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"The Range Problem" After a Century of Rangeland Science: New Research Themes for Altered Landscapes

Nathan F. Sayre,[1] William deBuys,[2] Brandon T. Bestelmeyer,[3] and Kris M. Havstad[4]

Authors are [1]Associate Professor, Department of Geography, University of California, Berkeley, CA 94720, USA; [2]Conservationist and Author, Chamisal, NM 87521, USA; and [3]Research Ecologist and [4]Supervisory Scientist, USDA-ARS, Jornada Experimental Range, Las Cruces, NM 88003, USA.

Abstract

The rangeland science profession in the United States has its roots in the widespread overgrazing and concurrent severe droughts of the late 19th century. These drivers contributed to rangeland resource degradation especially in the American Southwest—what E. O. Wooton (1908) called the "Range Problem." Although logical for the time, the scientific activities and resulting policies that arose out of this catastrophe were based on reductionist experimentation and productionist emphases on food and fiber. After a century of science and policy, there are two additional perspectives that shape our vision for the emphases of the future. First, rangeland landscapes are extremely heterogeneous; general principles derived from scientific experimentation cannot be easily or generally applied without adjusting to the distinct societal and ecological characteristics of a location. Second, rangeland management occurs at spatial scales considerably larger than those that have typically been addressed in range science. Scaling up science results is not a simple, additive process. The leading features of the emerging science are 1) research at landscape scales and 2) over longer time spans that 3) approaches conservation and management practices as treatments requiring scientific evaluation, 4) incorporates local knowledge, 5) is explicitly applied in nature, and 6) is transparent in its practice. We strongly argue for a science that supports resource management by testing hypotheses relevant to actual conservation practices and iteratively applying its findings in partnership with managers in an ongoing, adaptive fashion.

Resumen

La profesión de ciencia del pastizal en Estados Unidos tiene sus raíces en el sobrepastoreo y recurrentes y severas sequias a finales del siglo XIX. Estos factores contribuyeron a la degradación de los recursos del pastizal especialmente en el Suroeste de los Estados Unidos—a lo que E. O. Wooton (1908) llamo el "Problema del Pastizal." Aunque por la lógica del tiempo, las actividades científicas y políticas resultantes que surgen de esta catástrofe fueron basadas en experimentación reduccionista y énfasis en producción de alimentos y fibras. Después de un siglo de ciencia y políticas hay dos perspectivas adicionales que dan forma a nuestra visión para enfatizar en el futuro. Primero, el paisaje del pastizal es extremadamente heterogéneo, principios generales de experimentación científica no pueden ser fácilmente o generalmente aplicados sin ajustes en las marcadas características sociales y ecológicas del lugar. Segundo, el manejo del pastizal ocurre a escalas espaciales considerablemente mayores a aquellas que normalmente se aplican en la ciencia del pastizal. Dimensionar los resultados de la ciencia no es un proceso sencillo y aditivo. Las características importantes de la ciencia emergente son 1) investigación a escala del paisaje y 2) sobre periodos largo de tiempo que 3) abarque practicas de conservación y manejo como tratamientos que requieren evaluación científica, 4) incorporar conocimiento local, 5) ser explicito aplicado a la naturaleza y 6) ser trasparente en su práctica. Argumentamos fuertemente por una ciencia que apoye el manejo de los recursos por medio de evaluar hipótesis relevantes a las prácticas de conservación actuales y que aplique sus resultados en sociedad con manejadores de manera adaptiva.

Key Words: applied science, ecosystem management, range science history, science–management linkages, translational science

INTRODUCTION: OUR HISTORY

The range science profession in the United States has its roots in the closing decades of the 19th century, when widespread overgrazing and severe droughts resulted in acute episodes of livestock mortality, accelerated soil erosion, and loss of native forage plants across much of the western United States. The crisis was worst in the Southwest—western Texas, New Mexico, and Arizona—and beginning in the 1890s, the US Department of Agriculture (USDA) sent a handful of special agents to the region to assess the damages, study the causes, and identify potential remedies. The work of these scientific pioneers helped shape not only the emerging discipline of range science but also the laws, policies, and institutions affecting rangelands and rangeland management throughout the nation (and, later, in many other nations as well). Among the institutional outcomes of these early assessments and reports were large experimental ranges, beginning with the Santa Rita Experimental Range south of Tucson, Arizona, in 1903 (McClaran et al. 2003) and followed in 1912 by the Great Basin Experimental Range in Utah and the Jornada Experimental Range in south-central New Mexico (Havstad et al. 1996). It is the centennial of the Jornada that this special issue of *Rangeland Ecology & Management* commemorates.

E. O. Wooton's (1908) report "The Range Problem in New Mexico," which inspires the title of this introductory paper, helped define the questions that range science would subsequently ask and the kinds of solutions that it would propose.

Correspondence: Nathan F. Sayre, 507 McCone Hall, University of California, Berkeley, CA 94720-4740, USA. E-mail: nsayre@berkeley.edu

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Wooton combined policy prescriptions with biological and geographical descriptions of the expansive and diverse rangelands of the Territory (statehood was still four years away). He diagnosed the Range Problem in the same way that H. L. Bentley (1898), Jared Smith (1899), and David Griffiths (1904) had done in west Texas and Arizona and that others would later do for the western United States as a whole (e.g., Clements 1920): unrestricted, open access to public rangelands had created what would now be termed a tragedy of the commons. "The open range is public property, and being a gift to no one in particular, and every citizen having a right to use it, he who takes all he can and takes it most quickly, gets most of it. . . . The stockman cannot even protect the range from himself, because any improvement of his range is only an inducement for someone else to bring stock upon it, and he thinks he had better put the extra stock on himself" (Wooton 1908, pp. 31–32). The solution, he wrote, was "not only possible, but has already been devised" and was "already known to stockmen" (pp. 4–5): divide the range into fenced units and control access through leases. "There can be but one conclusion to be drawn from these conditions. Every interest at stake—the stockman's interest, the best interests of the range, and the interests of the general public demand that the range be given into the control of such citizens as desire to use it and use it properly, and who may be given definite legal rights with respect to it and held personally responsible for the proper use and care of what is really public property" (Wooton 1908, p. 33).[1]

The Range Problem of Wooton's time was too many livestock, too often, and for too long, and the solutions proposed involved gaining control of the open rangeland, proper classification of capacities of different land types for livestock use, and identification of conservation principles. Control meant several things: government retention of public rangelands (rather than disposal), fencing to allow control of livestock numbers, allocation of the range to individual ranchers by lease, and ongoing government administration and management in collaboration with—and for the benefit of—lessees. Range science was born to support these interlocking policy endeavors, and its first task, in Wooton's words, was "the careful classification of the lands, with definite information as to carrying capacity of the ranges, the character of the forage, and climatic and other conditions likely to affect their value" (Wooton 1908, p. 39). This was necessary to inform decisions about how large the resulting allotments (leased areas) would be: large enough to support a family—but no larger because the policy goal was to maximize the number of families the range would support. Finally, conservation meant both preventing further deterioration and restoring past conditions. Restoration, it was believed, would result from accurate classification of livestock carrying capacity and proper control of stocking rates because the ecological paradigm of the time was that the range would recover on its own if overgrazing were avoided.

The initial foundations of range science included several assumptions, some explicitly stated within these early publications and manifestos and others more or less taken for granted. The prevailing use of rangelands as grazing lands was not questioned because the lands themselves were seen as suited only for livestock production. "Stockraising is one of the most important industries of the Territory and probably always will be" (Wooton 1908, p. 4). This was a crucial premise of the entire system: secure tenure would give lessees strong incentive to conserve the range precisely because no other profitable use could be made of it. Because the settlers were seen as newcomers to the region—longtime Spanish and Mexican ranchers were by and large overlooked—range science was seen as necessary to identify principles and develop practices that newly created agricultural extension offices would use to educate livestock producers about how rangelands functioned. The goals were candidly productivist: "The whole plan is based upon the one idea of making the public grazing lands more productive" (Wooton 1908, p. 40). This was seen as benefiting not only livestock producers but everyone as well. "Whatever will improve the conditions of the stock business and increase the output of beef and mutton, fat cattle and lambs, wool, mohair, and hides, or horses and mules, is bound to increase the general prosperity of the Territory, and make our social conditions better and life more worth living" (Wooton 1908, p. 4).

The underlying paradigm for range science was taken for granted because it was the prevailing one of the time: an agronomic approach based on controlled experiments and hypothetico-deductive reasoning. Plant communities were assumed to be in equilibrium with static edaphic and climatic conditions unless disturbed by exogenous (usually human) drivers, and studies were designed within a mechanistic and reductionist framework that sought to isolate response variables such as forage production and livestock weight gains while minimizing the effects of temporal variability and spatial heterogeneity. Experimental results were analyzed for specific treatment effects in hopes of identifying general techniques and technologies that would be applicable anytime and anywhere in the region. Provided that such practices were economically feasible—or could be made feasible through government support—their relevance to management could thereby be assumed.

The resulting publications from these early investigations on subjects such as land tenure (Wooton 1922), methods of increasing cattle production (Jardine and Hurtt 1917), livestock management during droughts (Jardine and Forsling 1922), and estimates of livestock carrying capacities (Wooton 1916) likely influenced emerging policies. The policy measures that Wooton and other early observers recommended were in fact implemented between 1905, when the Forest Service was created within the USDA, and 1934, when the Grazing Service, the forerunner of the Bureau of Land Management (BLM), was established under the Taylor Grazing Act to administer the remaining public lands within the US Department of the Interior. Western public rangelands were systematically divided into allotments, fenced, and leased to private ranchers based on myriad considerations, including privately owned infrastructure (such as wells and private water rights) and the interests of the local ranchers (Skaggs et al. 2011). Range scientists

[1]This solution reflected more broadly held perceptions of both conservationists and national politicians such as Teddy Roosevelt at the end of the 19th century. However, Wooton's manifesto was rejected for publication as a USDA Bulletin because it was perceived by upper-administration bureaucrats as a proposal that would increase regulatory duties of the USDA. Wooton instead published his 1908 report through the Albuquerque Journal Printing Office in New Mexico.

classified the lands according to plant communities and developed quantitative measures of carrying capacity that were used to establish limits on livestock numbers that the US Forest Service and the BLM could enforce through lease agreements. The system was slowly and haltingly implemented—Depression-era jobs programs, as they emerged, provided the labor to build many of the fences, and the imposition of stocking limits was controversial and contested through most of the 20th century (Rowley 1985; Merrill 2002; Skaggs et al. 2011). Nonetheless, the new system established the scale at which range management was—and is—practiced throughout the West: ranches of 10^3 to 10^5 hectares in size, combining private lands and public allotments. Meanwhile, the discipline of range science grew rapidly, supported by public agencies, state land grant universities, and federal legislation such as the McSweeny-McNary Forest Research Act of 1928 (Chapline 1944). With the foundation of the American Society for Range Management in 1948, range science achieved a long-sought institutional identity distinct from both agronomy and forestry.

The institutional framework of US rangeland management has changed little since the mid-20th century, but our scientific knowledge of rangelands has changed dramatically. Experimental ranges, including the Jornada and many other academic and federal research locations, such as the Santa Rita and the Great Basin experimental stations, have played a major role in these changes, albeit in a somewhat roundabout and unanticipated way. One of the rationales for establishing large experimental stations and ranges was that they would match the spatial scale of actual ranches, helping to ensure applicability of their science. One unintended consequence, however, was that scientists did not have to do their research on functioning ranches. They had their own places to do experiments, and science and management thus became spatially segregated. The scales of science and management also diverged because in practice experiments were designed to meet the institutional demands of scientific production (i.e., peer-reviewed and published articles). As a result, research efforts typically lasted no longer than 10 yr and usually less than 5 yr to coincide with the duration of a graduate thesis or dissertation, a competitively funded research grant, or an intramurally supported research project while permitting publication of results in a timely fashion. Spatially, most experiments were carried out at relatively small scales—plots or sometimes pastures—because larger scales were too heterogeneous, unreplicatable, or prohibitively expensive to manipulate. The scale of experiments was therefore smaller than the scale of actual ranches or even management units within them, and larger scales, such as landscapes, were almost wholly neglected for most of the 20th century (Svejcar and Havstad 2009). This is a characteristic of field-based applied ecological research where often, regardless of the studied organism or process, a majority of experiments are conducted on small plots, often less than 1 m^2 (Kareiva and Anderson 1988). Such studies were valuable in identifying ecological mechanisms (e.g., plant recruitment or response to defoliation), but they could not capture larger-scale processes that might confound or override small-scale dynamics. However, within the prevailing paradigm of the time, scaling up from experiments to larger areas and longer time periods was seen as linear and unproblematic, especially once Clementsian successional theory and the quantitative methods that Clements helped to pioneer had gained stature within the discipline in the 1920s (Tobey 1981; Sayre 2010).

The Range Problem Today

Compared to the landscapes Wooton wrote about in 1908, we observe and write about altered landscapes through a lens of altered scientific perspectives. The fact that rangelands globally are often quite different, structurally if not functionally, from those seen in the late 19th and early 20th centuries is well documented (Grice and Hodgkinson 2002; Millennium Ecosystem Assessment 2005), and the array of ecosystem goods and services demanded from these landscapes is also now more diverse (Havstad et al. 2007). Even the term "range" was altered by the professional community of scientists and managers by the end of the 20th century to the more descriptive term "rangeland" to more appropriately reflect this distinct type of land. These developments are driving changes in both the questions being addressed and the methods employed to answer those questions by the rangeland science community.

Over the past century, rangeland scientists have discovered processes and principles important to understanding rangelands, but degradation persists (Herrick et al. 2010). In part, this reflects the discovery that a core assumption of past science and policy—that these systems would revert to their "original" condition if livestock were removed—was mistaken, as many areas (with and without livestock) still show the effects of events and uses that occurred more than a century ago. But there are also other reasons for continued degradation that stem more directly from rangeland science itself. First, rangeland systems are highly variable because of extreme rainfall variability, extended droughts, and low soil fertility (Grice and Hodgkinson 2002); low, highly variable primary productivity limits yields of ecosystem goods and services from rangelands (Costanza et al. 1997; Reynolds et al. 2007). Rangeland scientists now believe that there is neither a unifying set of general principles nor a general theory of rangeland management that can be employed everywhere given this highly variable nature (Anderies et al. 2006). If good rangeland management is an art as well as a science, the science has often been difficult to apply, and the art may be more important (Briske et al. 2008; Boyd and Svejcar 2009; Bailey and Brown 2011).

Second, until recently, the science failed to assess conservation practices systematically, and managers routinely failed to implement recommendations from available evaluations (Boyd and Svejcar 2009). For example, the science led to published guidelines for proper utilization of key livestock forage species (e.g., Paulsen and Ares 1961) but did not provide usable, affordable, and repeatable means to gauge utilization in the field. Extreme spatial and temporal variability made utilization a less coherent and tractable management tool than scientists (and agencies) seem to have realized until much later (Scarnecchia 1999), but managers have resisted employing *any* monitoring methodologies consistently over time. Only recently have tools and techniques and the corresponding commitment to evaluate conservation practices emerged from the science and its management partners (Briske 2011).

We still lack an adequate understanding of how rangeland landscapes function. In some cases, shortcomings can be traced to the institutionalized scales of research and management inherited from the past. Individual, small-scale, reductionist experiments identified tools and techniques whose application at larger scales was often impractical (e.g., seeding in southwestern semiarid grasslands; Cox et al. 1982). In spite of this limitation, the hundreds of individual experiments have collectively produced useful insights. It is now clear, for example, that many arid and semiarid systems change in response to "slow" variables (i.e., variables that significantly affect these systems but that change only gradually over long periods of time) that had rarely been monitored at the appropriate scales (Lynam and Stafford Smith 2004). We also know that we cannot assume linearity in extrapolating results across scales of space and time: findings from plots or pastures may not scale up to ranches or landscapes, and studies of < 10 yr in duration may not hold at other times or over longer periods. Nor can we assume that data from a handful of experimental ranges (even large ones) are sufficient to understand other places, even if they appear to be similar, and for that reason we can no longer ignore actual ranches as sites for producing knowledge. Today, scientists in many disciplines, including biology and its applications, are revisiting old data sets, compiling new ones, and collecting data sets from other locations in an effort to overcome these limitations (Peters 2010). Our capacities to identify patterns over much longer temporal scales and larger spatial scales than earlier experiments were designed to capture are growing rapidly and in previously unavailable (if not unimagined) ways (Reichman et al. 2011). The synergistic capacities of tools including Google Earth, Landscape Tracker, and ArcGIS to analyze and integrate spatial and temporal data have revolutionized the application of science to management.

As we have expanded our capacities to examine patterns, we have also learned that the processes are much more complex than Wooton and others envisioned a century ago. In the Clementsian successional paradigm, as adapted to rangelands by Arthur Sampson (1919) and E. J. Dyksterhuis (1948), livestock grazing—its timing, frequency, and intensity—served as the independent variable that determined vegetation responses at both the individual plant and the community level. Measures of carrying capacity were intended to capture and take advantage of this conveniently tractable and manageable driver. Here again, long-term observations at research facilities around the world have helped reveal the limitations of such an approach, and rangeland science now recognizes multiple, interacting drivers of change, including abiotic factors (especially climate) as well as biotic ones. Clementsian plant succession has given way to more complex theories in which thresholds distinguish multiple possible ecosystem states, some of which are undesirable and difficult to change (Westoby et al. 1979). Unfortunately, the recognition that rangelands are complex adaptive systems (Lynam and Stafford Smith 2004) and that they often cannot be fully understood via the reductionist framework that structured range science for most of the past century does not provide a rigorous scientific framework to address the rangeland problems of today. A key component of this complexity is humans—their knowledge, decisions, and actions—which make rangelands social-ecological systems with important slow drivers of change, often of a nonbiological nature.

Meanwhile, the socioeconomic assumption on which rangeland science was built in the United States—the conviction that rangelands "are, and probably always must be, of chief value for grazing" (Public Lands Commission 1905, p. 7)—no longer holds. Most privately owned US rangelands are now far more valuable for residential development than for livestock grazing, especially if they are owned in small or medium-sized parcels ($< 10^2$ hectares). Land used for grazing in the intermountain West declined by 6.4×10^5 ha $\cdot$ yr^{-1} in the 1990s, nearly half of which was converted to urban uses (Sullins et al. 2002). Low-density rural residential development is the most rapidly growing land use in the United States since 1950, resulting in significant fragmentation and threats to biodiversity (Hansen et al. 2005). Ranchers no longer represent a sufficient constituency for rangeland science, especially when compared to rapidly growing urban and "exurban" populations. Public rangelands are insulated from development, but their relatively modest contribution to regional and national economic output renders them—and the local economies that do still depend on access to them—increasingly marginal for the traditional goods and services they provide, while the "newer" goods and services of rangelands—such as watershed functioning, biodiversity conservation, open space, and wildlife—remain undervalued (Havstad et al. 2007).

Elsewhere in the world, rangelands and their inhabitants are increasingly marginalized for different but related reasons. According to the Millennium Ecosystem Assessment (2005), drylands are acutely threatened worldwide because of the combination of rapid population growth, low inherent productivity (net primary production), and low average gross domestic product (GDP). Some developing countries (e.g., Mongolia) derive large portions of their GDP from rangelands, but their low overall national GDP and impoverished populations are prompting calls for greater commercialization and "modern" production systems—which often means replicating the model implemented in the United States a century ago. Other developing countries, such as China, are rapidly intensifying rangeland exploitation in some regions through policies that privatize grazing rights, somewhat analogous to Wooton's remedies, with tragic results (Li and Huntsinger 2011).

The search for sustainable management approaches in dynamic and unique social-ecological systems defines the Range Problem after a century of rangeland science. If sustainability means conserving the capacity of ecosystems to support future generations (whose needs may differ from those of the present), then science can provide knowledge of general principles. But such knowledge alone is insufficient for application to the unique contexts of specific landscapes (Peters et al. 2012 [this issue]). Moreover, we also now recognize the fundamental role of human activities and needs in driving the ecological dynamics that we observe (Crane 2011). Thus, it is clear that rangeland science must retool itself to study the acts of actual managers, in specific social and ecological contexts, as the focus of inquiry, in methodical and repeatable ways.

Approaching rangelands as social-ecological systems requires understanding people's mental models of how ecosystems work and the processes by which those models change through learning and adaptation (Lynam and Stafford Smith 2004). Yet our management institutions are not designed to accommodate these key components, and the infrastructure for rangeland science is declining or, at best, maintaining itself. Meanwhile, the problems, uses, and societal demands placed on rangelands are very different today than they were in 1912. For example, newcomers continue to flow into the Southwest with new demands and expectations; meanwhile, the ranchers who have persisted are no longer naive about the landscapes they manage—in many cases, they have much deeper experience than rangeland scientists. Successful ranch managers can be found across the region, although their methods may fit uneasily (or not at all) with current scientific recommendations (Sayre 2001; Briske et al. 2011). Even while they are no longer a sufficient constituency, ranchers are a necessary one, not simply as recipients but also as sources of local knowledge and as managers of landscapes that contribute to meeting the newcomers' myriad needs. Sustainable management will require scientists to engage with local residents, learning from their experiences (Reynolds et al. 2007) and treating their management actions—past, present, and future—as long-term experiments that can help develop and test scientific hypotheses to address real problems (Bestelmeyer et al. 2011). The process of defining what sustainability means—its relevant components, measurements, threats, and beneficiaries—should be inclusive, deliberate, and explicit.

How to Understand Big Changes and Answer Big Questions: A Vision of Our Science

The Range Problem today calls not only for different experiments but also for a reformulation of the broader framework of the discipline away from its "normal science" roots—productivist, reductionist, and mechanistic. A "postnormal" (Funtowicz and Ravetz 1994) rangeland science is the application of the scientific method in diverse contexts and via direct interactions with managers and other stakeholders within those contexts. Therefore, this issue of *Rangeland Ecology & Management* is about the evolution of rangeland science and its methods—a postnormal vision of the science and the ideas that underpin it. Here we offer an outline of this vision in general terms. The articles that follow this introduction exemplify this vision while addressing "big questions" for the coming century of rangeland science.

First, we envision a science that addresses the scale of landscapes, with all of their hitherto unidentified and uncontrolled variations. Spatially, this means embedding smaller units—from individual plants to plots, pastures and ranches—within the surrounding landscape and recognizing the interconnections among processes operating at different scales. Hierarchy theory provides an initial framework for organizing observations and formulating hypotheses, but simple notions of "top-down constraints" or "bottom-up mechanisms" should be viewed as provisional and heuristic devices, not established theory.

Second, this multiscaled approach should also be applied on the temporal dimension: processes operating at different time scales interact, sometimes resulting in abrupt changes in system attributes (thresholds) that endure as historic legacies, strongly determining the possible subsequent changes and management opportunities. Above all, the science we envision needs to be spatially and temporally explicit about the processes and interactions that we observe and seek to understand.

Third, we envision a scientific method that incorporates tests of hypotheses in "retrospective" experimental designs using landscape treatments of the past as an affordable means to work at scales relevant to management (e.g., Teague et al. 2011). In addition to biophysical events with historic legacies (e.g., severe droughts, unusually wet summers, or hard frosts), we must capture information about the human activities and management practices implemented on rangelands over the past century (or in some cases even longer). For example, if the dates and locations of construction of fences, wells and water developments, erosion control measures, or vegetation manipulations (e.g., bulldozing, chemical applications, or seeding) can be accurately ascertained, such practices could potentially be analyzed as natural or quasi experiments whose effects can be evaluated using both newly gathered and historical records, such as aerial photographs, monitoring data, and weather records. Quasi experiments lack the strict controls and random assignment of treatments characteristic of fine-scale, classic experimentation, and historical data can be difficult to obtain, incomplete, or of uneven quality. Nonetheless, with these caveats in mind, matching comparisons using ecological sites and historical information about initial ecological states can be used to produce meaningful information about management practice effects at broad spatial and temporal scales. The development of systematic methods to design and interpret quasi experiments will be an important contribution for a rangeland science focused on landscapes and regions (Hargrove and Pickering 1992).

Fourth, scientists must engage land management professionals and ranchers, on both public and private lands, as collaborators and sources of information and knowledge, recognizing their long-term experience in specific sites and the potential value of that experience for improving scientific understanding. Existing data sets can yield information about patterns at large scales, but such analysis often cannot reveal the mechanisms that produced those patterns. Longtime residents and managers often possess site-specific documentation, such as rainfall records, memories of when changes occurred, and knowledge of any unique or unusual events that preceded or accompanied changes (e.g., flash floods; Knapp and Fernandez-Gimenez 2009). They also provide insight into the social variables that acted as causes or preconditions for management interventions, such as periods of prosperity that allowed ranchers to make large investments in fences, water, or vegetation manipulations (Knapp and Fernandez-Gimenez 2008). Such local knowledge can thus help to identify mechanisms of change to explain patterns discerned (or corroborated) from other sources. The resulting integration of diverse data, knowledge sources, and methods of investigation could be described as a science of consilience of facts and anecdotes across a landscape, thoroughly interdisciplinary in nature.

Fifth, we envision a science that is postproductivist: a science that recognizes a diversity of social objectives, including both traditional commodities and (as yet) nonmarket "ecosystem services" (e.g., Boody et al. 2005). Scientists must weigh and prioritize among objectives in open dialogue with interested

Table 1. Contrasting challenges for rangeland science before and after a century of research.

Normal, 20th-Century Rangeland Science	Postnormal,[1] 21st-Century Rangeland Science
Identification of cardinal principles of management	Contextualization of general principles to specific landscapes
Determination of livestock carrying capacities for discrete parcels of land	Identification and integration of values of multiple goods and services across entire landscapes
Conservation and recovery of herbaceous forage species	Management strategies for a world with less grass
Linear stages of plant communities in various but stable climates	Adaptive strategies for nonlinear change, extreme events, and changing climates
Practices for controlling invasive species	Strategies for adaptation to nonnative species
Impacts of biophysical drivers	Impacts of socioeconomic drivers interacting with biophysical drivers
Linear transfer of scientific knowledge to users	Scientists informed by local knowledge
Stored reductionist data for controlled analyses	Accessible data for open analyses
Minimal spatial dimensions to experimentation	Landscape-scale dimensions to experimentation
Management not integrated into experimental designs	Management as an integral part of hypotheses

[1]After Funtowicz and Ravetz (1994), where ''postnormal'' science is not the normal, reductionist approach to the scientific method but a science that emphasizes pragmatism and dialogue among scientists and practitioners in its application.

parties and seek to understand and communicate the complexity of the biophysical processes that interact to produce valued goods and services. Here the scientific emphasis is on *analytical services* that exploit the rapidly increasing layers of available data rather than on science-based technologies narrowly tailored to discrete goods.

Finally, we envision a science that is *public* in multiple senses of the word: a science whose practices and data are transparent and accessible as broadly as possible, that serves public needs and interests and is receptive to public participation, that is applicable as one of many inputs to policy, and that is communicated in ways that enable it to contribute to those policies and improved quality of life for the citizens who support it.

Taken together, these elements encompass and extend what is widely discussed under the term "adaptive management" (or "ecosystem management"). We echo and endorse adaptive management's call for greater integration of science and management, "learning by doing" (Walters and Holling 1990), and iterative processes of management, monitoring, and adaptation. We would add that greater institutional incentives are needed to enable truly adaptive science and management to become more widespread: it remains difficult for scientists or agencies to secure funding for the kinds of risk-taking, open-ended research-and-management initiatives that are at the heart of such an approach. Moreover, we believe that past management practices, as well as present and future ones, can contribute to learning, although this is not often mentioned in discussions of adaptive management. Finally, this combination of historical and contemporary inquiries into rangeland management is a necessary component for capturing multiple temporal as well as spatial scales of social and ecological dynamics.

MANAGEMENT IMPLICATIONS

On considering the past century of effort and the current state of the Range Problem, we see a need to redirect the dominant themes of rangeland research in order to continue what Wooton and others started over a century ago (Table 1). These themes are explored in the following articles in this issue. Conducting science to address questions linked to these themes will require approaches that are considerably different from those used in the past. These approaches are addressed in the synthesis article by Bestelmeyer and Briske (2012 [this issue]). We are arguing not only for a science conducted at larger spatial and longer temporal scales than those addressed in the past but also for a science that embraces the heterogeneity of these landscapes, including their inhabitants, and that is focused on how those inhabitants interact with specific landscapes. This focus additionally requires a science that is informed by local knowledge and management "experiments," both successful and not successful, in the past and the present. It is a science rooted firmly in testing the effects of management, guided by existing knowledge organized as models and hypotheses (Walker et al., 2002). In this fashion, the practices of management become scientific tests, and science provides the means for the public to learn from their actions and innovations.

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