, p.227.

changes and variations between one type of prairie and another type occurred so gradually that naked-eye observations could not determine accurately where the change occurred.

Only quantitative ecology provided access to reality. Sense impressions simply could not capture the complex variations and changes in nature. Clements and Pound therefore concluded they had to count the plants in order to reveal those changes. Realizing that counting all the plants on Nebraska's prairies was impossible, they developed the quadrat method. The quadrat was (and is) a plot measuring one square meter on which the ecologist counted all the plants. Clements and Pound established a string of those quadrats across Nebraska in order to measure accurately the changes and variations the naked eye could not reveal. Clements realized the quadrat excluded people who had played a prominent role in botany, the amateur naturalists, from the emerging discipline of ecology. He did not think this a loss, however, because the quadrat method enhanced the ability of the ecologist to examine nature.[10] The quadrat method thereby also reinforced the professionalization of American ecologists, and the experimental analyses it made possible enhanced the scientific authority of ecologists.[11]

The quadrat method enabled ecologists to determine the

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[10] See Tobey, 1981, pp.73-74.
[11] See Cittadino, 1980.

causal mechanisms responsible for changes. The ecologists, or the range scientist, could carefully measure changes in the environment and changes within the quadrat to correlate those changes and determine the causal relationship between various environmental factors and the vegetation. Clements developed five different types of quadrats, two of which would be important in range science, the permanent quadrat and the denuded quadrat. To measure changes from year to year or season to season accurately, the permanent quadrat remained in one location permanently, and the ecologist returned to the quadrat periodically to count and map the vegetation. He could then determine the causal connection between changes in the vegetation and changes in the environment. The ecologist could also initiate more direct experimental investigations to study changes in the vegetation by using the denuded quadrat. The ecologist removed all the vegetation within the quadrat, and then returned periodically to measure the changes of the vegetation that was reestablishing itself in that area.[12]

Sampson introduced the quadrat method because he wanted accurate knowledge of the changes in the vegetation so he could predict the effect of grazing on the range, thus instantiating the social definition of scientific research. Stockmen and forest rangers had relied on sense impressions of

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[12] On the quadrat method, see Tobey, 1981, ch.3; Hagen, 1988.

the condition of the range and the condition of their stock to determine the effect of grazing on the range. But such common sense observations did not satisfy Sampson. He wanted "to secure conservative and accurate experimental data," and he used the quadrats because "quadrats are of invaluable aid in securing reliable data on the changes in the type of vegetation."[13] The quadrats provided the only basis to predict the impact of grazing on the vegetation, and hence they provided the only "authentic records of the vegetative changes," enabling the range scientist to "state authoritatively whether or not a change should be made in a given system of management."[14] Consequently, the rangers and stockmen's observations were inadequate and they would have to rely on knowledge generated with the quadrat method. In contrast, Jardine's study in and of itself did not have this effect because he relied on common sense observation. Anybody could duplicate the observations he made. In fact, J.W. Emmons, an Oregon stockman, in 1907 initiated the same experiment. In 1909, Jardine gave an extensive description of Emmons' observations and results to underscore the practicality of the pasture system.[15] Sampson never described the observations of stockmen because it was

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[13] Sampson, 1908, pp.12, 13.

[14] Arthur W. Sampson, 1917a, "Succession as a Factor in Range Management," Journal of Forestry, 15:593-596, on p. 594.

[15] Jardine, 1909, pp. 32-37.

impossible to duplicate his observations without special training. Enhancing the researcher's predictive abilities thus reinforced lines of authority within the Forest Service and the Forest Service's power over the stockmen.

The Forest Service's categorization of range management provided the context to use the quadrat method. The focus on grazing systems to manage the stock and the stockmen emerging from that categorization told him what kind of data to collect, how to interpret the data, and thus how to use the quadrat method. Consequently, he knew how the method functioned from the perspective of human interactions with the range, and he did not experience the problems determining the meaning of the data Griffiths had encountered. Griffiths, in fact, recognized during an observational study of grazing in the Great Basin area in 1902, that "The whole question of preservation and maintenance of native pasturage ... is an administrative one."[16] This was before he undertook his clipping experiments, but Griffiths lacked the institutional context to link his research directly to the administration of the ranges. As a result, although in 1902 he also recognized the need for a grazing system to "... control the pasturing in such a way as not to injure the stand of grass," he was never

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[16] David Griffiths, 1902, "Forage Conditions on the Northern Border of the Great Basin," USDA, Bureau of Plant Industry, Bulletin 15, p.56.

able to develop such a grazing system out of his subsequent research.[17] In contrast, the institutional context of the Forest Service enabled Sampson to resolve the tension due to the dual orientation toward both science and society. Sampson collected data primarily on the life histories of plants, specifically the production of flower stalks, maturity of seeds, germination of seeds into seedlings ("actual counts were made of forage seedlings"), and the trampling of seedlings.[18] He then used that data as the basis for a grazing system.

That categorization of range management, and the resulting search for grazing systems, structured the breakdown of Sampson's study into questions. To develop a system of grazing that balanced the requirements of the stock and the vegetation, he had to understand the causal connection between various grazing methods and the vegetation's life history. He had to develop knowledge about the ways range plants revegetated, and about the factors that might affect that revegetation. As an applied scientist, Sampson wanted to know the relationship between human manipulations and the vegetation, and that focus determined what he wanted to know about the vegetation. Only then could he develop a system that satisfied the needs of both the vegetation and the stock.

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[17] Ibid., p.56.
[18] Sampson, 1908, p. 13.

He broke the problem to develop a grazing system down into three components. First, by following sheep around the range, he determined how important the various forage plants were in terms of "their abundance, distribution, seed habits, and forage value."[19] The identification of the most important forage plants set the stage to study their life histories. As a range scientist he did not merely study their life histories; instead, he studied their life histories "so far as concerns the handling of these areas as grazing areas."[20] Second, he wanted to determine how quickly and to what extent under the current grazing practices and under complete rest the valuable forage species re-appeared on the range. Third, he wanted to determine the course of succession under current grazing practices. He did not, however, have a theoretical conception of succession; rather, succession was an empirical phenomenon, the replacement of one group of plants by another group. He argued that physical factors acting upon plant formations are primarily responsible for changes in vegetation, and he carefully measured the humidity, temperature, and light intensity to determine their impact on the vegetation.[21] Yet these factors were not the primary

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[19] Sampson, 1908, 8.
[20] Sampson, 1908, 8.
[21] Sampson, 1908, 13. Sampson followed Clements, although without citing him, apparently accepting Clements' arguments as a matter of fact.

focus of his investigations. He believed that human actions such as the (mis)management of stock affected the vegetation by interfering in the life cycles of plants. This study was specifically designed to determine as accurately as possible how various management methods affected those life cycles, and how that in turn affected succession. Sampson wanted to examine if and how a grazing system that allowed plants to revegetate would lead to the reappearance of the most important forage plants, i.e., would foster their succession. He intended to use all this knowledge about the plant life on the ranges to provide the foundation for his fourth goal, "to develop a system of grazing from the information secured through these studies whereby the former productiveness of the ranges may be restored through natural reseeding."[22]

In contrast to Clements, Sampson systematically used the quadrat method in his experiments to test alternative ideas. Clements apparently used the quadrat method primarily to demonstrate conclusions he had already reached, e.g., on the basis of his theory.[23]

The theory of succession proved to be one source of the problem for Clements because the process of succession could take centuries, making it difficult to perceive how an ecologist could test alternative hypotheses using the

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[22] Sampson, 1908, 8.
[23] See Hagen, 1988, p.272.

permanent quadrat. Testing alternative hypotheses required several generations of ecologists recording and mapping the changes occurring within the quadrat. The denuded quadrat, furthermore, interfered with the natural process of succession, raising questions about the validity and significance of the data with respect to conclusions about the natural process. Clements, in fact, introduced a different concept for the kind of succession that occurred after all the vegetation had been removed from an area, whether by the experimenter or as the result of some catastrophe like a fire. He called it secondary succession, to contrast it with the regular process of succession. Finally, there were other constraints, in addition to the conceptual difficulties, that made the use of the quadrat impractical. The method required a lot of time, and partly as a result, was expensive. Time did limit the practicality of the quadrat method for Sampson, but he did not experience the same conceptual obstacles.

The applied orientation of range science, its search for a grazing system that balanced the requirements of the stock and the vegetation, enabled Sampson to use the quadrat method to generate and test ideas. In his observations and experiments he focussed on the immediate relationship between stockmen and the range, not on long-term processes. Furthermore, he did not examine the successional process, but focussed strictly on the relationship between human actions and individual plant species. An ecologist like Clements might want to study the replacement of one type of vegetation by another type in order to analyze the causal mechanism of succession. Sampson instead followed sheep around the range to determine the most valuable forage plants, which were mountain bunchgrass and some other grasses and a few non-grass species. He did not classify those species in terms of their place in the process of succession, but only in terms of their economic value. That classification set the stage to generate and test ideas. He did not search for a grazing system to balance grazing with the successional process. Instead, he sought to balance the relationship between grazing and the requirements of those species, although he assumed that if he achieved that balance, then those species would succeed on the range, regardless of the natural process of succession.

On the basis of observations made with the quadrat, Sampson proposed a grazing system, deferred-rotation grazing.[24] He observed the effect of grazing on the life histories of the most important forage plants, in particular when they produced flower stalks, when their seeds matured, and when their seeds germinated and produced seedlings. He established three hundred quadrats to observe the effect of the most common grazing practice, season-long grazing, on the

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[24] For a brief description of deferred-rotation grazing, see Chapter 3; or see Sampson, 1908, 1913a, 1913b, 1914.

plants' life histories. On the basis of those observations, Sampson concluded that "season-long grazing continued year after year seriously interferes with the growth of the vegetation, decreasing both the quantity and the palatability of the forage crop."[25] Furthermore, the quadrats allowed Sampson to determine the kind of plant species that established themselves, the pattern of succession under season-long grazing. However, he studied the pattern of succession strictly in terms of the grazing value of the plants that established themselves. Under season-long grazing, the palatable, perennial forage species were almost all being destroyed and replaced by annual weeds and other species of very low forage value, and hence the productivity of the range declined.[26] Sampson argued that for the range to revegetate itself with palatable species, their seedlings must have a chance to establish themselves in order for those plants to reproduce. Grazing should therefore be deferred until the seedlings had established themselves. Stockmen should divide their land into sections, defer grazing on one section for two years until the seedlings had established themselves, and then rotate their grazing pattern to defer grazing on another section.

Sampson then used the quadrat method in a clipping

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[25] Sampson, 1914, p.120.
[26] See Sampson, 1914, pp.117, 120.

experiment to test and compare deferred grazing with the most common grazing system, year-long or season-long grazing. He established a number of quadrats, and clipped the vegetation on the quadrats "just above the ground for three consecutive seasons," and compared the results of the clipping experiment with the vegetation's growth on a protected (unclipped and ungrazed) range.[27] On half the quadrats, he clipped the vegetation once a month during the grazing season (July-September) to simulate season-long grazing, the method stockmen used. On the other half of the quadrats, he tested deferred grazing by clipping the vegetation only after the seed had matured. The results demonstrated the advantages of deferred grazing over season-long grazing. While on the quadrats clipped monthly, "the vegetative growth decreased in abundance each successive season," on the other quadrats "the flower stalks were produced fully as early, as uniformly, and as profusely as in the case of the plants which had remained unmolested."[28] The clipping experiments also demonstrated that the vegetation was more vigorous on the plots clipped after seed maturity. The plants clipped monthly had lost all vigor, i.e., they produced no seed during the fourth season,

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[27] Ibid., p.103. The similarities with Griffiths' method are here obvious, though in the Forest Service context Sampson had the advantage of being able to determine how to use the method.

[28] Ibid., p.103.

when Sampson left the vegetation on all the quadrats undisturbed.[29] On the quadrats clipped after seed maturity, "the seed crop was fully as large ... as on lands from which stock were excluded."[30] Observations of actual grazing practices corroborated those results. Comparisons of the number of seedlings produced by different species under season-long grazing and deferred grazing showed that under deferred grazing more seedlings established themselves and that those were seedlings of the more palatable species.[31]

Systematic observations and experiments with the quadrat method also demonstrated the need for deferred grazing in order to exercise dominion or power over nature, which proved necessary to improve the range's productivity through natural reseeding. Sampson discovered that the germination rate of all species is low, but that the least palatable species had a much higher germination rate than the more valuable forage species. Consequently, the least valuable species possessed a competitive advantage. Another factor reinforced this advantage. The morphology of the seed also favored the reproduction of the least valuable species. The seeds of

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[29] Had he clipped it monthly, no seed would have been produced because the vegetation would have been clipped before it had a chance to produce the seed. He therefore had to leave the vegetation undisturbed to test the vegetation's vigor after three consecutive years of season-long grazing.

[30] Ibid., p.105.

[31] Ibid., p.126.

those species worked themselves into the ground easier because they were small and heavy, while the seeds of the more valuable species were larger and lighter. Therefore, although they were dropped in the autumn, in the spring the seeds of the more valuable species still lay on the ground. Only one of the valuable forage species, porcupine grass, established itself because its seed possessed an awn that "when moistened ... untwists vigorously, causing the bent, needle-like point at the lower end of the scale to bore into the ground, the stiff, backward-turning hairs holding it in the earth when once started."[32]

Several experiments underscored that humans have to change nature through systematic grazing or other interventions such as harrowing to ensure maximum productivity. Sampson protected a number of permanent and denuded quadrats from grazing and other human interventions, and discovered that without such intervention the least valuable species established themselves on the quadrats.[33] To ensure the natural revegetation of the most valuable species, humans therefore had to stir the soil to plant the seeds of the most valuable species. Grazing accomplished such planting of the seed.

Sampson also examined whether the advantage of systematic

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[32] Ibid., 109.
[33] Ibid., 121-125.

grazing outweighed the loss of seedlings due to trampling. He used the quadrat method to measure the causal connection between grazing and the vegetation by counting the density of the various species before and after moderate grazing. He could thus "determine the actual loss and injury to forage seedlings caused by trampling."[34] Sampson concluded that 32.7 percent of the seedlings are killed due to trampling, but he thought this was not a serious loss because "ample reproduction is assured," and the loss of seedlings is largely compensated for by the beneficial effect sheep have on reproduction when they trample seeds into the ground, enabling them to take root.[35] Finally, observations showed that physical factors such as low temperatures and lack of precipitation were the major causes responsible for the destruction of seedlings, and that such destruction occurred before he allowed the sheep on the range under the system of deferred grazing.[36]

Sampson used these results to take issue with the stockmen. arguing the deferred-rotation grazing system satisfied their economic criteria. Most stockmen still

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[34] Sampson, 1908, p. 14; see also the table on p.17, where Sampson collected the data generated with the quadrat method about the percentage of the ground covered with seedlings of various important forage species.

[35] Ibid., p. 19; on the beneficial effects of trampling, see p.18.

[36] Sampson, 1914, 109-112.

claimed the range required complete rest to revegetate naturally. Sampson argued complete rest would not achieve maximum productivity and was unnecessary. He also thought it was impractical. Due to the Forest Service's categorization of range management, Sampson, like Jardine, defined practicality in economic terms. He made that point explicitly when he described the goals of his research: "One of the chief aims was not to change the common grazing practice any more than was absolutely necessary, and thus avoid any needless economic disturbance that might follow if the stock-raising interests were deprived of the use of a large part of the forage crop."[37] Many Oregon stockmen depended on the range, and complete rest would not accomplish that goal. Such interference was unnecessary because stockmen could achieve their objective more efficiently if they deferred grazing until after the seed matured and the seedlings became established. Following the experiments with the quadrats from 1907 through 1910, he applied deferred-rotation grazing for two years on the Wallowa National Forest to convince stockmen they could revegetate the range and still utilize its forage production. Economically the stockmen benefitted by adopting deferred-rotation grazing. Some stockmen apparently thought,

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[37] Arthur W. Sampson, 1909, "Natural Revegetation of Depleted Mountain Grazing Lands," Forest Service Circular 169, p.6.

however, that the stock would not consume the forage after it dropped its seed because it would be too dry, but the application of deferred-rotation grazing demonstrated that "the herbage, while not succulent at that time, is eaten with great relish ... [and] lambs as well as dry sheep made satisfactory gains."[38] Finally, chemical analysis of the nutritive content of the herbage in the autumn corroborated those observations.[39]

Sampson also used these results to extend the power of the researchers over Forest Service employees, and through them over the stockmen, successfully arguing both had to change their practices. Although the Forest Service established grazing seasons when the stockmen could use the range on the National Forests as soon as it acquired the authority, it still allowed season-long grazing for a number of years. Sampson attacked that practice head-on. He argued that his studies had "been extensive and thorough enough to show clearly that the present regulations are ineffective in bringing about the natural revegetation of the valuable grazing plants."[40] In Oregon, for example, as was typical, the Forest Service initially permitted sheep to enter the Forests in July, but at that time the most palatable species

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[38] Sampson, 1913a, p.12.
[39] Ibid.
[40] Sampson, 1908, p.19.

had barely begun to produce their flower stalks. This practice prevented seed production because the stock consumed the flower stalks before they had produced seed, and it decreased the range's carrying capacity.[41] The stockmen, in short, should no longer practice season-long grazing; if they wanted to graze on the National Forests they would have to practice deferred-rotation grazing. That meant that stockmen would have to supervise their stock animals constantly, and move them around from one area to another all through the grazing season. Up until now, cattlemen had just taken their cattle onto the ranges and left them to graze a certain area for as long as there was some vegetation, and sheep herders had allowed their sheep to consume virtually all the vegetation in one area before they moved onto a new area of the range. The Forest Service should no longer merely supervise when stockmen entered and left the Forests. Instead, Forest Rangers should assume the role of 'professional sheep and cattle herders' and systematically supervise the stockmen to ensure they practiced deferred-rotation grazing.

Sampson in these experiments successfully intertwined the range, the stockmen, the Forest Service's power, and the epistemic values of the social definition of research. He had

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[41] See Sampson, 1914, 116.

developed a grazing system the Forest Service could use to conserve the range, and he had developed an alternative to season-long grazing. In the final paper on these studies, Sampson discussed "the comparative merits of the three gazing systems [yearling grazing, yearlong rest, deferred-rotation grazing], from the standpoint of the requirements of the range plants for growth and reproduction."[42] He considered yearlong grazing to be the antithesis of a grazing system.

> The term "grazing system" implies a definite plan of utilizing the forage crop in accordance with certain basic principles. Yearlong or season-long grazing, however, is characterized by a lack of system, since it fails to provide for the removal of the herbage at any particular time in any locality. Its ultimate results to stock and the range are not considered.[43]

Deferred-rotation grazing, on the other hand, utilized the herbage in accordance with the basic principles of plant growth and reproduction, while simultaneously increasing stockmen's economic returns. Samspon thus satisfied both poles of the dual orientation because deferred-rotation grazing satisfied the epistemic and economic criteria simultaneously.

Sampson also legitimated and enhanced the power of both the Forest Service and professional applied scientists by constructing a barrier between range science and society.

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[42] Ibid., p.116.
[43] Ibid., p.116.

Only the professional researchers could develop knowledge that satisfied the economic and epistemic criteria simultaneously. The quadrat method enabled Sampson to study the causal relationship between grazing and the vegetation's requirements, and the quantification the method made possible provided the basis to predict the effects of various grazing practices. With this ability to predict, humans could extend their power over nature and control it to transform it according to their goals. However, the quadrat method also drew boundaries between the researchers and the rangers and stockmen because it denied the validity of common sense observations. Only professional experts could develop grazing systems based on basic principles because lay people did not possess the necessary skills.

These boundaries were acceptable within the institutional context of the Forest Service because they enabled the Forest Service to achieve its goals within its categorization of range management. These boundaries enabled Samspon to construct authority for his claims. Samspon's authority in turn enabled the Forest Service to justify its authority to mandate particular grazing practices, and once local rangers learned the basic principles of the quadrat method, they could apply it in demonstrations to convince stockmen of the validity of their claims. Sampson therefore did not experience the same problems as Bentley and Smith. Deferred-rotation grazing was gradually adopted, and by 1914 it became mandatory on most National Forests, at least in principle. Specific applications of the system remained a matter of negotiation between local Forest Service officials and local sheep or cattlemen's associations. Nevertheless, deferred-rotation grazing and the principles behind it provided the baseline for those negotiations.

Boundary-Work: The Question of Practicality

Samspon left a potentially significant opening to renegotiate the boundary demarcating range science from the rangers and the stockmen. Like Jardine, he examined the practicality of the grazing system he proposed from the perspective of the stockmen, and then strictly in economic terms. Sampson discussed the application of this system in actual range management, focussing on the rules and criteria for applying the system, and explaining what the stockmen should do, when, and how.[44] However, within the context of the Forest Service, Sampson's audience consisted not only of the stockmen, but also and more prominently included local Forest Service rangers. The rangers had to implement the system and supervise the stockmen to ensure they deferred and rotated their grazing. Implementing the pasture system

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[44] See Sampson, 1914, 143-146.

changed Forest Service practices in ways it had not anticipated. But at least the required changes were fairly obvious, most prominently the need to provide stockmen with some form of financial aid so they could construct fences.

Sampson did not specify what the rangers should do or how they could determine whether the stockmen indeed abided by the regulations. Following every cattleman and sheep herder around the range on the National Forests to supervise their activities would have been too time consuming and expensive, and the Forest Service did not have the personnel to undertake such a massive task. The rangers, in short, needed information about the stockmen's grazing practices without following them around and supervising them all the time. They needed some indicator of the causal connection between grazing and the range that would provide them with information they could use to evaluate stockmen's grazing practices, especially because Sampson's research denied the validity of the rangers' common sense observations. Without such an indicator, range researchers' ability to predict the impact of grazing on the range did not help the rangers perform their management tasks. Yet Sampson neglected to provide them with such an indicator, and rangers challenged the boundary demarcating range research by questioning the practicality of deferred-rotation grazing from their perspective. As part of their response, Sampson and Forest Service administrators reaffirmed the validity of deferred-rotation grazing and strengthened the boundary.

The critique of the grazing system's impracticality took a few years to appear, and in the meantime Sampson moved from Oregon to Utah to become the first Director of the Utah Experiment Station. After 1910, the Forest Service further institutionalized its research efforts by establishing several experiment stations to produce the knowledge base for natural resource management. In 1912, it established the Utah Experiment Station on the Manti (now Manti-La Salle) National Forest near the central Utah town of Ephraim.[45] The name of the station would change several times during its history, but it became popularly known as the Great Basin Station (GBS).[46] The Forest Service appointed Sampson, at that time the USDA's chief investigator for range research, the first Director of the Station, and for a number of years he was its only full-time employee, although he had several assistants during the summer.[47] Under Sampson, the GBS became the premier range research center in the U.S.. Officially, determining the causes of summertime floods originating in the West's

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[45] See Alexander, 1987a and 1987b, 412; and Keck, 1972a, 1. Also in 1912, the USDA's Bureau of Plant Industry created the Jornada Range Reserve in New Mexico. In 1915, the USDA transferred the Jornada Range Reserve from BPI to the Forest Service; see Ares, 1974.

[46] See Keck, 1972a, 1-2. In 1918, the name was changed to Great Basin Experiment Station, and currently the official name is the Great Basin Experimental Range.

[47] See Keck, 1972a, 7.

mountains was the Station's first objective. However, the Station's work from the beginning included range management research, and Sampson examined the causes of the summertime floods in terms of the relation between grazing and erosion, thus linking the floods and range management.

As director, he repeated the goal of the Forest Service when he outlined the mission of the Station. First, he explicitly and as his first objective linked research with the administration of the forests, arguing that the center should solve "problems involving general principles which have a direct bearing upon administration."[48] He did not specify what he meant with "general principles," but seems to have had in mind principles that would not be bound merely to any one particular local context. He thought that much of range research could be centralized in Utah because "the geographical location as well as the equipment for work are highly favorable."[49] The station had access to ranges covering just about every natural environment in the West, and hence was located ideally in a geographical sense to develop knowledge that could be applied in many areas. Sampson also thought that such centralization of the research would make possible its standardization, and argued that "in the event

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[48] Annual Report of the Utah Experiment Station, December 1913, p.29.

[49] Ibid., 29.

that the project is of such importance as to justify getting experimental data elsewhere than at the Experiment Station the study should be carried out under the direction of the Director in order that the work may be standardized."[50] Finally, the station should attempt to solve "problems of local importance" such as poison plant control and artificial revegetation, in which endeavors he thought the researchers at the station should cooperate closely with Supervisors of the various forests.[51]

In 1913, Sampson also reproduced the boundary between researchers and stockmen when he outlined the procedures to communicate the researchers' knowledge to the stockmen. The local population quickly took note of the GBS, and observing the researchers using the quadrat method "counting grass", called them the "Grass-Counters."[52] Sampson received requests for articles from local newspapers and national magazines such as the National Wool Grower. Accommodating those requests would, Sampson thought, help establish the reputation of the GBS, although he did not have the time to write the articles during his first years as Director. Nevertheless, in the annual report he described in general

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[50] Ibid., 30.

[51] Ibid., p.29; see also p.30.

[52] See Albert Antrei, 1971, "A Western Phenomenon - The Origin of Watershed Research: Manti, Utah, 1889, The American West, 8(2):42-47, 59.

terms the content such articles should contain. The stockmen wanted number of specific questions answered. They were particularly "anxious to know if we amount to anything and this will be judged by what we are doing."[53]

To answer the stockmen's questions, Sampson drew a distinction on the basis of the kind of information they should contain between articles published in newspapers and magazines and those published by the government. While "news items of a general nature should be prepared in the future," he stated that in such articles "Full data, such as will be used in a final government report, should in no case be given; rather the practical application of experiments will be discussed."[54] Sampson never explained his reasons for this distinction, apparently presuming them obvious. He followed his own counsel in articles published in the National Wool Grower.[55] In those articles he never cited a government report, except for rare references to a Farmer's Bulletin. That publication adopted much the same attitude about the kind of information they should contain because the USDA intended

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[53] "Annual Report of the Utah Experiment Station, Dec. 1913," p.32, Intermountain Station

[54] Ibid.

[55] Sampson, 1913b; 1918, "The Great Basin Experiment Station," National Wool Grower, 8(4):19-21. In addition, in 1916 Sampson published 6 articles in the National Wool Grower describing various grasses and poisonous plants on the range, and in each of those articles he adopted the same attitude towards the knowledge stockmen should know; for references, see Keck, 1972a, pp.46-47.

it to communicate knowledge to a lay audience. Neither Sampson nor the Forest Service or the USDA ever attempted to determine whether perhaps the stockmen were interested in more than just a general discussion of practical applications. In 1922, the Forest Service discovered that "many people, especially stockmen, do not read the department and other bulletins in a very general way," but without examining why this situation existed.[56] A year later it discovered something about the bulletins that may explain why people did not read them. The bulletins and other reports "have only limited circulation and seem to be "pretty hard to take" even for Forest Officers."[57] As a result, educating the public proved to be a difficult undertaking, and in 1927 Forest Service administrators and researchers still wondered whether range management was being sold effectively.[58]

Consciously or not, Sampson thereby extended the power of the range scientists and the Forest Service over the stockmen. He denied the validity of the stockmen's common sense observations, and then did not provide them information about the researchers' methods and data. Other members of the Forest Service and the USDA shared this attitude, most likely

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[56] Annual Report, Investigative Committee District Four, 1922, p.129.

[57] Annual Report, Investigative Committee District Four, 1923, p.93.

[58] Annual Report, Investigative Committee District Four, 1926, pp.56-62.

because the USDA almost from its inception encouraged publishing information in a manner farmers and stockmen could understand.[59] Articles in stockmen's journals by Jardine, USDA and Forest Service grazing expert and director of the Forest Service's Grazing Section Albert Potter, assistant forester Will C. Barnes, and others all follow the same pattern.[60] At the same time, institutional procedures in the Forest Service mandated the use of scientific methods and data as the foundation for grazing practices. Through the content of their communications with the stockmen, however, Sampson and the Forest Service denied the stockmen easy access to that information. Unless they changed their information-gathering behavior, the stockmen could only judge what the researchers did or critique a grazing system on grounds of practicality.

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[59] See, e.g., Everett M. Rogers, 1988, "The Intellectual Foundation and History of the Agricultural Extension Model," Knowledge: Creation, Diffusion, Utilization, 9(4):492-510.

[60] James T. Jardine, 1918, "Improvement and Maintenance of Far Western Ranges," American Sheep Breeder and Wool Grower, 38(7):427-430; 38(8):498-501 (which is unusual because it is the only one of these articles to actually cite a government document, namely Sampson, 1913); 38(10):635-638; Albert F. Potter, 1913, "Cooperation in Range Management," American National Cattleman's Association Proceedings, 16:55; idem, 1916, "Grazing Experiments on Federal Range Reserves," American Sheep Breeder, 36(2):74-75; Will C. Barnes, 1918, "Cattle and Sheep on the Same Range," National Wool Grower, 8(2):16; idem, 1920a, "The Sheepmen and the National Forests, American Sheep Breeder and Wool Grower, 40(2):124-125, 160-161; C.E. Fleming, 1920, "About our Western Grazing Grounds," American Sheep Breeder and Wool Grower, 40(1):29-30,101-105; C.N. Woods, 1920, "Conserving Range Forage," American Sheep Breeder and Wool Grower, 20(3):184-186, 236-241.

If not in intent, at least in effect, this attitude toward the kind of information considered pertinent to the stockmen extended Sampson's and the Forest Service's power by controlling the stockmen's access to knowledge. It limited the stockmen's ability to participate in decisions having a direct bearing on their behavior and economic life.[61]

Range research at the GBS meanwhile grew rapidly, and in important ways continued the work Sampson had begun in Oregon. Sampson's first progress report describes four grazing studies and four silvicultural studies initiated in 1912. Three of the four grazing studies dealt directly with management: "Relation of grazing to erosion and streamflow," "Effect of Grazing on the Reproduction of Aspen," and "Natural Revegetation," a continuation of the study begun in Oregon. The fourth study dealt with artificial reseeding, a topic that attracted attention because of its potential promise to restore the ranges fairly quickly. Not surprisingly, therefore, the twelve new projects Sampson suggested for study in 1913 included seven that focussed on artificial reseeding.

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[61] The stockmen's response is another issue, which, although fascinating, is not the point here. The point is that while developing the applied science, which had power consequences of its own, Sampson and the Forest Service also systematically constructed social boundaries that had an effect on power relations. Those boundaries included institutional procedures such as the kinds of information considered in decisions and the kinds of information considered pertinent to stockmen. For some discussion of the stockmen's response, see Ch.6.

The other new projects analyzed whether the planting of trees and shrubs aided in the prevention of erosion, the effect of grazing on the local water supply's purity, the relation between oak poisoning and stock losses, and Sampson thought a researcher from the Biological Survey should test methods to eradicate rodents, whom he blamed for denuding range lands. Finally, Sampson proposed a range management project to build on Jardine's researches in Oregon. Samspon wanted to convince stockmen to apply Jardine's recommendations and to study their effects on the life histories of forage plants. He hoped to feed the results into his own work on natural revegetation. No further records of this study exist, indicating it probably was not approved. He later included a study of the best methods to eradicate poisonous plants and climatic observations that would aid in the interpretation of the data collected in other studies.[62]

At the GBS, natural revegetation research and the deferred-rotation grazing system became the subject of an ongoing research program. Sampson used quadrats to determine the effect of grazing on the vegetation, especially on the life histories of the most important forage species. In the fiscal year 1915 report, he said he used the quadrats for

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[62] See Arthur W. Sampson, "Report on Progress of Experiments Initiated in 1912 and Plans for New Projects for 1913, Utah Experiment Station," pp.32-36, Intermountain Station, Annual Report Files.

"detailed life history studies of the forage species" in order "To ascertain most efficient management of range and stock, especially applicable in working out further principles pertaining to the rotation grazing system."[63] A year later he again indicated the importance of life history studies when he explained how he recorded the life history data: "On the reverse side of the quadrat form space is provided for recording the life history of the various important species."[64] In 1917, Sampson published a paper based on data from his research in Oregon, but emphasizing plant life histories rather than the deferred-rotation grazing system. Towards the end of the paper, however, he still argued that "The data compiled relative to the life-history performances of the different forage species have made possible the adoption of what is known as the "deferred or rotation grazing system.""[65]

One element distinguished the natural revegetation study from the other studies: Sampson thought its results generalized beyond the experimental environment's local conditions. Although centralizing the research at the Great

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[63] Great Basin Experiment Station, Fiscal Year 1915 Annual Report, p.160f.
[64] Great Basin Experiment Station, Fiscal Year 1916 Annual Report, p.111.
[65] Arthur W. Sampson, 1917b, "Important Range Plants: Their Life History and Forage Value," USDA Bulletin 545, p.57.

Basin Experiment Station allowed him to standardize research methods, Sampson recognized it also meant the results would not easily generalize to other environmental conditions. For example, he performed research on the relationship between grazing and aspen reproduction in the Manti Forest in Utah. He thought he could not generalize the results to conditions in the Uintah National Forest in Utah, slightly to the north of Manti, because "range conditions may be so different that the same range management would not work out satisfactorily."[66] The same limitation did not apply, however, to extending the results of the natural revegetation study to other areas. He performed his research in the Manti National Forest, and claimed the results applied to all Forests in District 4 of the Forest Service, which included Utah, Nevada, parts of Idaho and Wyoming, and a small section in the northwest of Arizona, and possibly to areas outside Region 4 as well. Environmental conditions vary considerably between Wyoming and northern Arizona or Nevada, and range conditions can differ drastically. Discussing the utility of the results from the natural revegetation study, he nevertheless claimed that "The methods of handling stock which give the best results to range, forage, and stock may be

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[66] Utah Experiment Station, Annual Report Dec. 1913, 39.

applicable in many localities, particularly in District 4."[67]

Sampson had reasons to think the results would generalize to different environmental conditions. He had success with the deferred-rotation grazing method in Oregon. In 1915, after three years of experimentation with natural revegetation in Utah, he claimed that "the range is reseeding satisfactorily and most important forage plants reproducing as rapidly as non-palatable species."[68] By 1916, Sampson's research into the deferred-rotation grazing system thus stretched back nine years and had shown its success in two very different environments, although he recognized one limitation. The local environment could influence how long a range took before revegetation was accomplished, and it took longer in Utah than in Oregon.[69] Another researcher also demonstrated the validity of Sampson's system. In 1915, L.H. Douglas reported on a demonstration experiment performed in the Hayden National Forest in Wyoming from 1910 to 1913. He designed the experiment to test whether the deferred-rotation grazing system could be applied in Wyoming's different natural environment. Douglas claimed deferred-rotation grazing worked in Wyoming: "The results of the test were essentially the same

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[67] Ibid., 113.

[68] Great Basin Experiment Station, Fiscal Year 1915 Annual Report, 160f.

[69] See Great Basin Experiment Station, Fiscal Year 1916 Annual Report, p.110.

as the results of the more intensive natural reseeding studies in Northern Oregon."[70] In 1917, Jardine and grazing examiner L.C. Hurtt reported on experiments performed at the Jornada Range Reserve in New Mexico between 1912 and 1916. On the basis of systematic observations with quadrats, they claimed their results demonstrated the possibility of naturally reseeding ranges through deferred-rotation grazing.[71]

Nevertheless, in 1916 two rangers in Wyoming questioned Sampson's system on grounds of both its practicality and its validity in Wyoming's environmental conditions. In 1916, Clarence E. Favre and W. Vincent Evans, two range managers at the Caribou National Forest located at the border between Wyoming and Idaho and part of District 4 of the Forest Service, directly challenged deferred-rotation grazing. Favre and Evans found this grazing system impractical. As Thomas Alexander points out, the critique "was practical rather than ideological."[72] Precisely, they did not question the Forest Service's right to manage the stockmen, but argued they could

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[70] L.H. Douglas, 1915, "Deferred and Rotation Grazing," National Wool Grower, 5(10):11-14, on p.14.

[71] James T. Jardine and L.C. Hurtt, 1917, "Increased Cattle Production on Southwestern Ranges," USDA, Bulletin 588, pp.4-9. It is not clear how early Sampson knew of their results, but knowledge was communicated informally in the Forest Service. Sampson, for example, cited Douglas' study a year before it was published; in Sampson, 1914, p.94.

[72] Alexander, 1987a, p.81. Alexander analyzes Favre and Evans' study in some detail. My summary is based on Alexander, but he does not analyze the response of Samspon and other members of the Forest Service.

not perform that task. Without some tool to determine whether stockmen practiced deferred-rotation grazing, rangers could only supervise the stockmen by following them around the range. It therefore required "an extra large amount of supervision."[73] In addition, they questioned whether deferred-rotation grazing was appropriate for all types of ranges. Although it might be appropriate for grass ranges, they argued it was not appropriate for weed ranges like the Caribou. Consequently, they totally rejected rotation grazing, and limited deferred grazing to instances where the range had been badly overgrazed.

Their critique undermined the role and status of range science in the Forest Service because they claimed Sampson had failed to satisfy the dual orientation of range science as defined in the Forest Service. On the one hand, by questioning deferred-rotation grazing's general validity, they denied Sampson had satisfied the epistemic values of the social definition of research. Range science had not proven able to predict the impact of grazing on all ranges, and hence had not proven able to develop a universally valid grazing system. On the other hand, by questioning the system's practicality, they denied Sampson had developed a system enabling rangers to manage the stockmen so the stockmen could

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[73] Quoted in Alexander, 1987b, p.421.

maximize their profits. Range science, for all the claims that it could develop general knowledge with predictive power while simultaneously satisfying the stockmen's economic needs, had produced an impractical system from the rangers' perspective. Their attack thus struck at the heart of the way Sampson had instantiated the social definition of research. Yet the Forest Service used range science as the basis for its actions and to justify its authority and actions. They did not have to wait long for a response.

In December of 1916, Samspon and the supervisors of Favre and Evans in the bureaucratic hierarchy, collectively known as the Investigative Committee of District 4 of the Forest Service, decided to discontinue this study. The Forest Service in 1912 created a Central Investigative Committee to oversee its research efforts. To decentralize the Forest Service's work, including its research, so it could respond more effectively to local problems, each District then established its own Investigative Committee. Part of each Investigative Committee's task consisted in distinguishing between "investigative studies," which performed research on fundamental principles, and "administrative studies," defined as studies seeking to fit knowledge (e.g., grazing systems) into local conditions. But as Favre and Evans' critique indicates, the Committee had not yet successfully established the boundaries between investigative and administrative studies. In 1915, Henry Graves, Chief of the Forest Service, abolished these Investigative Committees and established the Branch of Research in order to separate research and administration and give research the recognition he felt it deserved.[74] However, at the GBS and in District 4, the Committee continued to function.[75] Furthermore, the Committee in 1916 argued explicitly that it should continue to function because it enabled the establishment of links between research and administration. According to the Committee, "The present method of procedure in reviewing all investigative work, both past, present, and contemplated, will ensure a better correlation of research work with the administrative work."[76]

The Investigative Committee justified its action on several grounds. First, it reaffirmed the benefits of deferred-rotation grazing. The Committee argued the success rangers had achieved in naturally revegetating the range was due to "the application of the principle of deferred grazing."[77] They then drew their own conclusion from the Caribou study, arguing that "further refinement in the

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[74] Steen, 1976, 137.

[75] Alexander, 1987b, 413, shows that Graves still "expected field research to function in cooperation with the regional foresters."

[76] Annual Report, Investigative Committee District Four, Dec., 1916, 14.

[77] Annual report, Investigative Committee District Four, Dec. 1916, 56.

practice of deferred grazing on sheep allotments [at the Caribou] is unnecessary."[78] The deferred grazing system was universally valid, irregardless of local environmental conditions. The Committee recommended that rangers apply the principle "on all areas in need of revegetation or on areas where the grazing is close enough to warrant protective measures."[79] Therefore, through their support of the system they declared the challenge invalid, without, however, providing any arguments or data to support their claims. The expertise of Sampson and the other members of the Committee apparently justified their authority to make such judgments.

The Committee drew a social boundary to demarcate range science from the rangers' job on the basis of the qualifications of the investigators. Favre, to whom the project was officially assigned, was not a researcher, and the study would not lead to general knowledge. "Since the grazing assistant assigned to this work will be doing administrative work mainly and since the study does not aim at the determination of principles of general application it is deemed advisable to consider this project in the future as administrative in character."[80] Administrative work could include administrative studies, and as such the project

[78] Ibid.
[79] Ibid.
[80] Ibid.

continued, but it could no longer provide the basis for a challenge to the researchers' knowledge. Furthermore, no one raised any objections to Favre when he initiated the study to determine the grazing system best suited to the Caribou and tested deferred-rotation grazing in 1915, or when he and Evans continued the study in 1916. Favre's qualifications became an issue only after he and Evans challenged deferred-rotation grazing. It is possible the Committee took their interpretation of the results of their study as an indication that they were not qualified to challenge deferred-rotation grazing, but there is no evidence in the records to support such a view. Favre also clearly was a competent range manager. The challenge did not hurt his career; in the last years of the decade the Forest Service appointed him supervisor of the Humboldt Forest, where under different environmental conditions he applied deferred-rotation grazing, and by 1939 he was the assistant regional forester in charge of wildlife and range management for the Intermountain Region of the Forest Service.[81] In any case, this social boundary had epistemic consequences: the challenge to the practicality and generality of deferred-rotation grazing was not worth pursuing further because the person who raised it lacked the necessary qualifications.

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[81] See Alexander, 1987a, p.81.

The Committee also re-articulated the purpose and objective of range research in the Forest Service and defined who should perform it. They limited its scope, arguing previous research had focussed on too wide a variety of topics and that not all of them had equal importance. To correct that problem they thought their "future tendency should be toward a concentration on a few of the fundamentally important studies," although it did not define what it meant with fundamental studies.[82] Not everyone should engage in such research, and Graves' reorganization to enhance the status of research may well have facilitated this move. Prior to this time, no one at the GBS or in the Intermountain Region had claimed explicitly that only certain people could perform certain investigations. Now, however, only "special research personnel" should undertake those studies.[83] Local rangers should not worry about these type of studies but should focus instead on administrative studies, and develop ways to apply the knowledge of range research.

The Committee thereby completed the institutionalization of range research, and only other members of the select group of research personnel could question, critique, or challenge knowledge developed by range scientists. Just like the

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[82] Annual report, Investigative Committee District Four, Dec. 1916, 2.
[83] Ibid., 2.

stockmen, local rangers could not legitimately challenge the researchers because they lacked the necessary knowledge and credentials and therefore did not belong to that select group. The blame for lack of improvement in the condition of a range, moreover, would consequently also fall on the shoulders of local rangers. If a range failed to improve, it must mean rangers had failed to take the actions mandated by the general principles developed in fundamental studies. Sampson was very clear on this point. Noting the lack of improvement achieved on the Manti National Forest after six years, he blamed the rangers for failing to take the appropriate actions, while in this case absolving their supervisor. "This lack of action ... is not believed to be due to inactivity on the part of the Supervisor, who, I believe to be in sympathy with the Station's deductions and recommendations concerning this matter, but rather to the person in charge of the particular Forest district concerned."[84]

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[84] Utah Experiment Station, Annual Report Fiscal Year 1918, p.4. Two unknown readers of this document made some interesting comments in the margin. One agreed with Sampson, calling it "strong language," and claiming "we need more of it." The other disagreed with Samspon and with the first commentator, noting that the problem resided in a lack of cooperation. Underneath the first reader's comments, he stated: "not strong language [;] but better cooperation." Apparently, at least the assignment of blame remained somewhat negotiable.

Range Science as Applied Ecology, and a New Management Tool

Finally, the negotiations over the boundary affected what happened inside the boundary: in response to the critique by Favre and Evans, Sampson placed range science on an applied ecological foundation. Sampson moved succession from the background to the foreground, and the concept of succession became the theoretical foundation for deferred-rotation grazing and for range science. In range research, Smith in 1899 already linked deferred grazing to succession, but Sampson never credited or cited him on this point. In the context surrounding range research in the Forest Service, the theory of succession simultaneously solidified the boundary. Challenges to deferred-rotation grazing and other knowledge range scientists produced henceforth required an understanding of succession. At the same time, Sampson's modifications of the theory of succession changed it from an ecological to an applied ecological theory and thereby served to distinguish the range scientists from the plant ecologists.

From the theory of succession, Sampson derived the concept of plant indicators, and plant indicators made deferred-rotation grazing practical by providing rangers with the tool they needed to manage the stockmen. The concept of plant indicators was nothing new, and Smith had already linked it to the succession of plant species and applied it to range management in 1899, but here again Sampson never cited nor credited Smith.[85] Under this concept, plants served as a measure of the cause-effect relationship between grazing and the vegetation. The effect, the vegetation, served as an indicator of the range's condition, and thus served as an indicator of the cause that caused the condition. In a review of the concept in 1939, Sampson stated that "The plant indicator concept is based on a cause-effect relationship, where the effect is taken as a sign of the cause."[86] Initially in 1919, due to the focus on grazing systems, for Sampson the cause was primarily grazing. Rangers could then determine if stockmen changed their behavior, abided by Forest Service policies, and practiced deferred-rotation grazing simply by looking at the vegetation, provided they learned to look through the eyes of a range scientist.

To emphasize succession, Sampson appealed to the theory of succession in the nascent discipline of ecology. Frederic

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[85] Smith, 1899, p.28; Smith discussed how as a result of overgrazing the blue stem or sage grasses had first been replaced by the dog grasses or needle grasses and then by the mesquite grasses, and then concluded that "The occurrence of any one of these as the dominant or most conspicuous grass is to some extent an index of the state of the land and of what stage in overstocking and deterioration has been reached." In addition, H.L. Shantz, a USDA plant physiologist, had already argued that the vegetation could serve as an indication of the condition of the land, in particular whether it could produce crops; H.L. Shantz, 1911, "Natural Vegetation as an Indicator of the Capabilities of Land for Crop Production in the Great Plains Area," USDA, Bureau of Plant Industry, Bulletin 201. Sampson did cite Shantz' study.

[86] Arthur W. Sampson, 1939, p.156.

Clements, Sampson's teacher, developed the theory of succession as the first theory for plant ecology. He developed his concept of succession between 1900 and 1916, first publishing Research Methods in Ecology in 1905, and in 1916 publishing Plant Succession, which quickly became one of the most important books in plant ecology. In it, he argued that succession was the "universal process of plant formation development."[87] Clements also acknowledged that Smith in 1899 had already used a concept similar to succession and to plant indicators.[88] At about the same time, Henry Chandler Cowles, a University of Chicago biologist, developed an alternative concept of succession, but Clements' theory of succession came to dominate American plant ecology. Sampson, however, did not just adopt either his teacher's or Cowles' version of succession. In 1919, he developed a hybrid concept of succession out of their conceptions. In the process he rejected significant elements of both men's views, though particularly of Clements' theory, bringing his views closer to Cowles' concept.[89]

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[87] Frederic 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.

[88] See Clements, 1928, p.225.

[89] On Clements and Cowles, see Cittadino (1980), Tobey (1981), and Hagen (1988); what follows is based on these authors. Tobey, however, implies that Clements' theory provided the basis for the applied ecological foundation of range science, and Rowley (1985) follows Tobey on this point.

Frederic Clements had a very deterministic, organismic, and dynamic view of succession and of nature. He thought of plant formations as organisms that developed through a life cycle: growth, maturity, decline, and death, when they were replaced by the next plant formation, which invaded the area being vacated by the previous formation. On the basis of the quadrat method, Clements identified two primary causal agents responsible for changes in vegetative formations, for succession: climate and the impact of the vegetation on the environment preparing the habitat for the next vegetational stage. Joel Hagen therefore characterizes Clements' view as a stimulus-response model of succession.[90] A climatic stimulus such as light intensity or humidity caused the invasion of new species into the plant formation. The plant formation responded by reacting on its physical habitat, changing the habitat in the process, and thereby preparing the habitat for the invasion of new species. Under the influence of those causal agents, successive plant formations succeeded each other in stages. Clements thought of the course of development as virtually inevitable and clearly directional, progressive rather than random, ultimately culminating with

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In many ways, they thereby inadvertently perpetuate the image that applied science is merely the application of ideas developed in the sciences. But Sampson developed his own conception of succession, and it had more in common with Cowles' theory than Clements's.

[90] Hagen (1988).

the 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."[91] In the climax stage the plant formation was in equilibrium with its environment.

Regressive or retrogressive succession could not occur, and Clements explicitly ruled out the possibility that human actions like grazing could cause regressive succession. For Clements, humans occupied a position separate from nature, and hence could not interfere drastically in the processes of nature. At best, humans could cause a temporary interruption in those processes. "It is conceivable that lumbering, grazing, and fire might cooperate to produce artificial regression, but there is nowhere evidence that this is the

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[91] Clements, 1928, 147. 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). Therefore, to gain a deeper understanding of the history of ecology, an interesting question, beyond the scope of this analysis, is to what extent Smith influenced Clements.

case."[92] 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 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.[93]

H.C. Cowles disagreed with Clements on some fundamental points, in particular Clements's determinism and his claim that succession would ultimately produce a stable climax.[94] In contrast to Clements, Cowles in a more Darwinian vein emphasized the competition inherent in the struggle for survival as the primary cause of succession, and down-played the causal efficacy of climate. As a result of this emphasis on competitive relationships, Cowles argued that succession was far less deterministic than Clements claimed. There was nothing inevitable or progressive about the succession of plant formations. Rather, they succeeded each other as different species gained an advantage in the struggle for

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[92] Ibid.

[93] 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.

[94] The following discussion of Cowles criticisms is based on Tobey (1981, pp.99-108), and Hagen (1988, pp.270-271).

survival. Cowles also read Clements's determinism as a form of teleology, which bothered him because he thought the idea of a purpose in nature would hurt the reputation of ecology. Finally, Cowles claimed that the stable climax was an illusion. As Ronald Tobey argues, this rejection of the deterministic march of succession towards the stable climax was a rejection of basic principles of Clementsian succession: directionality and irreversability.[95]

Sampson constructed an alternative conception of succession out of these two views. In contrast to Clements, he argued that retrogressive succession occurred and that humans could cause it, and in contrast to Cowles, he accepted a stable climax in equilibrium with its environment. In 1916, Sampson brought succession to the foreground to protect and justify his system. However, at this time he did not propose an explicit conception of succession. Nor did he distance himself explicitly from either Cowles or Clements, and he would never openly take issue with Cowles. Instead, at this stage, he merely justified deferred-rotation grazing on the basis of ecological succession, although he already differed clearly from both men. But not until 1919 did he articulate precisely how succession as he conceived it related to range management, and only then did he use it to develop a

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[95] Tobey, 1981, p.104.

management tool rangers could use.

In December 1916, the Investigative Committee prepared its annual report, and deferred-rotation grazing suddenly became linked explicitly with succession. Sampson never mentioned it in any of his reports before. Now, however, the goal of the natural revegetation study became to determine "the most expedient method of reseeding depleted ranges to natural range plants, and especially to ascertain the rate of succession of the climax species."[96] Determining the rate of the climax vegetation's succession had never before been part of the research, which had focussed solely on developing a grazing system to balance the requirements of the stock with the growth requirements of the vegetation.

Succession also served to justify the general validity of deferred-rotation grazing, and thus to answer Favre and Evans'

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[96] Annual report, Investigative Committee District Four, Dec. 1916, p.57. Keck, 1972a, p.8, claims that a review of the diaries of one of Sampson's assistants, Leon H. Weyl, reveals that they were performing succession studies in 1916 and 1917. That would fit with the argument presented here. Furthermore, in 1920 Sampson claimed he began the succession studies in the Spring of 1917; see annual report, fiscal year 1917, p.14. My argument that Favre and Evans' challenge caused Sampson to emphasize the theory of succession (and then to develop the concept of plant indicators as it applied to range management), to some extent commits the post hoc, ergo propter hoc fallacy. However, too many factors point to this causal connection, in particular because the ideas of succession and of plant indicators had already been available in the literature, even before Sampson began his experiments in Oregon, yet he never explicitly appealed to them before the challenge.

critique that it might not apply on the Caribou. Research with the quadrats had shown the existence of three types of succession: progressive, retrogressive, and problematic succession. Progressive succession occurred on a range that showed improvement, and retrogressive succession on a range that was declining, usually because the soil was eroded or eroding and thus less fertile. Succession was problematic when it could go either way, which tended to occur on ranges with soil of intermediate fertility and where grazing interfered with the palatable vegetation's seed crop. Improving the soil was the first requirement for reseeding depleted lands. "The depleted soils will have to be improved by the elimination of destructive grazing and by deferred and rotation grazing." Deferred-rotation grazing made possible "The usual succession of annual weed and shallow rooted perennial species." Ultimately, through the reaction of these new plant formations on their habitat, "The vegetable matter annually accumulated will then pave the way for the invasion of the climax perennial forage type."[97] Deferred-rotation grazing provided the only way to ensure progressive succession. Therefore, only deferred-rotation grazing could improve or maintain the condition of the range, depending on

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[97] Annual report, Investigative Committee District Four, Dec. 1916, 58; Sampson repeated these arguments again in the Annual report fiscal year 1917, 7-8.

whether it was still moving towards or had already reached the climax stage. By shifting from the micro level of plant life histories to the macro level of succession, Sampson showed the necessity of applying deferred-rotation grazing, regardless of the local environment.

Early in 1917, Sampson argued succession provided the crucial scientific foundation for range science and range management. In February, he read a very programmatic paper on succession before the Botanical Society of Washington, D.C.. He stressed the need to use quadrats in range studies. Generating data, however, was only half the process of knowledge development; the researcher needed to "analyze and crystallize the data into tangible and usable form."[98] Up until 1916, the need to develop a grazing system had provided the interpretive background, but no longer. Instead, he claimed he found it useful to "divide the data obtained from the various quadrats into three groups" depending on whether the data demonstrated progressive, retrogressive, or problematic succession.[99] He briefly explained those concepts, and concluded by emphasizing the importance of succession. Range management was "gradually being placed upon a scientific basis in which applied ecology, and particularly

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[98] Arthur W. Sampson, 1917a, p.594.
[99] Ibid.

succession plays a prominent and highly important part."[100]

The turn to applied ecology also reinforced the boundary demarcating range scientists from society and enhanced their authority. Utilizing ecology required expertise. "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 coupled with their growth requirements."[101] Only range scientists or ecologists could develop such knowledge, and only they could develop or critique grazing systems based on that knowledge.

In 1919, Sampson published his major paper on succession, and disagreed with Clements on conceptual grounds. Sampson argued succession moved through a number of stages, distinguished on the basis of the dominant vegetation they contained. Succession proceeded from the pioneer stage through the transitional stage to the first weed stage, then to the second weed stage, and finally to the climax stage. This climax stage was the grass stage, and hence from the perspective of ecology really a subclimax, as Sampson recognized. The true climax stage, the tree stage, however, was not important from the perspective of range science and

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[100] Ibid., p.596.
[101] Ibid., 595.

management, and Sampson often called the grass stage the climax, and at other times the subclimax. Stages could succeed each other in both directions, from the first to the second weed stage to the climax, but also in reverse.

Sampson explicitly said his use of the concept and term therefore differed from Clements, and explained why. Although he had earlier said the data indicated retrogressive succession, he now said he had a conceptual problem with Clements's theory arising from the etiology and definition of the word.

> Coming as it does from the Latin verb "succedo," meaning literally "I go under," the word "succeed" originally had nothing to do with the superiority of one crop over another. Thus, succession is here considered in the sense to "follow," "take the place of," etc., and is applied in a vegetative invasional sense.[102]

Nothing in the definition of the word implied invasions could

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[102] Sampson, 1919b, p.5, n.2. We are left wondering why Sampson chose to take issue with Clements in this fashion. A plausible explanation may be because Clements did not interpret the data in the same way, instead developing "rather convoluted explanations for changes in vegetation that appeared to other ecologists to be retrogressive."(Hagen, 1988, p.273) Sampson circumvented Clements' interpretations by shifting the debate from the data to the definition of the concept. Clements continued to think succession could only progress, and thus apparently was not convinced by Sampson's argument. Unfortunately, Clements never responded publicly, Sampson apparently considered the issue closed and never returned to the issue, Sampson's private papers have been lost, and although the Clements papers contain some letters between Clements and Sampson, they never contain any scientific discussions; Frederick E. Clements Collection, American Heritage Center, University of Wyoming, Laramie, Wyoming.

only be progressive; instead, they could be either progressive or retrogressive.

Primarily due to the management orientation of his research, Sampson did think the process of succession ultimately culminated in a stable climax vegetation. In that sense he agreed with Clements rather than with Cowles. In part, his training under Clements may perhaps explain why he accepted the stable climax. If that was the case, the management orientation of range science reinforced his training. After all, the Forest Service had set conservation as its goal; i.e., to manage the stockmen in order to maximize their profits while simultaneously improving or maintaining the condition of the range to guarantee future maximum economic returns. This goal presupposed that order could somehow be brought to the range, that researchers could develop a grazing system balancing the requirements of the stock and the vegetation so the stockmen could use the range now and in the future. It presupposed, in short, that researchers could strike a stable balance between the stock and the vegetation. The balance Sampson struck between Clements and Cowles by conceiving of succession in terms of a process that could either progress or regress and that could potentially progress to a stable climax thus reproduced the institutional goal of research.

In this paper, Sampson related succession to range management in order to develop a measurement of the relationship between grazing and changes in the vegetation. Faulty management such as yearlong grazing or overstocking had decreased the carrying capacity of the range. The problem was recognizing faulty management. Rangers and stockmen lacked "a means of recognizing overgrazing in its early stages."[103] As a result, they could not change their faulty management in time to prevent the carrying capacity from decreasing, necessitating more drastic actions later.

Developing that measurement required the denial of common sense observations and their replacement with observations based on applied ecology. Until this time, "the stockmen and those regulating grazing have essentially relied upon general observations of the abundance and luxuriance of the forage supply and upon the condition of the stock grazed."[104] However, these measures were not sensitive enough. Realizing the problems with these observations, "Enterprising stockmen and those concerned with the administration of grazing," recognized the need for "a finer discrimination than mere observation of the density of the plant cover and the condition of the stock."[105] Understanding the principles of succession provided that finer discrimination. Overgrazing

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[103] Sampson, 1919, p.1.
[104] Ibid.., p.1-2.
[105] Ibid., p.2.

caused successional changes, and therefore the earliest indication of a change in the stage of succession would provide rangers and stockmen the measure they needed. The invasion of a new species into a plant formation provided that measure. According to Sampson, "the incoming species are the most reliable indicators of small departures from the normal carrying capacity of the range."[106]

This applied orientation shaped Sampson's discussion of succession, and as a result economics and ecology became intertwined such that the concept satisfied both simultaneously. Sampson equated the range's ability to produce economic benefits, its carrying capacity, with its ecological condition.[107] At the same time, if humans scientifically managed their stock they could manipulate the ecological condition to maximize their economic returns. However, an ecological change would have immediate economic consequences, and an economic change would have immediate ecological consequences.

Sampson classified the vegetation into four major classes, called consociations, three of which matched successional stages and one that fell between two stages.

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[106] Ibid.

[107] On this point, see also E. Lamar Smith, 1984, "Use of Inventory and Monitoring Data for Range Management Purposes," in National Research Council/National Academy of Sciences, Developing Strategies for Rangeland Management (Boulder: Westview Press), pp. 808-842.

From the subclimax down, he called these the wheat-grass consociation, porcupine-grass-yellow-brush consociation, foxglove-sweet-sage-yarrow consociation, and the ruderal-early-weed consociation. He characterized these consociations according to the dominant species. The wheat-grass consociation was the (sub)climax, the foxglove-sweet-sage-yarrow consociation belonged to the second weed stage, and the ruderal-early-weed consociation belonged to the first weed stage. These three thus belonged to the stages of development that succession proceeded through on its way to the climax. The porcupine-grass-yellow-brush consociation did not fit any one of those stages. It fell between the climax stage and the second weed stage, and he proposed a separate stage for it, the mixed-grass-and-weed stage.

The goal of range management became essentially fostering progressive succession to achieve the highest successional stage and thus to maximize economic returns. Sampson analyzed the ecological characteristics and economic value of each stage. He described the preferred habitat, the conditions for the growth and reproduction of the predominant species, their physiology, and factors that could disturb them. To determine the economic value of each stage, he determined how much forage each produced, which he measured in terms of the amount of dry weight. He also followed cattle and sheep around the range to determine the palatability of the forage in each stage. In general, he concluded that "The grazing value of the vegetative covers [characteristic of each stage] is essentially determined by the stage of succession."[108] To achieve maximum economic returns, therefore, the range should be managed to establish the highest stage of succession.

However, when Sampson first outlined the stages of succession, he only considered the causal relationships between the vegetation and its habitat. Although many factors contributed to the change from one stage to the next, Sampson considered the reaction of the plant formation on the soil the most important. "The plants themselves, by adding humus to the soil through the decomposition of their tissues, and in this way changing the physical and chemical composition of the soil, prepare the way for a new and higher form of life, hence in a way work out their own destruction."[109] As a result of this causal relationship, the vegetation proceeded from the pioneer stage through the first weed stage and into the second weed stage. The second weed stage then prepared the soil for the climax stage. Sampson was very specific on this causal connection: "By the time the second weed stage has had its growth and has thus prepared the way for the next stage of plants the soil is sufficiently decomposed and contains sufficient organic matter and soil moisture to make possible

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[108] Sampson, 1919b, p.72.
[109] Ibid., p.3.

the establishment of the climax or the subclimax grass cover."[110] The vegetation would then be in a stable equilibrium with its habitat.

That causal scenario was too simplistic because it did not include humans, and hence was not an applied ecological explanation of the causes of succession. Humans occupied a position outside of nature when he described the causal relationship responsible for succession in terms of the reaction of the vegetation on its environment. In that sense, this was an ecological analysis a la Clements, rather than an applied ecological analysis. Yet his previous studies, and the range's deterioration as a result of the stockmen's grazing practices, demonstrated that human actions are a part of the environment. Sampson therefore had to adjust his causal account. Although he recognized other factors could also play a role, he then constructed a virtually mono-causal account that attributed retrogressive succession almost solely to grazing. He claimed that "overgrazing or other faulty management is usually accountable for the retrogression on range lands as a whole."[111] Throughout his study, he therefore focussed almost exclusively on grazing as the cause for succession, and the impact of grazing on succession was mediated through its effect on the soil.

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[110] Ibid., p.5.
[111] Ibid., p.6.

When Sampson included humans in the causal process, he had to re-classify the stages of succession and modify management practices accordingly. When he systematically observed the interrelationship between grazing and the vegetation, he recognized another type of vegetation that did not fit the stages of succession he described initially, the porcupine-grass-yellow brush consociation. Hence he added another stage to the successional process, the mixed-grass-and weed-stage. As a result, he had to qualify his statements about managing the range. Management should not attempt to achieve the climax stage because the climax stage did not consider humans as part of the causal relationships. If the condition of the range had deteriorated to the second weed stage or below, management should promote progressive succession. Nevertheless, although the wheat-grass consociation represented the highest type in terms of the successional process, management should not promote progressive succession until the range had achieved the climax stage. "In general, there is little or no justification for handling the lands so as to maintain a more or less pure wheat-grass cover."[112]

To maximize economic returns and to maintain ecological stability on the range, range management should attempt to

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[112] Ibid., p.74.

achieve or maintain the mixed-grass-and-weed-stage. The climax stage did not satisfy human purposes and ecologically was not the most stable stage when Sampson included grazing in the successional process. Instead, the porcupine-grass-yellow brush consociation "is the second highest and the most stable forage type," and it also "affords the most nutritious forage cover, and will probably support more stock than will any other stage of plant development."[113] Sampson made an error in his reasoning at this point by equating palatability and nutrition. He had not measured nutrition. Instead, he measured palatability, the preference of stock for certain kinds of vegetation. Those two concepts are very different and they measure something different, and his claim that this consociation provided the most nutritious vegetation was

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[113] Ibid., pp.68,69. Sampson has been misunderstood on this point by subsequent range researchers and range managers (see e.g., the review in E. Lamar Smith, 1984). They think Sampson simply wanted management to foster progressive succession until the climax had been reached because the climax was the most productive stage. They also argue that management to achieve the climax introduced a "cow bias" into range management because the grasses typical of the climax are more palatable to cows than to sheep. Although these critics realize that Samspon based the economic value of the range on its ecological condition, they do not realize that Sampson recognized that the climax was not the most productive stage for all classes of stock. He recognized that the climax was neither economically nor ecologically stable when humans were considered part of the causal process, and he therefore did not advocate management to achieve the climax. At any rate, this misunderstanding demonstrates that (range) scientists do not control how others will use the knowledge they propose, or the meaning others ascribe to it.

therefore not justified.[114] Nevertheless, this assumption was crucial to justify his claim that managers should manage the stock, and thus the stockmen, to achieve the mixed-grass-and-weed stage. It enabled Sampson to equate the ecological and economic condition of the range.

Only the mixed-grass-and-weed stage was economically and ecologically stable. If, for example, a stockman grazed only cattle and horses, he might still want to attempt to foster or maintain the climax because that stage produced the most palatable dry feed for those animals. Conversely, the second weed stage produced the most palatable feed for sheep. However, a stockman could not maintain either of those stages while cropping the vegetation exclusively with cattle or sheep.

> "As a rule, when the most stable grass type is cropped by cattle and horses alone, it is soon sufficiently opened up to permit the establishment of at least a moderate proportion of weed plants, most of which are highly palatable to sheep but which may be grazed little or nor at all by cattle or horses. Likewise a weed cover grazed exclusively by sheep will sooner or later change to the grass stage."[115]

Both the climax and second weed stage (and all lower stages) were economically and ecologically unstable. Grazing only cattle on the range caused the invasion of weeds and then

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[114] After all, many people find ice cream and chocolate very palatable, but they are certainly not the most nutritious foods.

[115] Ibid., pp.72-73.

reduced the economic returns to the stockmen because the cattle would not benefit as much from the changed vegetation on the range. When Sampson included grazing in the causal process, only the mixed-grass-and-weed stage was stable, whether from an economic or ecological perspective.

Achieving the mixed-grass-and-weed stage meant controlling and changing human actions. In part, that meant deferred-rotation grazing because it interfered least in plant life histories, thus enabling the desired species to reproduce themselves. When necessary, deferred-rotation grazing would foster progressive succession; and if management had already achieved the mixed-weed-and-grass-stage, this grazing system would maintain the range in this optimum condition.

In addition to deferred-rotation grazing, the equation of ecology and economics also meant "common-use" of the ranges. Stockmen should no longer just graze either cattle and horses or sheep on the ranges. The Forest Service, furthermore, should not assign a certain section of the range on a National Forest exclusively to one stockman if that stockman grazed only cattle and horses, or grazed only sheep. Such a grazing practice would not maintain the range nor produce the greatest economic returns. As a result, those practices were not efficient, and Sampson thought range management should achieve the greatest possible efficiency. He argued that "The highest grazing efficiency consists in getting the greatest possible use out of the range from year to year."[116] Grazing only cattle and horses, or only sheep, did not extract the greatest possible use from the range because either class of animals would leave some of the vegetation untouched. Nor would grazing only one class of animals produce the greatest returns from year to year. Therefore, "the highest grazing efficiency is obtained through "combination" or "common-use" grazing, that is, the grazing of cattle, sheep, and horses."[117]

From this redefinition of succession, Sampson derived the concept of plant indicators, which simultaneously indicated the range's ecological and economic condition. The basic idea behind the idea of plant indicators was very simple. Sampson characterized each consociation on the basis of the dominant species within it. Since Sampson equated ecology and economics, those dominant species then served to indicate simultaneously the ecological and economic condition of the range. Furthermore, Sampson focussed almost exclusively on grazing as a cause of succession. The appearance or invasion of a species characteristic of another stage thus revealed the "effect of a given method of grazing on the plant cover."[118] For example, if a species of the second weed stage invaded a range characterized primarily by the mixed-grass-and-weed-

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[116] Ibid., p.54.
[117] Ibid., p.69.
[118] Ibid., p.73.

stage, it meant the ecological and economic condition of the range was deteriorating under the current system of grazing. On the other hand, if a species of the second weed stage invaded a range characterized primarily by species of the first weed stage, it indicated that the current grazing system worked because the range's ecological and economic condition was improving.

The plant indicator concept provided rangers with the tool they needed to supervise the stockmen. It had obvious management implications that Sampson could teach the rangers: "Where the negative indicators are crowding out the more permanent and desirable species, remedial measures should be adopted with a minimum loss of time."[119] The rangers could simply look at the invading species to determine the condition and trend (change in condition) of the range. Since grazing was the primary cause behind succession, the plant indicator served as a measurement indicating whether or not the stockmen practiced the required grazing system. Not surprisingly, therefore, the rangers embraced plant indicators enthusiastically. According to Sampson, "Probably the principles developed in connection with the "plant indicator" study were received with more enthusiasm than were those of

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[119] Ibid., p.73.

any other experiment."[120] Once they learned how to establish and read small plots to measure the ecological condition and trend, rangers could practice deferred-rotation grazing, and they could also quickly determine whether a stockman had overstocked a section of range.

In many respects, the plant indicator concept brought to ranching the transformations that had already swept through most American industries. To the newly trained eye of the ranger, the range revealed all. The flow of information could now proceed unhindered, from the range scientists to the rangers, from the rangers to the stockmen, and then from the range back to the rangers, from whom it flowed again to the stockmen. This concept thereby transformed the range, the stockmen, and their interactions into one machine, much like McCallum had transformed the railroads into a machine more than a half century earlier. The Forest Service's administrators and rangers could now oversee and manage the machine. In that sense, we can perhaps say range science industrialized ranching by rationalizing and organizing the stockmen's interactions with the range. Ranching was now part of an organized and rationally managed industrial economy.

Sampson's studies at the GBS also laid the foundation for further research, in some of which the tension between science

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[120] Annual Report, FY 1918, p.80.

and society reappeared. Sampson realized his research should develop knowledge that satisfied economic and scientific criteria simultaneously. But at times he wanted to derive goals and questions for future research from his previous research. The Forest Service, on the other hand, wanted the researchers to continue to derive their questions from the originally established goal and within the framework of the categorization of range science that went along with it. It apparently believed researchers could satisfy the dual orientation only by sticking with this research strategy.

Sampson proposed to study the process of succession in more detail. He hoped thereby to develop the plant indicator concept further, and to examine the causes of succession and the rate at which plant formations succeeded each other as a result of various causes. Although elements of this project were approved, it suffered from a lack of funding. The more applied aspects of the project finally received funding, but not as a separate project, as Sampson wanted. Instead, researchers should perform their investigations of the relationship between grazing and succession in the context of the natural revegetation study. The study of succession as a phenomenon in and of itself, without human activities as part of the causal analysis, never received funding and was finally cancelled. It may well have been considered too technical and fundamental, and therefore not appropriate to the mission of the GBS. Sampson himself at times worried about the possibility that his research could be construed as focussing too much on fundamentals and technical details. Responding to some critiques along those lines apparently made by rangers, he stated that "we do not want to grow stale at the Station by working out "alleged fundamentals" to the extent of becoming academic; we would like to aid in handling administrative problems concerning which we possess expert knowledge."[121]

Yet at other times Sampson went ahead with projects his superiors had cancelled because they though them not practical enough. Sampson initiated a study to examine how the frequency of grazing, simulated by clipping the vegetation with shears, effected the vigor of the vegetation. His superiors thought he should not spend his time on this investigations because "of its being rather technical and detailed."[122] Sampson proceeded with the experiment anyway, and the results apparently convinced his superiors of its practical implications.[123] C.L. Forsling, Sampson's successor as director of the GBS, continued the project, but measured the effect of actual grazing rather than clipping

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[121] Ibid., p.40.
[122] Ibid., p.21.
[123] Sampson published the results of this study in A.W. Sampson and Harry E. Malmsten, 1926, "Grazing Periods and Forage Production on the National Forests," USDA Bulletin 1405; and used the results to determine when the grazing season should begin and end on the range.

the vegetation in order to make the project more practical.

Nevertheless, at least with the plant indicator concept, Sampson completely satisfied the dual orientation. With that concept, he satisfied the needs and concerns of all the different audiences for range research in the Forest Service. The theory of succession and the plant indicator concept also enhanced the power of range scientists. They reinforced existing lines of authority and enabled those lines of authority to be put into practice. In addition, it extended the power of range scientists by further denying the relevance of common sense observation. The use of plant indicators depended on knowledge of succession, and both concepts thereby served to enhance the power of range scientists. Range scientists could dismiss rangers' or stockmen's challenges to their claims as irrelevant if they were not based on scientific knowledge, as potentially could the Forest Service with respect to challenges from stockmen. Both concepts had the effect of restricting the extent to which people who lacked the requisite skills could participate in the decision making process. Except for the range scientists or perhaps other scientists, only the stockmen could still legitimately challenge the researchers' knowledge, and then only on grounds of practicality (i.e., economics). These concepts even ruled other scientists out of the debate to some extent because their knowledge did not satisfy ecology and economics simultaneously. Clements, for example, attempted on several occasions to derive practical implications for range management from his theory of succession, but this work had virtually no impact on range science or management.

However, Sampson's subsequent experiences at the GBS already demonstrated that constructing knowledge claims that satisfied both economic and ecological criteria simultaneously did not resolve the tension once and for all. Further research could upset the simulteneity, requiring it to be reconstructed. Similarly, since the knowledge claims satisfied social and epistemic criteria simultaneously, a change in the social context would also necessitate a reconstruction of simulteneity.

Chapter 6
CONTINUITY AND CHALLENGE: THE CONTEXTUALITY OF RANGE SCIENCE, 1920-1960

Between 1920 and 1960, different groups responded differently to the knowledge Sampson had developed. In the Forest Service, Sampson's research provided the foundation for a research program. In particular from the early 1930's on, Forest Service researchers introduced some important modifications of Sampson's arguments. Nevertheless, they performed their research in a similar context as Sampson, and consequently their research clearly shared many similarities with Sampson's approach to range science. Serious challenges to Sampson's research came not from Forest Service researchers but from groups who occupied a different context.

Two groups emerged outside the Forest Service who challenged Sampson's accomplishments, stockmen and researchers at Utah Agricultural College and Experiment Station (UAC; later Utah State University - USU). Sampson had accomplished his achievements within a certain context. They depended on the Forest Service's categorization of range management, and reflected its authority to manage the range and the stockmen to improve the condition of both. As a result of that context, Sampson and other range researchers acquired power over nature and over the stockmen, at least at the level of research. Sampson therefore could reintroduce a research method and a grazing system that had failed in other contexts, and introduce a management tool. Yet he achieved those successes in the context of his research, and the question remained whether in particular the grazing system was practical outside that context. In another way, his accomplishments also reflected his background as an applied ecologist. He equated economics and ecological succession, thereby ascribing economic value to the various stages of succession.

Both the stockmen and the UAC researchers occupied a different context compared to Sampson because a crucial relationship was different. The knowledge range researchers produced entered political negotiations between the Forest Service and the stockmen, and the researchers then lost control over their knowledge. The stockmen occupied a different relationship with respect to the range than the researchers, and in those negotiations they challenged deferred-rotation grazing's practicality. Yet the change in the relationship to the range from researcher to stockmen in and of itself did not determine the system's practicality. Other factors surrounding the stockmen's relationship with the range played a crucial role. Sheepmen in their negotiations with the Forest Service attempted to use deferred-rotation grazing to their advantage over the cattlemen. In short, deferred-rotation grazing and the research on which it was based became a tool in the ongoing political struggles between the Forest Service and the stockmen and between the cattlemen and the sheepmen.

At UAC, on the other hand, the researchers and stockmen occupied a different political relationship with respect to each other. In many ways, the stockmen possessed power over the researchers at UAC, rather than the researchers over the stockmen as in the Forest Service. Research at UAC remained a barely supported side-bar to teaching for a number of years until researchers developed an approach to performing range science research that reflected the power relationship. Nevertheless, at UAC the research also reflected the dual orientation of range science. Their knowledge consequently had to satisfy epistemic and economic criteria simultaneously. UAC researchers just had to develop an alternative way of performing research to accomplish their goal, and they challenged the equivalence of ecology and economics. They did not, however, challenge every aspect of the knowledge developed by Forest Service range researchers. Instead, they attempted to articulate a position somewhere between the Forest Service and the stockmen. They recognized the need to conserve the range, but placed more emphasis on the needs of the stockmen than Forest Service researchers. As a result, they meant something different when they argued the range should be conserved. From that position, and based on the knowledge they developed within that definition of their role, UAC researchers attempted simultaneously to convince the stockmen to practice conservation and to serve as advocates for the stockmen in their negotiations with the Forest Service.

The Forest Service

The Forest Service adopted the knowledge Sampson developed, and over time Forest Service researchers extended and in the process modified a number of Sampson's achievements. Their research, and research performed by scientists working for an agency with a similar mission, the Soil Conservation Service, produced modifications in the use of indicators. In addition, Forest Service researchers developed an alternative grazing system. In 1931, A.L. Hormay and M.W. Talbot, initiated research in California that thirty years later produced the rest-rotation grazing system. Yet all this research build on the foundation Sampson had constructed and shared many similarities with his approach to range science.

Reflecting the changes occurring in American business, the Forest Service argued research provided the only means to improve ranching so ranchers could share in the economic successes achieved in other industries. For example, in a 1927 address before the Idaho Woolgrowers' Convention, C.L. Forsling, Sampson's successor as Director at the GBS, drew an analogy with giants in American industry to convince stockmen to support research and apply its results. It is not clear whether he recognized that Sampson had created a flow of information very similar to the flow of information in modern industrial bureaucracies, and it is therefore not clear whether he realized how appropriate his analogy was. At any rate, he argued research provided the primary avenue for progress, i.e., increased economic returns, in modern industry. Stockmen risked remaining stuck in an outmoded way of doing business unless they adopted the practices of those successful enterprises. "The use of experimental methods and scientific study," he claimed, "has become so thoroughly fixed as an essential means to progress in modern day industry and life that it must hold similar promise for progress in the grazing industry."[1] In fact, those companies could not have become as successful without science because "the present day mass production in some industries that has made possible a better product as a lower selling price in spite of higher wages than have ever been paid before, is to a large extent the result of the developments made possible thru [sic] scientific study or research."[2] To conclude, he underscored

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[1] C.L. Forsling, "The Solution of Problems in the Management of Grazing Lands," address delivered before the Idaho Woolgrowers' Convention, Weiser, Idaho, January 17, 1927, p.17; Historical Files, Intermountain Station.

[2] Ibid.

the importance modern industries attached to research by describing how many millions of dollars companies like AT&T, General Electric, General Motors, Goodyear, and Eastman Kodak invested in research every year. They certainly would not do that "if these expenditures were not increasing dividends."[3]

Forest Service administrators and researchers then used a number of means to convince stockmen to adopt the deferred-rotation grazing system and other range management principles it had developed. In 1926, W.R. Chapline, the Inspector of Grazing, and in charge of grazing research, advocated the use of deferred-rotation grazing before a meeting of the USDA Extension Conference. According to Chapline, the Forest Service would educate the stockmen through its own local rangers as well as by using the Extension Service, but GBS researchers also presented the arguments for deferred-rotation grazing in a variety of other places.[4] C.L. Forsling visited stockmen's conventions and published a number of articles in stockmen's magazines. In his address to the Idaho

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[3] Ibid., p.18.

[4] W.R. Chapline, "The Relation of Grazing Research to the Range Extension Program," address delivered before the USDA Extension Conference, December 15, 1926; copy at Intermountain Station, historical files. I will discuss the stockmen's response in a later section of this chapter. Deferred-rotation grazing was quickly adopted by other scientists in the USDA, e.g., in the Bureau of Animal Husbandry, which thus served as another avenue to get the message out; see Virgil V. Parr, 1925, "Beef Cattle Production in the Range Area," USDA Farmers' Bulletin 1395.

Woolgrowers, he argued artificial reseeding was often not cost-effective, but with deferred-rotation grazing "Man can work with nature to advantage to obtain maximum forage production, and at the same time secure a fuller use of the forage than would otherwise be possible."[5] The message about deferred-rotation grazing nevertheless bore repeating a number of times over a number of years.[6] Finally, the GBS organized field days where stockmen could come to the Station and examine its activities themselves.[7]

The Forest Service initially also adopted Sampson's plant indicator concept without serious modification. Sampson's successional stages became range condition classes, and the Forest Service initially adopted plant indicators as the primary criterion to judge the condition of the range. Its 1937 Range Plant Handbook listed the significance of some of the plant species as indicators and stated that "Proper

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[5] C.L. Forsling, "The Solution of Problems in the Management of Grazing Lands," pp.6-7.

[6] See, e.g., Lincoln Ellison, 1948, "Bettering Management of Utah's Range Lands," The Utah Magazine, 10(9):8-13, 25-27.

[7] These means of providing the stockmen may still not have been sufficient to educate them. In 1940, Nilo Deming of the Department of Interior's Grazing Service, created under the Taylor Grazing Act of 1934, argued that "Present methods of spreading knowledge of proven practices are inadequate," and that to improve the methods of disseminating information "Probably administrative agencies have particular need to strengthen their means of contact." Nilo H. Deming, 1940, "The Place of Range Improvement in Utilization of Open Range," paper presented at the Western Grasslands Conference, Salt Lake City, Utah, July 15, 1940; copy in WPA Grazing Notes, Box 3, fd. 8.

grazing capacity of range lands, periods and degrees of use, and class of livestock to which a particular range is best suited are determined largely by the character and composition of the range vegetation and the life habits and values of the plants themselves."[8] There was some debate whether individual plant species or communities of plants provided more reliable indicators, but in a 1939 review of the concept Sampson concluded that "the grazing indicators are the best understood and the most extensively and successfully used."[9]

Some researchers, however, began to question the accuracy of plant indicators as indications of the condition of the range and made minor modifications. Sampson assumed that the composition of the vegetation in and of itself served as an indication of the range's condition; i.e., that all the other factors, such as the condition of the soil, were reflected in the vegetation. Researchers gradually began to question this assumption, and in 1936, two Forest Service researchers cited the need for a "simple, usable measure of range condition."[10] By 1944, a paper by GBS researchers Lincoln Ellison and A. R.

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[8] U.S. Forest Service, 1937, Range Plant Handbook, (Washington D.C.: U.S. Government Printing Office); quoted in Sampson, 1939, p.174.

[9] Sampson, 1939, p. 220.

[10] M.W. Talbot and E.C. Crafts, 1936, "The Lag in Research and Extension," The Western Range, 74th Congress, 2nd session, Senate Document 199, pp.185-192, on p.192.

Croft listed a total of nineteen indicators of range condition.[11] Considering all nineteen of those factors would have made determining condition and trend (change in condition) incredibly time consuming. The list therefore became reduced to four or five factors crucial to judging range condition: plant composition, forage density, vigor of the vegetation, soil stability/erosion, and amount of litter on the soil (which helped to prevent erosion and maintain soil structure).[12]

From the perspective of the management of the range, a subtle yet more important change crept into the way the Forest Service used the applied ecological version of succession. For Sampson, management should foster progressive succession until it reached the mixed-grass-and-weed stage; it should definitely not manage to achieve the climax because the climax was not ecologically stable and did not produce the greatest economic returns. But in a clear case demonstrating that a scientist does not control what happens to the knowledge he

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[11] Lincoln Ellison and A.R. Croft, 1944, "Principles and Indicators for Judging Condition and Trend of High Range-Watersheds," Intermountain Forest and Range Experiment Station, Research Paper Number 6. See also the first paper that added several indicators to be considered, M.W. Talbot, 1937, "Indicators of Southwestern Range Condition," USDA Farmers' Bulletin 1782.

[12] See, e.g., R.R. Humphrey, 1949, "Field Comments on the Range Condition Method of Forage Survey," JRM 2(1):1-10; Kenneth W. Parker, 1954, "Application of Ecology in the Determination of Range Condition and Trend," JRM 7(1):14-23.

proposes, the objective of management became achieving the climax, and researchers did not recognize they had introduced a significant change.[13] One researcher also proposed an alternative management objective not based on ecology, but his peers did not accept his arguments because he did not base his system on ecology. R.R. Humphrey, initially with the Soil Conservation Service and later a member of the faculty at the University of Arizona, proposed determining management objectives in terms of the productivity of the forage and classifying sites in terms of their potential forage production rather that in terms of the stage of succession.[14] Other researchers disagreed because, according to Forest Service researcher Kenneth Parker, it had "little or no ecological basis," and because it would only work, according to Soil Conservation Service scientists E.J. Dyksterhuis, "if range condition classes were simply production classes."[15]

In both the Forest Service and the Soil Conservation Service, researchers claimed managing to achieve the climax still also increased the stockmen's economic returns. Much

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[13] This may have been due to ambiguities in Sampson's use of language. He would say that in general management should foster progressive succession, and then qualify it by saying that it should not attempt to achieve the climax.

[14] See R.R. Humphrey, 1947, "Range Forage Evaluation by the Range Condition Method," Journal of Forestry, 45(1):10-16; Humphrey, 1949.

[15] Parker, 1954, p.16; E.J. Dyksterhuis, 1949, "Condition and Trend of Range Land Based on Quantitative Ecology," JRM, 2(3): 104-115, on p.106.

like Sampson, researchers argued that "in general forage production in the upper condition classes greatly exceeds that of the lower."[16] A certain range site should then be compared to the potential for that site, its climax stage, to determine whether stockmen needed to change their management practices. To accomplish that comparison, rangers should compare a grazed area on the site with an ungrazed area, which Sampson had also done in his experiments. But the ungrazed area now acquired a specific name. Range scientists started to call them "natural areas," and managers should thus determine the potential or climax state of a site and adjust the management of the range by comparing the range's condition "with natural areas that have never been grazed."[17]

Other research performed by Forest Service range scientists during these years produced an alternative grazing system. From 1936 to 1951, A.L. Hormay and M.W. Talbot performed research in northern California. Their research ultimately led them to propose the rest-rotation grazing system. They noted that cattle preferred certain species and

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[16] Parker, 1954, p.16. On this point, see also E. Lamar Smith, 1984, pp.816-821.

[17] Lincoln Ellison, 1949, "The Ecological Basis for Judging Condition and Trend on Mountain Range Land," Journal of Forestry, 47(10):789-795. This paper also contains the first use of the ecosystem concept in range science, but the concept apparently did not catch on because no one who cited Ellison's paper picked it up, and it did not shape the remainder of Ellison's analysis.

areas of the range over others, corroborating the observations of others before them. The cattle's preferences caused uneven utilization of the range, leading to the destruction of certain areas while other areas remained under-utilized. Since they could not change the behavior of the cattle, they developed a grazing system to accommodate their behavior.

Their rest-rotation grazing system consisted of a cycle of four steps, to be carried out over a number of years. Over those years, the rest-rotation system rested the range at crucial times for the vegetation's development: to restore vigor, and to ensure seed production and seed establishment. First, a stockman would graze an area one season, and then rest the area one or more seasons to restore the vigor of the vegetation. Under the third step of the system, the stockman would defer grazing his cattle on that area of the range until after the seed had ripened, at which point grazing would benefit the range because the cattle would trample the seed into the ground, thereby planting them. Finally, under the fourth step, the stockman would rest the range until the reproduction had a chance to establish itself, which might take from one to three years. The cycle would then start over. The major advantage of this system was that it allowed and aided the vegetation to complete its life-cycle. Stockmen, furthermore, could stock the range with more cattle than under other systems. Rest-rotation grazing forced cattle to eat the less palatable species and enter the less accessible areas, thus ensuring the trampling of seeds into the ground in areas "where reproduction is most needed."[18] This system also allowed for heavier use because "the range is rested at critical times."[19]

Nevertheless, all these changes Forest Service researchers wrought were modifications within the framework Sampson established. Hormay and Talbot acknowledged that Sampson incorporated rest in range management, and Sampson had linked it to the growth requirements of the vegetation. Their major modification extended the amount of time stockmen should rest the range, and, they claimed, therefore under their system "grazing is eliminated as an environmental factor" because of the rest it allowed the range at crucial moments.[20] More importantly they worked within the same categorization of the problem. Since its inception, they said, Forest Service research had "sought grazing methods that

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[18] A.L. Hormay and M.W. Talbot, 1961, "Rest-Rotation Grazing ... A New Management System for Perennial Bunchgrass Ranges," U.S. Forest Service, Production Research Report 51, p.35.

[19] Ibid.

[20] Ibid., p.40. However, this claim is somewhat of an overstatement. At best they could have claimed that they eliminated grazing as a destructive environmental factor. The cattle trampled the seed into the ground, and as Sampson had already recognized, and they reiterated this observation, the cattle thereby served as an environmental factor influencing the kind of vegetation that reproduced itself on the range.

will improve the range and increase livestock and wildlife production."[21] And in the concluding section of their paper, they explained how managers could use the vegetation as a indicator to judge the effectiveness of rest-rotation grazing. Finally, under their system, "species become established in normal successional patterns."[22] Their system, and the Forest Service's modifications in the use of the concept of succession as a tool for range management, thus still satisfied economic and ecological criteria simultaneously in the same way as Sampson's research had.

In fact, all this research build on Sampson's solution to the tension due to range science's dual orientation because it satisfied economic and ecological criteria simultaneously. In 1926, the Investigative Committee of District Four realized their approach to range science and management had been one-sided. "We have talked too much from the standpoint of doing certain things to prevent injury to the range rather than emphasizing the possibilities of increasing forage production and making it contribute more to economical livestock production."[23] They did not, however, take any action based on this realization, and thirty years later another Forest Service researcher repeated their argument. In 1956, David

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[21] Ibid., p.1.
[22] Ibid.
[23] Annual Report, Investigative Committee District Four, 1926, pp.58-59.

Costello recognized that ecologists had begun to question what exactly the climax is and that perhaps the climax is not the preferred condition from the perspective of range management. He agreed that the productivity of the various stages needed more study "in order that productivity may be tied directly to animal output and rancher income."[24] Nevertheless, he concluded by reinforcing the status of ecology in range science as the foundation for judging range condition and hence for developing management plans.

Around 1940, two Forest Service researchers proposed to extend research into a new area by focussing on the productivity of the range, but they could not convince their superiors. They argued the Forest Service should not only study the impact of animals on the range, but should also study the impact of the vegetation on the animals. Although they did not recognize that Sampson had made en error when he equated palatability with nutritional content, they argued the Forest Service should investigate the vegetation's nutritional value in much more depth. One of them, Kenneth Parker, argued the nutritional content could function as a deciding factor in decisions between grazing systems, e.g., when deciding whether to apply deferred grazing, rotation grazing, or deferred-

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[24] David F. Costello, 1956, "Factors to Consider in the Evaluation of Vegetation Condition," JRM, 9(2):73-74, on p.74.

rotation grazing.[25] The Forest Service did not pursue this research, perhaps in part because it lacked scientists with the required skills.[26] Parker recommended that the Forest Service cooperate in these experiments with the state agricultural colleges because they had animal nutritionists on their faculty. But at least in Utah, the agricultural college researchers would emerge as a force on the side of the stockmen and in opposition to the Forest Service.

Stockmen, the Forest Service, and the Question of Practicality

The issue of deferred-rotation grazing's practicality resurfaced when the Forest Service required stockmen to apply it on the National Forests. Ultimately, most stockmen agreed the Forest Service's management of the ranges in the National Forest improved their condition dramatically.[27] Initially,

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[25] Kenneth W. Parker, "Animal Nutrition Studies," in Proceedings of Range Research Seminar, held at the Great Basin Branch of the Intermountain Forest and Range Experiment Station, Ephraim, Utah, July 10-22, 1939; copy at GBS. See also George Stewart, 1940, "Forest Service Range Research Seminar," Journal of the American Society of Agronomy, 32(3):235-238.

[26] For example, in 1956, Lincoln Ellison still argued knowledge of the palatability, not nutritive content, of plant species was crucial to the use of plant indicators of range condition. See Lincoln Ellison, 1956, "Grazing Standards in Range Management," New Zealand Grassland Association, Proceedings of the Eighteenth Conference, at Canterbury Agricultural College, Lincoln, November 29-29, 1956; reprint in historical files, GBS.

[27] See the statements by stockmen in the interviews performed by staff associated with the WPA Grazing Project in the 1940's, e.g. the statements by W.P Madsen, Daniel Gull,

however, the question became how well Sampson captured or mirrored the actual relation between the range and the stockmen who leased land from the Forest Service. Sampson demonstrated the practicality of deferred-rotation grazing within the context of his experiments by demonstrating that stockmen could increase their economic returns if they practiced deferred-rotation grazing. The question of the system's practicality under actual operating conditions would be answered in negotiations between the Forest Service and the stockmen.

The new institutional context of grazing constrained the stockmen's arguments in their negotiations with the Forest Service. They could, of course, challenge the legitimacy of the new institutional context, which they did on several occasions. According to the stockmen, the Forest Service's management of the range was an illegitimate extension of centralized power, abrogating the rights of individuals, local communities, and states, and they also argued it interfered with God's mandate to subdue and conquer the earth. Powerful institutions such as the Mormon Church supported the stockmen when they cast their arguments in these terms.[28] But the

C.A. Mattsson, W.R. Coleman, W.P. Coleman, H.T. Goodman, B.Y. Green, Gus Williams, and L.E. Despain; WPA Grazing Notes, Box 4, fd. 1.

[28] See Jay Melvin Haymond, 1972, "History of the Manti Forest, Utah: A Case of Conservation in the West," unpubl. Ph.D. dissertation, University of Utah.

Supreme Court decisions of 1911 had already settled this issue in favor of the Forest Service.

Forced to negotiate within the new institutional context, they had to abide by its rules and values. Emerging in the context of late-nineteenth and early-twentieth century America, the Forest Service had institutionalized the values of the new order, in particular the values mandating organization and rational management. Its research branch provided the means to instantiate those values. As a result, the stockmen could not appeal to their own common sense, rule-of-thumb knowledge because the Forest Service had institutionalized experts to provide it with the knowledge necessary to manage the range. The stockmen's knowledge had become invalid. Within the new institutional context, they could appeal to the knowledge of the experts, to range science. Or they could challenge the practicality of that knowledge, and hence its rationality. They could argue that the knowledge was irrational outside the experimental context because it did not satisfy their economic criteria, and hence did not satisfy the Forest Service's categorization of range management. In their negotiations with the Forest Service, different groups pursued either or both of those strategies.

The outcome of those negotiations over deferred-rotation grazing's practicality depended on the context surrounding different groups of stockmen. Cattlemen would have to change their practices more drastically than sheepmen under deferred rotation grazing. Before the Forest Service began to manage the ranges, the cattlemen just took their cattle to the range and left them there to fend for themselves. Now they would have to start managing their stock while on the range and rotate them from section to section. The sheepmen, on the other hand, already managed their sheep while the animals grazed the range. They just had to change their management practices and manage their animals more intensively. This difference, however, did not become the crucial factor. Differences in the context surrounding these different groups became more important. Because of differences in the context surrounding sheepmen and cattlemen, the Forest Service considered the system less practical and from an economic standpoint irrational for the cattlemen. As a result, it gave the cattlemen more leeway. In response, the sheepmen tried to argue the Forest Service thereby violated the general principles its own research branch had developed, but with only mixed success.

The Forest Service during these years never just mandated the kinds of practices stockmen had to adopt but considered the impact of its policies on the stockmen. In 1919, acting district forester L.F. Kneipp explained the Forest Service's views to the president of the Heber Horse and Cattle Growers Association, whose members grazed their horses and cattle on the Uinta National Forest in Utah. The Forest Service understood the stockmen's position, he said, and it would not "take action that will tend to militate against the interests of the stockmen involved further than is found absolutely necessary to insure proper protection for the range."[29] He reiterated this claim when he explained how the Forest Service incorporated the stockmen's interests in its deliberations to set opening dates, when stockmen could enter the ranges on the National Forests. Acknowledging the opening dates might cause some inconveniences, he strove to present the Forest Service as flexible on this issue because "In establishing seasons ... it has always been the policy to give as much weight to the needs of the communities as is consistent with the permanent welfare of the National Forest."[30] The stockmen, apparently, did not think the Forest Service was flexible enough.

Controversy erupted over the date stock could enter the National Forests. Cattlemen used to graze their cattle on the winter ranges in the valleys until the snow began to retreat from the mountains, when they took the cattle from the valleys to the mountains and turned them loose. The cattle then

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[29] L.F. Kneipp to George A. Fisher, Feb. 20, 1919, p.1; Heber Cattle Company Collection, box 1, fd. 1, Utah State Historical Society.

[30] Ibid., p.2.

followed the snow line up the mountain until they reached their summer ranges. This practice destroyed much of the vegetation because the cattle trampled many of the young plants to death before they had a chance to establish themselves firmly, let alone reproduce. Yet this practice was necessary because stockmen overstocked the winter range and by then most of it had been exhausted for the season. Sheepmen also entered the mountain ranges as early as possible to secure forage for their sheep in competition with the cattlemen and with each other. There was no question of deferring or rotating grazing, and without opening dates the vegetation would never stand a chance to reproduce itself on any section of the range. The Forest Service therefore established opening dates, and it required the stockmen to practice deferred-rotation grazing on those ranges, meaning the stock could not enter certain sections of the range until late summer when the vegetation had reproduced.

As the Forest Service learned more about the impact of grazing on the vegetation, it delayed the opening date of the grazing season on the National Forests. Until 1919, it allowed cattlemen to enter the National Forests on May 1. Experience taught rangers the range could not absorb the effect of grazing at such an early date, and they should probably delay the opening date until May 15. In 1919, they proposed opening the season on May 8, but spring came early and they allowed the cattle on the range a week earlier. However, in 1919, local rangers also performed a study to determine the date the range could accommodate the cattle (called a period study). The cattle, this study observed, did not confine themselves to the section of the range opened up for spring grazing and followed the snow line up the mountain, thereby reaching the summer range weeks ahead of schedule. Fencing would prevent this, but the rangers thought it impractical because it was too expensive. Therefore, "the practical solution and the one which has been decided upon, is to defer the date upon which cattle and horses shall enter the Forest."[31] The new opening date became June 1, and the cattlemen should then rotate their cattle among sections of their range allotment to defer grazing on at least one section until after the seed had ripened.

The cattlemen protested, and won by arguing the policy was irrational because it was impractical. That is, they claimed it imposed undue economic hardships on them. They did not waste any time. One month after the Forest Service announced the June 1 opening date for cattle, a committee representing seventeen livestock associations in Utah passed a resolution opposing the new date. The new policy would cause such severe economic hardships for most cattlemen "that

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[31] W.W. Blakeslee, "Supervision, Uinta," August 17, 1919, p.2; Heber Cattle Company Collection, box 1, fd.14.

a majority of them would be forced to abandon this pursuit of making a livelihood" because they could not "feed their cattle upon their fields and growing crops until June 1 ... [nor] feed them in the feed-yards."[32] In September 1919, representatives of the cattlemen who grazed their cattle on the Uinta and Wasatch National Forests had an audience with Secretary of Agriculture D.F. Houston. In March 1920, a committee representing the permittees of the Wasatch National Forest send a letter to Secretary of Agriculture Edwin T. Meredith. They claimed the range had improved and challenged the period study as "superficial and incomplete."[33] They repeated the claim that the economic hardships suffered by cattlemen would put most of them out of business, and argued the policy would in effect give sheepmen an advantage over the cattlemen.

> "The farmers grazing cattle on these forests raise alfalfa and sugar beets on their farms and ... the injury that would be done these growing crops by allowing the cattle to graze on them until the first of June would be so great that the farmer would dispose of his cattle, and thus the range would soon revert to the large sheep man, while the cattle man would impoverish his farm by lack of fertilizer.[34]

The claims about potential economic hardships convinced the

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[32] "Resolution" (passed September 9, 1919); Heber Cattle Company Collection, Box 1, fd. 15.

[33] Committee representing the Permittees of the Wasatch National Forest to the Secretary of Agriculture, March 6, 1920, p.2; Heber Cattle Company Collection, Box 1, fd. 3.

[34] Ibid., pp.3-4

Forest Service to change its policy. In July 1920, Forest Supervisor W.W. Blakeslee announced the 1921 season for cattle would open May 16.[35]

The cattlemen's victory upset the sheepmen. Initially, sheepherders could enter the Forests May 10, but investigations demonstrated stockmen should not graze the range until June 1, and the sheepmen agreed with the rangers' assessment. In the fall or winter of 1920, Blakeslee announced the 1921 grazing season for sheep would open June 15. The sheepmen opposed that policy strenuously. Experienced sheepmen, they argued, agreed the range was ready by June 1, and often even by May 15. Like the cattlemen, the sheepmen argued the policy was irrational because it did not reflect the economic realities they faced. Economically the new policy would impose "great hardship" because the permittees would "have to secure early range for their sheep and would have to re-adjust their business."[36] Learning that the cattlemen would enter the range in May, they became even more upset, and in January, 1921, they passed a resolution arguing all types of livestock should be allowed to enter the

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[35] W.W. Blakeslee to L.C. Montgomery, July 19, 1920; Heber Cattle Company Collection, Box 1, fd. 4.

[36] J. Thomas et al to W.W. Blakeslee, Dec. 23, 1920, p.1; Uinta Sheep Grazers' Association Correspondence, Box 1, fd. 10, Utah State Historical Society.

range on the same date.[37] They claimed the distinction between cattle and sheep discriminated against the sheepmen because the range was either ready or not, and if it was ready, then the Forest Service should allow both sheep and cattle to enter the range at the same time.

The Forest Service denied the sheepmen's appeals because the appeals did not recognize the different context of the cattlemen and sheepmen. Blakeslee claimed the sheepmen adopted a very narrow perspective, while the Forest Service had to "consider the situation in a broad and general way."[38] Seen in a broader context, the sheepmen and cattlemen occupied a different relationship with respect to the range on the National Forests, and this difference justified the Forest Service's policy. In particular, the sheepmen had acquired grazing lands where they could graze their sheep until the grazing season on the Forest opened. The cattlemen, in contrast, did not possess such grazing lands, and therefore needed access to the Forest at an earlier date. The sheepmen could of course claim the cattlemen had the opportunity to acquire the necessary grazing lands, but Blakeslee argued "The facts remain, however, that they did not acquire grazing land,

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[37] Uinta Sheep Grazers' Association to W.W. Blakeslee, Jan. 25, 1921; Uinta Sheep Grazers' Association Correspondence, Box 1, fd. 10.

[38] W.W. Blakeslee to Uinta Sheep Graziers' [sic] Association, Jan. 28, 1921, p.1; Uinta Sheep Grazers Association Correspondence, Box 1, fd. 10.

and ... the situation must be met upon present conditions rather than upon what might have been done."[39] This difference between the sheepmen and cattlemen provided the sole answer to the sheepmen; Blakeslee did not respond to their other claims. Apparently, he did not consider the economic cost as imposing an undue hardship, and in the context of the Forest Service the sheepmen's experience could not provide the basis to challenge the claims of its experts. The sheepmen later claimed the cattlemen had taken advantage of the sheepmen and of the Forest Service because "The records will indicate that many of the cattlemen had sold their spring range to sheepmen, and then requested early entry upon the Forest because they had disposed of their spring range."[40] This strategy, however, was also unsuccessful.

The sheepmen then argued the Forest Service's policies were irrational because they were inconsistent with the growth requirements of plants and violated the principles of deferred-rotation grazing. The controversy continued for more than a decade, and reached a climax in 1934. Again protesting the distinction between cattlemen and sheepmen, the sheepmen claimed the Forest Service's management of the cattlemen violated the knowledge developed by its own researchers. Not

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[39] Ibid., p.2.

[40] Jas. A. Hooper to Chas. DeMoisy, Jr., January 3, 1934, p.3; Uinta Sheep Grazers' Association Correspondence, Box 3, fd. 9.

only did it allow the cattlemen to enter the range too early, it also failed to manage the cattlemen so they would keep their stock of the areas where grazing should be deferred until after the seed had ripened. Citing a number of studies by Forest Service range researchers, they claimed it was "absurd that the above experiments and experiences are to be entirely lost sight of and the range permitted to be destroyed by early entry."[41] Being "firm believers in deferred and rotation grazing," they also claimed the cattlemen and Forest Service violated this system because "This ... can only be done by protecting that portion of the range where deferred and rotation grazing is to take place ... from grazing until the seeds are matured."[42] They therefore concluded "It is neither fair nor practical to attempt a deferred area grazing by permitting one class of livestock to be on the range promiscuously and at will."[43]

The sheepmen achieved at least partial success with this

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[41] Ibid., p.6.

[42] Ibid.

[43] Ibid. This was apparently a common complaint of sheepmen. Already in 1920, two sheepmen who grazed their sheep on Manti-LaSal National Forest filed affidavits with the USDA. They complained the Forest Service should manage the cattlemen more closely so they would manage their cattle because the cattlemen allowed or even actively encouraged their cattle to drift onto areas of the range the sheepmen had set aside to defer grazing. See J.D. Meyrick, Affidavit, filed Feb. 1, 1920; and John H. Seeley, Affidavit, filed Feb. 9, 1920; Supervision, General, Manti, copy at Utah State Historical Society, file A-606.

strategy. Relying on the same justification as before, the Forest Service still maintained it had to permit cattle access to the ranges on the Forests at an earlier date than sheep. The Forest Service had to acknowledge, however, "that there has not been sufficient control of cattle." It hoped to remedy this situation in the future by "division of the range, fencing and better management of cattle and horses.[44]

Both sheepmen and cattlemen started to practice deferred-rotation grazing on a much more systematic basis. By 1942, four hundred and twenty-nine sheep outfits in Utah practiced this system, and only eighty-eight did not.[45] Comparable data are not available for cattlemen. However, a statement made by an old cowman to Forest Service administrator Keith E. Evans about grazing on the National Forests clearly demonstrates the cattlemen changed their practices. According to that cowman, the biggest impact of Forest Service management of the ranges had been that "Now I have to keep my horses shoed all year."[46] Cattlemen used to shoe their horses in the Spring to take the cattle to the mountain, where they left the cattle to fend for themselves, and they then removed the horseshoes because they no longer needed their

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[44] Chas. DeMoisy, Jr. to Jas. A. Hooper, March 5, 1935, p.3; Uinta Sheep Grazers' Association Correspondence, Box 3, fd. 11.

[45] Charles M. Lindsay, summary of statement collected from District Forester R.H. Rutledge, WPA Grazing Notes, Box 2.

[46] Interview with Keith E. Evans, July 5, 1990.

horses. They would re-shoe the horses in the Fall to take the cattle from the mountains onto the winter ranges in the valleys. However, when the Forest Service started to require that they practice deferred-rotation grazing more systematically, they had to keep their horses shoed because they had to manage their cattle while the cattle grazed the mountain ranges.

At about the same time, the stockmen, both cattlemen and sheepmen, acquired a new ally in their negotiations with the Forest Service. Beginning in the 1930's, but especially from the 1940's on, researchers at what was then called Utah Agricultural College (UAC) and later became Utah State University (USU), emerged as a force in range science. Those researchers would occupy a position between the stockmen and the Forest Service and other land management agencies, such as the Bureau of Land Management (BLM).

RANGE RESEARCH AT UTAH AGRICULTURAL COLLEGE

Education in range management followed on the heels of the Forest Service's institutionalization of range research, and in 1914, UAC offered the first range management course in the country. The Forest Service needed trained personnel, and the colleges, especially the agricultural colleges, soon established curricula to satisfy that need, often by creating forestry schools. 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.[47] Meanwhile, in 1928, UAC established a curriculum leading to the bachelor's degree in range management, and it awarded the first of those degrees in 1930. In 1938, UAC awarded its first master's degree in range management. Fourteen years later, USU had awarded one hundred and ninety-seven bachelor's and seventeen master's degrees. Its graduates found employment in a number of institutions; forty-one worked for the Soil Conservation Service, thirty-six became administrators in the Forest Service and thirteen earned jobs at the Forest Service's Forest and Range Experiment Stations, twenty-seven joined BLM, fifteen were ranchers, eleven became college professors, while others found employment in a wide variety of institutions.

Changes in the faculty at UAC reflected this steady growth of range science. Professor George R. Hill, a member of the faculty in the School of Agriculture, taught the first range management course. In 1918, UAC hired professor Raymond J. Becraft specifically to teach range management and

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[47] These figures are compiled from information in Arthur W. Sampson, 1954, "The Education of Range Managers," <u>JRM</u>, 7(5):207-212.

forestry, and to perform research in those areas. He resigned in 1935, and UAC hired Laurence A. Stoddart to replace him. Stoddart build one of the premier range programs in the country at UAC. Two years later, Arthur D. Smith joined Stoddart, and they collaborated to write what soon became the premier textbook in range management.[48] In 1942, C. Wayne Cook replaced Smith, who left to serve in the Army, but returned in 1946. By 1952, the range department at USU's main campus in Logan, Utah, was staffed by four people, and a fifth faculty member worked at its Branch Agricultural College in Cedar City, Utah.[49]

Yet the picture of steady growth hides more than it reveals because range science and range management for a long time occupied at best a tenuous position at UAC. Range science became firmly established only when researchers satisfied the mission of the agricultural college and served Utah's stockmen. It took until the 1940's before UAC researchers developed a role and research orientation that

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[48] Laurence A. Stoddart and Arthur D. Smith, 1943, Range Management (New York and London: McGraw-Hill Book Company, Inc.); this textbook quickly replaced Sampson, 1923, and in revised editions, this is still the standard textbook in range management.

[49] Information on USU in this and the previous paragraph is a summary of USAC School of Forestry, Range, and Wildlife Management, 1952, "History of the School of Forest, Range and Wildlife Management," and Lewis M. Turner, 1957-8, "History of Forest, Range and Wildlife Management Instruction at USU," both at Merrill Library Special Collections, USU, Logan, Utah.

satisfied their institutional context. As a result of their institutional context, UAC range researchers categorized the interaction between the stockmen and the range differently, and therefore developed an approach to range science different from the Forest Service.

The Institutional Context at UAC

Created in 1888 as Utah's agricultural college, with the state experiment station attached to it, UAC's primary mission was to serve Utah's citizens and especially its agriculturalists.[50] Since Utah is a desert state where ranching is the most feasible form of agriculture, serving Utah's ranchers occupied a large part of the institution's task. To ensure the college met its responsibilities, ranchers and their friends, such a local bankers, played an important role in UAC's administration. These features of the context at UAC shaped many of the administrator's and range researcher's experiences at the college and experiment station. A third important feature pertained to the research facilities available to UAC. Range research required access to range land, and for a long time UAC researchers simply did not possess such access. The college's administrators

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[50] On the school's history, see Joel E. Ricks, 1938, The Utah State Agricultural College, A History of Fifty Years, 1888-1938 (Salt Lake City: The Deseret News Press).

attempted to acquire land for a numbers of years, but did not succeed until the early 1940's. This lack of land limited the ability of UAC researchers to perform independent research, and its researchers often depended on the Forest Service for access to land during the early years of range research at the college.

UAC's mission linked the school directly to the economic and social concerns of the State's agriculturalists. Administrators and faculty designed the curriculum to prepare Utah agriculturalists to use the benefits of research and to raise the status of agriculture as a profession. Courses leading towards a degree in the school of agriculture included "the sciences that underlie practical agriculture," and such other subjects as were necessary to "develop the agricultural student to the intellectual level of the educated in the other professions."[51] The goal of the agricultural experiment station always remained undertaking studies "in the hope that truths of great value to the farmer may be discovered."[52]

The relationship with the stockmen and focus on agricultural production did not mean UAC administrators and researchers opposed conservation. Utahans in fact took the lead in the movement to place the remaining public lands under

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[51] 1919-1920 Catalogue, Utah Agricultural College, p.58; at Special Collections, Merrill Library, USU.

[52] Ibid., p.49.

some form of government control. The Forest Service controlled the National Forests, but the federal government still owned most of the land in the West in lieu of private ownership. Those lands provided the stockmen's fall, winter, and spring ranges, and without any agency exercising control over those ranges, they continued to deteriorate.

In 1927, Utah congressman Don Colton submitted a bill for the regulation of grazing to Congress. That bill contained many features later incorporated into the Taylor Grazing Act of 1934, which placed the remaining public lands under the control of the Department of Interior and created the Grazing Service, which later became the BLM. Had Colton, a Republican, not lost his seat in the Democratic landslide of 1932, it is possible the Taylor Grazing Act would now be called the Colton Grazing Act. At any rate, Colton received the support of William Peterson, the Director of the Utah Extension Service, attached to the agricultural experiment station, who later became the Director of the experiment station. Colton and Peterson communicated frequently about the problem of controlling grazing on the public domain, and in 1929, President Hoover selected Peterson to become a member of the Commission on the Conservation and Administration of the Public Domain.[53]

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[53] For the communications between Colton and Peterson, see Director's Files, Utah Agricultural Experiment Station, box 9,

Utah stockmen for the most part supported the control of grazing on the public lands, primarily because they and stockmen from other states ensured it reproduced their interests. At their annual convention in 1930, the Utah Cattle and Horse Growers Association resolved "that we endorse government control of the public domain in the Western States."[54] The deteriorating condition of the public lands undoubtedly provided a major motivation to support some form of control. Most stockmen acknowledged the need for some form of rational management of the public range. The question remained what form such management would take.

It became clear very early on that control of the public domain would be based on a somewhat different definition of conservation then used in the Forest Service. Although the Forest Service considered the stockmen's economic interests, it would curb present use in order to protect the range for future use. Usually, it limited current use by reducing the number of stock it allowed the stockmen to graze on the

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fd. 2. On Colton and on Utahans' attitudes toward conservation, see also Thomas G. Alexander, "From War to Depression;" John F. Bluth and Wayne K. Hinton, "The Great Depression," and Charles S. Peterson, "Natural Resource Utilization," all in Richard D. Poll, et al., eds., 1978, <u>Utah's History</u> (Provo, UT: Brigham Young University Press), on pp. 463-480, 481-496, and 651-668, respectively.

[54] Utah Cattle and Horse Growers Association, Twelfth Annual Convention, Salt Lake City, April 4-5, 1930, Resolution No. 11; Utah Agricultural Experiment Station, Director's Files, Box 8, fd. 4.

National Forests. Stockmen intensely disliked this policy because they saw it as interfering with their economic well-being.[55] The Commission on the Conservation and Administration of the Public Domain adopted a somewhat different perspective, primarily because the stockmen managed to exert significant influence on its members. The Commission's chairman, James R. Garfield, stated in 1930 that the Commission hoped to achieve "conservation that does not interfere with present use."[56] Western stockmen managed to maintain control of the process leading up to the passage of

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[55] Nationally, stockmen sometimes questioned the need for any control or management of the ranges, in particular by the Forest Service, arguing for example that no form of management could restore the ranges because the problem was due to a lack of rain. See, e.g., F.E. Molin, 1938, If and When it Rains: The Stockman's View of the Range Question (Denver: American National Livestock Association). As the following excerpt from a poem that appeared in a Utah livestock magazine indicates, Utahans were also impressed by the power of rain, which they attributed to God's benevolence:

> It rained all through the night and morning
> A soft and steady rain
> And where yesterday's wind spun a column of dust
> The grass is growing again
> ...
> Stockmen and farmers are thanking God
> For the grass that is growing again
> ...

Peter Spraynozzle, 1934, "The Grass is Growing Again," The Ogden Livestock Digest, July 5, 1934, copy in WPA Grazing Notes, Box 1, fd. labeled "Published Works." However, there is no evidence that they used this argument against management of the range.

[56] "Statement by James R. Garfield, Chairman, Committee on the Conservation and Administration of the Public Domain, at conclusion of meeting June 5, 1930," p.5; Utah Agricultural Experiment Station, Director's Files, Box 6, fd. 9.

the Taylor Grazing Act, and subsequently quickly captured first the Grazing Service and then its successor, BLM. As a result, those agencies were much more sympathetic to stockmen, interfered much less with their current use of the range, and UAC's Laurence Stoddart therefore called the Taylor Grazing Act "the first law to help the livestock man in Utah."[57]

The attitude toward conservation expressed by members of the Board of Trustees and faculty members at UAC clearly reflected the stockmen's concerns with production. Frederick P. Champ, president of the Utah Mortgage Loan Corporation and of the Cache Valley Banking Company, served on the Board throughout the 1930's and was its president in 1940. In 1940, he argued bankers had learned the importance of conserving the grass resource. The ranchers owed the bankers significant amounts of money, which they would not be able to repay if they destroyed the range. Bankers therefore had come to realize the importance of conservation not only for the ranching industry but also for themselves, and for the economic and social structure of many communities in the West. However, although the bankers therefore supported scientific management, they and the stockmen needed to know more to evaluate the rationality of such management. They needed more

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[57] James E. Newes, "Interview with L.A. Stoddart, 11-4-40," WPA Grazing Notes, Box 3, fd. 9; on the Taylor Grazing Act, see also Rowley, 1985, pp.152-153.

information about the costs involved, "and whether the ultimate increased returns will more than equal this sacrifice and justify temporary curtailment."[58] He not only thought an economic limit to conservation existed, but also argued bankers and ranchers needed to know more about improving the productivity of the range. The Forest Service should develop management plans for "the rates of use of range and pasture resources which will yield maximum production."[59] This attitude remained fairly constant at USU, and was repeated by a member of the faculty almost thirty years later in a course on technical problems in range management.[60]

This attitude toward conservation was due to the direct influence of stockmen because the institutional structure at UAC reflected its relationship with Utah's agriculturalists. To ensure the college served the agricultural community, and especially the ranchers, stockmen and their friends occupied powerful positions on its administrative boards. The Board of Trustees was subdivided into a number of committees, including a livestock committee. That committee consisted primarily of ranchers. Throughout the 1930's and into the 1940's, for

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[58] Frederick P. Champ, "Grass as a Banker Sees It," Address delivered at the Western Grassland Conference, Salt Lake City, Utah, July 15-17, 1940, p.5; Historical Files, Intermountain Forest and Range Experiment Station.

[59] Ibid..

[60] "Technical Problems [RS 164]," p.20; Stoddart Papers, Box 8, fd. 4.

example, Charles Redd played a prominent role on the livestock committee. Redd was the largest stockman in Utah and perhaps in the nation, and at one time served in the Utah legislature. Reflecting the changes that had occurred in ranching, Redd took an active interest in rational management of his stock. He followed new developments in science closely, always hoping to find some new knowledge he could use to increase his profits. According to one of his cousins, Keith Redd, "He was everlastingly studying, and he would get all the information that anybody had on a new program that would come out in the livestock business."[61] Not only did he read all the bulletins and go to the college to talk to researchers, he also "would have their people down to the ranch to visit for a weekend."[62] He then tested the new knowledge himself, with a clear goal in mind. Always hoping to increase his production, "expense didn't mean a thing to him if he was improving his stock."[63]

The agriculturalists also had powerful allies on the Board of Trustees. Board member and banker Frederick P. Champ in a letter to the president of UAC clearly defined the role

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[61] Keith Redd, interviewed by Gregory P. Maynard on July 23 and 27, 1973, Charles Redd Oral History Project sponsored by Charles Redd Center for Western Studies, Brigham Young University, Utah State University, and the Ronald V. Jensen Living Historical Farm, transcript at USU, p.8.

[62] Ibid..

[63] Oscar Whiting, interviewed by Gregory P. Maynard on August 9, 1973, Charles Redd Oral History Project, pp.8-9.

of the College as serving the needs of agriculturalists. "The future safety and development of the College rests upon ... service to the State, and the loyal support of these fundamental agricultural groups," he argued, "rather than necessarily upon an expansion of our enrollment or curriculum in the field of formal education, which does not have the same fundamental relationship to the economy of the State."[64] In short, UAC should help the state's agriculturalists by providing the knowledge necessary to allow the farmers and ranchers to place their business on a rational, scientific basis so they could increase their profits.

To enable UAC to perform its mission and serve agriculture, stockmen and members of the board of Trustees actively supported administrators' attempts to acquire land for the college to perform research. Range research required a lot of land, and the initial grant UAC received as a land grant institution proved insufficient. In 1930, the College tried unsuccessfully to convince the Forest Service to establish a research station on its campus.[65] In 1934, it tried again to solve this problem when President Peterson

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[64] Frederick P. Champ to President E.G. Peterson, April 13, 1939, Papers of E.G. Peterson, Box 176, fd. 2; Special Collections, Merrill Library.

[65] See E.G. Peterson to C.L. Forsling, July 1, 1930, and Acting Secretary of Agriculture (name illegible) to President E.G. Peterson, July 10, 1930; Papers of E.G. Peterson, Box 88, fd. 9.

supported by the Trustees and the Utah Woolgrowers' Association requested Congress to grant the College additional land.[66] They could not convince Congress. In 1939, Frederick Champ suggested to President Peterson they "bring to the attention of the Secretary of Agriculture our need for additional land."[67]

Unsuccessful at acquiring its own land, UAC researchers cooperated with the Forest Service. In 1923, they drew up a tentative plan for a cooperative experiment, and at about the same time UAC researchers began to attend the annual meetings of the Forest Service's Investigative Committee for District Four.[68] Becraft cooperated with the Forest Service in a reseeding study, and when he wanted to modify the project to study the feasibility of reseeding with native rather than with introduced species, he first "conferred with the U.S. Forest Service."[69] Stoddart continued this study, and established a number of other projects in cooperation with the Forest Service, including studies to improve range sheep and sheep management practices, on range and pasture development

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[66] E.G. Peterson to Congressman Abe Murdock, February 5, 1935; papers of E.G. Peterson, box 99, fd. 6.

[67] Frederick P. Champ to E.G. Peterson, April 13, 1939.

[68] Annual Report, Investigative Committee District Four, 1923; the 1928 report states that "The Utah Agricultural College has been represented at the District Investigative Committee each year for the last four or five years."(p.7)

[69] P.V. Cardon to E.W. Allen, 16 October, 1929; Utah Agricultural Experiment Station, Progress Reports, Box 10, fd. 1.

and use, the development and use of spring and fall ranges, and range inventories.[70] Nevertheless, the land shortage continued to be a problem, and in 1942, Stoddart called the results of a range revegetation research project "discouraging because of inadequate experimental land."[71]

This strategy may well have influenced the stockmen's perception of range science at UAC, and they did not always support it to the extent they had when they endorsed the College's attempts to acquire land. In fact, the stockmen were at times openly hostile to range science at UAC. They expected the college's researchers to help them solve their problems, increase their productivity and thus their income, and side with them in their negotiations with the Forest Service over what they considered illegitimate interference in their rights. Instead, however, they perceived the range researchers at UAC as somehow linked with the Forest Service, and therefore as supporting the changes the Forest Service expected them to make.

The stockmen's perception of range science and management affected range scientists' position at the College. They apparently totally withdrew their support in 1926, when range

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[70] Summaries of these projects can be found in the Stoddart Papers, Box 12, fd. 3 and fd. 4; for additional cooperative projects see Box 15, fd. 9.

[71] Annual Progress Report, Federal Grant Projects, 1942, Project 185, "Range Revegetation;" Stoddart papers, box 15, fd. 4.

management ceased to exist as an independent department in the School of Agriculture. During the 1926-1927 and 1927-1928 academic years, UAC offered only four range and forestry courses, as compared to six the previous years. More significantly, the Department of Botany offered those courses. During the Fall of 1927, however, a rumor reached President Peterson that the University of Utah wanted to establish a Forestry Department. He immediately called a meeting of the Board of Trustees and requested they reestablish a Forestry School or Department. The Board agreed, and in the Fall of 1928 created a Department of Forestry and Range Management, which offered nine courses in range management.[72] The reasons for this decision are not entirely clear. It may well have involved competition between the two schools for state and other funds. The nature of the two schools, and in particular their relationship with the stockmen, may however have been a more important factor in this decision. The University of Utah was not the agriculturalists' school; UAC was. The Board may well have thought the agriculturalists could never exercise the same amount of control over researchers at the University of Utah as they possessed at UAC. And they may not have considered the University of Utah

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[72] Lewis M. Turner, unpubl., 1957-8, p.6.

as sympathetic to the agriculturalists' interests.[73] Although we may never know precisely why the Board agreed to reestablish the Forestry and Range Department, these event had a dramatic effect on the researchers.

The changing fortunes of the department clearly affected the researchers' understanding of their role and of where the power resided at UAC. In 1957, Lewis M. Turner wrote a history of the School of Forest, Range and Wildlife Management. Turner was a member of the faculty and later dean of the School of Forestry, created in 1933, and consisting at the time of two departments, forestry and range management. The seat of power at UAC, Turner recognized, was the Board of Trustees, and the stockmen occupied dominant positions on the Board. In a rather lengthy statement, he explained the effect

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[73] If this was indeed the line of reasoning of the Board, they must have felt vindicated in the 1930's and the 1940's. During those years, the ecologist Walter P. Cottam of the University of Utah critiqued stockmen's grazing management practices, predicted dire ecological consequences unless the stockmen changed their practices, and cooperated with and corroborated the arguments of the Forest Service, most notably in his 1947, "Is Utah Sahara Bound?" in W.P. Cottam, ed., Our Renewable Wildlands - A Challenge (Salt Lake City: University of Utah Press), pp.1-52. This was the culmination of arguments Cottam had proposed for years, and it extended claims made by another ecologist, Paul Sears, who had analyzed the Dust Bowl; see Paul B. Sears, 1935, Deserts on the March (Norman: University of Oklahoma Press). Utah stockmen, working through the Mormon Church, then attempted to get Cottam dismissed; see McIntosh, 1985, p.305; on relations between the Mormon Church and the stockmen, and the Church's advocacy of stockmen's positions, see also Jay Melvin Haymond, 1972.

of this distribution of power on the forestry school.

> The particular difficulty associated with this matter of support has been the Board of Trustees. There have always been a considerable number of men on the Board who are either directly engaged in livestock production ... or who have been indirectly associated with it. These men ... were unable to dissociate the forestry school from the U.S. Forest Service. As is well known, many of these people were openly antagonistic to the Forest Service, and their rancor carried over into a dislike for and a lack of interest in forestry schools. Our school certainly suffered from this unhappy association.[74]

Statements by Stoddart and lecture notes indicate the stockmen continued to influence range science and management well into the 1960's. In 1965, Stoddart explained to an assistant to the Secretary in the Department of Interior that " the University is under constant pressure from stockmen to act in their defense against what they feel is government infringement upon their "rights.""[75] In a range science course taught in or after 1969 entitled "Technical Problems," the faculty member teaching the course explained the position of range science at UAC. Ranchers and sheepmen looked to USU for help, he claimed, because they did not trust the Forest Service, and USU therefore had to occupy a position between the stockmen and the Forest Service.[76]

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[74] Turner, unpubl., 1957-8, p.52.

[75] L.A. Stoddart to Karl S. Landstrom, September 20, 1965; Stoddart Papers, Box 3, fd. 2.

[76] "Technical Problems [RS 164]," pp. 1,2; Stoddart papers, Box 8, fd. 4.

The budgets for research in range science and management also clearly indicate the stockmen's changing opinion whether researchers successfully satisfied their mission. The stockmen and their allies had considerable power over those budgets through their influence on the Board of Trustees and on the state government. For the 1925 and 1926 biennium, Becraft requested a total of $4,050.00 exclusive of salaries, but for the 1925-26 academic year he received only $400 exclusive of salaries.[77] In 1931, that amount had risen to the sum of $700.[78] By comparison, at the GBS, the Forest Service spend $7,984 on range research in 1922, $11,550 in 1929, and in 1930 the budget for range investigations increased by $14,500 as a result of the McSweeney-McNarry Forest Research Act.[79] UAC researchers could not realistically hope to receive comparable funding during those years; nevertheless, the pittance they received underscores the low level of importance attributed to range research. That would change, and during the 1956-58 period the

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[77] Director's Files, Utah Agricultural Experiment Station, Budgets, Box 1, fd. 2.

[78] P.V. Cardon to R.J. Becraft, June 11, 1931; Director's Files, Utah Agricultural Experiment Station, Budgets, Box 1, fd. 2.

[79] See Annual Report 1922, Investigative Committee District 4, p.17; Annual Report 1929, Investigative Committee District 4, p.3; and Report of the Great Basin Experiment Station and District Investigative Committee, 1930, p.2.

agricultural experiment station spend $124,532 on range research.[80] Inflation by itself can not account for that change. Instead, it reflected a change in the research performed at USU, a change which brought the research more in line with the stockmen's interests, and which was rewarded with tremendous increases in the allocation of state funds and with grants from private sources such as the meatpacking giant Swift and Company.

Research at UAC, and the Challenge to the Forest Service

Under the leadership of Laurence Stoddart, UAC researchers gradually developed their own approach to range science, which satisfied the demands placed on them by their institutional context. Born in Colorado in 1909, Stoddart received an education both in management-oriented disciplines and in ecology. In 1931, he received his bachelor's in forestry from Colorado State University, and a year later that institution awarded him a M.S. in range management. Stoddart then moved to the University of Nebraska, where he studied under a former student of Frederic Clements, John E. Weaver, with whom he performed a study of the prairie's response to

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[80] A Brief Review of the Budget Requests of the Utah Agricultural Experiment Station, Utah State University, Logan," Director's Files, Utah Agricultural Experiment Station, Budgets, Box 1. Unfortunately, the records of the budgets are incomplete.

the devastating droughts of the 1930's, which also became the topic of his dissertation.[81] In 1935, Nebraska awarded him the Ph.D. in plant ecology and soils. Dr. Stoddart had a distinguished career in range science, earning such honors as the presidency of the American Society of Range Management, which he helped to establish in 1948, and being named the chairman of the Range and Pasture Committee of the National Research Council. Except for a brief stint with the Soil Conservation Service in 1934-35, and a brief stay at Texas A&M in 1945-46, he spend his entire career at UAC. It shaped his career in ways he probably did not anticipate in 1935, when he became a member of its faculty.

UAC researchers categorized range management as a practical problem facing the stockmen, meaning the objective should be to increase the stockmen's profits. Not surprisingly considering its mission, UAC administrators and researchers defined the role of science in very utilitarian terms. Franklin S. Harris, the director of the agricultural experiment station between 1916 and 1921, and president of UAC in the 1940's, articulated perhaps the clearest statement of that attitude in a speech to the Utah Academy of Sciences. Reviewing the history of science, he concluded by repeating an

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[81] J.E. Weaver, L.A. Stoddart, and W. Noll, 1935, "Response of the Prairie to the Great Drought of 1934," Ecology 16(4):612-629.

argument Francis Bacon had made centuries earlier and extolled the virtues of science as a tool to achieve human purposes by dominating nature, which he considered a prerequisite for progress. "With the aid of science," he argued, "man is able to become master of his environment; he may harness the forces of nature and use them to advance his own welfare as well as to make the earth an abiding place worthy of his God-given intelligence."[82] This utilitarian orientation also influenced the researchers at the college and the experiment station. The role of the experiment station, according to range researcher Arthur Smith was "to provide means of increasing this [the agriculturalists'] income."[83]

UAC instantiated that categorization by studying the impact of the range on the stockmen's animals. UAC researchers focussed explicitly on increasing the productivity of the range. According to Stoddart in a lecture delivered to the faculty at UAC in 1945, "in the formative years of the science, there was an unfortunate emphasis placed upon

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[82] F.S. Harris, (1917) 1918, "The World Without Science," Transactions of the Utah Academy of Sciences, Volume I (1908-1917), 178-184, on p.184. As a member in good standing of the LDS Church, he also strengthened the link between science and religion, thus justifying the domination of nature not only in terms of human goals but also providing it with religious sanction. "Best of all," Harris continued, "he [man] is enabled to obey that part of the first command wherein he was given dominion over the earth and was required to subdue it."

[83] A.D. Smith, unpubl, n.d., "Range Research at the Utah Experiment Station," p.1, Stoddart Papers, Special Collections, Merrill Library, USU, Logan, Utah, box 15, fd. 6.

conservation at the expense of production."[84] Indicating that at some level he recognized the cause behind UAC's orientation, he also claimed "stockmen have felt keenly and have openly resented the lack of production emphasis on the part of range management technicians."[85] Eleven years later, C. Wayne Cook, who performed many of the actual experiments, repeated the message. "Technical range managers need to be better informed in matters dealing with the nutritional qualities of the various plant species," Cook argued, "so that management considers not only the effect of livestock upon the forage but also the effect of the forage upon the livestock."[86]

Ironically, Cook's statement indicated the potential complimentarity of UAC's perspective and the Forest Service's perspective. But initially, that was not to be. UAC researchers accepted much of the knowledge developed by Forest Service researchers and cooperated with the Forest Service in certain projects. However, they relied on the studies of the vegetation's nutrient content to side with the stockmen in

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[84] Lawrence A. Stoddart, 1945, "Range Land of America and Some Research on its Management, Fourth Annual Faculty Research Lecture," (Logan, Utah: The Faculty Association, Utah State Agricultural College), p.8.

[85] Ibid..

[86] C. Wayne Cook, 1956, "Range Livestock Nutrition and Its Importance in the Intermountain Region, Seventeenth Annual Faculty Research Lecture," (Logan, Utah: The Faculty Association, Utah State Agricultural College), pp.23-24.

their negotiations with the Forest Service, and Stoddart also took issue with Forest Service research on methodological grounds. The two orientations clashed head on in the late 1950's and early 60's.

Developing their own role as researchers took time, and for a number of years researchers at UAC performed exactly the same research as the Forest Service. Becraft proposed two main range research projects in the 1920's. One examined the possibilities of reseeding the range, and the other was a range survey to determine the condition of Utah's ranges and the impact of current range management practices. He performed both in cooperation with the Forest Service, and during these years UAC functioned in many ways as an extension of the Forest Service. The Forest Service usually used such surveys to propose or mandate changes in management practices and reductions in the number of stock stockmen could graze on a certain area. These surveys were therefore not popular with stockmen, and their unpopularity may well have affected this project at UAC. In 1929, within one year after the reestablishment of the Department of Range Management, P.V. Cardon, at the time the director of the Agricultural Experiment Station, reviewed the project and decided to cancel it. Although Becraft had proposed the project in 1918 when he arrived at UAC, "for various reasons practically nothing has been done with it," primarily "owing to lack of personnel and supporting funds."[87]

Within a few years after the reinstatement of the Department of Range Management, UAC researchers began to study the impact of the range on the stockmen's cattle and sheep. UAC researchers started to develop their own research program in the 1930's by making better use of the resources available at the school. In cooperation with the Animal Husbandry and Chemistry Departments, Becraft initiated studies to improve the productivity of sheep and cattle on the range. In 1931, they undertook a study of corn and cottonseed cake as a dietary supplement for sheep on the winter range in the desert. Three years later they added a study of the phosphorous content of summer range forage. On land made available by the Forest Service, which also served in an advisory capacity, the researchers grazed cattle, collected blood samples from the cattle and forage samples, which they "analyzed for phosphorous content to determine the correlation of [weight] gains, feed utilization, and phosphorous content."[88] The results of the project would indicate, they hoped, whether stockmen should provide their cattle with

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[87] P.V. Cardon to Dr. E.W. Allen, 16 October, 1929; "Progress Reports," Utah Agricultural Experiment Station, Box 7, fd. 1.

[88] Project 162, "The Phosphorous Content of Summer Range Forage and its Relation to Range Cattle Maintenance," 1934, p.1; Utah Agricultural Experiment Station, Progress Reports, Box 27, fd. 6.

supplemental feed to compensate for phosphorous deficiencies, which indeed turned out to be the case.

So far, UAC's research complemented the Forest Service's, but in the 1940's research by Stoddart began to test some of the Forest Service's knowledge. Stoddart initially continued Becraft's projects along he same lines. In 1940, however, he greatly expanded the scope of his research by revising and combining both projects into a much more comprehensive study of the "Nutritive Value of Range Forage in Utah." Six years later he also dropped the qualifying reference to Utah, and revised the project to become a study of "The Nutritive Value of Range Plants." The objective of these studies, performed in cooperation with the Animal Husbandry Department, remained examining whether stockmen should provide their animals with supplemental feed. Significantly, however, the Forest Service did not participate in these studies at all, and one of the justifications for the study related the vegetation's nutrient content to the time of year the stock grazed the vegetation. Previous research at other universities had shown that "the stage of maturity and season of the year influences very markedly the amount of nutrients in forage that are available for livestock."[89] The question thus already emerged whether

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[89] Project Outline, Project 132 (Revised), 1940, p. 3; Progress Reports, Director's Files, Utah Agricultural Experiment Station, Box 20, fd. 15.

the management practices the Forest Service required the stockmen to use, especially the postponement of grazing under the deferred-rotation grazing system, did not cause the animals' nutrient deficiency. This research therefore potentially had major implications for the management of the range. Stoddart subsequently proposed to study fluctuations in nutrient content during the course of the season because such knowledge "might prove to be important indexes to the response of livestock production to deferred and rotation grazing."[90]

The establishment of a research program based on UAC's categorization of range management, and the direct testing of Forest Service range science, really took off when UAC acquired its own land for research. In 1943, the college was finally able to purchase the land it had long needed. It bought land in southern Utah, near the town of Cedar City, and immediately outlined a program for range research at its Branch Agricultural College there. The land consisted of summer range in the mountains, and it still needed access to winter ranges in order to mirror as closely as possible the stockmen's practices during the course of an entire year rather than merely one season. The Interior Department's

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[90] "Suggested Outline for Regional Research Project - Range Improvement," n.d., p.3; Stoddart Papers, Box 16, fd.5 & fd.6.

Grazing Service was cooperative enough to give them access to winter ranges on the land it controlled.

At the Branch Agricultural College, research focussed predominantly on production. Articulating "a philosophy" to provide the basis for its research, UAC researchers argued "The field of research in range management as it relates to sheep production is relatively new."[91] Its research at Cedar City would rectify this problem, and in the process critically examined Forest Service researchers' claims. Continuing the articulation of their philosophy, they linked management to the productivity of the range, claiming "The technique and procedures of sheep management and range production are plainly and thoroughly interwoven."[92] To achieve their objective, they proposed continuing the studies of the chemistry and nutrition of forage species, and extended these studies into analyses of management systems proposed by the Forest Service. Those nutrition studies should not only analyze the effect of various species on the stock animals, but should also include analyses of "herding compared to free movement ... and [of] rotation versus continuous grazing," to determine which systems increased production the most.[93]

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[91] Memorandum for the Files, Subject: The Cedar City Research Program, 19 November 1943, p.1; Papers of E.G. Peterson, Box 154, Fd. 5.

[92] Ibid., p.3.

[93] "Outline of a Range Sheep Production Research Program At Cedar City," p.3; Papers of E.G. Peterson, Box 154, fd. 5.

Forest Service researchers at the GBS resented the college's independent research efforts at Cedar City. In 1943, Stoddart send a memo to the dean of the School of Forestry, Lewis Turner, complaining about the Forest Service's behavior towards the research at the Branch Agricultural College. He claimed the Forest Service had in the past "blocked attempts of the college to secure land," forced cooperation on UAC researchers "through appeals to Washington and President Peterson," and now attempted to block UAC's "cooperation with the Grazing Service."[94] There was indeed some competition between the two federal land management agencies, and previous studies of the USDA indicated its researchers sometimes tended "to consider the Experiment Station workers with whom they have cooperative relations merely as field representatives of the Department."[95] Forest Service researchers thus had reason to fear losing their cooperative arrangements with UAC's researchers.

But conscious of the integrity of his research and of his audience, Stoddart maintained his distance. Stoddart thought UAC should not cooperate with the Forest Service in these studies because it would affect both the actual research and

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[94] Memo, L.A. Stoddart to Lewis M. Turner, November 23, 1943; Stoddart Papers, Box 12, fd.3.

[95] Rowley, 1985, pp.155-159; "Report of the Committee on Federal-State Relations in Agricultural Research," Director's Files, Utah Agricultural Experiment Station, Box 3, fd. 38.

the stockmen's perception of the research. "Active cooperation with an agency concerned with administration of federal ranges and an agency with well defined policy to contend with," he argued, "colors research both actually and in the minds of stockmen."[96] Cooperation with the Grazing Service apparently did not pose the same problems, perhaps because it had not yet established a well-defined policy, and because the stockmen, after all, exerted significant control over the Grazing Service. UAC researchers, including Stoddart and Cook, also continued to cooperate with the Forest Service in a number of other projects, including some nutrition studies to determine the kinds of supplemental feeds stockmen should use. Nevertheless, at least in this research they wanted to maintain their independence from the Forest Service.

It would be a mistake, however, to consider the UAC range scientists merely as conduits for the stockmen's interests, as merely wanting to satisfy the stockmen's economic criteria, because like their peers in the Forest Service, they also wanted to satisfy the epistemic criteria of science. For example, measuring the effect of the range on the stockmen's animals proved more difficult then they at first anticipated. They could not satisfy the criterion of predictive power. Following the animals around the range, observing the kinds

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[96] Ibid.

and amounts of forage they consumed, and then later analyzing the chemical composition of the relevant vegetation and of the contents of the animals' stomachs, they thought they had measured the stock's forage consumption. However, this method did not provide data they could use to make predictions about the effect of the range on the animals because "no trend of correlation was found."[97]

Because both the method and the data it produced were therefore unusable, they abandoned this method and developed another one. Collecting samples of the vegetation before and after grazing, they measured the difference in weight and chemical composition between the two samples, which served as a "measure of the nutrient content of the ingested forage."[98] But they realized the ingested forage did not give a measure of the digestibility of the nutrients in the forage, therefore did not equal consumed forage, and therefore did not satisfy the criterion of accuracy. Chemically analyzing the animals' feces provided a measure of the undigested forage. Once they possessed this measurement, they could calculate the effect of the forage on the animal by subtracting the undigested nutrients from the ingested nutrients. This combination of

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[97] "Final Report, The Nutritive Value of Range Forages in Utah," p.1; Progress Reports, Director's Files, Utah Agricultural Experiment Station, Box 20, fd. 15.

[98] "Annual Project Report and Work Plan, The Nutritive Value of Range Plants, Project 260, 1949," p. 6; Stoddart papers, Box 14, fd. 3.

measurements enabled them to determine accurately and then predict the effect of all major forage species, and hence of most range conditions, on the stockmen's animals.

Out of this research emerged a critique of Forest Service range management and the science it was based on. USU researchers questioned whether managing to achieve the climax should be the objective for lands managed for grazing. In 1960, Stoddart argued range managers should consider the nutritive value of the vegetation as a factor when determining a range's carrying capacity, or its stocking rate, as he preferred to call it.[99] He made his strongest objections in 1967 or 1968, though the claims he made then had provided the basis of his and Cook's research since they read their papers before the UAC faculty.

He challenged both the equation of ecology and economics and the way the Forest Service used the "natural area" concept. Stockmen thought the "natural area" concept worthless because of its "implied objective; namely, having economic, producing ranch land appear the same as unused land." And indeed, range managers used the concept in that way because they concluded "that climax vegetation is a "must" objective in range management." As a result, "Range condition classification ... measures deviation from natural condition."

[99] L.A. Stoddart, 1960, "Determining Correct Stocking Rate on Range Land," JRM 13(5):251-255.

This, Stoddart argued, was seriously flawed. On lands used for grazing, managers should not attempt to achieve the natural climax because "After all, cattle and sheep themselves are not natural." It was also "wrong to imply that climax ranges are necessarily the best ranges" because research at UAC had shown the climax vegetation did not provide the most nutritious vegetation. In fact, what management considered a deteriorating range from the perspective of its ecological condition might provide more nutritious forage "because of better balanced diets." To the stockmen he explained the managers should nevertheless continue to use the concept because it provided a means to distinguish a range with very little vegetation of low vigor due habitat limitations from a range where mismanagement had caused that condition. He also warned the managers to take care when they used the concept. Economic use of the range was "not always consistent with the esthetics of "naturalness."" "The because-its-natural-its-best attitude is rather belied," he argued, "by the tripled forage production sometimes attainable in Western America by use of completely unnatural [i.e., introduced] grasses such as crested wheatgrass."[100]

In short, managers should judge the condition of the land

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[100] L.A. Stoddart, unpubl., "The Role of Natural Areas in Managing Ranges," outline of a paper presented at the American Society of Range Management meeting in Albuquerque, NM, Feb. 14, 1968; Stoddart Papers, Box 4, fd. 2.

in terms of its use and its productivity with respect to a certain kind of use, not in terms of some theoretical standard such as its ecological condition. It did not make sense to require that land used for grazing exhibit the natural process of succession toward the climax because grazing was an unnatural interference. Rangers could, however, require that land used for other purposes satisfy those processes. Stoddart did not deny the validity of all the knowledge the Forest Service researchers had produced. He merely wanted to distinguish between ecology and economics and judge range conditions not in terms of their ecological condition but in terms of their economic condition, their ability to produce. Managers should therefore also adjust the meaning of conservation according to the use of the range. On areas used for grazing, conservation did not mean conserving the range as an ecological entity. Instead, it meant conserving the range as a producer of a forage. Stoddart wanted to transform the sections of the Western range used for grazing into either artificially or naturally revegetated unfenced pastures containing the most nutritious species. Conservation then meant preserving those species to prevent erosion and sustain production over time, while simultaneously maximizing production in the short term.

Stoddart also critiqued the stockmen, although in general UAC's research program reflected their interests. They had to conserve the range because "it is our wealth and our future."[101] Applying the appropriate range management techniques, including rotation grazing, therefore only served the stockmen's own best interests. To accomplish conservation, he advocated government ownership of the land because it provided "the land with the services of technical managers." If those experts decided to cut the number of animals allowed to graze a section of range, the stockmen should shoulder some of the blame (although that did not mean he would not support the stockmen if he thought it right). "Federal range permit cuts" he argued, "are as likely to result from poor management as from any inherent productivity limitation on the part of the range itself."[102] This attitude remained fairly constant at USU, and was repeated, for example, in 1969, when a professor told his students in a range management course that stockmen should embrace conservation even more completely than the Forest Service.[103]

Once they established their own role, UAC researchers build a successful research program that satisfied economic and epistemic criteria simultaneously. They published both methodological papers and the results of their studies in a variety of peer-reviewed journals, experiment station

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[101] Stoddart, 1960, p.251.
[102] Ibid., p.255.
[103] "Technical Problems [RS 164]," Stoddart Papers, Box 8, fd. 4; emphasis in original.

bulletins, and in sources more accessible to the stockmen, such as Farm and Home Science.[104] More important given the institutional context of their research, they thought they had satisfied the stockmen. "We feel that the range research which has done to most for the direct use of the livestock industry," Stoddart told W.R. Hanson, a Canadian forester, "is the cooperative study between our department and the animal husbandry department dealing with range nutrition."[105] The stockmen and their allies agreed, if we can take the funding of range research as an indication of their opinion. In the early 1940's, the Department lost its accreditation because it was understaffed due to a lack of funding. It never experienced that problem again once USU researchers began to study the effects of the range on the stock animals systematically, and the budgets continued to grow. From $700 in 1931, and $124,000 in 1956-58, a two-year period, the budget more than doubled to $157,000 in 1966-67, a one-year

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[104] L.A. Stoddart, 1941, "Chemical Composition of Symphoricarpos Retundifolius as Influenced by Soil, Site, and Date of Collection," Journal of Agricultural Research 63(12):727-739; idem, 1944, "Gains made by Cattle on summer Ranges in Northern Utah," Utah Agricultural Experiment Station Bulletin 314; idem, 1946, "Some Physical and Chemical Responses of Agrophyron spicatum to Herbage Removal at Various Seasons," Utah Agricultural Experiment Station Bulletin 324; idem, 1948b, "Supplementary Feeding of Sheep on the Winter Range," Farm and Home Science 9(3):8-18; idem, 1952, "Range Nutrition Techniques," Journal of Animal Science 11(1):181-190.

[105] L.A. Stoddart to W.R. Hanson, October 2, 1962; Box 2, fd. 3.

period, and to $266,000 in 1973-74. Most of the funding came from the state, the general university operating budget, and private grants, all sources over which the stockmen could have exercised control, had they wanted to cut the budget.[106]

Grazing Controversy: UAC versus the Forest Service

UAC had resolved the tension due to range science's dual orientation in a radically different way than the Forest Service, and a clash was inevitable. It came as the result of a grazing controversy between the stockmen and the Forest Service that festered all through the 1950's. Throughout that decade, the Forest Service wanted to cut the size of the herds the stockmen grazed on the National Forests, mainly

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[106] Director's Files, Utah Agricultural Experiment Station, Box 1. USU researchers also performed some research that on the face of it conflicted with the stockmen's interests, although it was consistent with the researchers basic orientation. For example, in the early 1950's, sheepmen in southern Utah and the surrounding states lost a number of sheep, perhaps due to radiation fall-out from nuclear testing, or so they thought. USU range scientists, veterinary scientists, and animal husbandrymen, in cooperation with the U.S. Atomic Energy Commission, undertook a study, entitled "The Effect of Level of Nutrition on the Pathology and Productivity of Range Sheep," to determine whether the losses could be due to the effect of the range on the sheep (Stoddart Papers, Box 15, fd. 10). On the one hand, it made sense to rule out all alternative causes; but potentially this study could absolve the government of all liability, leaving the sheepmen with a bunch dead sheep on their hands. Apparently, in this case it did not matter that USU researchers cooperated with an agency that had a definite policy objective, although the AEC at this time did not have the same connotations in the minds of the stockmen as the Forest Service.

because the ranges were overgrazed, and partly because Utah's and America's expanding urban population put pressure on the Forest Service to set aside more land for hunting and other forms of recreation, and for wilderness and watershed protection.

Seeing their livelihood threatened, the stockmen reacted in a number of ways, including appeals to traditional American values. Through the Mormon Church, they appealed to values that had been prominent since the American Revolution, during Andrew Jackson's administration, and were at the heart of several protest movements questioning the modernization of America, most notably the Populist movement and Huey Long and Father Coughlin's protest movements during the Great Depression. This aspect of American culture was the fear of centralized power and the loss of individual and local control.[107] The Church in the 1950's attacked big government. Hoping to soften its stance somewhat, the Forest Service send two representatives, Ivan Dyreng and Julian Thomas, to meet with Church officials. Meeting with Church

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[107] Bernard Bailyn, 1967, The Ideological Origins of the American Revolution (Cambridge: Harvard University Press), argues the fear of centralized power was one of the causes of the Revolution; and this same fear was also very strong during the Jacksonian Era, see Leonard L. Richards, 1970, Gentlemen of Property and Standing (New York: Oxford University Press). On Populism, see Goodwyn, 1978. On Long and Coughlin, see Alan Brinkley, 1982, 1983, Voices of Protest: Huey Long, Father Coughlin, and the Great Depression (New York: Vintage Books).

official Henry D. Moyle in 1950, they hoped to avoid getting dragged into its fight against big government, and to point out that "F.S. Administration of local problems does not come from Washington."[108] They were unsuccessful. Moyle agreed the stockmen had overstocked the ranges, but wanted "local (community) government to control it," claimed "stockmen who use the range have the only right to it," argued "we would have to get democratic control by local people even if we have to sacrifice the natural resources," and attacked the federal government as dictatorial because "local people do not have chance to select local government officials or setting policies [sic]."[109] In 1957, the Uintah Sheep Grazers Association also attacked the Forest Service as "dictatorial," a charge repeated later that same year by J. Reuben Clark, Jr., a Utah cattleman, former law officer for the U.S. Department of State, and former ambassador to Mexico.[110]

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[108] Memorandum for the files, "Points of Approach," Manti-LaSal National Forest, Correspondence, Supervision, Church Affiliations, 1950; Discussions on Land Control Between LDS Church and Forest Service Officials, 3/22/50, item 4. Copy at Utah State Historical Society. Emphasis in the original.

[109] Memorandum for the files, "Henry D. Moyle," Ibid.. Emphasis in the original.

[110] [Uintah Sheep Grazers Association] to Floyd Iverson, Regional Forester, April 5, 1957; Uintah Sheep Grazers Association, Correspondence, Box 4, fd. 10, Utah State Historical Society. J. Reuben Clark, Jr., "Grazing on the National Forests," address delivered before the Utah Cattlemen's Association Convention, Salt Lake City, December 13, 1957; Director's Files, Utah Agricultural Experiment Station, Box 8, fd. 4.

This strategy had not worked before, it did not work now, and the stockmen turned to USU; in the new context of grazing, their best hope lay in countervailing expertise.[111] The "new order" had been firmly established. Traditional values had certainly not become irrelevant, but the new values were firmly institutionalized and had at least become equally influential in American culture, if not more influential. The objectives people hoped to achieve, and the institutional context where they worked, then exerted a tremendous influence on people when they decided which values to apply. Certainly in the Forest Service and other governmental scientific and management agencies, the new values took precedence over the old. Even Ezra Taft Benson, an influential member of the Church hierarchy and later its President, ruled against the stockmen when he served as Secretary of Agriculture during the Eisenhower administration. The controversy, however, was not finished, and the stockmen possessed an ally who had institutionalized those same values, but in a different manner. Among USU's departments, the Extension Service became involved first. USU had appointed its first range management extension specialist in 1955. During the height of the controversy in the late 1950's the extension specialist was

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[111] What follows is based on Alexander, 1987a, pp.167-168. However, Alexander misrepresents USU as merely a mouthpiece for the stockmen because he does not analyze the research on which USU's claims were based.

John Valentine, and he defended the stockmen's interest. But he could not convince the Forest Service, and the stockmen next called on Cook and Stoddart to testify on their behalf.

The issue centered to a large extent on the condition of the range, and to some extent on the economic consequences of the proposed cuts. Looking at the same range, Forest Service and USU experts interpreted what they saw very differently. The Forest Service, judging the range in terms of its ecological condition and managing the range to achieve the climax, argued the stockmen had to accept the cuts. Cook, Stoddart, and Valentine disagreed. They argued the range contained vegetation and hence erosion would not be a serious problem, and they judged the vegetation's condition in terms of its current versus its potential productivity. Judged in those terms, the range was in a good condition, as evidenced by the condition of the stock. Stockmen had used the stock's condition as an indicator of range condition since before the USDA began to perform range research in 1897, and it had proven a very unreliable guide. Now, however, that indicator was based on Cook and Stoddart's research.

The resolution of the controversy did not settle much between USU and the Forest Service, though it must not have pleased the stockmen. First Floyd Iverson, the regional forester, ruled against the stockmen on the basis of research by Forest Service range scientists. The stockmen appealed to the Chief of the Forest Service, Richard E. McArdle. Reproducing the Forest Service's categorization of range management, he also decided against the stockmen. Although the USU researchers raised valid questions about the Forest Service's methodology for judging range condition that merited further consideration by his own research branch, McArdle did not think them sufficient to overrule his own researchers and range managers.[112] In short, although McArdle did not question the validity or legitimacy of the USU researcher's claims as range science, their knowledge nevertheless could not help the stockmen win in their negotiations with the Forest Service. At least for now, the major contribution of USU research from the stockmen's perspective became the advice its range science scientists could give them about nutrient deficiencies and supplemental feeds for animals grazing the range. On the other hand, the research at USU and the Forest Service in may ways complimented each other. USU studied the impact of the range on the stock animals, while the Forest Service studied the impact of the stock animals on the range. Nevertheless, due to the context surrounding these two research programs, in particular the different relationship each occupied with respect to the stockmen, they could not complement one another.

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[112] Alexander, 1987a, 168.

Conclusion

SCIENCE AND POLITICS: THE DUAL ORIENTATION OF RANGE SCIENCE

The crucial factor for the development of range science was the construction of an equivalence between social and scientific criteria so the knowledge satisfied both sets of criteria simultaneously. Range science became linked with specific political objectives, usually centered around the economics and management of ranching. Those political objectives provided the motivation to institutionalize range science and a justification for performing range research.[1] Yet range science was also oriented toward science. Range scientists had to satisfy a social definition of science, which mandated that their claims satisfy scientific or epistemic criteria such as accuracy, explanatory and predictive power, etc.. In short, early in its development, the discipline acquired a dual orientation toward both science and society.

The dual orientation toward both social and epistemic criteria introduced a tension into range science, and range researchers had to resolve that tension in order to develop knowledge. It was not obvious how researchers could or should simultaneously satisfy the criteria arising from both

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[1] In an unpublished manuscript, Thomas R. Dunlap presents a similar argument. He argues the relationship with political authority was crucial for the institutionalization of wildlife management.

orientations. Researchers in different institutional contexts developed distinct resolutions to that tension. In this case, the most significant differences between the institutional contexts was a difference in the social relations, in particular the power relations, between range researchers and their audience. Hence the politics of range science involved not only its political objectives but also included the social relationships in which this discipline was embedded. The power relations defined the social criteria and provided the background to judge the acceptability of particular resolutions of the tension. They thereby enabled and constrained researchers' attempts to satisfy social and scientific criteria simultaneously.

This conclusion undermines the traditional image of applied science. According to the predominant image, applied science is a linear and sequential process; it is, simply, the use of science to solve society's problems. First a science develops new knowledge, and applied scientists then apply that knowledge to solve society's problems. Applied scientific knowledge development, according to this view, consists of finding ways to use science. The only exception is if the sciences have not yet developed the necessary knowledge, in which case the applied scientists first develop the scientific knowledge and then apply it. At any rate, according to this linear and sequential model, there is a sharp distinction between epistemic or scientific criteria and social criteria. Knowledge first has to satisfy the epistemic criteria of science, and applied scientists next use this certified knowledge in ways that also satisfies social criteria. This view then identifies two problems facing the sequential process: maintaining the autonomy of science and finding ways to use science's knowledge to solve society's problems.[2]

But this model does not fit the history of range science. Instead, the practice of applied scientific knowledge development is a simultaneous process rather than a consecutive process. Range scientists simultaneously satisfied the epistemic criteria of science and the social

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[2] For example, Dupree (1957) argues there are not "two forms of science, pure and applied, but only science and the application of science."(p. 155). Dupree thus clearly articulated a linear and sequential model of applied science, as does Margeret Rossiter (1979) in her review of the history of the agricultural sciences. I do not mean to pick on just a few people. This presumption has been typical in many analyses of agricultural science until recently. And recent models of applied science that reject the linearity of this traditional view nevertheless still present a sequential picture of applied science and maintain a sharp distinction between epistemic and social criteria. See, e.g., Matti Sintonen, 1990, "Basic and Applied Sciences - Can the Distinction (still) be Drawn?" Science Studies, 3(2):23-31. This traditional model has also provided the basis for most accounts of range science; see Rowley, 1985; Alexander, 1987a, 1987b; Tobey, 1981; McIntosh, 1985; and Frank N. Egerton, 1985, "The History of Ecology: Achievements and Opportunities, Part Two," Journal of the History of Biology, 18:103-143, argues explicitly that for a study to count as a history of applied ecology it should not consider "either the social context that led to scientific investigation or the policies that were implemented as a result of the investigation."(pp. 103-104)

criteria that flowed from different political objectives and different power relations between researchers and ranchers. Range science was therefore not just the application of science to solve social problems, and it also was not just politics by another means.

Range science developed at a time when America increasingly looked to science to solve economic, political, and social problems in the hope that science's ability to predict could solve those problems by providing the basis for organization and rational management. The context and sheer size of the ranching operations in the West and Southwest had created a situation where individual ranchers caused severe problems for themselves and for the livestock economy as a whole by destroying the range. Federal government agencies and scientists stepped in to solve those problems.

Initially, range researchers could not develop authoritative knowledge to solve those problems because they could not satisfy both sets of criteria simultaneously. Knowledge about the range that satisfied epistemic criteria, such as Smith's argument for deferred-rotation grazing and Griffiths' data, did not satisfy the social criteria. In that sense, the knowledge these researchers produced was invalid as applied science. Either the stockmen rejected it as a solution to the problems they faced on the range, or the researcher himself could not determine the meaning of the knowledge for the management of the range because he lacked the necessary social criteria to make that determination. However, other scientists, in particular Frederic Clements in ecology, did respond favorably to the knowledge because this same knowledge, and the methods that produced it, were epistemically valid. We may therefore have to turn the traditional image of applied science on its head. That is, it appears that a crucial factor for the development of the science of ecology may have been its relation to applied science, in particular range science, rather than vice versa.[3]

Two different categorizations of the problem on the range, one first developed in the Forest Service and the second developed in response to the first by researchers at USU, enabled and constrained range researchers' development of knowledge that satisfied both sets of criteria simultaneously. When the Forest Service institutionalized range science and linked it directly to its policy objectives, it provided a justification and stable foundation for research. Categorizing range management as a problem it faced, the Forest Service empowered the epistemic values because it used range science to justify its management of the stockmen who

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[3] Ronald Tobey, 1976, "Theoretical Science and Technology in American Ecology," Technology and Culture, 17:718-28, argues explicitly that applied concerns motivated Frederic Clements, but he does not analyze the connections between this range research and Clements' work.

used the range. Range scientists' ability to measure, analyze, and predict replaced economic, political, and social considerations as the basis to manage the range, at least in the sense that the knowledge developed by range scientists provided the basis for the negotiations between the Forest Service and the stockmen. However, this knowledge and the Forest Service's attempt to industrialize ranching did not just impose itself on the stockmen. They responded in various ways to the Forest Service's attempt to manage them, in the end turning to countervailing expertise to justify their position. They found such expertise at the agricultural colleges, and the political objectives of the stockmen in their negotiations with the Forest Service gradually provided the justification and stable foundation for research at USU when USU researchers categorized range management as a problem facing the stockmen.

Along with the epistemic criteria, these categorizations embedded social criteria at the core of range science due to their focus on production. The Forest Service defined the stockmen as economic maximizers, and the Forest Service therefore set maximizing the range's production as its goal. Consequently, the knowledge Forest Service range scientists produced had to satisfy epistemic criteria and social criteria simultaneously. The epistemic criteria ensured the epistemic validity of the knowledge the Forest Service relied on to manage the stockmen. At the same time, the social criteria the knowledge also satisfied ensured the knowledge's applied scientific validity and justified the Forest Service's claim that it had the best interests of the stockmen at heart. The Forest Service's use of the concept "production," and the role it played as source of criteria to judge the validity of knowledge, thus indicated that the Forest Service was concerned about the same issue as the stockmen and that it attempted to solve the problems facing the stockmen. In contrast to the linear, sequential model of applied science, therefore, there was no sharp distinction between social and epistemic criteria in range science. The problem to increase production clearly shaped the practices of the Forest Service's range scientists and the knowledge they produced. Sampson argued the deferred-rotation grazing system satisfied both epistemic and social criteria simultaneously, and Hormay and Talbot later argued for the rest-rotation grazing system on the same grounds.

Once the social criteria became institutionalized, the Forest Service enhanced its power by constructing boundaries to demarcate the range scientists' research and knowledge from the activities and knowledge of non-researchers. The Forest Service, and consequently its researchers, already occupied a position of power vis-a-vis the stockmen because the Forest Service possessed discretionary power. In addition, due to the nature of the link between research and policy objectives, autonomy became a very important consideration for these researchers, and for the Forest Service. The autonomy of research served as one means to create the objectivity of the knowledge the range scientists produced by removing the appearance of non-epistemic influences on the knowledge development process. Creating objectivity was a necessity if range science was to function as the politically neutral body of knowledge that justified the Forest Service's authority to manage the stockmen. But the Forest Service institutionalized particular social objectives at the same time it institutionalized range science, and the construction of boundaries therefore did not eliminate the social criteria from the applied science knowledge development process. In fact, quite the contrary: the social criteria themselves played a crucial role in enhancing the Forest Service's power.

The way Forest Service's researchers and administrators used the social criteria in their practices reinforced their power. The way the Forest Service used the concept "production" had power effects because it severely limited the role of the stockmen in their debate with the Forest Service and located expertise with the Forest Service range researchers. The stockmen traditionally measured productivity in terms of the condition of the livestock; i.e., they used the concept to refer to the livestock. But Forest Service researchers and administrators used the concept to refer to the condition of the range as determined according to the applied ecological version of the theory of succession. Considering the condition of the ranges when the Forest Service initiated range research, its use of the concept to refer to the productivity of the range was certainly understandable. This use of the concept also provided a source of social criteria to judge the validity of knowledge claims the range scientists produced.

Nevertheless, the rationality of its use of the concept and the validity of the knowledge should not obscure the power effects of this use of the concept and of the knowledge, even though most Forest Service researchers and administrators most likely were not aware of these power effects. While the stockmen could measure the condition of the animals, they did not possess the requisite skills to measure the condition of the range and hence could not satisfy the newly institutionalized epistemic criteria. Sampson drove both of those points home on several occasions. Thus, even while using the concept "production" in both its range research and range management practices enabled the Forest Service to claim it served the stockmen's interests, the way it used the concept enhanced the authority of its researchers and at the same time undermined the authority of stockmen's claims about productivity. It thus served to exclude many of the stockmen's arguments from the debate.[4]

Stockmen, who could not question the measurements of productivity, questioned whether the Forest Service's range scientists actually solved their problems. In his history of range management, Rowley implies range scientists faced such questions only during the initial phases of range research.[5] But stockmen did not just raise these kinds of questions during the very early history of range science; they kept raising them. The Forest Service's researchers continued to use increasingly more specialized skills in their research, and argued such scientification of research was the only way to develop knowledge to solve the stockmen's problems by increasing the productivity of the range.[6] In response, in the 1950's two stockmen published papers in the official journal of the American Society of Range Management questioning whether the range scientists solved the problems on the range as the stockmen understood them. They argued

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[4] Such use of terms in practice is an example of Lukes' (1974) second dimension of power because it can serve as an effective means of limiting the extent to which people can participate in decision making.

[5] Rowley, 1985, p.111.

[6] See, e.g., Arthur W. Sampson, 1936, "Research Problems Pertinent to Better Range Management," The Michigan Forester, 17(6):10-15; idem, 1955, "Where We have Been and Where We are Going in Range Management," JRM, 8(6):241-246; E.J. Dyksterhuis, 1953, "Some Notes and Quotes on Range Education," JRM, 6(5):295- 298.

range scientists should receive a more practical education and they saw the further scientification of research as a problem rather than as a solution.[7]

However, these arguments did not convince the Forest Service. They suffered the same fate as the stockmen's appeals to traditional American values. The Forest Service attempted to convince the stockmen it indeed tried to solve their problems by increasing productivity, and if that did not work, it simply brushed such questioning aside. In the new context of ranching that emphasized organization and rational management and that institutionalized range science, the stockmen became forced to rely on a different kind of counter-argument. To achieve their political objectives in the negotiations with the Forest Service, they needed arguments based on their definition of productivity that satisfied social and epistemic criteria simultaneously.

The stockmen found the requisite countervailing expertise at USU, an institution where they occupied a position of power vis-a-vis the researchers and therefore determined the categorization of the problem. As a result of this power relationship, USU range scientists categorized the problems on

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[7] See, e.g., George E. Weaver, 1953, Range Education from the Ranchers' Viewpoint," JRM, 6(5):307-308; E.O. Moore, Jr., 1954, "Range Management on the U Ranch near Carlsbad, New Mexico," JRM, 7(1):23-24. I have found one range scientist who agreed with these stockmen; see H.R. Hochmuth, 1954, "A Continuing Appraisal of Range Management," JRM, 7(4):147-148.

the range as problems facing stockmen rather than as problems facing professional managers. This categorization in turn influenced their definition of "production." When USU researchers talked about production, they referred not to the condition of the range as determined ecologically but to the condition of the livestock. Defining the problem of increasing productivity in these terms securely institutionalized range science at USU because it provided a justification that satisfied the stockmen who controlled the fortunes of the discipline at the college. It matched the objectives of the range scientists with those of the stockmen. The stockmen did not think that was the case in the way the Forest Service practiced range science and range management because, although "Range management should be as welcome to the rancher as rain," according to Stoddart, "Too often he associates it with curbs and controls - he fears it."[8] Nevertheless, he continued, "I do not believe we are basically different in our objectives," and he thought range scientists should engage in a "Fearless examination of principles, techniques and policies"[9] to achieve that unity.

On the basis of their categorization of range management, USU researchers initiated an alternative range research

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

[9] Ibid..

program that satisfied social and scientific criteria simultaneously and critiqued the Forest Service's construction of simultaneity. It would therefore be a mistake to consider the USU researchers merely as conduits for the stockmen because in their research they also satisfied the epistemic criteria of science. Attempting to measure the livestock's productivity, they relied on epistemic criteria when they decided to abandon one research method and develop an alternative. Nevertheless, both the epistemic criteria and the social criteria they derived from their definition of production simultaneously shaped their research practices, and the resulting knowledge therefore satisfied both sets of criteria at the same time. Similarly, Stoddart based his critique of Forest Service methods, concepts, and policies on knowledge that satisfied both sets of criteria simultaneously and on his categorization of the problems on the range.

These two categorizations of range management complimented each other. Managing the range was a problem facing both the professional experts of the Forest Service and the stockmen. Furthermore, range scientists studied the interactions between humans and the range, and for ranching, those interactions flowed in two ways: the effects of grazing on the range, and the effect of the range's vegetation on the livestock.

However, due to the contexts surrounding both, they could not be accommodated within the graded structure of one category. Because of the political objectives and the relationship between researchers and stockmen characteristic of the institutions where researchers performed range research, the graded structure of the category was simply not that flexible. The Forest Service's categorization of the problems on the range, for example, was flexible enough to allow for debate among its range scientists and to incorporate research performed by other range researchers. It was not flexible enough to accommodate studying productivity in terms of the impact of the vegetation on the livestock, even when its own researchers advocated such research. Approaching the problem from that perspective would not have provided the Forest Service with additional tools to manage the stockmen. As its own researchers recognized, such research might in fact produce results that could undermine the validity of the grazing systems the Forest Service used to manage the stockmen. It might, that is, undermine the Forest Service's categorization of the problem on the range by providing the stockmen with additional arguments in their negotiations with the Forest Service. Not surprisingly, therefore, this research became institutionalized in a context where the researchers categorized range science as a problem facing the stockmen. In short, as a result of the different contexts, what could have been accommodated within the graded structure of one category became alternative and opposite categorizations.

The knowledge developed within each categorization consequently called into question and challenged rather than complemented the knowledge developed in the other, and the ensuing controversy centered around the different constructions of simultaneity. That is, the debate was not about issues such as a paradigm, a theory, what counts as a replication of an experiment, or other issues often negotiated in scientific disciplines. Instead, it centered on the question which of the two approaches satisfied both social and epistemic criteria simultaneously. The Forest Service used the knowledge its researchers had developed to justify its authority to mandate changes in the stockmen's range management practices. In terms of the link between productivity and the condition of the range, the Forest Service saw a deteriorating range because the range was not in its optimum ecological condition.

USU range researchers disagreed on the basis of the link they had constructed between productivity and the condition of the livestock, and they questioned the validity of the Forest Service's connection between productivity and the ecological condition of the range. They thought the Forest Service should divorce economics from ecology because the link between ecology and economics did not satisfy the social criteria: the optimum ecological condition did not produce the greatest economic returns. The Forest Service should therefore determine the condition of the land in relation to the way it was to be used, rather than in terms of some abstract ecological standard. Stoddart and Cook argued the range was in good condition because it produced nutritious feed, as evidenced by the condition of the livestock. The stockmen therefore did not need to change their range management practices.

To resolve the controversy, the Forest Service constructed closure between the two camps at the level of range management. The Forest Service did not question the validity of the USU researchers' claims, acknowledged its own researchers should perhaps incorporate the USU perspective into their research, although without explaining how. Still it insisted the stockmen had to change their range management practices. Perhaps that was a foregone conclusion, given the Forest Service's categorization of the problems on the range. Within that categorization, furthermore, its decision was also rational and valid.

On the other hand, it did not construct closure at the level of range research. At that level, the controversy did not decide between USU and the Forest Service. Both sets of knowledge were valid applied science within their respective categorizations of the problems on the range. Controversy erupted when a range scientist or manager used knowledge developed within one categorization and that therefore simultaneously satisfied both sets of criteria in one way, to judge claims made within the other categorization that satisfied both sets of criteria in another way. But so long as the contexts surrounding those categorizations remained the same, both groups would continue on their own course, and the knowledge each produced remained separate rather than complementary.

In short, the meaning of each group of range scientists' practices and of the knowledge they produced was socially located in their respective institutional contexts. The institutional context determined range scientists' categorization of the problems on the range on the basis of the institution's political objectives and of the relationship between the institution and the stockmen. Those political objectives and power relations defined the social criteria and provided the background to judge the acceptability of particular resolutions of the tension. They thereby enabled and constrained researchers' practices and the knowledge they developed.

Yet the social location of meaning did not mean each group's practices and knowledge claims were socially determined. The social criteria were not primary or decisive in and of themselves. Those practices and the knowledge claims simultaneously had to satisfy epistemic criteria. The practices and knowledge developed by both the Forest Service and USU range scientists replaced the practical knowledge the stockmen had accumulated on the basis of their experience precisely because the stockmen's knowledge did not satisfy the requisite epistemic criteria. As a result, the range scientists at both institutions could determine the accuracy, generality, testability, explanatory and predictive power, etc., of their claims about the relationships between humans and the range, something the stockmen could not do nearly as well. Compared to the stockmen, the range scientists therefore did have privileged access to those relationships.[10] Nevertheless, due to the social location of meaning, reconciling the knowledge claims and practices of the two groups of researchers required a change in the social context.

Beginning around 1960, the contexts of range research began to change and disrupted the simultaneity researchers had constructed between social and epistemic criteria. As a

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[10] Philosophical debates about realism, i.e., whether or not scientists and applied scientists have privileged access to nature or to relationships between humans and nature in an absolute sense, seem senseless. The best we can do is determine in relative terms whether one person or one group has privileged access as compared to some other person or group. The interesting questions then become how such privileged access is used and the effects it has.

result of social, economic, and cultural changes such as increasing urbanization, the rise of personal income, and the emergence of the environmental movement, range scientists and managers had to redefine the social criteria.[11] Various groups in society pressured the range scientists and managers because they wanted to use the range for recreational purposes or because they wanted to preserve the range in its unspoiled condition.

Multiple-use management replaced single-use management, which meant the Forest Service and BLM could no longer consider economic factors as the only relevant social criteria when developing range management policies. However, researchers at both institutions had used economic criteria as the sole social criteria to determine the meaning of their knowledge claims and their practices because they had accepted maximizing productivity as the only goal of range management. Both groups of researchers had therefore equated the condition of the range with the productivity of the range, although they disagreed about the meaning of productivity. But as a result of the shift to multiple-use management, they could no longer equate the condition of the range with its productivity. They had to redefine their social criteria to incorporate the new

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[11] On these changes in American society and culture and their relation to environmental management, see Petulla, 1977; Worster, 1977; Nash, 1982; Rowley, 1985; Hays, 1987; and Alexander, 1987a; Dunlap, 1988.

uses of the range, and as a result they had to re-establish simultaneity.

Recognizing the complementarity of Forest Service and USU research allowed them to re-established simultaneity between epistemic and social criteria. Range scientists gradually, very gradually in fact, reached the conclusion that the value of the range depended on the goals of management. Many factors, including political, economic, and environmental considerations, could provide the basis for those goals, and hence the basis to determine the value of the range. Yet up until this point in time, the Forest Service, due to the equation of economics and ecology, had determined the value of the range in terms of the range's ecological condition. Range scientists began to argue their knowledge and ability to make predictions about the ecological condition of the range provided management agencies with one, but only one, basis to determine the value of the range, in addition to economic, environmental, and etc., considerations. This approach became firmly institutionalized only in the 1980's, even though the pressure had existed for two decades and range scientists had gradually accommodated those pressures. They argued determining range condition should become a two-step process. "The first would recognize the ecological status of plant communities ..... The second ... would be oriented toward use by cattle, wildlife, watershed, recreation, and so on, and would rate ecological stages by value relative to specific uses."[12] Determining range condition and developing management plans would thus incorporate both the effect of the livestock on the range and the effect of the range on the livestock.[13]

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[12] James O. Klemmedson, Minoru Hironaka, and Bobbi S. Low, 1984, "Inventory of Rangeland Resources: Summary and Recommendations," in National Research Council/National Academy of Sciences, 1984, pp.571--592, on p.585.

[13] Based on my argument that range scientists attempt to satisfy social and epistemic criteria simultaneously, I would also expect that changes in science require a reconstruction of simultaneity. However, changes in science do not appear to have been involved in constructing the complimentarity between Forest Service and USU research. Furthermore, the impact on this process of the major change that has occurred, the switch to ecosystem ecology, is somewhat ambiguous. The first use of the ecosystem concept in range science, Ellison (1949), had no discernable impact. The ecosystem concept became widely used in ecology only with the publication of E.P. Odum, 1953, Fundamental of Ecology (Philadelphia: Saunders). In 1969, George Van Dyne argued for the adoption of the ecosystem concept in range and other natural resource management; George M. Van Dyne, ed., 1969, The Ecosystem Concept in Natural Resource Management (New York: Academic Press). But the ecosystem concept was adopted in most range management curricula only in the 1970's; see R. Nicholson, 1988, "Range Management from Grassland Ecology," in P.T. Tueller, ed., 1988, Vegetation Science Applications for Rangeland Analysis and Management; part 14 of Handbook of Vegetation Science, H. Lieth, ed. in chief (Dordrecht: Kluwer Academic Publishers), pp.399-424, on p.414. The ecosystem concept is now accepted by most range scientists. However, a more detailed analysis of the impact of this concept will have to await further study.

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