Chapter 15
Rexford F. Daubenmire and the Ecology of Place: Applied Ecology in the Mid-Twentieth-Century American West

Adam M. Sowards

Introduction

In 1952, Rexford F. Daubenmire, a botany professor at the State College of Washington in Pullman, concluded his *Ecological Monographs* article with this paragraph:

> The ideal management of forest lands involves a balanced consideration of their value in timber production, grazing capacity, wildlife production, and watershed protection. On account of the complexity of the problem and the fact that changing demands will undoubtedly call for frequent modifications of plans, it is difficult to see how such a multiple use policy can become effective until the fundamental potentialities of the major ecosystems are understood by all who are charged with the responsibility of planning land management.[1]

In two sentences, he captured much of his life's work, even though he was not quite at his career's midpoint. In sum, humans required much from natural communities; their demands would inevitably evolve and shift, so policies would fail unless managers could determine an ecosystem's ultimate potential. In the Pacific Northwest's interior grasslands and forests, Daubenmire spent more than four decades closely studying natural places to discern how ecosystems functioned and adjusting his ecological theories to fit what he found. With such knowledge, hard-won through meticulous fieldwork, Daubenmire's science could inform land management and reduce the likelihood of costly failures for farmers and foresters, ranchers and range managers.

Daubenmire worked as an ecologist from the 1930s through the 1970s, a time when plant ecology in the USA matured and evolved from its founding generation's

---

[1] Daubenmire (1952, p. 327).

A. M. Sowards (✉)
Department of History, University of Idaho, 875 Perimeter Drive, MS3175, Moscow,
ID 83844-3175, USA
e-mail: asowards@uidaho.edu

D. Phillips, S. Kingsland (eds.), *New Perspectives on the History of Life Sciences and Agriculture,* Archimedes 40, DOI 10.1007/978-3-319-12185-7_15

roots.[2] Daubenmire serves as a window through which to examine how the discipline asked and answered questions and debated certain central concepts. Ecologists of his generation inherited dominant ideas that guided the field, but they did not accept all ideas uncritically or use them identically. Analyzing Daubenmire's research program and contemporary scientific debates show the ways a new generation of scholars accepted and extended, challenged and rejected, their teachers' ideas. Between the founders at the turn of the twentieth century and later ecologists who helped inspire the environmental movement of the 1960s and 1970s, the group Daubenmire represents has received comparatively little scholarly attention. This work, then, helps flesh out the history of ecology in that era. Recently, in *Measuring Plant Diversity,* ecologist Thomas J. Stohlgren claimed that "Daubenmire epitomized the science of vegetation ecology in the 1960s (and for many plant geographers and ecologists today)."[3] Daubenmire thus serves as an effective case study for a generation (or more) of applied ecologists solving problems related to land use in rural places.

The place where Daubenmire devoted his professional life also underwent important transformations while he worked there, changes shaped by economic activities and guided by science. Daubenmire focused most of his attention on the Columbia Plateau, the area between the Cascade Mountains and the Rocky Mountains that encompasses forested mountains and foothills, as well as an open plain that includes grazing rangeland, irrigated cropland, and dryland farmscapes. Hardly a pristine landscape in the 1930s when Daubenmire arrived, the region was poised for greater ecological disturbances with new agricultural possibilities and expanding timber production because of midcentury population growth and technological innovations. To best achieve these goals, conservationists promised that scientifically informed management would reduce wasteful, inefficient resource use.[4] Ecologists such as Daubenmire would provide the necessary understanding of nature to guide agricultural practices and resource development. Doing so required that Daubenmire and others like him reckon with past ecological disturbance to understand environmental impacts on regional landscapes and understand how an area's natural components fitted together so as to be able to predict the land's responses to various management possibilities. In looking at the land in the present, then, ecologists both looked backward and forward in time, accounting for change and forecasting the future. Daubenmire made this very point in 1953, writing, "a given condition of vegetation allows extrapolation into the past as well as prediction into the future."[5] His numerous publications offered relevant data and explanations useful to rural

---

[2] Overviews of ecology's founding and development include Bowler (1992); Golley (1993); Hagen (1992); Kingsland (2005), Tobey (1981); Worster (1994). Also, Real and Brown (1991).

[3] Stohlgren (2007, p. 34).

[4] The classic statement is Hays (1959). Although Hays' focus extended only to 1920, these priorities remained important much longer. For how these ideas evolved into the New Deal Era, see Fox (1981), esp. pp. 183–217; Maher (2008).

[5] Daubenmire (1953, p. 17).

land managers and users, especially his innovative approach to predicting a given habitat's *potential* vegetation.

However, Daubenmire was not just a lone scientist conducting case studies in an out-of-the-way part of the North American West. He trained with reputed ecologists and engaged widely in the profession's intellectual debates and institutions. Indeed, he led ecology's leading organization (the Ecological Society of America, ESA), earned national awards and recognition, and published textbooks on the fundamentals of plant ecology and geography for botany and ecology students.[6] Although not a widely recognizable name today in the history of ecology, Daubenmire was a substantial intellectual presence and can shine a light on central ideas and problems in ecology in the mid-twentieth century.

The approach taken here—studying one scientist in one place—helps develop the scholarly discussion of ecology of place. "Ecology of place," as characterized recently by scientists Ian Billick and Mary V. Price, describes a research approach that "pursues general understanding through…detailed understanding of a particular place."[7] Daubenmire illustrates this approach, for he mostly worked in the inland Northwest on both small-scale and landscape-scale research but consistently kept in mind larger questions about how vegetation units anywhere assembled and functioned and what factors affected them. He worked at intersections—the local and universal, the basic and applied—in understanding how his place fit within larger scientific and environmental frameworks. To interrogate the life sciences, agriculture, and the environment, we can investigate those who operated in the field trying to make sense of that very nexus. Daubenmire is an exemplar.

Vegetational Units in Early Ecological Theory

Ecologists inherited from biogeographers such as Alexander von Humboldt an interest in understanding how and why species were distributed across and interacted with the landscape.[8] They surveyed and mapped regions around the globe in ever-increasing detail with different methods and preferences developing over the course

---

[6] A brief biography that highlights Daubenmire's professional achievements is Hoffman (1996). The textbooks are Daubenmire (1947), *Plants and Environment;* Daubenmire (1959b), *Plants and Environment;* Daubenmire (1968b), *Plant Communities;* Daubenmire (1974), *Plants and Environment;* Daubenmire (1978), *Plant Geography*.

[7] Billick and Price (2010, p. 4). This approach also resembles Jeremy Vetter's discussion of field scientists "scaling up" their work from their local field sites to the regional scale (or even beyond) to reach broader claims of knowledge. See Vetter (2011) in his introduction, esp. pp. 2–3, as well as Chap. 14 in this volume.

[8] On Humboldt's work as a precursor to ecology, see Bowler (1992, pp. 205–208); Nicolson (1987); Worster (1994, pp. 133–137). Plant geography is discussed in various contexts in this volume in Chaps. 2 (Phillips), 3 (Güttler), 5 (Horan), and 16 (Lavelle).

of the nineteenth century and beyond.[9] In North America, and to a lesser extent in Great Britain, ecologists at the start of the twentieth century conceived of nature in discrete communities, forming through a process they eventually called succession.[10] After a disturbance like a fire or a landscape change like a receding glacier, new plant species would colonize an area, followed by another suite of species, and then another. Thus, nature was not static. As the theory's cofounder Henry Chandler Cowles of the University of Chicago put it in 1899, "Ecology is, therefore, a study in dynamics."[11]

Scientists differed in their theories about what caused succession and where it led. Some, like Cowles who had a background in geology and geography, saw the physical world as too dynamic to ever produce a stable array of species.[12] Succession was real enough, for he observed successive plant types as he walked from the sands along Lake Michigan through grasses and then into shrubs and inland toward forests.[13] Others, like Frederic E. Clements, a botanist from Nebraska, believed succession with any given locale's climate would lead to a single plant community that existed in self-replicating equilibrium, provided no disturbance or human interference disrupted the natural order of things. So coherent was this climax or monoclimax, he described it as an organism, or super-organism: "The unit of vegetation, the climax formation, is an organic entity. As an organism, the formation arises, grows, matures, and dies."[14]

The stability and predictability implied by Clements' vision failed to convince other ecologists, who proposed less deterministic alternatives. Rather than seeing succession as proceeding along community lines in an inherently progressive fashion, American ecologist Henry A. Gleason saw it driven by individual plants and plant species subject to unique environmental conditions and migration dynamics. As he concluded an influential 1926 article, "[I]t may be said that every species of plant is a law unto itself, the distribution of which in space depends upon its individual peculiarities of migration and environmental requirements…. A rigid definition of the scope or extent of the association is impossible, and a logical classification

---

[9] Ecologists have accounted for how scientists classified communities variously across the globe in Kendeigh (1954); Whittaker (1962).

[10] Robert Kohler contextualized American plant ecologists' classification activities from the 1890s to the 1930s within biology's other classification practices; see Kohler (2008).

[11] Cowles (1899, p. 95).

[12] For Cowles, see Cittadino (1993). Rumore (2009, pp. 84–86) describes Cowles' educational background.

[13] Cowles published his influential study in four successive 1899 journal issues: Cowles (1899).

[14] Clements' classic statement is in Clements (1916), quoted on p. 124. Daubenmire's characterization of Clements' theory is clear and helpful. In *Plant Communities,* he wrote: Clements hypothesized "that within a given area all differences among habitats due to soil and topography are eliminated with the passing of time, so that all the area is ultimately taken over by the same climax association, the nature of which reflects primarily the climate. His *monoclimax hypothesis,* as it later came to be known, therefore demanded that every piece of vegetation in a landscape be fitted into one or more seres, all of which converge in a common climax." Daubenmire (1968b, p. 240; original emphasis). Seres are transitory states of vegetation prior to reaching the climax state.

of associations into larger groups, or into successional series, has not yet been achieved."[15] If plant species had not assembled in a recognizable pattern but rather in a statistically random distribution caused by chance, then it made little sense to speak of plant communities at all. And if that were the case, little prediction was possible and efforts to control or improve nature would be difficult if not impossible for ecologists to recommend.[16] In the mid-twentieth century, ecologists centered at the University of Wisconsin-Madison picked up and extended this individualistic critique in a way that struck at the heart of Daubenmire's work (see below).[17]

Another alternative concept was the ecosystem. In a significant 1935 article in *Ecology,* Oxford botanist Arthur G. Tansley analyzed and criticized the "use and abuse" of various ecological terms and ideas within the Clementsian tradition but also proposed a novel way of thinking about the environment. To a greater extent than his predecessors, Tansley coupled the physical and biological: "Though the organisms may claim our primary interest, when we are trying to think fundamentally we cannot separate them from their special environment, with which they form one physical system." He continued: "It is the systems so formed which, from the point of view of the ecologist, are the basic units of nature on the face of the earth." He called these "basic units" ecosystems, a more diverse and integrated unit than the monoclimax. Qualifying Clementsian perspectives, Tansley allowed that ecosystems might organize into stable states determined by factors other than climate. Sometimes called polyclimax theory, it recognized that at times soil, topography, fire, or grazing created and maintained ecosystems in relative equilibrium, respectively known as edaphic, physiographic, fire, or biotic climaxes.[18] A critical distinction was that Tansley's system could incorporate disturbances and human activity as part of an ecological system, whereas Clements' approach excluded humans and saw disturbances as setbacks on a community's progress toward climax. It took ecologists two decades before the ecosystem became a widely adopted concept with methods devised to study the integrated system Tansley described. Nevertheless, Daubenmire and others such as his mentor William Skinner Cooper at the University of Minnesota saw this version of the natural world with biotic and abiotic factors "braided" together to more adequately represent complex nature.[19]

---

[15] Gleason (1926), quoted on p. 26; and Gleason (1939), where he is clearer and more insistent on his view's incompatibility with community ecology. Commentary is in Nicolson and McIntosh (2002).

[16] Kingsland (2005) also emphasizes how Gleason's concepts undermined ecologists' abilities to predict and thus be socially useful in *Evolution of American Ecology,* p. 160.

[17] On the resurgence of Gleason's influence in the 1950s, see Barbour (1995). Rumore has challenged the notion that there was as sharp a divergence as Barbour describes, because Barbour (and others) overemphasized the dominance of Clements. See Rumore (2009, pp. 10–11).

[18] Tansley (1935, p. 299).

[19] Daubenmire adopted "ecosystem" early and employed it throughout his career. His first use was in Daubenmire and Colwell (1942, p. 32). Cooper (1926, p. 397) famously compared dynamic vegetation communities to a "braided stream." Rumore (2009) has examined this effectively in "A natural laboratory."

Daubenmire immersed himself in these larger intellectual issues, and they shaped his scientific worldview and practice. Recognizing how these debates unfolded around him contextualizes his Northwest ecological fieldwork. Furthermore, it warrants emphasizing that his careful and data-rich approach followed that of Cooper.[20] In her study of Cooper and his work at Glacier Bay, Alaska, historian Gina Rumore characterized him as a careful ecologist working tirelessly to match theory with data. By contrast, Clements' organism framework and his notion of progressive change toward climax were dogmatic, much like the man himself.[21] Instead, Cooper practiced and instilled in his students care with ecological terms, avoidance of teleology, and careful tests of theories with field data. Cooper and his students still sought natural laws (unlike Gleason) to explain the constancy of ecological change, but their approaches closely integrated data with theory and readjusted them when data required (unlike Clements) and incorporated multicausal explanations and models (like Tansley) that changed over time.[22] Daubenmire seldom cited Cooper's influence, yet his undogmatic approach searching for underlying causes bears Cooper's intellectual imprint. For example, Daubenmire's dissertation study of Minnesota's Big Woods sought to understand the structure and physical limits to the biological community, incorporating climate, soils, and fire—this final factor being an innovative factor he later would develop further.[23] With this solid academic mooring, Daubenmire took his ecological practice west to the University of Idaho.

Besides the various personalities and schools of thought that have been recounted in the history of ecology, a central crux to the scientific debate at this time was whether plants existed in discrete objective units that could be described scientifically and what impelled their changes over time. For a half century and more, ecologists debated these tenets. Plant communities were real entities that could be delineated scientifically. Or not. Climax communities were homogenous states determined by climate in the absence of interference. Or not. These positions had practical and philosophical consequences. If real, plant communities could be classified scientifically—that is, objectively, or quantitatively. If not, they were merely

---

[20] Daubenmire studied with Stanley Cain as an undergraduate at Butler University. Cain worked at the University of Chicago at the same time Cowles taught there, although Cowles was not Cain's supervisor. Later, Cain was an assistant secretary of the Department of the Interior. Thomas (1995); Barbour (1995, p. 253). Daubenmire also took a master's degree at University of Colorado, working with Francis Ramaley. Hoffman (1996, pp. 143–144); Stout (1995, p. 85).

[21] Barbour relates a revealing story from Daubenmire about Clements' dogmatism. The two botanists were scouting plant communities in the Palouse when Clements misidentified a plant and announced it as a climax species. When Daubenmire corrected Clements and noted the plant was evidence of disturbance, Clements replied that "There's a negligible difference," suggesting how Clements might have overlooked details to fit his theories. Barbour (1995, p. 248).

[22] Rumore (2009, pp. 206–241) shows these ideas and influences in practice in Cooper's Glacier Bay fieldwork.

[23] Daubenmire (1936) was a pioneer researcher in fire ecology. His text, *Plants and Environment,* reportedly was the first plant ecology text to devote a chapter to fire as an ecological factor; see Hoffman (1980, p. 34). Also, in 1968, he published a review essay on fire in grasslands that remained a classic for a generation; see Daubenmire (1968a).

human contrivances and delimited by a given scientist's subjective interests. If they were real and predictable, then ecologists could diagnose problems and prescribe remedies based on natural laws. If they were not predictable but simply the result of various contingencies and historical accidents liable to move in any number of future directions, then scientists could make few relevant predictions and prescribe no effective policies. For ecologists such as Daubenmire who worked on applied questions in agriculture or forestry, the implications were tremendous. In 1936, when he relocated to the Northwest, he grappled with these questions and their applications in place.

Applied Ecology and Agriculture

Classifying landscapes was embedded within virtually all early ecologists' work. They sought to identify different confluences of biological and physical factors to capture their characteristics, especially those related to successional phases and climax states. Historian of science Robert E. Kohler has argued that the first generation of ecologists tried to create a vegetation type classification system, much as biologists had with species taxonomy, only to abandon the project by about 1940 when empirical data revealed that vegetation types were not like species.[24] However, Daubenmire engaged with questions surrounding classification throughout his career, never yielding the assumption that vegetation communities existed and therefore could be characterized, understood, and managed.

An early Daubenmire publication challenged one of North America's first classification systems, C. Hart Merriam's life zones.[25] Merriam built on a long tradition. Classifying vegetation groups began with Humboldt who, as the nineteenth century dawned, made plant geography modern by studying vegetation in relationships rather than just compiling individual lists of flora as followers of Carl Linnaeus had done.[26] By the end of the nineteenth century, such efforts expanded. As part of the US Department of Agriculture's Division of Ornithology and Mammalogy and later the Bureau of the Biological Survey, Merriam took up the Humboldtian mantle and examined the distribution of species, identifying six main life zones in North America. In perhaps his most famous study in 1890, he investigated Arizona's San Francisco Peak and saw life zones matching patterns based on altitude but determined mainly by temperature.[27] Such work was useful but lacked scientific rigor.

Writing in *The Quarterly Review of Biology* in 1938, Daubenmire summarized Merriam's biotic distribution and criticized it. Temperature, mapped onto latitude,

---

[24] To be sure, ecologists still named vegetation groups for pragmatic reasons, but, as Kohler wrote, "they no longer constructed *systems* of classification, nor inquired too deeply into biological meaning of their categories." Kohler (2008), quoted on p. 107 (original emphasis).

[25] Daubenmire (1938); Merriam (1898).

[26] Nicolson (1987).

[27] Merriam (1890). Worster linked Merriam to Humboldt in *Nature's Economy,* pp. 195–197.

was all Merriam used to explain patterns and included virtually no quantitative data. Testing Merriam's theory in the field, ecologists had found vegetation types to be evidence of coherent zones more than climatological data. In other words, plant communities indicated biological coherence better than climate readings. For instance, some places where instrumental data (e.g., temperature readings) suggested the existence of a new zone also contained the same biota and so "certain natural entities were artificially split," or the contrary, "very diverse vegetation types were at times lumped together," simply because they shared a common climate. Another problem with Merriam's perspective was that he relied on a single factor—temperature—while Daubenmire contended that "we now hold the environment to be such an intricate complex of interdependent factors that it is exceedingly difficult, if indeed not an impossibility, to attempt to evaluate the individual influences." This emphasis on myriad factors grew out of his ecosystem perspective and would remain consistent throughout Daubenmire's career. Ultimately, although he credited Merriam with stimulating new research and for being the first to use climatic data, he ultimately found the explanations "fallacious."[28] For his work, Daubenmire received an admiring letter from Joseph Grinnell, the eminent field biologist who directed the University of California, Berkeley's Museum of Vertebrate Zoology. Although Grinnell had used Merriam's schema and "feel a sort of responsibility for defending that concept," he found Daubenmire's "summation and appraisal… thought-provoking, hence worthy."[29] One can only surmise such praise would be gratifying to a young assistant professor.

Even as Daubenmire wrestled with continental-scale classification questions, he zeroed in on local landscapes. Daubenmire arrived in the inland Northwest during the Great Depression, which also corresponded with national concern over environmental problems and ambitious conservation programs. The Dust Bowl of the southern plains with its massive soil erosion brought to a national audience a concern about poor land-use decisions that resulted in both ecological and economic ruin. Clements used the opportunity to showcase ecology as an applied science, vocally criticizing agriculture's role in disturbing the plains' biological community and advocating ecologists' potential to advise conservation work.[30] Depression-era conservation focused on two arenas: One would ameliorate existing problems; the other would plan new projects scientifically to avoid repeating mistakes. Like ecologists and conservationists elsewhere, Daubenmire believed that science could and should guide human–land relations, and a large conservation project in the region offered him a timely opportunity to be useful.

---

[28] Daubenmire (1938), quotations on pp. 330–332. He developed his own assessment of zones in the Rocky Mountains not long after this publication; see Daubenmire (1943). Later, Daubenmire demonstrated shortcomings to other climate-based classifications in Daubenmire (1956a).

[29] Washington State University Manuscripts, Archives, and Special Collections; Rexford F. Daubenmire papers (unprocessed) MS-1997–05 (hereafter RFDP); J. Grinnell to Prof. Rexford F. Daubenmire, 11 November 1938.

[30] Worster (1979, 1994) accounts for Clements's work surrounding the Dust Bowl in *Nature's Economy,* pp. 221–253; and *Dust Bowl,* pp. 198–209.

The Columbia Basin Project was the impetus for much of Daubenmire's early Northwest work. Authorized in 1933, this project sought to transform the Columbia Plateau by, among other things, bringing water from the Columbia River to the plateau's rich, but dry, soil often hundreds of feet above the river. The western part of the Columbia Plateau's 63,000 square miles is flat, dry sagebrush plain where wildlife, then Native Americans' horses, and then Euro-American livestock, especially sheep, grazed. With Grand Coulee Dam as its centerpiece, the Columbia Basin Project promised to convert a million acres or more of that plain into irrigated cropland, bringing more intensive agriculture to the region. Yet this would displace some grazing lands.[31]

This environmental history set the stage for Daubenmire's research. As the Columbia Basin Project reconfigured the plateau's geography of agriculture, many were invested in doing it scientifically to avoid disaster. Daubenmire explained how this agricultural frontier would avoid the "misguided history" of the Dust Bowl and "follow a course dictated by the findings of scientific research. These findings must be the synthetic product of specialists: ecologists, soil scientists, agronomists, engineers, etc."[32] This multidisciplinary synthesis bespoke Daubenmire's ecological vision and exuded confidence in specialists, a faith representative of the Progressive-era conservation movement and its continuation in President Franklin D. Roosevelt's New Deal.[33] This work, as Daubenmire put it, offered an opportunity for "man to practice what conservation principles he has learned by his past mistakes."[34] As an applied ecologist, he would explain past impacts and advise on future use for project areas. Launching fieldwork in the region in the mid-1930s, Daubenmire initiated what became four decades of intensive research during which he became arguably the region's unrivaled botanical expert.

From the start, Daubenmire's ecology of place engaged with disturbed lands, larger ecological questions, and implications concerning land use and its impacts. Overgrazing had already produced on the Columbia Plateau "sorry conditions. So badly have they been overgrazed that no one knows just how much forage such lands are capable of producing under less injurious treatment."[35] Further study revealed four effects of overgrazing on plant communities. First, the characteristic climax plants—native bluebunch wheatgrasses (*Agropyron spicatum*)—declined, as grazing pressure destroyed perennial plants' capacity for photosynthesis, weakened their overall vigor, and prevented seed production in annuals all the while removing larger plants' protective coverage that helped grasses thrive. Second, a new set of plants that could withstand trampling and were generally hardier than native bunchgrasses thrived in overgrazed lands, but they were "woolly" or bristly

---

[31] Material on the region's history is synthesized well in Meinig (1995 pp. 3–25); Duffin (2007, pp. 16–31). For grazing, see Dwire et al. (1999); McGregor (1982). For the Columbia Basin Project, see Pitzer (1994).

[32] Daubenmire (1939, p. 33).

[33] Fox (1981); Maher (2008).

[34] Daubenmire (1940b, p. 8).

[35] Daubenmire (1939, p. 33).

or "otherwise distasteful," and thus seen not only as a biotic regression but also as economically worthless and undesirable. Third, a transitory plant community appeared as grazing removed competitors that allowed these plants to grow, but they were "not very well adapted" to the larger habitat and ultimately did not remain in significant numbers. Fourth, grazing did not affect some minor plant communities in frequency or distribution.[36] The upshot: A stable, productive grassland was being replaced by a disturbed, unpalatable one.

Daubenmire's joined other studies about grassland ecology but reached somewhat different conclusions, demonstrating the value of place-based inquiry. Native bunchgrasses on western rangelands had been a great boon to ranchers, but by the mid-twentieth century, overgrazing deteriorated prairies over much of western North America. Daubenmire reported that selected plants in Washington's bunchgrass prairies did not behave as expected based on observations elsewhere: Russian thistle (*Salsola kali* L.) was not present, despite its ubiquity in other regions; cheatgrass (*Bromus tectorum*) could dominate as it did elsewhere, but the relationship with grazing could not be drawn directly; and sagebrush (*Artemisia tridentata*) often invaded grazed lands, but in Washington it appeared complementary to, not competitive with, bunchgrasses. The conundrum facing range managers tasked with balancing livestock numbers and available forage was obvious. Ninety percent of the biological output in this ecosystem—measured by dry weight—came from just two plant groups, *Agropyron* and *Bromus,* but Daubenmire's study demonstrated that those plants declined markedly in overgrazed ecosystems. In fact, *Agropyron* was only negligibly present with the annuals that replaced it being "valueless as forage." Thus, grazing reduced and replaced over time the very grasses required or preferred by livestock. Daubenmire recommended cutting and curing grasses for hay later in the year, removing annuals to reduce competition and promote perennial vigor, and resting land from grazing, especially during spring growth. His experiments and observations in the field suggested that a haphazard grazing system would inevitably continue to destroy the range required to sustain livestock.[37]

Plants' successional responses to overgrazing were only one relevant element; to construct a more complete understanding of the ecosystem, Daubenmire also turned to the effects of overgrazing on soil. Such research was necessary, because while it was common knowledge that overgrazing caused "vegetational retrogression," few scientists investigated what it did to the soil. He examined two comparable plots only 50 m apart, one severely overgrazed, while the other protected from grazing for nearly three decades because of a railroad cut. In comparing these two virtually identical soil samples, Daubenmire found significant changes that could only be attributed to grazing. The annual plant communities that colonized heavily grazed

---

[36] Daubenmire (1940a), quoted on p. 60. He had presented these four stages in preliminary form in Daubenmire (1939, pp. 35–36).

[37] Daubenmire (1940a), quoted on p. 62. Earlier, he had recommended minimal spring grazing and relying on cured shoots in fall and winter for feedstock to ameliorate overgrazing's effects; see Daubenmire (1939, p. 36). Daubenmire's work related to cheatgrass invasion is contextualized in Young and Allen (1997).

land possessed shallower root structures, which in turn changed the way water accumulated and was absorbed in the soil, weakened soil aeration, and reduced soil aggregation. The evidence seemed clear: Grazing worsened soil functioning. Combined with his earlier study of grazing's botanical effects, Daubenmire recognized a causal chain from grazing that extended beyond obvious biotic reconfigurations to "secondary, or even more remote, effects of grazing."[38] His research had begun capturing in detailed scientific terms the negative consequences of the region's prevailing agricultural practice of maximizing production.

Meanwhile, Daubenmire sought to bring ecology's insights to other agricultural problems. To do so, he identified natural plant communities, seeing in them ecological clues to what the best crops for a habitat might be. This research constituted an outgrowth of his life zones work and ecology's general classification project, and he spent the bulk of his career classifying plant communities as a prerequisite to understanding an environment's subtle "potentialities."[39] The Columbia Basin Project's lands might appear uniform: "Apparently all that is needed is to grid the area into tracts of 40 acres as is now planned, supply irrigation water, and let the success of the project rest entirely upon the diligence of the farmers." But Daubenmire warned of greater complexity, "But nature has not endowed this area with uniform soil conditions, and farmers who settle tracts of good soil will prosper while their nabors [sic] may have a difficult time of finding subsistence on a tract of equal size and with the same amount of irrigation water."[40] In two separate studies—one brief and impressionistic, the other lengthy and statistical—he used "virgin and near-virgin relics" often found in cemeteries that had been protected from disturbances like plowing and grazing to determine natural plant communities and which environmental factors controlled their structure. Most important on the Columbia Plateau were soil types, which closely corresponded with the observed vegetation communities.[41] From this information, Daubenmire offered practical agricultural advice. For instance, the saltgrass-type community would be ideal for sugar beets or alfalfa, while sagebrush and rabbitbrush indicated good habitat for orchards. Understanding these botanical communities paid practical dividends for farmers, since the same environmental conditions affected any plant, even crops. "Native vegetation represents the final outcome of the operation of ecologic factors which have influenced plants throughout centuries and which are operating today not only on the remnants of the original flora, but on our crop plants as well," Daubenmire reasoned.[42] Knowing and implementing this ecological information, farmers might experience greater success and avoid expensive failures.

---

[38] Daubenmire and Colwell (1942), both quotations on p. 32.

[39] Daubenmire (1940b, p. 8. He used "potentiality" in various forms, including "biotic potentiality" or "crop potentiality" in many publications.

[40] Daubenmire (1940b, p. 8).

[41] Daubenmire (1940b); Daubenmire (1942), quoted on p. 60.

[42] Daubenmire (1940b, pp. 9–10, quotation on p. 10). He made a similar statement in Daubenmire (1942, p. 75).

To a large degree, that was the larger point: Ecologists sought practical applications for their work. This first spate of Daubenmire's Northwest work, rooted in questions surrounding the reclamation project's potential, found those outlets. Seeking to understand past impacts and future potentialities, the ecologist supported the region's agricultural interests. At the very least, Daubenmire believed in putting farming on an ecologically secure foundation that served farmers, although any suggestion of reducing grazing or questioning the inevitable success of irrigation across the project may have irritated farmers. Inland Northwest agriculture was in transition with expanding irrigation and increased mechanization meeting a landscape already showing signs of significant wear and tear. The science also was in transition, finally giving the region attention. In his major study of plateau vegetation in *Ecological Monographs* in 1942, Daubenmire noted that only two other scientists had examined the region's plants.[43] His study of grazing's impact on soil similarly brought attention to a question that had received little scientific investigation.[44] That Daubenmire was among the first to describe the region's ecosystems scientifically indicates just how recent was the conjunction of science and agriculture to this place. These detailed studies summarized field research but also engaged with ecology's larger questions of plant communities, illustrating Daubenmire's emerging ecology of place.

Habitat Types

Moving into the post–World War II era when Daubenmire relocated to the State College of Washington (renamed Washington State University in 1959), his growing research program found him still in agricultural fields, but also increasingly in the forested foothills, trying to make sense of timbered ecosystems. Diverse and disturbed landscapes challenged botanists, for succession's fundamental dynamism made classification difficult with constantly shifting biotic communities.[45] "The delimitation of natural sociologic entities in a complex and largely disturbed vegetation is by no means an easy task that can be resolved to simplicity in a short time"; Daubenmire explained in 1952, "even a small area of vegetation may contain thousands of species which, at first seem to form a chaotic pattern."[46] Finding the patterns in the chaos became his task. Paradoxically, by figuring out what nature might be like without human activities (i.e., disturbances), ecologists such as Daubenmire believed they could bring to bear scientific insight on environmental questions and guide natural resource development.

Daubenmire maintained his focus on natural vegetation communities and included larger landscapes from which he added more data and refinement to his earlier

---

[43] Daubenmire (1942, p. 55).

[44] Daubenmire and Colwell (1942, p. 32).

[45] Daubenmire (1946, p. 33).

[46] Daubenmire (1952, p. 321).

observations. Physiography and climate were insufficiently accurate indicators, he found; only biotic distribution worked, and since animals ultimately depended on plants, vegetation was the best criterion. The plants used to characterize the communities needed to be climax species, for otherwise the community's character would change with each successional stage. To Daubenmire, these "vegetation zones are fundamental natural entities." He acknowledged that individual species might be found in other zones, for a species' presence or absence was not what constituted a unique community. Their groupings and interactions, as well as their relative abundance, created "highly distinctive" communities that could be discerned and classified. Knowing these zones' characteristics allowed foresters, range experts, and game managers to understand potential biota and the "possibilities for controlling vegetational change" in each unique zone for various management goals.[47] Focusing on *potential* vegetation became a hallmark of Daubenmire's ecology, because planning to use a landscape over time required managers to know what could grow in a given type rather than what occupied the ground at the moment, which could be merely a transitory product of disturbance.[48] An early seral association, for instance, would be comparatively short-lived with the climax association ultimately dominating the area. Using existing cover type for classification might show foresters where commercially valuable trees were, but it would be subject to frequent change and was not an ecologically sound method.[49] Ecologists and managers required more basic and permanent classification schemes.

Daubenmire expanded and clarified these perspectives in a major study of northern Rockies forests, published in 1952 in *Ecological Monographs*.[50] It extended the geographic range of his earlier work from the west to the east, the ecological range from steppe vegetation to forests, and the economic focus from agriculture to natural resources more broadly. He had large ambitions for this project—nothing less than an exemplar of a universal scheme for vegetation classification. At the outset, Daubenmire labeled different natural units. *Unions* (also termed *synusias*) included a species or closely related species with similar environmental requirements; these were the smallest structural components. *Associations* were the basic units in classifying vegetation and included all unions in the same area characterized by climax species. He named associations binomially with the dominant and subordinate union identified, such as the *Pinus ponderosa/A. spicatum* association where ponderosa pines dominated with bunchgrasses as subordinates. *Zones* included areas of

---

[47] He explains his reasoning clearly in Daubenmire (1946), quotations on pp. 37, 36, 37, respectively.

[48] Daubenmire's approach to classification is contextualized historically for managers in Bailey et al. (1978); Franklin (1980); O'Hara et al. (1996). Both Franklin and Pfister (a coauthor in Bailey et al.) worked with Daubenmire for their doctorates. Daubenmire's approach is an antecedent to what is sometimes called potential natural vegetation (PNV). An explanation and application is found in Henderson et al. (2011), esp. pp. 2–5.

[49] Daubenmire (1952), "Forest vegetation of northern Idaho and adjacent Washington," p. 324.

[50] Ibid., pp. 301–30.

closely related associations, such as grasslands.[51] Using this nested system, while paying attention to climax and seral stages, Daubenmire could effectively describe ecosystems. This framework was foundational to field ecology, he concluded: "We should look upon complex ecosystems as the only natural units, and that macroscopic vegetation in its entirety comprises the best criterion of ecosystems."[52]

Daubenmire proposed using the *habitat type* as the basis of classifying land.[53] Habitat types included various environmental factors of a given place, including climate and soil. In effect, they provided the basic ecological context for the unions, associations, and zones. These would not fundamentally change because of natural disturbances like fire or human disturbances like logging. Habitat types were practically permanent and thus strong indicators for long-range planning; thus, they could be a valuable and welcome tool for land managers. He provided an example of mismanagement that could have minimized by using the habitat-type method. After fires moved through one Idaho stand, foresters planted ponderosa pines, which grew quickly but then stalled and declined. Meanwhile, natural regrowth of western white pine (*Pinus monticola*) started slowly but then far surpassed the ponderosas. Had managers understood the locale's true habitat type, they could have saved time and money by not planting trees likely to be supplanted. As he had done when advising for the Columbia Basin Project, Daubenmire searched for explanations to predict likelihoods and avoid costly efforts. "The trial and error method of ascertaining habitat potentialities of forestlands is very costly because of the many years that are needed to determine the ultimate effect of different practices as the tree crop matures," Daubenmire explained, "so that the habitat type concept has much to offer by indicating the degree to which each experiment can be extended throughout the mosaic of forest associations." In short, scientists could map habitat types that reflected ecological qualities so that wherever a certain habitat type was found—whether disturbed or pristine—managers could look into the future to see how that forest or rangeland would likely develop.[54]

---

[51] His system is described in ibid., pp. 302–303. Similar summaries are found in Daubenmire (1953), "Classification of the conifer forests," pp. 17–19; Daubenmire (1954), "Vegetation classification."

[52] Daubenmire (1952), "Forest vegetation of northern Idaho and adjacent Washington," quotation pp. 324–35. This definition differed from contemporaneous work that took a systems approach toward how and what moved through ecosystems; see Golley, *History of the Ecosystem Concept,* esp. pp. 35–108; Hagen, *Entangled Bank,* esp. pp. 78–145; Kingsland (2005), esp. pp. 185–99, 206–19; Worster, *Nature's Economy,* esp. pp. 301–15.

[53] Daubenmire described the origins of this idea in a paper given in 1987, see Rexford Daubenmire, "The roots of a concept," a paper presented at the Symposium Land Classifications Based on Vegetation: Applications for Resource Management," pp. 17–19 November 1987 (found in RFDP).

[54] Daubenmire (1952), "Forest vegetation of northern Idaho and adjacent Washington," quotation from p. 326. More on mapping habitat types is found in Daubenmire (1973), "A comparison of approaches to the mapping." Other publications also show Daubenmire attempting to predict ecological trends for managers, see Daubenmire (1956b), "The use of vegetation to indicate grazing potentials." Daubenmire (1976), "The use of vegetation in assessing the productivity."

Habitat typing would become an important practical tool.[55] Daubenmire believed that "land units defined on the basis of their potential or actual climax can and will play an increasingly more important part in the ecologic sciences as the use of uncultivated lands becomes intensified."[56] He was correct. In the postwar era, Northwest forests experienced significant harvest increases and intensified management, especially on federal lands. By 1960, Congress codified that national forest timber harvests be conducted on a sustained yield basis, while those lands would be managed for multiple uses, including timber, wildlife, watershed protection, and recreation. These competing goals, as well as pressures caused by a growing population's consumer and amenity demands, required the best and most informed management possible.[57] Scientists like Daubenmire tested and refined inherited ideas and formulated new approaches to assist this work.

Working foresters welcomed Daubenmire's efforts. Noted ecologist Frank E. Egler, then working at the research site Aton Forest in Connecticut, and often a critic of community ecology, praised Daubenmire's engrossing article in *Ecological Monographs* as a "mile-stone paper" and assured Daubenmire that he would be quoting it in the future.[58] Northwestern private foresters praised it, as did researchers abroad.[59] Wildlife experts also recognized the impact the classification system would have on their work.[60] Given the high proportion of Forest Service lands in the Northwest (today more than 20 million acres in Idaho alone),[61] perhaps the most significant praise came from a federal forester, Fred W. Johnson, who wanted 150 reprints of the article to distribute to all regional field officers. In particular, Johnson appreciated the applied ecological approach: "Your ecological interpretation of vegetative associations will form the basis for much of the silvicultural, range and wildlife habitat management which will be accomplished on the national forests of northern Idaho and eastern Washington in the future. Such a basis has long been needed." He continued by suggesting "your paper will go a long way toward selling

---

[55] Daubenmire, "Roots of a concept," (unpublished); Bailey et al. (1978); O'Hara et al. (1996);; Pfister and Arno (1980); Stout (1995); Hill Williams, "Shrubs, Herbs Used in Classing Forests," unnamed and undated newspaper article contained in RFDP; Hinz (1975).

[56] Daubenmire (1953, p. 17).

[57] Daubenmire made this very point about competing demands in Daubenmire (1973, pp. 87–91). An overview of these trends in the region is in Sowards (2007, pp. 176–82).

[58] RFDP; Frank E. Egler to Daubie, 2 December 1952. For Egler's position as a Clementsian critic in favor of the individualist school, see Whittaker (1962, pp. 82 and 124).

[59] RFDP; John H. Fagan (to Rexford Daubenmire, undated). Although it is not specified, Fagan was likely employed by Potlatch Corporation. Another Potlatch forester inquired about reprints to distribute to the company's foresters; see RFDP; Royce G. Cox to Dr. R. F. Daubenmire, 6 February 1953. RFDP; M. E. Solomon to Dr. R. Daubenmire (undated). Solomon worked in the Department of Scientific and Industrial Research, Pest Infestation Laboratory, Slough, England. RFDP; Lucy B. Moore to Dr. R. F. Daubenmire, 7 April 1953. Moore worked in the Botany Division of the Department of Scientific and Industrial Research in Wellington, New Zealand.

[60] RFDP; Paul D. Dalke to Dr. R. F. Daubenmire, 19 December 1952.

[61] Statistics derived from information on US Forest Service website, (http://www.fs.fed.us/) accessed 30 July 2013.

an ecological approach to forest land management of the area described."[62] Together, these comments demonstrate that Daubenmire offered generalized knowledge, useful to those in Pennsylvania, New Zealand, or England, where plant species and communities were quite distinct from the inland Northwest. But they also show practical, local applications from an ecological perspective on public and private forestlands. Daubenmire clearly conducted work that bridged, or at least appealed to, both sides of the basic and applied divide.

The Continuum Theory Challenge

Yet, while Daubenmire and coworkers traipsed through the forests finding climax or near-climax communities, other ecologists devised distinct approaches. Since its founding, the discipline struggled for acceptance, and one way it sought to enhance credibility was to develop greater rigor. Moving beyond what some saw as descriptive and subjective methods, a new school of ecology used statistical methods to create supposedly objective descriptions of plant ecology. Their innovations were part of a general quantitative turn in ecology, moving it more in line with so-called hard sciences, a self-conscious desire that seems to run throughout ecology's history.[63] Led by University of Wisconsin-Madison professor John T. Curtis, this school helped revive Gleason's individualist perspective. Working first in the Midwest, field-workers selected random plots and collected data on the vegetation and then arranged it along several axes tied to various environmental gradients.[64] The data revealed that distinct plant communities did not exist, but rather that vegetation grew in continuous variation—a continuum—whereby as one moved through a landscape, a species would appear, increase in quantity, then decline, and disappear, but in no particular pattern.[65] The continuum school constituted a significant shift in the 1950s, amounting to a paradigm shift according to plant biologist Michael

---

[62] RFDP; Fred W. Johnson to Dr. R. F. Daubenmire, 21 November 1952. The principal silviculturist from the Northeastern Forest Experiment Station in Pennsylvania concurred with the need to tie plant sociology with forest management, see RFDP; M. Westveld to Dr. R. Daubenmire, 8 December 1952. Perhaps too much can be made of the supportive statements by foresters, since historian Paul W. Hirt has shown that timber management in the region at this time initiated a disastrous set of unsustainable practices, see Hirt (1999).

[63] Kohler (2002) explored various efforts in biology to bring statistical and other methods into fieldwork around the turn of the twentieth century in *Landscapes and Labscapes*. Other histories of ecology note the quantitative shift. Kingsland (2005) focuses on how ecologists adopted systems perspectives to study ecosystems in *Evolution of American Ecology,* pp. 206–231. McIntosh explores a range of quantitative topics in *The Background of Ecology,* pp. 107–145. Also, Bowler, *Earth Encompassed,* pp. 535–46.

[64] The classic methodological paper is Bray and Curtis (1957).

[65] Explanations and context for Curtis' work can be found in McIntosh (1985), esp. pp. 137–45; Nicolson (2001).

G. Barbour.[66] It was a Gleasonian world without distinct communities behaving in predictable ways.

For the most part, Daubenmire's disagreement with the continuum school remained implicit in his own conclusions. Indeed, he frequently noted how there were good things to take from competing schools of thought.[67] However, his article in the prestigious journal *Science* directly criticized the continuum school. Describing Curtis' approach, he sardonically noted that the statistical methodology "makes the results more satisfying to a mathematician than to a botanist."[68] Daubenmire recognized what was at stake; without an organizing principle, vegetation science would be unable to predict and thus furnish useful information. As he once put it, "Without classification there can be no science of vegetation."[69] Daubenmire reported on his own Columbia Plateau research which revealed marked discontinuities among four vegetation zones. Rather than resting his case there, he provided a contrary reading of evidence. Random sampling, as Curtis advocated, in the same region could well have yielded islands of atypical plants—those growing on steep slopes, for instance. This was why Daubenmire advocated sampling, subjectively, from representative areas that were relatively homogenous in climax or near-climax states. This approach did not produce random objectivity but did generate an accurate characterization within the broader landscape.[70]

Furthermore, the continuum school focused on tabulating species' distribution and abundance, but Daubenmire pointed out that such a method was too simple, "as much a part of taxonomy as of synecology. In synecology we must come to grips with matters of more fundamental biologic importance, especially population structure and dynamics." The continuum school quantified plants as they existed in one moment of time, while the community-based approach examined how they interrelated with each other across space and time. Doing so required ecologists to pay closer attention to such factors as age structure and competition within stands, factors that revealed succession patterns post-disturbance. Curtis' statistical methods might have been ecologically innovative and mathematically sound. But the results, according to Daubenmire, merely showed "that continuum advocates have used disturbed vegetation mosaics in which seral mixtures can provide frequent bridging between otherwise reasonable distinct stable types, or in which degradation has

---

[66] Barbour (1995). The degree to which the shift truly represented a paradigm change is debatable, depending on one's comparative framework. Nonetheless, a revival of Gleason's influence was indisputable.

[67] For instance, Daubenmire (1952, p. 302); Daubenmire (1968b, p. x.)

[68] Daubenmire (1966, quoted on p. 291).

[69] Daubenmire (1960, p. 24). This paper was based on remarks at a Symposium on Forest Types and Forest Ecosystems during the IX International Botanical Congress in Montreal, 24 August 1959. It includes some of his most direct criticisms of the continuum school.

[70] Daubenmire (1966), esp. pp. 291–95. In 1959, Daubenmire published his own methodology, which explained his field approach in detail. Daubenmire (1959a). This article was widely cited (according to Google Scholar, nearly 2000 citations) and earned status as a "citation classic" for ecology; in fact, it was the 13th most cited article in ecology between 1947 and 1977. See McIntosh (1989). With modifications, his method continues to be used; see Bonham et al. (2004).

proceeded to a relatively stable network of variation that is infinitely simpler than the mosaic which replaced it." Daubenmire conceded *flora*—that is, the individual plants—represented a continua, for surely plants changed imperceptibly as one moved through the landscape; however, *vegetation*—that is, the cumulative plants in relationship with each other and the environment—was something arranged in distinct units.[71]

The *Science* article was a strong critique, and judging from the responses Daubenmire received privately, ecologists cared deeply about its implications. Although scientists have reputations for being rational, objective researchers, these letters of support exuded a combination of bellicosity and acclamation. Some described how Daubenmire "struck a blow" for the community perspective, characterized the article as a "rallying point" for community ecologists, and gave him the proverbial "Good show!" as if this were a schoolyard contest and not a set of scientific questions.[72] These reactions support one report that at least some of this rancorous debate was "maybe due to the delight in fighting each other that some people have."[73] Indeed, comments against the continuum school were often uncharitable, calling it "nonsense" or saying the method included "little of ecological value, mostly a maze of statistics." A zoologist at Curtis' university who had not been "entirely indoctrinated" sided with Daubenmire that "the study of ecology should not be reduced to numerical abstraction, in spite of the temptations our high speed machines offer in terms of data analysis."[74] The inimitable Frank Egler was sure that "This paper will go down in history as the long-overdue come-uppance for the continuumophilists."[75] Longtime Yale forester Harold Lutz enthusiastically "endorsed" Daubenmire's views but could not share his ideas about the continuum perspective "[w]ithout resorting to campfire language." More importantly, Lutz offered what was no doubt a common, though unscientific, point of view: "Plant communities are very real things to me; I have seen them, felt them, walked in them and *know* they are real and meaningful. The same is true for the climax concept and for succession."[76] Despite efforts to be objective, then, some ecologists still relied on a

---

[71] Daubenmire (1966), esp. pp. 292–96, quotations from pp. 295, 296, 298. His focus on space and time, as well as the emphasis on landscapes' mosaics, are all legacies of Cooper's teaching. Nicolson showed that Humboldt first distinguished between floral vegetation in "Humboldt, Humboldtian Science."

[72] All in RFDP; Francis C. Evans to Dr. R. F. Daubenmire, 26 January 1966 (struck a blow); John [no last name] to Dauby, 15 February 1966 (rallying point); Dr. Robert Linn to Dr. Rexford Daubenmire, 21 January 1966 (good show).

[73] Helen Buell, quoted in Barbour (1995, p. 242).

[74] All in RFDP; Ronald O. Kapp to Dr. R. Daubenmire, 25 January 1966 ("nonsense"); Philip V. Wells to Dr. R. F. Daubenmire, 15 February 1966 ("abstract nonsense"); Lawrence C. Bliss to Dr. Rexford Daubenmire, 28 February 1966 (little ecological value); James W. Drescher to Dr. Rexford Daubenmire, 19 April 1966 ("entirely indoctrinated").

[75] RFDP; Frank (Egler) to Daubie, 26 February 1966.

[76] RFDP; Harold Lutz to Dr. Daubenmire, 26 January 1966 (original emphasis). Lutz continued, "It may be smugness, but sometimes I wonder about the field experience of those who have trouble with these concepts."

felt sense of the way things were in nature. Too many scientists to list from throughout the USA and as far away as Costa Rica and India attested to Daubenmire's eloquence and discipline in publishing this important scholarship.[77]

A final comment from Richard S. Driscoll, a principal plant ecologist for the US Forest Service at its Rocky Mountain Forest and Range Experiment Station, made a critical point. Even though he knew others disagreed, classification could be done and in fact was essential: "I feel vegetation grouping is very necessary if we are to provide a rational scientific and factual basis for land use and management."[78] When managing vegetation, whether in forests or farms, the plant community concept offered something useful and necessary. For some at the time, that proved the essence of the debate. Jerry Franklin, a Daubenmire graduate student who finished his doctorate the year before the *Science* article appeared and who later became a central figure for the Forest Service in the spotted owl controversy of the 1980s and 1990s, recalled a continuum partisan telling him at the time, "This community stuff is OK for you managers, but I'm interested in the truth."[79] Even if it was not "truth," a continuum ecologist just might allow that it was useful for management.

The Ecology of Place

By the late 1960s and into the 1970s, after more than three decades in the inland Northwest, Daubenmire had witnessed much change in the region, not to mention in the science of ecology. Questions about power (hydroelectric and nuclear), the intensifying use of natural resources, and the preservation of wilderness made the Northwest a politically and economically contentious region centered on questions related to environmental quality.[80] Nationally, the environmental movement had emerged, and federal legislation like the National Environmental Policy Act (1970) and the Endangered Species Act (1973) reshaped Americans' legal and ethical relationship with nature.[81] Ecology played a role in this activism, providing data about the harm certain economic activities caused to natural systems. This context suffused the work Daubenmire conducted in the last stage of his career.

As well as anything, two major studies, *Forest Vegetation of Eastern Washington and Northern Idaho* in 1968 and *Steppe Vegetation of Washington* in 1970, exem-

---

[77] Others appreciated Daubenmire's account because it affirmed their own research findings and thus lent support against the wave of continuum studies. For instance, all in RFDP; Henry S. Conrad to Dr. Daubenmire, 22 January 1966; Donald Caplenor to Dr. R. F. Daubenmire, 23 February 1966. The Daubenmire Papers include dozens of supportive letters. *Science* published two letters from Curtis students, explaining their disagreements with Daubenmire's methods and interpretation of continuum perspectives; Vogl et al. (1966).

[78] Richard S. Driscoll to Dr. R. Daubenmire, 14 November 1966.

[79] Quoted in Barbour (1995, p. 241). A list of Daubenmire's graduate students is available in RFDP.

[80] A regional overview is found in Sowards (2007), esp. pp. 167–209.

[81] There are numerous studies that trace the contours of the environmental movement; for a representative introduction, see Rothman (1998).

plified Daubenmire's ecology of place and demonstrate how he had become more assured so that he could comment more openly, if briefly, on these broader environmental concerns.[82] The bulk of each study defined the myriad forest and steppe vegetation habitat types on the Columbia Plateau, the culmination of more than three decades of fieldwork. There were few surprises in these studies, as Daubenmire rehearsed his typical methods, his basic assumptions about the reality of plant associations, and his preference for sampling representative climax communities. Indeed, he noted that this work only strengthened his earlier conclusions. However, suggesting the era's zeitgeist, he included an impassioned rationale: "Remnants of primeval forest representing most of the associations are still to be found. However, as more and more of the land is brought under management, these stands are the first to suffer, for in terms of timber production they are 'overmature' and 'decadent.' Simple economics dictate their replacement by young and vigorously growing trees. Thus the possibility of making such a study as this is rapidly dwindling and another useful purpose, the historical, is served by recording the character of the primeval forest."[83] One readily senses Daubenmire's sense of urgency and passion for the place, as well as his impatience for "simple economics."

In *Steppe Vegetation of Washington,* Daubenmire offered less dramatic prose, but his criticism may have been more subversive. Ostensibly, the study could inform range management for maximum sustained yield.[84] Daubenmire proposed an ecologically informed grazing regime that differed from the "narrow view" typical of North American range managers who focused on just a few species, arguing that "plants of low economic value can have very high indicator significance."[85] Focusing on the entire ecosystem and not just economically valuable species improved management. For example, ranchers and range managers wanted to remove sagebrush to promote grasses. However, such so-called range restoration really was about increasing the productivity of specific grasses favored by livestock and was rooted in an ethos of maximized production and intensive agriculture. Ecological considerations were different. Sagebrush protected perennial grasses; thus, eradicating it would make grasses even more vulnerable to overgrazing. Little evidence existed that removing sagebrush did anything more than increase grass productivity

---

[82] Daubenmire and Daubenmire (1968); Daubenmire (1970). The forest vegetation study included Daubenmire's wife, who had earned an MSc degree, as a coauthor. She accompanied him on much of his fieldwork, and he faithfully acknowledged her assistance in numerous publications. This was their only coauthored piece. Unfortunately, the dynamics of their scientific partnership remain elusive in the extant record. Letters from his students contained in his papers frequently mention Jean, suggesting that she was an active and visible partner.

[83] Daubenmire and Daubenmire (1968, pp. 1–2). Much as anthropologists practice salvage anthropology where artifacts or communities are imminently threatened, what Daubenmire is describing here can be likened to salvage ecology: gathering as much ecological data as possible before the natural community was destroyed.

[84] Maximum sustained yield was a common managerial goal, although environmental historians have criticized its actual practice; see, for example, Hirt (1994); Langston (1995, pp. 157–200); McEvoy (1986, p. 6).

[85] Daubenmire (1970, p. 1). For further criticism of narrow management, see Daubenmire (1984).

in the short term; long-term effects were still unknown. Nor was there evidence about what ramifications there may be for soils, although Daubenmire hypothesized several negative consequences (e.g., declining mineral cycling). Sagebrush also furnished excellent bird habitat, which in turn aided in keeping insects in check. It also held snowpack longer in spring, which increased soil moisture during hot summers in the interior Northwest. He also warned against using powerful new herbicides, because these chemicals killed broadleaf plants indiscriminately, eliminating other plants that were economically unimportant but which could be ecologically significant. In short, Daubenmire challenged an article of faith—that removing sagebrush was beneficial because it enhanced economically valuable and palatable plants—by shifting the economic criteria to ecological values. More and more such conclusions permeated some ecologists' work, showing how a long career and widening perspectives promoted in Daubenmire a strong ethic of place.[86]

Daubenmire's decades of work earned recognition and accolades from his colleagues on a national level. He presided over the ESA in 1967, joining such other notable American ecologists as Aldo Leopold and Eugene Odum, both of whom had also served as ESA president. The Northwest Scientific Association honored him as their "Outstanding Scientist" in 1970. Daubenmire enjoyed national awards from the ESA who named him as the Eminent Ecologist for 1979, from the Society of American Foresters who awarded him the 1980 Barrington Moore Award, the Society for Range Management who gave him a "Special Award" in 1986 recognizing his "extraordinary contributions to the Society for Range Management and the range profession," and the Nature Conservancy granted him honorary lifetime membership. This recognition indicated both the esteem in which fellow scientists held Daubenmire and his diverse interests and expertise spread across forests and rangelands. Significant scientists also walked through his classroom and recognized Daubenmire's teaching influence, including F. Herbert Bormann (the longtime Yale researcher who worked on the notable Hubbard Brook Ecosystem Study), Tom Tidwell (the current US Forest Service chief), and Jerry Franklin (the erstwhile chief plant ecologist for the Forest Service's Pacific Northwest Research Station). A legacy of students and a full curriculum vitae meant that by traditional academic standards Daubenmire finished his career as a great success.[87]

After more than four decades examining the Columbia Plateau's varied and changing landscapes, Daubenmire had accomplished much. He applied ecological thinking to a place theretofore barely examined with modern scientific methods. He determined habitat types throughout the interior Northwest with an eye toward potential vegetation. He did this all caring how local results fit within broader schemes

---

[86] Daubenmire (1970, pp. 79–80), quotation on pp. 80. Knobloch points out that restoring overgrazed ranges was always about increasing economic productivity, not any ecologically based goal; see Knobloch (1996, pp. 99). Knobloch explores the chemical focus of weed eradication on pp. 136–142; see also Duffin (2007, pp. 102–26).

[87] Burgess and Ellstrand (1983); Hoffman (1980, pp. 34–35); Hoffman reviews his awards in Hoffman (1996). See also Bormann (1996, p. 3); Anonymous (n.d.). On his retirement, many students wrote letters of appreciation that revealed the deep admiration they felt for their mentor. This correspondence is bound and contained in the RFDP, which also contains the actual awards.

for organizing the world's vegetation. This meant he was engaged deeply in the local while simultaneously contributing to larger ecological projects, exemplifying the ecology of place approach to the discipline.

Conclusion

Daubenmire represents those many scientists seeking connections between generalized theories, local conditions, and practical problems. These contexts are important when investigating the intersection of life sciences, environment, and agriculture. In this case, ecology formed the scientific framework for Daubenmire's work. Yet the discipline changed during his career from a relatively immature science with few competing theories to one where changing methodologies and philosophies added nuances, challenges, and intellectual competition. Tracing Daubenmire's engagement with ecological debates during the transitional era between the 1930s and 1970s reveals some of these contours. Meanwhile, when lands opened to intensive agriculture or when forests opened to increased harvests, the regional environment transformed. The desire to avoid expensive trial-and-error approaches to growing plants and the hope to harvest nature's products sustainably held managers' and scientists' attention and drove Daubenmire and others to conceive of ways ecology could promote greater environmental quality. To ignore the policy or management dilemmas facing natural resource systems or to neglect the environmental changes and pressures in a landscape is to miss a prime motivating factor for many working ecologists. It is essential that historians of science keep in mind the practical and material contexts in which ecologists worked in addition to the ideas they developed and debated.

In *Plant Communities,* Daubenmire contextualized ecological work like his. "A major objective in any science is to predict and control," he claimed. "Since vegetation is dynamic, it is only through careful study of successional processes that man gains an ability to predict natural trends and to develop feasible objectives in modifying them, both of which are essential for success in managing vegetation."[88] Here, he summed up his work's raison d'être. Ecologists were not modern natural historians describing landscapes and listing species. Nor were they just discerning biological mechanics to determine plants' functioning. For ecologists such as Daubenmire, the necessary work they did served broader society by allowing natural resource decisions to be scientifically informed. For him, this grew organically out of his practice of the ecology of place, developed in the field, over time, and with deep engagement.

Acknowledgments The participants at the workshop provided a helpful, supportive, and engaging environment. Sharon Kingsland challenged me and forced me to clarify concepts and prose in a way that vastly improved this essay. I am grateful to Laura Kross for her research assistance

---

[88] Daubenmire (1968b, p. 25).

and many conversations about Daubenmire and ecological methods. Ian Chambers proved to be an excellent reader of multiple drafts. Andrew Duffin provided both invaluable research assistance and intellectual companionship; in an earlier iteration of this essay, he served as my coauthor, an experience I relished. Finally, to Kelley Sowards who listened to my findings and ideas and read countless drafts, I am grateful as we develop our own ecology of place here.

References

Anonymous. n.d. U.S. Forest Chief Tom Tidwell: 'WSU education taught me how to think'. Washington State University, Alumni and Friends website. http://cahnrs.wsu.edu/alumni/profile/u-s-forest-chief-tom-tidwell-wsu-education-taught-me-how-to-think/. Accessed 20 July 2013.

Bailey, Robert G., Robert D. Pfister, and Jan A. Henderson. 1978. Nature of land and resource classification—A review. *Journal of Forestry* 76:650–655.

Barbour, Michael G. 1995. Ecological fragmentation in the fifties. In *Uncommon ground: Toward reinventing nature,* ed. William Cronon, pp. 233–255. New York: W. W. Norton.

Billick, Ian, and Mary V. Price, eds. 2010. *The ecology of place: Contributions of place-based research to ecological understanding*. Chicago: University of Chicago Press.

Bonham, Charles D., Daryl E. Mergen, and Sam Montoya, 2004. Plant cover estimation: A contiguous Daubenmire frame. *Rangelands* 26:17–22.

Bormann, F. H. 1996. Ecology: A personal history. *Annual Review of Energy and the Environment* 21:1–29.

Bowler, Peter J. 1992. *The earth encompassed: A history of the environmental sciences*. New York: W. W. Norton.

Bray, J. R., and J. T. Curtis. 1957. An ordination of the upland forest communities of southern Wisconsin. *Ecological Monographs* 27:325–349.

Burgess, Robert L., and Norman C. Ellstrand. 1983. Of interest to ecologists. *Bulletin of the Ecological Society of America* 64:17–18.

Cittadino, Eugene. 1993. A 'marvelous cosmopolitan preserve': The dunes, Chicago, and the dynamic ecology of Henry Cowles. *Perspectives on Science* 1:520–559.

Clements, Frederic E. 1916. *Plant succession: An analysis of the development of vegetation*. Washington, D.C.: Carnegie Institution of Washington.

Cooper, W. S. 1926. Fundamentals of vegetational change. *Ecology* 7:391–413.

Cowles, Henry C. 1899. The ecological relations of the vegetation on the sand dunes of Lake Michigan. *Botanical Gazette* 27:95–117, 167–202, 281–308, 361–391.

Daubenmire, R. F. 1936. The 'big woods' of Minnesota: Its structure, and relation to climate, fire, and soils. *Ecological Monographs* 6:233–268.

Daubenmire, R. F. 1938. Merriam's life zones of North America. *The Quarterly Review of Biology* 13:327–332.

Daubenmire, R. F. 1939. Contributions to the ecology of the Big Bend area of Washington: Forage resources of the Scabland Prairies and the effects of grazing upon them. *Northwest Science* 13:33–37.

Daubenmire, R. F. 1940a. Plant succession due to overgrazing in the *Agropyron* bunchgrass prairie of southeastern Washington. *Ecology* 21:55–64.

Daubenmire, R. F. 1940b. Some forest-soil relationships: Contributions to the ecology of the Big Bend Area of Washington: II. Indicator significance of the natural plant communities in the region of the Grand Coulee irrigation project. *Northwest Science* 14:8–10.

Daubenmire, R. F. 1942. An ecological study of the vegetation of southeastern Washington and adjacent Idaho. *Ecological Monographs* 12:53–79.

Daubenmire, R. F. 1943. Vegetational zonation in the Rocky Mountains. *Botanical Review* 9:325–393.

Daubenmire, R. F. 1946. The life zone problem in the northern intermountain region. *Northwest Science* 20:28–38.

Daubenmire, R. F. 1947. *Plants and environment: A textbook of plant autecology*. New York: Wiley.

Daubenmire, R. F. 1952. Forest vegetation of Northern Idaho and adjacent Washington, and its bearing on concepts of vegetation classification. *Ecological Monographs* 22:301–330.

Daubenmire, R. F. 1953. Classification of the conifer forests of eastern Washington and northern Idaho. *Northwest Science* 27:17–24.

Daubenmire, R. F. 1954. Vegetation classification. *Veröffentlichungen des Geobotanischen Institutes Rübel in Zürich* 29:29–34.

Daubenmire, R. F. 1956a. Climate as a determinant of vegetation distribution in eastern Washington and Northern Idaho. *Ecological Monographs* 26:131–154.

Daubenmire, R. F. 1956b. The use of vegetation to indicate grazing potentials of forest land. In Converting the old-growth forest: Proceedings of the Society of American Foresters Meeting, October 16–21, 1955, 35–36. Washington: Society of American Foresters.

Daubenmire, R. F. 1959a. A canopy-coverage method of vegetational analysis. *Northwest Science* 33:43–64.

Daubenmire, R. F. 1959b. *Plants and environment: A textbook of plant autecology*. 2nd ed. New York: Wiley.

Daubenmire, R. F. 1960. Some major problems in vegetation classification. *Silva Fennica* 105:22–25.

Daubenmire, R. F. 1966. Vegetation: Identification of typal communities. *Science* 151:291–298.

Daubenmire, R. F. 1968a. Ecology of fire in grasslands. In *Advances in ecological research,* ed. J. B. Cragg, vol. 5, pp. 209–266. London: Academic.

Daubenmire, R. F. 1968b. *Plant communities: A textbook of plant synecology*. New York: Harper and Row.

Daubenmire, R. F. 1970. Steppe vegetation of Washington, Technical Bulletin 62. Pullman: Washington Agricultural Experiment Station, Washington State University.

Daubenmire, R. F. 1973. A comparison of approaches to the mapping of forest land for intensive management. *The Forestry Chronicle* 49:87–91.

Daubenmire, R. F. 1974. *Plants and environment*. 3rd ed. New York: Wiley.

Daubenmire, R. F. 1976. The use of vegetation in assessing the productivity of forest lands. *Botanical Review* 42:115–143.

Daubenmire, R. F. 1978. *Plant geography with special reference to North America*. New York: Academic.

Daubenmire, R. F. 1984. Viewpoint: Ecological site/range site/habitat type. *Rangelands* 6:263–264.

Daubenmire, R. F., and W. E. Colwell. 1942. Some edaphic changes due to overgrazing in the Agropyron-Poa prairie of southeastern Washington. *Ecology* 23:32–40.

Daubenmire, R. F., and Jean B. Daubenmire. 1968. *Forest vegetation of eastern Washington and Northern Idaho*. Pullman: Washington State University Extension.

Duffin, Andrew P. 2007. *Plowed under: Agriculture and environment in the Palouse*. Seattle: University of Washington Press.

Dwire, Kathleen A., Bruce A. McIntosh, and J. Boone Kauffman. 1999. Ecological influences of the introduction of livestock on Pacific Northwest ecosystems. In *Northwest lands, northwest peoples: Readings in environmental history,* ed. Dale D. Goble and Paul W. Hirt, pp. 313–335. Seattle: University of Washington Press.

Fox, Stephen. 1981. *The American conservation movement: John Muir and his legacy*. Madison: University of Wisconsin Press.

Franklin, Jerry F. 1980. Ecological site classification activities in Oregon and Washington. *The Forestry Chronicle* 56:68–70.

Gleason, H. A. 1926. The individualistic concept of the plant association. *Bulletin of the Torrey Botanical Club* 53:7–26.

Gleason, H. A. 1939. The individualistic concept of the plant association. *American Midland Naturalist* 21:92–110.

Golley, Frank B. 1993. *A history of the ecosystem concept in ecology: More than the sum of the parts*. New Haven: Yale University Press.

Hagen, Joel B. 1992. *An entangled bank: The origins of ecosystem ecology*. New Brunswick: Rutgers University Press.

Hays, Samuel P. 1959. *Conservation and the gospel of efficiency: The progressive conservation movement, 1890–1920*. Cambridge: Harvard University Press.

Henderson, Jan A., Robin D. Lesher, David H. Peter, and Chris D. Ringo. 2011. A landscape model for predicting potential natural vegetation of the Olympic Peninsula USA using boundary equations and newly developed environmental variables, General Technical Report PNW-GRT 841. Portland, Oregon: United States Department of Agriculture, Forest Service, Pacific Northwest Research Station.

Hinz, Sue. 1975. Daubenmire: A botanist 365 Days a Year. Hilltopics (October), 14.

Hirt, Paul W. 1994. *A conspiracy of optimism: Management of the national forests since World War Two*. Lincoln: University of Nebraska Press.

Hirt, Paul W. 1999. Getting out the cut: A history of national forest management in the Northern Rockies. In *Northwest lands, northwest peoples: Readings in environmental history,* ed. Dale D. Goble and Paul W. Hirt, pp. 437–461. Seattle: University of Washington Press.

Hoffman, George R. 1980. Eminent ecologist for 1979, Rexford Daubenmire. *Bulletin of the Ecological Society of America* 61:34.

Hoffman, George R. 1996. Rexford Daubenmire, 1909–1995. *Bulletin of the Ecological Society of America* 77:143–144.

Kendeigh, S. Charles. 1954. History and evaluation of various concepts of plant and animal communities in North America. *Ecology* 35:152–171.

Kingsland, Sharon E. 2005. *The evolution of American ecology, 1890–2000*. Baltimore: Johns Hopkins University Press.

Knobloch, Frieda. 1996. *The culture of wilderness: Agriculture as colonization in the American West*. Chapel Hill: University of North Carolina Press.

Kohler, Robert E. 2002. *Landscapes and labscapes: Exploring the lab-field border in biology*. Chicago: University of Chicago Press.

Kohler, Robert E. 2008. Plants and pigeonholes: Classification as a practice in American ecology. *Historical Studies in the Natural Sciences* 38:77–108.

Langston, Nancy. 1995. *Forest dreams, forest nightmares: The paradox of old growth in the inland west*. Seattle: University of Washington Press.

Maher, Neil M. 2008. *Nature's new deal: The Civilian Conservation Corps and the roots of the American environmental movement*. New York: Oxford University Press.

McEvoy, Arthur F. 1986. *The fisherman's problem: Ecology and law in the California fisheries, 1850–1980*. Cambridge: Cambridge University Press.

McGregor, Alexander Campbell. 1982. *Counting sheep: From open range to agribusiness on the Columbia Plateau*. Seattle: University of Washington Press.

McIntosh, Robert P. 1985. *The background of ecology: Concept and theory*. Cambridge: Cambridge University Press.

McIntosh, Robert P. 1989. Citation classics of ecology. *Quarterly Review of Biology* 64:31–49.

Meinig, D. W. 1995. *The Great Columbia Plain: A historical geography, 1805–1910. Reprint 1968 edition*. Seattle: University of Washington Press.

Merriam, C. Hart. 1890. Results of a biological survey of the San Francisco mountain region and desert of the Little Colorado in Arizona. *North American Fauna* 3:1–136.

Merriam, C. Hart. 1898. *Life zones and crop zones of the United States*. Washington, D.C.: Government Printing Office.

Nicolson, Malcolm. 1987. Alexander von Humboldt, Humboldtian science and the origins of the study of vegetation. *History of Science* 25:167–194.

Nicolson, Malcolm. 2001. 'Towards establishing ecology as a science instead of an art': The work of John T. Curtis on the plant community continuum. Web Ecology 2:1–6. http://www.web-ecol.net/2/1/2001/we-2-1-2001.html. Accessed 30 July 2013.

Nicolson, Malcolm, and Robert P. McIntosh. 2002. H. A. Gleason and the individualistic hypothesis revisited. *Bulletin of the Ecological Society of America* 83:133–42.

O'Hara, Kevin L., Penelope A. Latham, Paul Hessburg, and Bradley G. Smith. 1996. A structural classification for inland northwest forest vegetation. *Western Journal of Applied Forestry* 11:97–102.

Pfister, Robert D., and Stephen F. Arno. 1980. Classifying forest habitat types based on potential climax vegetation. *Forest Science* 26:52–70.

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

Real, Leslie A., and James H. Brown, eds. 1991. *Foundations of ecology: Classic papers with commentaries*. Chicago: University of Chicago Press.

Rothman, Hal K. 1998. *The greening of a nation? Environmentalism in the United States since 1945*. Fort Worth: Harcourt Brace College Publishers.

Rumore, Gina M. 2009. A natural laboratory, a national monument: Carving out a place for science in Glacier Bay, Alaska, 1879–1958. Ph.D. dissertation, University of Minnesota.

Sowards, Adam M. 2007. *United States west coast: An environmental history*. Santa Barbara: ABC–CLIO.

Stohlgren, Thomas J. 2007. *Measuring plant diversity: Lessons from the field*. New York: Oxford University Press.

Stout, David. 1995. R. F. Daubenmire, 85, botanist whose books inspired ecologists. *New York Times,* September 8.

Tansley, A. G. 1935. The use and abuse of vegetational concepts and terms. *Ecology* 16:284–307.

Thomas, Robert M Jr. 1995. Stanley Cain, ecologist 92, led the field. *New York Times,* April 2.

Tobey, Ronald C. 1981. *Saving the prairies: The life cycle of the founding school of American plant ecology, 1895–1955*. Berkeley: University of California Press.

Vetter, Jeremy, ed. 2011. *Knowing global environments: New historical perspectives on the field sciences*. New Brunswick: Rutgers University Press.

Vogl, Richard J., Grant Cottam, and Robert P. McIntosh. 1966. Vegetational continuum (letters to the editor). *Science* 152:546–547.

Whittaker, Robert H. 1962. Classification of natural communities. *The Botanical Review* 28:1–239.

Worster, Donald. 1979. *Dust bowl: The southern plains in the 1930s*. New York: Oxford University Press.

Worster, Donald. 1994. *Nature's economy: A history of ecological ideas*. 2nd ed. Cambridge: Cambridge University Press.

Young, James A., and Fay L. Allen. 1997. Cheatgrass and range science: 1930–1950. *Journal of Range Management* 50:530–535.

Adam M. Sowards is associate professor of history at the University of Idaho. He concentrates on environmental history in the North American West. He has written or edited several books, including *Idaho's Place: A New History of the Gem State* (2014) and *The Environmental Justice: William O. Douglas and American Conservation* (2009). Currently, his research centers on how scientists have come to understand particular places across changing scales.