Demarcating Nature, Defining Ecology: Creating a Rationale for the Study of Nature's "Primitive Conditions"

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The proper place of humans in ecological study has been a recurring issue. I reconstruct and evaluate an early twentieth century rationale in ecology that encouraged the treatment of humans as apart from natural processes, and I unearth the interests and assumptions, both epistemic and non-epistemic, that fostered it. This rationale was articulated during the early years of the Ecological Society of America, particularly through its Committee on the Preservation of Natural Conditions. Committee members advocated for the preservation of what they considered epistemologically foundational and functionally normal objects of study—nature's "primitive conditions"—and in doing so collapsed two conceptually independent categories of unnaturalness: the artificial and the pathological. As these ecologists demarcated what counted as nature, they were, in the process, defining ecology as a science in ways that had lasting repercussions.

> The relationship of man himself to his environment is an inseparable part of ecology; for he also is an organism and other organisms are a part of his environment. Ecology, therefore, broadly conceived and rightly understood, instead of being an academic science merely, out of touch with humanistic interests, is really that part of every other biological science which brings it into immediate relation to human kind. (Forbes 1922, pp. 89–90)

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1. Introduction

The proper place of humans in ecological study has been a recurring issue for ecology. Of course humans are part of Nature writ large, but are they part of the ecologist's nature? The question bothered biologists even before the discipline of ecology was firmly institutionalized. Thomas Henry Huxley argued in 1876 that because man was a living creature, he and "all his ways" should properly be considered under the province of biology; yet, biologists, he felt, are a "self-sacrificing" bunch, for whom non-human nature is sufficient disciplinary territory (Huxley [1876] 1897, pp. 270–71).

Twenty-first-century ecologists are apparently less self-sacrificing. Confessing that they have traditionally neglected anthropogenic factors, many recent ecologists have argued that this has been for the worse.[1] The reason they offer is that our world, or at the very least our understanding of it, has changed (Ellis et al. 2013). We now recognize that humans are a dominant force in most ecosystems, transforming ecological processes and patterns around the globe, and current ecologists are afraid that a methodological position that discounts humans would lose its global relevance. They feel that they "cannot effectively contribute [to recent discourses] without models that incorporate the activities of our own species" (Collins et al. 2000, p. 418).[2]

As a consequence of this disciplinary self-reflection, commentaries abound in the contemporary ecological literature. Ecologists Boris Worm and Robert Paine, for example, write that "Humans have historically been treated as an externality, as if their effects belong in a separate category compared to other species and their interactions" (Worm and Paine 2016, p. 604).[3] Such commentaries emphasize, first, that ecologists continue to systematically bias the study of "protected" areas—areas protected from humans—over the human-disturbed (Martin et al. 2012). But, second, and more interesting from a philosophical perspective, they indicate that this is not just a matter of contingent institutional, financial, or sociological reasons, but also a matter of epistemology. Martin et al. (2012) argue that most ecological practice assumes that non-human environments "better represent ecological and evolutionary processes and are therefore better objects of study" (2012, p. 198). Ecologist James Brown has argued that such biases are the result of mistakenly associating the epistemic values of "rigor and objectivity" with "natural areas" alone. "The study

1. See Rees 1997, Haila 1999, Grimm et al. 2000, and O'Neill and Kahn 2000 as older but influential examples. See the citations below for more recent examples.

2. The history of "human ecology" speaks to the controversial place of humans in ecology. See Cittadino 1993a, Tjossem 1994, and Kingsland 2005. Steiner (2002), a recent proponent of human ecology, calls human ecology "the subversive subject."

3. For other recent examples, see Inkpen (2017).

of humans and their interrelationships with the rest of the natural world has been left to the 'social' and the 'applied' sciences," he writes, "both of which have been viewed with disdain by many of those who practice 'pure' ecology" (Brown 1995, p. 205). But *why* associate "natural areas" with rigor and objectivity? *Why* treat "the natural" and "the artificial" as distinct domains, and why think that an area is epistemologically superior because it involves less human interference?[4]

Critical historical study can help to answer these questions by going back and reconstructing the rationales or styles of reasoning that encouraged the practice of treating anthropogenic factors as improper research objects. In this essay, I will explore the origin of just such a rationale in ecology. This is not the story of a true origin, as the roots of the idea that nature is separate from the human world can be traced much further back.[5] But it is the story of the role this rationale played during the origin of American ecology as a "self-conscious scientific enterprise," when the rationale was articulated clearly and influentially, and when it served as an intellectual adhesive for the emerging science (Ilerbaig 1999, p. 456).

I argue that this rationale has roots in the early history of the Ecological Society of America (ESA), particularly through its Committee on the Preservation of Natural Conditions (1917–1945). Members of the Society attempted to protect what they considered to be epistemologically and methodologically foundational objects of study, what they called nature's "primitive conditions": "the conditions which existed before man came upon the scene [and] the conditions which would again supervene if the human inhabitants were withdrawn" (Adams 1913, p. 30). These conditions could be found or reestablished only on large tracks of undisturbed—meaning un-human-disturbed—land, such as in a nature preserve. As these ecologists demarcated what counted as nature's primitive conditions, they were also, in the process, defining the science of ecology in ways that had lasting, but largely unacknowledged, repercussions for the methods and practices of field ecology.

In recent years, a handful of historical and sociological studies have touched on this committee (Croker 1991, Tjossem 1994, Kinchy 2006, Kupper 2009, Barrow 2009, Rumore 2012). These studies have shown, first, that early ecologists were not just influenced by environmental

4. Brown's comments are striking also because historians and philosophers of twentieth-century science commonly point to highly artificial laboratory work as the current ideals of rigour and objectivity. See: Shapin 1988, Gooday 1991, Hacking 1991, Knorr Cetina 1992, De Chadarevian 1996. On the laboratory-field boundary, see Benson 1992, Kohler 2002, Kingsland 2009; 2015, Vetter 2012.

5. See, for example, Glacken 1967, Williams 1980, Oelschlaeger 1991, Worster 1994, Descola 2013.

movements, they were active participants: understanding early ecology requires more than identifying parallelisms between ecology and these wider movements, it involves appreciating that ecologists articulated their scientific rationale as members of these movements, sharing many of their assumptions. Secondly, this literature, and in particular Rumore (2012), has shown that the preservation of nature in its "primitive conditions" was important to ecologists because they viewed these conditions as necessary for the advancement of their science. In other words, these conditions were in particular epistemologically valuable. But the mystery, and the question which this essay addresses, is why? What was it about "primitive conditions" that made them so epistemologically valuable?

The current essay departs from the previous literature by taking both a broader and more philosophical approach. I aim to reconstruct the scientific rationale articulated and advocated by these ecologists and to unearth the assumptions and interests, both epistemic and non-epistemic, that fostered it. In doing so, I will draw on and extend Elliott Sober's analysis of the "natural state model" of scientific reasoning (Sober 1980). I will argue that their rationale was a variant of this style. I will also argue that their rationale implied a collapse of two conceptually independent ways of being unnatural: pathological and artificial. Being artificial was for them simply one way of being pathological.

My aim is unabashedly presentist. I ultimately desire to better understand how ecologists got to where they currently are, that is, to explain how rationales articulated in the past have become *de rigueur* and encouraged the practice of favoring nonhuman research sites today.[6] In the end, I will briefly discuss how this rationale fares in the light of modern ecological interests and assumptions, again both epistemic and non-epistemic.

This essay will proceed as follows. Section 2 historically situates the Committee for the Preservation of Natural Conditions within early twentieth-century environmental thinking. Section 3 illustrates the meaning of nature's "primitive conditions" through an analysis of nature preserve architectural plans. Section 4 argues that ecological succession theory, with its guiding metaphor of orderly development, encouraged the treatment of humans as disturbances to the normal sequence of ecological development. Section 5 analyzes their treatment of humans as disturbances and argues that the rationale they offered is a variant of the "natural state model" of scientific

6. The historical methodology utilized in this paper has been described as analogous to developing a phylogeny in biology. See Lennox (2001) for a full account of this methodology. Maienschein et al. (2008, p. 348) argue for a similar methodology, apparently independently of Lennox. For lucid accounts that don't make reference to phylogeny, see Hacking 2004 and Vogel 2015, pp. 33ff.

reasoning. Section 6 argues that underwriting the perceived functional normality of nature's primitive conditions were non-epistemic assumptions about its multifarious superiority. Section 7 demonstrates further methodological implications. Section 8 argues that these ideas were widely held, even among those less committed to the theory of succession. And Section 9 concludes with a critique of the value of their rationale.

2. Preservation for Science

Following its founding in 1915, the ESA quickly assumed a role in nature preservation, aiming to protect North America's "primitive conditions" through its Committee on the Preservation of Natural Conditions (Shreve 1917; Shelford 1938; Dexter 1978). Convened during the Society's second year, this committee concentrated its efforts on listing all of the preserved and preservable areas in North America, explaining the scientific need for preservation, and attempting to secure the preservation of each area. Victor Shelford, an ecologist at the University of Illinois and the ESA's first president, was appointed chair of the committee, a position he would hold intermittently until the committee disbanded in 1945 (Croker 1991). Shelford's appointment was understandable: as first president of the ESA, he argued that the preservation of natural conditions was just as important for the establishment of ecology as professional communication through annual meetings or a journal (Tjossem 1994, p. 18). From 1917 to 1945, the committee consisted of many prominent members of the ecological scientific community—in fact, six of the first seven presidents of the ESA served on the committee—but to eliminate unnecessary complexity, I will focus primarily on Victor Shelford, Charles C. Adams, Francis Sumner, and Joseph Grinnell.

Readers familiar with environmental literature will recognize that the name of the committee is significant. Environmental historians recognize two subspecies of early twentieth-century environmental thought: conservation and preservation.[7] Conservation implies human management: conservationists argue that a governmental body needs to actively manage natural resources for long-term human use, whether for recreation, like hunting, or for the "wise" management of essential resources like lumber. Preservationists, on the other hand, argue for a passive, non-interventionist approach that involves protecting the land from any human interference whatsoever. The committee's name—and its motto, "An Undisturbed Area

7. On the conservation-preservation distinction, see Shelford 1933, Pinchot [1947] 1998, pp. 319ff, Nash 1967, Dunlap 1980, Worster 1994, and Rumore 2012.

in Every National Park and Public Forest"—reveals its commitment to the latter cause (Croker 1991, p. 124).

The committee wanted to preserve nature in its "primitive" state for scientific reasons. As Gina Rumore (2009) has argued, this is often overlooked in environmental histories and histories of ecology.[8] One might think that this committee was formed so that ecologists would have a venue for entering into the political sphere—for using scientific knowledge and authority to influence political decisions about land management. This is in part true, and certainly this was part of Shelford's overall plan (Croker 1991, p. 120ff). But it is not the whole story. The committee was officially established to help protect what ecologists considered to be epistemologically foundational objects of study in ecology: nature's primitive conditions. It was a professional society committee concerned to promote scientific research.[9]

Despite its scientific focus, the committee was not alone in its attempts to protect nature from human exploitation. In fact, it was one of a handful of similar committees formed around the same time.[10] A prevailing narrative in environmental history argues that the proliferation of such committees was due to anxieties over the closing of the American frontier, and this narrative is worth briefly recounting to contextualize the claims made in later sections of this essay (Nash 1967). Until the early twentieth century, the "frontier"—that westward-migrating area of reaction "between wilderness and the edge of expanding settlement"—had been a defining American idea (Turner 1932, p. 183; see Nash 1967, p. 146; Cronon 1996). To many, including famously the historian Frederick Jackson Turner and President Theodore Roosevelt, the frontier had created a uniquely American mentality and set of ideals. Living in the wilderness, what Turner pertinently called "the return to *primitive conditions*," led to the admirable American trait of independence and a democratic society (Turner 1896, p. 289; my emphasis). According to Turner's narrative, struggling against the wilderness set Americans apart from their European ancestors, and set

8. See also Warren 2008 and Barrow 2009 for this emphasis.

9. From the beginning, the ESA focused on field work and the training of ecologists from different areas in the diverse natural communities throughout America (Shelford 1938).

10. See Kupper (2009) for an international perspective. In Switzerland, for example, a similar Committee was created around the same time, the Committee for Nature Protection. These national movements also had connections, especially through the American botanist Harvey Hall, who spent a year in Europe learning about European approaches to nature reserves. See also similar movements in England, for example, Arthur Tansley's work *Our Heritage of Wild Nature* (1945).

them apart for the better. Roosevelt similarly argued that "Under the hard conditions of life in the wilderness," Americans shed their European roots and became new "in dress, in customs, and in mode of life" (Roosevelt, quoted in Nash 1967, p. 149).[11] According to this narrative, America, both the idea and the place, was a product of westward pioneering expansion through, and ultimately with the overcoming of, wilderness. It was pervasive and persuasive in Progressive Era America (Hays 1959).

The writings of members of the ecological committee testify to the power and ubiquity of this narrative; it recurs throughout their work. Charles C. Adams—committee member, seventh president of the ESA, and director of the New York State Museum—wrote that, "Historians [like Turner] have shown us how much our American democratic institutions have been a direct outgrowth of our pioneering, and how this has tended to encourage independence, self-reliance and other traits which have contributed so much toward our institutions and our ideals" (Adams 1929, p. 40). If these ecologists focused on the value of nature's "primitive conditions" for science it was not, in other words, because they overlooked or underappreciated its other non-epistemic, culturally sanctioned values.[12]

Turner declared the close of the frontier in 1890, and by the early twentieth century, the narrative of the value of wilderness was intimately linked with anxieties about its disappearance (Nash 1967, p. 147). Turner himself wondered whether American ideals had "acquired sufficient momentum to sustain themselves under conditions so radically unlike those in the days of their origin" (Turner 1903, p. 91). Part of the reasoning behind early preservation movements, the National Park Service, and city parks, was to protect nature in its "primitive conditions" so that Americans could revisit the conditions that made them who they were. The most pressing issue was urbanization: many Americans had moved to cities, where unnatural living conditions were believed to result in degeneration. Landscape architects, like Charles Eliot, Frederick Law Olmsted, and Jens Jensen, who tackled these problems head on, argued that city life caused

11. Theodore Roosevelt was influential on the thinking of the ecologists of the Committee. For example, see Victor Shelford's long and favorable quotations from Roosevelt in *Animal Communities in Temperate America* (1913, p. 6ff). Joseph Grinnell's cousin, George Bird Grinnell, was a founding member, with Roosevelt, of the Boon and Crockett Club, a conservation organization dedicated to protecting America's wilderness, largely for hunting. See Haraway (1989) for an examination of Roosevelt, his part in these "Nature Movements," and his influence on natural history.

12. In fact, the influence goes both ways, since Turner himself drew on the science, and in particular biology, of his day (Coleman 1966).

exhaustion, depression, and emasculation—as "we grow more and more artificial day by day"—warranting the protection of "natural" areas for recreation and regeneration.[13]

Alongside these social, political and moral meanings, to members of the committee like Shelford and Adams, vanishing wilderness raised a particular problem for the new science of ecology: objects of study were vanishing with it. Using phrases already laden with cultural significance, the committee argued that the "primitive" or "primeval" conditions of nature—those existing before the encroachment of man—were being lost as the North American landscape was urbanized or adapted for agriculture. Joseph Grinnell, the committee's livestock "grazing advisor," wrote that "as the settlement of the country progresses, and the original aspect of nature is altered, the parks will probably be the only areas unspoiled for scientific study" (Grinnell and Storer 1916, p. 379; Shelford et al. 1921, p. 4). "The science of ecology," wrote another member, "depends upon undisturbed patches of nature as its 'material'" (Shelford et al. 1921, p. 10). The vanishing frontier meant vanishing research objects.

A conveniently slippery buzzword, "primitive conditions" spoke to more than just ecologists at this time; it also harkened to these broadly popular cultural ideas about American exceptionalism and the health of a nation and its people. Their particular understanding of the idea of nature's primitive conditions is best explained through a discussion of their landscape architectural plans.

3. Demarcating Nature's "Primitive Conditions"

According to the committee, preservation was intended "to allow nature to take her own course, with as little interference by man as is possible" (Adams 1929, p. 38). In September of 1917 the committee mailed out information cards to its members in order to secure a preliminary data set (Shreve 1917; figure 1). Those who knew of areas desirable for preservation were encouraged to fill in as much information about these areas as possible, including the location, area, and ownership, but also the type of ecological habitat and species therein. The committee could then prioritize and secure as much natural variability as possible, spanning prairies and mountains, swamps and deserts.

13. Stanley White, a landscape architect at Shelford's home institution, the University of Illinois, contributed a piece to the Committee's publication *Naturalist's Guide to the Americas* entitled "The Value of Natural Preserves to the Landscape Architect." Adams referred to the work of Jens Jensen, the landscape architect and committee member (Adams 1929, p. 41). On Jensen and preservation, see Grese 1992. On Jensen and his connection to Henry Cowles, graduate supervisor of both Adams and Shelford, see Cittadino 1993b.

ECOLOGICAL SOCIETY OF AMERICA
COMMITTEE ON THE PRESERVATION OF NATURAL CONDITIONS FOR ECOLOGICAL STUDY.

Tract already to be preserved.....acres, square miles, located near.....in the state or province of....., county of....., ...miles from.....P. O.
Remarks :.....

CLIMATIC VEGETATION
1. Deciduous forest
2. Deciduous Savanna
3. Prairie
4. Steppe
5. Coniferous forest
6. Eastern
7. Semi-desert
8. Mountain
9. Northwestern
10. Semi-desert
11. Evergreen Savanna
12. Chaparral
13.
14. .....
15.
16. Desert
17. Sage brush
18. Cactus
19. Alkali
20.
21. Broad leafed evergreen
22.
23.
24. Alpine meadow
25.

EDAPHIC VEGETATION (Dependent on Soil)
26. Sand areas
27. Flood plain
28. Talus slope
29. Gravel slides
30.
31.
32.
33.
34.
35.
36.

TOPOGRAPHY, SOIL, ETC.
37. Mountain
38. Level plain
39. Ravine
40. Canyon
41. Hill
42. Bluff
43. Shore
44. Sandy
45. Rocky
46. Glacial
47.
48. .....
49.

AQUATIC SITUATIONS
50. Swamp
51. Marsh
52. Pond
53. Lake
54. River
55. Spring
56.
57.

SEMI-NATURAL SITUATIONS
58. Road sides
59. Shrubby
60. Prairie
61. Swampy
62. Railway margins
63.
64.
65.

ANIMALS
66. Bison
67. Beaver
68. Deer
69. Elk
70. Moose
71. Sheep
72. Goats
73. Antelope

WHO SHOULD PRESERVE?
74. Private
75. Gift to
76. State
77. Trustees
78. Society
79. School
80. College
81. University
82. Municipality
83. County
84. State
85. Nation
86. Province
87. Dominion

OBJECTIONS LIKELY TO BE RAISED
88. Commercial development hindered
89. Cost
90. Not artistic
91. Mosquitoes
92. Yellow fever
93. Malaria
94.
95. Wolves favored
96. Coyotes favored
97. Skunks
98. Insect pests
99.

MATTERS NEEDING ATTENTION
100. Grazing
101 Cutting
102. Drainage
103. Fires

Fill out by crossing out irrelevant words in the heading; check characteristics just after the numbers; star important matter; FILL OUT IN DUPLICATE; notes on back.

Figure 1. An information card, mailed out by the committee to members of the ESA in September 1917 (Shreve 1917).

In its first report, published in 1921, the committee specified diagrammatically how one might go about preserving natural areas from the "encroachment of civilization" (Shelford et al. 1921; figures 2 and 3). These suggested management plans demonstrated how to preserve the "Natural"—represented in the figures by a particular symbol and distinguished from "stream," "fence," and "shrubs"—by building a buffer zone around its borders. To discover landscape plans within a committee document is unsurprising given that landscape architect Jens Jensen was a "co-operating member" of the committee in the Chicago area and that, as suggested above, landscape architecture was a key element of many preservation initiatives at the time (Cittadino 1993b, pp. 526ff). Each nature reserve, they argued, should be sub-divided into three distinct areas: a "Nature sanctuary," a "buffer area of partial protection," and an area for the development of human uses, where this is one of the aims of the reserve (Shelford 1933, p. 245). These plans thus also represent attempts to reach compromises between various economic and environmental interests.

These diagrams are important because they reflect the committee's own ideas about what nature was like—namely, that it was a physical location, containing an assemblage of species, free of human interference—where it might be found, and that it could be cordoned-off and contained. The

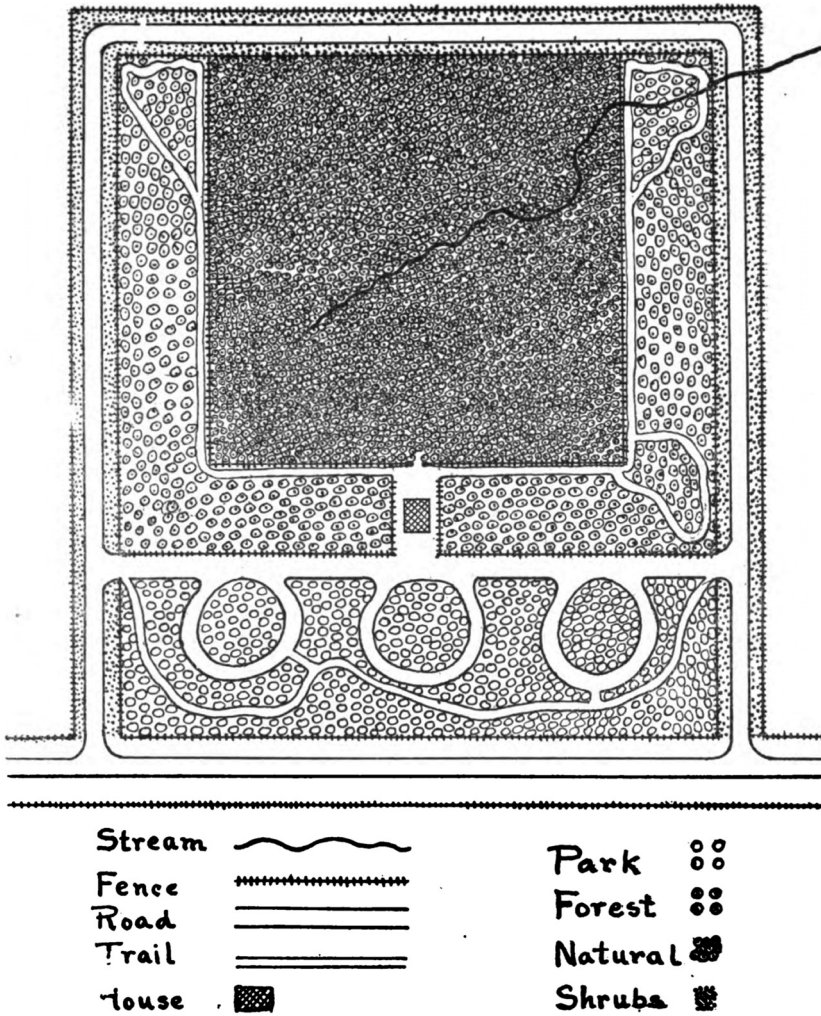
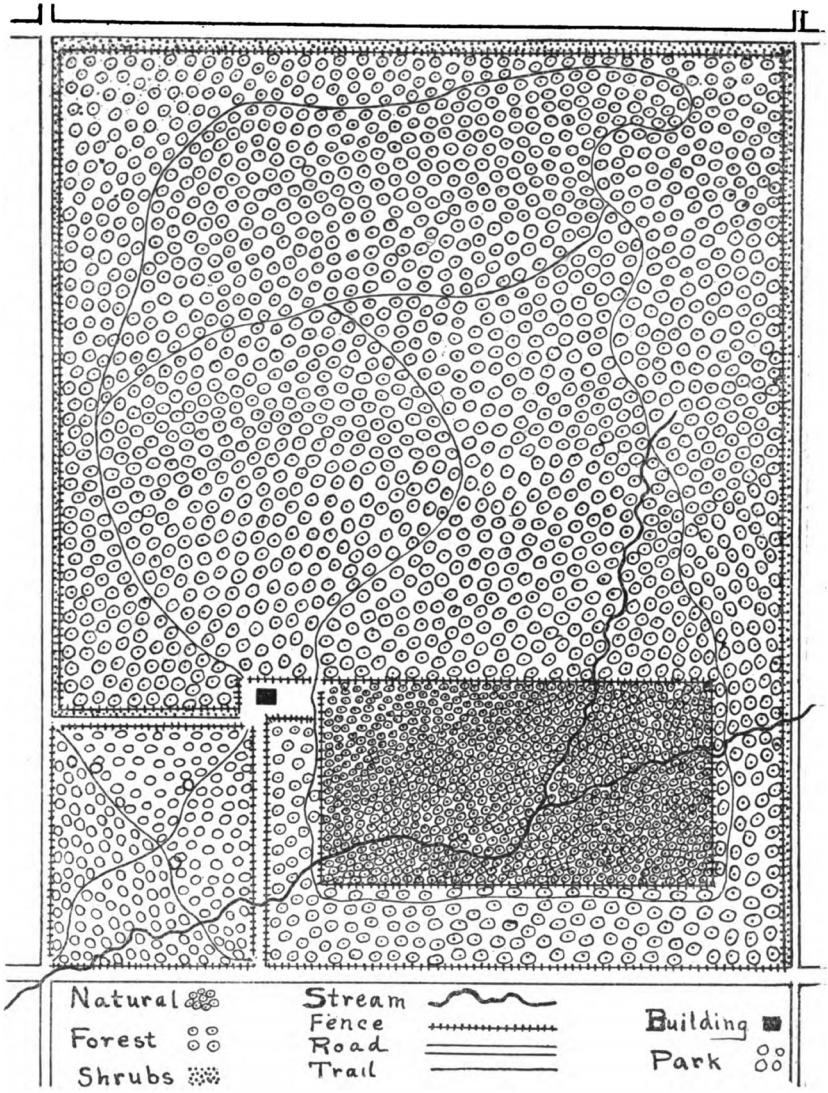


Figure 2. A suggested management plan for a sixty acre tract of forest (Shelford *et al*. 1921).



15. Plan of management of a 1,600 acre tract to serve for the most part as a timber growing project. 120 acres is set aside as a public park for camping, hunting, etc. A somewhat larger tract serves as a natural preserve and a game sanctuary and is surrounded by a single wire. Such a tract will support a number of deer and in fact should have them to maintain a condition such as was originally present.

Figure 3. A suggested management plan for a 1600 acre tract of forest (Shelford *et al*. 1921).

"Natural" area was an area where, in the absence of human disturbance, nature's primitive conditions would develop or express themselves. That the committee ultimately rested upon a distinction between "natural" and "human," despite possessing a rich vocabulary for describing the variability of ecological associations—as shown in figure 1—is a testament to their belief in the homogeneity of urban or agricultural spaces and the degenerative effects of human influence (figure 4). Some argued, and I will return to this below, that agricultural pests, such as scale insects, existed only in human-disturbed places, and not in nature itself (Shelford 1913, p. 18; figure 5).

As I've said, the committee's primary goal in the report and through subsequent work was not just to demarcate nature, but to articulate a scientific rationale for the study of this nature. It would be misrepresentative to focus on one set of factors giving rise to their treatment of nature's "primitive conditions" as epistemologically foundational. A constellation

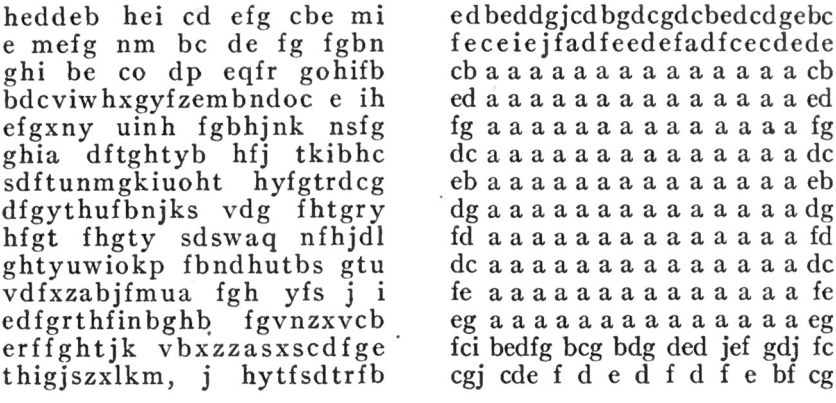
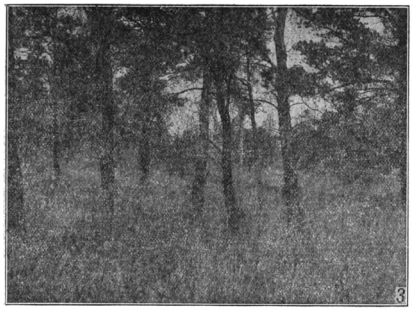


DIAGRAM 1.—Showing the arrangement of plants and animals on a plot of ground under primeval conditions. The letters are fortuitously chosen to represent the fortuitous arrangement of plants and accordingly the animals associated with them. Thus m, n, x, and z may be taken to represent oak, maple, basswood, and cherry, respectively, and the animals associated with each. The other letters may be taken to represent herbs and shrubs and the animals associated with them.

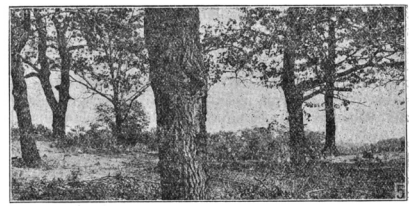
DIAGRAM 2.—Showing the arrangement under agricultural conditions. Here the plants which are put out in rows are represented by a's arranged in rows. There are certain animals associated with such plants and the a's represent these also. Land is not usually cultivated close to the fences and thus each field is surrounded by a border of original shrubs, herbs, and sprouts from the original trees. These and the animals associated with them are still fortuitously arranged.

Figure 4. Two diagrams showing the differences in arrangements of plants and animals under primitive conditions and agricultural conditions (Shelford 1913).

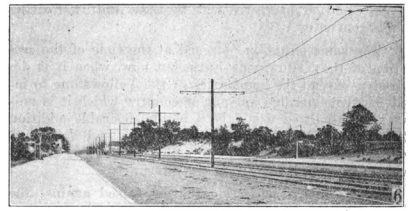


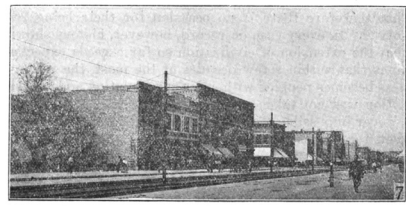
3. Early stage of forest development once common in northern Illinois and Indiana, now almost gone. This picture was taken on the site of Gary in 1905.

4. A later stage than that shown above, once common on the site of Chicago. Taken on the site of Gary in 1905.



5. Some of the oak woods in 1910 in a place similar to that shown in figure 4 showing needless destruction of vegetation.





6 and 7. Showing remnants of this vegetation along the main street of Gary in 1911.

Figure 5. We can get a sense of the perceived degenerative effects of urbanization by considering the committee's repeat photographs of Gary, Indiana following the establishment of a U.S. Steel plant. The photographs depict the progression of deforestation and urbanization of Gary from 1905 to 1911, which the committee saw as a process of de-naturalization (Shelford *et al*. 1921).

of mutually reinforcing arguments supported this belief, as the next sections will show.

4. Ecology's "Laws of Orderly Sequence"

In the early-twentieth century, two styles of ecology were distinguished from one another and these styles began to develop along separate investigative lines (Mitman 1992, p. 42ff).[14] On the one hand, there was autecology: the investigation of the development "of the structure, function, and behavior of *a given individual* or kind of animal from the standpoint of

14. Early ecological textbooks by Adams and Shelford both repeat this division (Adams 1913; Shelford 1913). See also Shelford 1926, p. 57.

its relations and responses to the complete environment" (Adams 1913, p. 3; emphasis added). On the other hand, there was synecology: the investigation of ecological communities, that is, relatively stable assemblages of species coexisting in a particular location.

Autecology was highly amenable to laboratory investigation: it involved subjecting individual organisms to various environmental stimuli and measuring responses—what Shelford called an individual's "physiological life history" (Shelford 1915, p. 2). Synecology, however, was much harder to conduct under laboratory conditions. Maintaining an entire community in the laboratory was infeasible, and so most of these studies were descriptive field studies. In other words, if one wanted to understand ecological relations above the individual level, one was forced to focus on natural communities and this couldn't be done well in the laboratory. Criticizing autecology, Shelford wrote, "Perhaps one reason why nature study has been unsuccessful is because too often it is not the study of nature but of single natural objects or groups of objects which constitute a small part of any natural aggregation" (Shelford 1933, p. 241). In order to study the whole community, what Shelford simply refers to as "nature," one needed to go outside.

Adams and Shelford were both trained at Chicago, and their early work on communities took them both outdoors. Adams' doctoral work focused on variation in gastropod shells in the Tennessee River system (Adams 1901; Adams 1902; Sears 1956; Ilerbaig 1999, p. 446). Shelford's approach, as would be his approach for the rest of his life, combined laboratory experiments and field study, to investigate Tiger beetles on dunes along the Lake Michigan shoreline (Shelford 1908; Shelford 1911; Croker 1991, p. 15ff; Mitman 1992, p. 38; Cittadino 1993b).

Thus, synecology provided one impetus for the preservation of nature's primitive conditions.[15] This reason, to be more specific, is natural historical in character. Ecologists were interested in collecting, cataloguing, and comparing the various natural communities of organisms throughout North America to understand the order of the natural world.[16] As the landscape was claimed by humans, these natural communities were disappearing. Preservation was required to save or regain these objects before they disappeared entirely. As Shelford straight-forwardly put it in the committee's dense, 761-page magnum opus, *Naturalist's Guide to the Americas*

15. Synecology became so important that by the mid-1930s it was sometimes used synonymously with ecology, see Clements and Shelford (1939, p. 2).

16. On the history of natural history and its methods, see Jardine *et al.* (1996) and Farber (2000).

(1926), "A branch of biological science which obtains its inspiration in the natural order in original habitats must depend upon the preservation of natural areas for the solution of many problems" (Shelford 1926, p. 3).[17]

But nature's primitive conditions were more than just important, vanishing objects for collection in this natural historical sense; they were epistemologically foundational objects, requisites for the science of ecology. To appreciate why, one has to realize that most of the attention in synecology at this time was devoted to the problem of ecological succession. As Greg Mitman has argued, the theory of succession, developed at the University of Chicago by Charles M. Child and Henry C. Cowles, incubated in a context which emphasized progressive, goal-directed, and orderly processes (Mitman 1992, p. 31).[18] This was true of many burgeoning biological sciences, like developmental biology and evolutionary biology, and it was true of the early theories of community ecology, including succession theory.

Succession begins with barren ground following some disturbance, like a forest fire. As new species arrive a sequence of stages of community composition unfold until the mature "climax" condition is arrived at. As Shelford wrote, "Cowles found that in the Lake Michigan sand area cottonwoods precede pines, pines precede black oaks, black oaks precede red oaks, red oaks are usually followed by sugar maple and beech" (Shelford 1912, p. 60). This process was progressive and goal-directed, and early ecologists thought of it as analogous to the development of an organism: from unfertilized egg, through a number of differently organized stages, ending at the adult individual. Shelford and Adams would lead the way in applying this approach to animal communities. Development "is the basic process of ecology," Shelford later wrote, "as applicable to the habitat and community as to the individual and species" (Clements and Shelford 1939, p. 3). The metaphor of development ran deep.

The rise of succession theory can be partly explained by the promise it offered: it gave ecologists a chance to make their science more than simply

17. There was also a sense among these ecologists that other biological sciences had falsely underestimated the problem of vanishing primitive conditions. Shelford, for his part, thought that evolutionary theory had "retarded the progress of biology" because it turned biologists' attention largely to types of work that could be done in museums and laboratories (Shelford 1926, p. 3).

18. Greg Mitman has argued that these ideas were underwritten by a Spencerian philosophical position commonly held by many at Chicago (Mitman 1992). Adams, for example, later wrote that "The physical world is undergoing constant change, living organisms are likewise undergoing similar changes, and the constant interaction between these major series of processes, as Herbert Spenser long ago pointed out, constitutes the essence of the living organism" (Adams 1951, p. 39).

about descriptive patterns and instead about dynamic, ordered processes (Cittadino 1993b). In an influential paper, Cowles, a graduate teacher of both Shelford and Adams, put the point as follows: the ecologist "must study the order of succession of the plant societies in the development of a region, and he must discover the laws which govern the panoramic changes. Ecology, therefore, is a study in dynamics" (Cowles 1899, p. 95). Eighteen years later, Adams called this "dynamic" science "the new natural history," to set it apart from the static, natural historical approach that dominated the previous century, and that I referred to above (Adams 1917, p. 493). He authoritatively cited the nineteenth-century philosopher of scientific method, John Stuart Mill, in this regard: "If the signs of the times are not read correctly, the most striking advance in scientific methods of thinking during the century will be in the direction of interpretation from the standpoint of processes—dynamically" (Adams 1913, p. 82). "Modern ecology," Shelford wrote in agreement, could finally answer the "epithet of a famous contemporary zoologist who closed a discussion of habitats with the words 'developing hodge podge'" (Shelford 1926, p. 3). With the discovery of "orderly sequences," ecological knowledge could "be organized into [a] science," with ecologists working to uncover the "laws of orderly sequence" of community development (Shelford 1926, p. 3; Adams 1913, p. 92). Once these laws were discovered, they could then be used to inform policy decisions and make sensible choices about nature's management (Grinnell and Storer 1916; Sumner 1920; Adams 1925; Moore 1925; Shelford 1926; Adams 1929; Shelford 1933).

Succession theory, with its guiding metaphor of orderly development, had a perhaps unintended consequence: it encouraged, possibly even justified, the treatment of humans as disturbances to the normal sequence of ecological development. And it was mobilized to establish nature's primitive conditions as epistemologically and methodologically foundational, and thus their preservation and study essential to ecology.

5. Much Ado about Humans

In his introductory ecology text, Adams provides an articulation of the idea that humans are disturbances within the framework of the theory of succession (Adams 1913; Adams 1929; Pritchard 1999). Like many ecologists at the time, he compared ecology to physiology—ecological communities to organisms—and argued that the study of human-disturbed environments was like the study of pathological organisms (Haraway 1976; McIntosh 1998). He wrote,

> Some appear to think that an interest in such original conditions is of no particular scientific value [...] But if we come to consider that the

> original primeval conditions give us our best conception of the normal processes of nature and are comparable to the normal health of an organism, it puts the subject in another light. [...]
>
> To study disturbed, artificial, and "pathological" conditions, without an adequate knowledge of the normal and original conditions of both the organisms and the environment, is an attempt to interpret the abnormal and artificial in terms of itself, rather than in terms of the normal. (Adams 1913, pp. 26–8)

He concluded that ecology needed a prior understanding of nature's normal development, what he called a "bionomic base line," a phrase Adams borrowed from another ecologist, Orator Fuller Cook (Adams 1913, p. 30). The bionomic base line would provide "an idea of the conditions which existed before man came upon the scene, and the conditions which would again supervene if the human inhabitants were withdrawn" (Adams 1913, p. 30).[19] This reasoning depends on, first, the intelligibility of the distinction between "normal" states of nature and "pathological" states of nature, second, that humans are a cause of ecological pathology, third, that no sound inferences can be drawn from "pathological" conditions to "normal" conditions, and fourth, that the normal is epistemologically prior to the pathological.

Adams' reasoning is a variant of a style of scientific reasoning that Elliott Sober has called the "natural state model" (Sober 1980). According to this style, a distinction is drawn between natural states of nature (or, more specifically, natural states of kinds of objects) and unnatural states of nature; the latter being produced by subjecting a natural state of nature to a disturbing force. Natural, in this context, means functionally normal: the natural state may not be common, that is, may not be statistically normal, but is functionally normal in the normative sense that it is in a state of proper, correct, or healthy functioning.[20] It is, in other words, not pathological. Scientific investigation proceeds by first determining and describing the natural state, and then enumerating the disturbances that interfere with the expression of this state. The explanatory payoff of this model is that all variation—in this case variation in the development of ecological communities—can be explained as the result of deviation from a natural

19. Shelford seemed to agree, citing ecologist and Committee member Stephen Forbes: "There is a general consent that primeval nature, as in the uninhabited forest or the untilled plain, presents a settled harmony of interaction among organic groups which is in strong contrast with the many serious maladjustments of plants and animals found in countries occupied by man" (Shelford 1913, p. 17).

20. See Wachbroit 1994, Siipi 2008, and Dussault 2016 for a discussion of types of normality and their connection with naturalness.

state by a disturbing force. Sober provides an example of this style of reasoning from Aristotle's *Generation of Animals*:

> According to Aristotle's theory of sexual reproduction, the male semen provides a set of instructions which dictates how the female matter is to be shaped into an organism. Interference may arise when the form fails to completely master the matter. This may happen, for example, when one or both parents are abnormal, or when the parents are from different species, or when there is trauma during fetal development. Such interferences are anything but rare, according to Aristotle. Mules—sterile hybrids—count as deviations from the natural state. (Sober 1980, pp. 361–62)

Committee members considered nature's "primitive conditions"—nature's un-human-disturbed conditions—to be a natural state in Sober's sense, and this grounded and organized their research. As the natural state, nature's "primitive conditions" were considered both epistemologically and methodologically foundational. Epistemologically, they defined what counted as functionally normal and thus what counted as deviation from normality; they provided a "bionomic base line" against which pathological states of nature could be interpreted and ultimately healed. And methodologically, they were the proper investigative starting point: ecology begins, first, by determining the laws governing the natural state, and then proceeds to understand how unnatural states are created through disturbances. To do otherwise, as Adams puts it, would be to erroneously attempt "to interpret the abnormal and artificial in terms of itself, rather than in terms of the normal" (Adams 1913, pp. 26–8). What is significant is that on this account of the theory of succession and how it is to be studied, humans come out as disturbing forces—given that they are by definition not part of nature's primitive conditions—and non-human nature comes out as the place where the normal processes of nature can be discovered. The easy slippage in Adams' quotation between "artificial" and "pathological" is one symptom of this thinking.[21]

In this regard, it should be noted that there is no necessary connection in Sober's model between "natural" meaning "functionally normal" and "natural" meaning "nonhuman." By treating nature's primitive conditions as the natural state, these ecologists were in essence collapsing two conceptually

21. What at first may seem paradoxical is that Adams was an early proponent of urban biology (Adams 1951). It is clear from his discussion of unhealthy conditions in urban environments that this doesn't go against the belief that nature's primitive conditions are epistemologically foundational.

independent senses of unnatural: pathological and artificial. The functionally normal state, or normal developmental sequence, did not as a matter of definition involve humans. In other words, being artificial—being human-disturbed—was simply one way of being functionally abnormal. I will return to this in the next section.

These ecologists employed a distinction between primary and secondary (also "man-made") communities to distinguish between nature's "primitive conditions" and human-disturbed nature. Primary communities housed nature's "primitive conditions." Secondary communities—to which ecologists were often forced to resort—were those that had been disturbed, and could be grouped in the order of their degree of difference from primary communities. Communities of roadside were less disturbed than those of pastures, pastures were less disturbed than farmlands, farmlands were less disturbed than orchards, orchards were less disturbed than "communities of buildings," and so on (Shelford 1913, p. 16). This established a continuum of more-to-less epistemologically appropriate research objects. An important philosophical point is worth making here. As Steven Vogel has argued, recasting the human-nature binary as a continuum "doesn't render it less dualistic, it just extends the dualism along an axis whose poles [...] remain fundamentally opposed to each other" (Vogel 2011, p. 96). Naturalness, in this case the normal sequence of development, is simply measured along an axis whose negative pole is human domestication. Domestication is a disturbance, it's just that some humans are less domesticating.

But how much interference or domestication was too much? This question introduces a level of racialized complexity and tension in the committee's analyses, as they tried to account for degrees of human interference. For example, Shelford's wife, Mabel Brown Shelford, contributed a section to his *Animal Communities in Temperate America* (1913), which reads, "When the white man first appeared near Chicago no secondary community existed, as the aborigines lived almost entirely by hunting and fishing. They cultivated the land only a little, and are accordingly to be ranked with the larger animals as a part of the original communities" (1913, p. 13). And Shelford himself later wrote that although "ecology has advanced beyond the simple distinction of the natural and the artificial," there is still "an important difference in the reactions and coactions exerted by man at various culture levels" (Clements and Shelford 1939, p. 24). "In pastoral areas," for example, "man perhaps is still to be reckoned as a constituent of the biome" (Clements and Shelford 1939, p. 24).

When these ecologists referred to "civilized" humans in studies of succession, they were often directly referred to as disturbing factors. Cowles believed, according to Shelford, that the superior climax community of the

Indiana dunes region—the beech-maple forest—had failed to actualize because of human influence: "man eliminated," the forest would "occupy the entire territory east of the Mississippi and Illinois rivers" (Cittadino 1993b, p. 551). In this case, the normal state of the area, the beech-maple forest, was displaced by a disturbing force: humans. Frederic Clements, a plant ecologist not on the Committee but whose influential work on succession was well-known, argued more forcefully that humans and their co-domesticates, notably sheep, gave rise to successions resulting in "disclimax" (Clements 1936, p. 265).[22] "[T]he significance of this term," wrote Clements, "is indicated by a prefix, *dis*-, denoting separation, unlikeness or derogation, much as in the Greek *sys*, poor, bad" (Clements 1936, p. 265). Disturbance "by man or domesticated animals" resulted in the "modification or replacement of the true climax" (Clements 1936, p. 265).[23]

In 1939, when Clements and Shelford co-wrote *Bio-ecology*, their opening remarks explain how they hoped humans would one day properly be synthesized into the study of succession. But the status of humans as disturbances that give rise to disclimaxes was not called into question throughout their analysis, and one wonders in the end whether this ecological study of humans would simply be the study of disturbances. They worried about which humans were a part of nature and about how domesticated was too domesticated, but the status of humans as disturbances was taken for granted.

The style of scientific reasoning advocated and articulated by these ecologists privileged nature's primitive conditions—essentially non-human nature—as the normal state, against which disturbances could be defined and enumerated. This seems to me to be less a matter of "axiomatic misanthropy," as historian Matthew Chew has argued, and more the valorization of nature's primitive conditions: these ecologists weren't arguing directly that we shouldn't study "artificial" environments, but that the study of nature's primitive conditions was the proper starting point (Chew 2009). This was an epistemological valorization: the motivation behind focusing on non-human nature was that it reveals the functionally normal (non-pathological) sequence of community development. And this was in turn important because it held open the prospect of creating an ecological science about dynamic ordered, predictable processes—perhaps laws—rather than simply descriptive patterns. Variation in the development of ecological communities across locations might be explained, rather than

22. Shelford referred to Clements' succession theory as "representing the essence of modern synecology" (Shelford 1926, p. 58).

23. Thanks to Matt Chew for this reference. See also, Chew 2009 and 2011.

simply described, as the result of deviation from the natural state caused by a disturbing force, such as humans.

6. Whence Normal?

While the reasons motivating the search for a natural state (or natural states) were largely epistemic, the decision to treat nature's primitive conditions as the natural state—as the functionally normal state—was underwritten by a constellation of non-epistemic factors. The decision to treat these conditions as normal was a normative judgment, and so we should ask, whence the normal?

This question can be answered at varying degrees of fineness. Since these points have been raised elsewhere by historians of ecology and the environment, my exegesis will be brief. At the coarsest level, the assumption that humans were disturbances was widely held among many at the time, as mentioned in Section 2. The massive industrialization and closing of the frontier in the nineteenth century and urbanization in the twentieth century, caused many to worry about the degenerating effects these trends would have on humans and the natural world. This degeneration touched on all aspects of human life: economic, in the sense that wild land was more productive; moral, in that wilderness inculcated positive values; spiritual, in that wilderness had the power to provide religious experience; aesthetic, in that it could provide inspiration for literature, poetry (the committee frequently cited poems by the American romantic William Cullen Bryant), and landscape painting (in the style of Albert Bierstadt). Primitive nature was broadly taken to hold the antidote to the malaise of urban life. Statements made by members of the committee—for example, that nature was a remedy to "restore to the human organs the normal balance which special or artificial conditions of life disturb" (Grinnell and Storer 1916, p. 375)—show that ecologists were, to varying degrees, sympathetic to these wider cultural sentiments. As historian Greg Mitman has said, these ecologists "did not want to heal nature as much as be healed by it" (Mitman 1992, p. 8).

At a finer grain of analysis, many recent historians have drawn attention to how the experiences of urban and agricultural life of specific ecologists influenced their views of humans as disturbances. Historians Sharon Kingsland and Donald Worster have both shown the influence of environmental and economic problems in the Midwest on the ecology of Clements (Worster 1994; Kingsland 2005). Kingsland has argued that, "he was deeply impressed by the degeneration of the land and what this impoverishment implied for human society" (2005, p. 148). Clements, she continues, "connected the breakdown of the destruction of [prairie land] to moral degeneracy and the breakdown of society" (2005, p. 148).

Many ecologists at this time had similar experiences and shared similar perspectives.

The influence of an important American philosophical tradition is also relevant here. The committee's understanding of the relationship of humans to nature which placed humans at odds with natural processes was also widely-known to these ecologists through the American Transcendentalism of Henry David Thoreau, Ralph Waldo Emerson, and John Muir.[24] For these philosophers, nature's primitive conditions were important not just for narrow human ends, like recreation, but served "higher purposes": reverence for God's work, Nature. As one member of the committee put it, preserving nature offers "a source of health and recreation which leads one's thoughts away from the mundane affairs of this world 'Through Nature up to Nature's God'" (Shelford 1921, p. 13). Domesticated or civilized humans were set apart from nature, as living artificial, and thus morally, spiritually, and physiologically, inferior lives.

As environmental historians have noted, Muir had a considerable influence in early twentieth-century America. And we can see his ideas within the committee by considering his discussions of sheep and social Darwinism. Muir had first-hand experience as a sheepherder in California and he didn't care for them much. Following Darwin, he argued that natural selection was similar in form, but superior in product, to artificial selection or domestication (Worster 2008, p. 288).[25] This was particularly apparent in the case of sheep; the domestic sheep was not only harmful to the natural environment—he called them "hoofed locusts"—but also aesthetically "expressionless, like a round bundle of something only half alive" (Nash 1967, p. 130; Muir 1874, p. 359).[26] And as the ending line to one of Muir's articles indicates, he had more than just domestic sheep in mind: "A little pure wildness is the one great present want, both of men and sheep" (Muir 1875, p. 366). The committee held and reinforced Muir's negative assessments of sheep; their first grazing advisor, Joseph

24. These ecologists also knew Thoreau as one of the first to study ecological succession—though there were obviously significant differences between Thoreau's and their approaches (Thoreau 1887).

25. I should say, Muir considered this analogous to Darwin's position. It actually seems more intellectually connected to the very disparaging remarks made about domestic species by Darwin's co-discoverer, Alfred Russel Wallace. See, Darwin and Wallace (1858, p. 60). See Lennox (2000) for a discussion of the intellectual relationship, or lack thereof, between Charles Darwin and John Muir.

26. This assessment was connected to the sheep's domesticity. Wild sheep, in contrast, were a part of nature: "the wild is elegant as a deer, and every muscle grows with life. The tame is timid; the wild is bold. The tame is ruffled and soiled; the wild is trim and clean as the flowers of its pasture" (Muir 1874, p. 359).

Grinnell, summarized the "disastrous results following upon close sheeping" (Shelford et al. 1921, p. 14).[27]

Of Muir, Adams wrote fondly, deeming him the original preservationist: "The older champions of our national parks, as John Muir, were among the leaders in this country to see in a broad way the value of preserving wild areas, but in recent years there has been an intense movement to get vast crowds of people in the national parks, and at such a rate that vast areas of the parks are without question being severely injured" (Adams 1925, p. 562). Adams saw Muir as America's "first and greatest champion" of nature study, "who exemplified the benefits derived from the appreciation of the wilderness. He was a naturalist, an artist, and from the wilderness he derived science, art, education, recreation" (Adams 1929, p. 40). One member of the Committee, William S. Cooper, considered abandoning his scientific research career to become a nature writer in the style of Muir; and even after reconsidering, his decision to spend years of field research in Glacier Bay, Alaska, was influenced by reading Muir's *Travels in Alaska* (1912). With Cooper's help Adams notably built his presidency of the ESA on protecting Glacier Bay as a national monument (Lawrence 1980; Barrow 2009, p. 214; Rumore 2009).

Thus, the belief in the functional superiority of nature's primitive conditions—a normative judgment underwriting the normality of these conditions—was influenced by a host of historically-specific assumptions about domesticated humans and their ill effects on nature. As I've said above, this involved the collapsing of the categories of pathological and artificial, two conceptually independent senses of unnatural. Being artificial was one way of being pathological.

7. Implications for Experimental Method

Nature's primitive conditions were treated as foundational, but some saw further implications, not just for what we should study, but how we should study it. At this time in the early-twentieth century, the growth in status and scale of academic and privately-funded laboratory biology created a context ripe for disputes about the advantages and disadvantages of different experimental methods (Allen 1979; Rainger et al. 1988; Kingsland 1991; Kohler 2002).[28] The committee's ideas about nature's primitive conditions often meant that they emphasized field over laboratory study,

27. Apparently Grinnell met Muir as a boy, when the latter came to view the boy's impressive collection of bird skins (Grinnell 1940, p. 5).

28. See Pauly (1987), Rainger, Benson, and Maienschein (1988), and Benson, Maienschein, and Rainger (1991). See Gooday (1991) on the rise of the laboratory in the late 19th-century. See also Kohler 2002.

at least for synecology. Unsurprisingly, the methods they recommended were often non-interventionist, like "natural experiments."[29] In a natural experiment, one studies the results of an experimental perturbation, but the perturbation is natural—e.g., a forest fire or hurricane—rather than induced by the experimenter. Adams' own example is extreme. What he called "nature's vast experiments," for example, were by-definition impossible to discover on areas of human-disturbed land, since these would cease to be nature's experiments at all (Adams 1913, p. 8). These experiments, he thought, avoided the problem that human manipulation spoils the object of study, rendering nature pathological. Other members held similar positions, perhaps not quite as extreme. Victor Shelford, an advocate of mixed-experimental approaches influentially defended in his *Laboratory and Field Ecology* (1929), argued that human experiments could be conducted in "buffer areas," but that Nature's experiments, found in the "nature sanctuary," were more epistemologically valuable because they allowed one to grasp nature in the absence of such disturbances (see Ilerbaig 1999).

At the time laboratory biologists also rhetorically drew on the artificial-natural distinction, but to opposite effect. For example, the physiological plant ecologist of Johns Hopkins University, Burton Livingston, tactfully argued that the "older reverence for natural or 'normal' phenomena has largely disappeared" (Livingston 1917, p. 9). "We have learned that the range of conditions offered by nature," he continued, "does not generally happen to be great enough to allow adequate experimental interpretation of plant processes." "[I]f a student has not a liking and talent for creating physical and chemical conditions such as never have occurred in nature, he should not cast his lot with plant physiologists, for the next generation" (Livingston 1917, p. 10). "Where would the chemist be," he asked rhetorically, "if he were constrained to study his salts always as they occur in nature?" (Livingston 1917, p. 10). Livingstone is here exploiting the same cultural rhetoric as committee ecologists, but for a different purpose.

Discussions of experimental methodology, such as these, also introduce another, more practical, epistemic reason why nature's primitive conditions were judged to be the natural state. They were also judged to be

29. Natural experiments emerged as a contentious theme in these disputes, and were advocated by biologists as diverse as E. B. Wilson, Frederic Clements, Charles C. Adams, Victor Shelford, Joseph Grinnell and Francis Sumner, many of whom also served on the Committee for the Preservation of Natural Conditions (Clements 1905, pp. 149, 306). See also Adams (1915, p. 8), Sumner (1915, p. 696), Grinnell (1919, p. 472), Clements (1934, pp. 41–2, 46), Shelford (1934, pp. 491–92), Nash (1967, p. 198ff), Kohler (2002, p. 216), Kingsland (2005, pp. 146–47), and Inkpen (2014).

superior on grounds of experimental or cognitive tractability. Humans were considered not just pathological disturbances, but also unpredictable.

Barrington Moore—a forest ecologist, committee member, and fourth president of the ESA—provides an example of this reasoning. Moore argued, like Adams, that the more nature is "kept free from man's interference, the better is the area for scientific study" (Moore 1925, p. 49). Before we can "avoid the fatal consequences of running counter to nature's laws," he wrote, "we must know what those laws are; and before we can use nature's help in attaining our ends we must better understand nature's processes" (Moore 1925, p. 49). But the problem for Moore wasn't just that humans were pathological, it was their unpredictability. The processes of nature, Moore wrote, "are so delicately adjusted that when man interferes in one respect he sets up a chain of consequences the end of which no one can foresee" (Moore 1925, p. 49). Comparing the situation to the popular British nursery rhyme, he wrote, it's "like 'the house that Jack built,' only the consequences may be almost endless" (Moore 1925, p. 49). This required, he reasoned, "both searching laboratory tests and thorough field studies in areas on which nature has been undisturbed" (Moore 1925, p. 50). Again, nature's "primitive conditions" are considered epistemologically foundational, but for the methodological reason that humans are unpredictable and so are the results of their actions. Their actions are, in other words, not orderly, not law-like, enough.

8. Two Integrative Examples

So far this discussion has treated the committee as a whole and as intellectually homogeneous. This is obviously an oversimplification. Ecologists at this time held the above positions to varying degrees and were influenced by events particular to their own lives. Through two brief sketches—of Francis Sumner and Joseph Grinnell—I would like now to show how these themes were instantiated in the thinking of particular individuals, and ones that were part of the committee, but not synecologists of the Chicago school, like Adams and Shelford. This latter point is important: although I believe that it was within synecology and its central organizing theory—succession—that this rationale was articulated, its influence was broad. In other words, the epistemological centrality of nature's primitive conditions permeated outwards, transgressing into other styles of ecology less directly committed to the theory of succession. Grinnell, trained as a taxonomist, advocated what he called "dynamic zoogeography," which focused on the comparative study of geographical ranges of animals (Ilerbaig 2009). Sumner, trained in laboratory physiology, advocated an evolutionary approach combining genetics and systematics of mice populations. Both nonetheless argued for the epistemic importance of nature's primitive

conditions as a natural state, and both grounded this judgment in a mix of epistemic and non-epistemic reasons.

Sumner studied as a graduate student under the famed cell biologist E. B. Wilson at Columbia. After a short stint at a College in New York, Sumner began his research career working in the laboratory of the Bureau of Fisheries measuring the response of fish to extreme environments (Provine 1979; Kohler 2002). After a number of years of frustrating laboratory work, Sumner turned to biogeographical field studies of the deer mouse: "I have had enough to do with the experimental method in zoology to make me realize its rigid limitations," he wrote, "I am therefore disposed to attach considerable importance to what have been called 'Nature's experiments'," the method championed by Adams (Sumner 1915, p. 696).

Sumner was well-suited for the ESA's Committee and fieldwork, given his love of "natural areas" and his explicit stance against urbanization. The "love of nature," he wrote, "includes vastly more than the appreciation of natural scenery. It includes that deep-rooted feeling of revolt [...] against the noise and distraction, the artificiality and sordidness, the contracted horizon and stifled individuality, which are dominating features of life in a great city" (Sumner 1920, p. 238). Sumner had first-hand experience of urban life: after beginning his career unhappily teaching in New York City, he moved to the Scripps Research Institute in La Jolla, California, only to witness the urbanization of the San Diego area. Of urbanization, he wrote, "If this is the real trend of human evolution, we who represent the 'unfit' type, may well pray for a speedy extermination" (Sumner 1920, p. 239).[30]

Sumner's views about preserving natural conditions, and about what counted as "natural conditions," were closely connected to his views about proper biological practice. He thought laboratory biologists could not understand nature, if they did not take what happened in nature seriously. They were frequently happy to study domestication products, but the problems of ecology, genetics and evolution, he wrote, "are not all to be solved by rearing pedigree-cultures of the fruit-fly and evening-primrose. We must study the actual products of evolution as they have arisen in nature" (Sumner 1921, p. 11). In fact, he even doubted the results of one of his own experiments simply because of the perceived "pathological character" of the lab-raised mice involved (Sumner 1924, p. 504). He preferred

30. Sumner's views didn't change much throughout his life. He wrote in his autobiography, published the year he died, "One of the glorious features of the desert landscape is the relative scarcity of that hopelessly unesthetic creature, man. But, even here, he has begun to stream in over a maze of newly constructed automobile roads … Alas, poor Desert!" (Sumner 1945, p. 223). This occurred in a chapter titled "Man versus Nature."

to study "nature's experiments," in which she had subjected the same species of mice to different environmental conditions, along the California coastline, for long periods of time.

Furthermore, for Sumner, it wasn't simply that laboratory biologists were scientifically misguided; they were also inclined to promote—or at least disinclined to speak against—what he considered to be undesirable environmental policies. Since they could not appreciate nature, they didn't understand the need to protect it; something he thought that biologists, of all scientists, should grasp. As such, he believed them to be failing in their responsibilities as biologists. "That both our native fauna and flora and our natural scenery are disappearing at an appalling rate is obvious to all," he wrote, "except those whose interests and outlook are bounded by the walls of their laboratories" (Sumner 1921, p. 39).

This same interconnected triad of method, nature's primitive conditions, and preservation, can also be found in Joseph Grinnell's writings. Unlike Sumner's laboratory beginnings, Grinnell had been an enthusiastic naturalist from a young age (Hall 1939; Grinnell 1940; Runte 1990). By eighteen he had already gained a reputation as an able ornithologist, contributing a section on birds to the *History of Pasadena* (1895)—the author of which wrote, "Young Joseph Grinnell [...] has won the reputation of having captured, preserved, labeled and classified more specimens of our native birds than any other person" (Grinnell 1940, p. 4). Like Sumner, Grinnell drew attention to the dangers of modern city life, and to the need for preserved lands in combatting these dangers. Life "in the cities," he wrote, "is so far removed from the surroundings of preceding human generations that it is conducive to serious nervous and mental fatigue" (Grinnell and Linsdale 1936, p. 152).

Nature, for Grinnell, is the antithesis and remedy for the artificial. Natural "processes are capable of maintaining an area with all the desirable qualities just to the extent they are allowed to do so by not interfering with them," he wrote, "Artificial help is not required" (Grinnell 1940, p. 16). The "existence of any artificial change," he concluded, "is the basis for harm," since it is a hindrance to the expression of nature's primitive conditions (Grinnell and Linsdale 1936, p. 153).

When it came to nature's primitive conditions as a research object, Grinnell drew on the close connection between "natural" and "normal"—and pathological-artificial—that I've discussed above. Nature, the antithesis of artificial, is also the proper object of ecological study. When discussing a study of the responses of white-crowned sparrows to environmental changes, Grinnell advocated for the necessity of making such observations under natural conditions in a nature preserve. As visitors to the Point Lobos reserve, sparrows are not interesting in and of themselves, "for

this is a common bird in our backyards at home" (Grinnell and Linsdale 1936, p. 153). They are interesting in relation to the natural environment. Grinnell was "intensely interested in seeing the normal responses of this species of bird to its natural environment" (Grinnell and Linsdale 1936, p. 153). "If the area is allowed to make its own changes these responses will be natural," Grinnell continued, "but with human influence it becomes another, artificial backyard" (Grinnell and Linsdale 1936, p. 153).

And, finally, Grinnell's thoughts about what nature is like and why it should be preserved support his scientific methodology. An advocate of natural experiments, he argued that objections to their use based on their perceived lack of rigor—that "'factors' of the environment are not sorted out"—were based on false characterizations of the experimental alternatives (Grinnell 1919, p. 472). In "the breeding cage," he argued, "there are always 'unknown' factors; so let us admit the existence of those in the wild as not invalidating the 'experiment'" (Grinnell 1919, p. 472). The advantage, he argued, was that the natural experiment provided an indication of an organism's behavior under relatively undisturbed—i.e., healthy—natural conditions, where they can, as he later said, "play their normal rôle in nature" (Grinnell and Linsdale 1936, p. 153).

For Sumner, Grinnell, and other members of the preservation for science movement, the scientific rationale they were creating, one that treats nature's primitive conditions as epistemologically and methodologically foundational, was inseparable from a number of wider, historically-specific trends. Although the rationale was articulated clearly through the theory of succession, and through the metaphor of normal development, it had a much further reaching influence. The point is not to call into question the importance of the intellectual contributions made by each of these ecologists. The point is that they both adhered to a model of science that treated non-human states of nature as functionally normal and foundational. Given the methodological, philosophical, and political diversity that many historians have shown to exist among these ecologists, one wonders whether this belief in the epistemic value of nature's primitive conditions is one of the strongest threads pulling them together.

9. Conclusion

The committee disbanded in 1945. Certainly one important factor was internal tension within the ESA (Kinchy 2006). Shelford, as he had argued from 1917 on, wanted the committee, acting on behalf of the ESA, to lobby for the preservation of particular areas. In the years leading up to 1945, the Society's council members increasingly came to believe that lobbying efforts and direct political action might jeopardize the "objective" status of the Society and the science of ecology. The committee was disbanded,

and Shelford and a number of other ecologists formed the Ecologists Union in 1946—which eventually reorganized as The Nature Conservancy in 1950—to continue such pursuits outside the auspices of the ESA. Aside from internal political tension, there were also methodological factors. Post-war ecology changed directions: short-term laboratory studies were favored over long-term field studies of succession, and community ecology came into competition with other schools of ecology, such as ecosystem ecology, population ecology, and systems ecology (Palladino 1991; Rumore 2012). As important as these reasons are for why the committee disbanded, for my purposes, the absence of a particular reason is more important: the disbanding was not the result of a general belief that field ecologists should question the epistemological centrality of nature's primitive conditions and start considering the role of humans in their ecological studies as anything other than pathological disturbances.

Let me end with a brief summary and two reflections. My aim has been to reconstruct the scientific rationale advocated by the Committee for the Preservation of Natural Conditions and to unearth the interests and assumptions, both epistemic and non-epistemic, that fostered it. I argued that their rationale was a variant of the natural state model of scientific reasoning. These ecologists considered nature's "primitive conditions" to be a natural state, and this grounded and organized their research: these conditions were both epistemologically and methodologically foundational. Epistemologically, they defined what counted as functionally normal and thus what counted as deviation from normality. And methodologically, they were the proper investigative starting point. Treating nature's primitive conditions as the natural state involved a collapse of two conceptually independent ways of being unnatural: artificial and pathological. More accurately, we might say that these ecologists subsumed the artificial under the pathological. While the reasons motivating the search for a natural state may have been largely epistemic—they wanted to build an ecological science that was explanatory and grounded in laws—their decision to treat nature's primitive conditions as the natural state—as the functionally normal state—was underwritten by a constellation of epistemic and non-epistemic factors (an example of the former being that humans were unpredictable, an example of the latter that human influence was, perhaps by definition, pathological). As these ecologists demarcated natural from artificial—normal from pathological—they were at the same time, both chronologically and methodologically, defining ecology as a science in the process. They were not the only ecologists writing in the early-twentieth century, and their vision of ecology was not shared by all. But their influence on field ecology is still felt today because their idea that "natural" states of nature are epistemologically and methodologically foundational persists and still

informs the choice of research sites. As Adams suggested, "The natural starting point [pun intended?] seems to be in as nearly natural normal environments and associations as possible" (Adams 1913, p. 34).

Now for two reflections. First, discussions of the vagaries of the concept "natural" mostly focus on bioethics, environmental policy, or environmental ethics (Siipi 2008; Vogel 2015). For example, the question of whether we have an obligation to preserve what is natural. I think this essay demonstrates that there is another dimension. In biology at least, concepts of "natural," and the artificial-natural distinction, are entwined in answers to basic research questions. How we decide what counts as worth studying, and in the end what we know, is influenced by how the concept of "natural" is demarcated.

Second, I've attempted to show how this rationale was a product of its time: it should be historically-situated within the interests and assumptions of a particular set of early-twentieth-century ecologists. This historical situating certainly emphasizes the contingency of their perspective. But it does more than that. Unearthing the assumptions and interests that fostered this rationale leads to the question of its value: if we find that the assumptions are no longer tenable, or that a science of ecology without this rationale would fit better with our current interests and assumptions, then we can legitimately question the current value of this style of scientific reasoning. Perhaps the best way to put this is as a series of questions: do we believe that humans are often or typically pathological? That ecological communities have functionally "normal" developmental sequences, analogous to the development of an organism? That domestication and urbanization often or typically lead to degeneration? That "ecological laws" can only, or more safely, be discovered in a category of geographical places deemed "natural"? Do we take for granted the intelligibility of finding nature's primitive conditions? For one thing, we have come to recognize that nature is constantly in flux in the absence of human interference (Hobbs et al. 2006). For another, we now recognize that humans were substantially influencing North American landscapes long before the arrival of Europeans (Cronon 1983). If not by a version of the natural state model, how do current ecologists justify their bias in favor of the study of non-human nature? In other words, if many or all of the assumptions that supported this rationale have in fact fallen away, what is left in their place? I do not want to answer these questions, but use them to show that this sort of history can help us better approach our current predicament by helping us to understand the assumptions that led us to where we are.

Furthermore, we can also reflect on how the natural state model more generally fares in light of the recent shift in ecology towards treating

anthropogenic factors as legitimate objects of study. There seem to be two options. The first is to reject the natural state model altogether on the grounds that there is nothing that can be usefully called the natural state of an ecological system. For example, even those optimistic about the current usefulness of succession theory provide analyses that emphasize the context-sensitivity of ecological communities and the numerous, diverse drivers of ecological succession (Meiners 2015). These analyses seem to provide little justification in choosing a natural state for an ecological community or its development because they are not committed to one successional state being a better candidate for functional normality than others. The second option is to accept a version of the natural state model with two significant modifications: (a) acknowledge that, because of the points just made, the natural state must be relative and stipulative, and (b) keep the artificial and the pathological as conceptually distinct categories such that an ecological community can be human-disturbed without being considered functionally abnormal. This would mean developing an account of functional normality that is highly context-relative but in some sense objectively grounded. To do so would be to provide an account of ecological normality analogous to the account of medical normality developed by philosopher Georges Canguilhem (Canguilhem 1991; see Dussault 2016 for the outlines of such a project).

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