THE LEGEND OF ORDER AND CHAOS: COMMUNITIES AND EARLY COMMUNITY ECOLOGY

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> "We must admit that a *stand* of vegetation is a concrete entity." "Extent, boundary, uniformity: these are the *sine qua non* of every community." Henry A. Gleason (1936)

> "No serious student of succession (a process) has ever claimed that a succession is made up of 'discrete units.'" E. Lucy Braun (1958)

A community, for ecologists, is a unit for discussing collections of organisms. It refers to collections of populations, which consist (by definition) of individuals of a single species. This is straightforward. But communities are unusual kinds of objects, if they are objects at all. They are collections consisting of other diverse, scattered, partly-autonomous, dynamic entities (that is, animals, plants, and other organisms). They often lack obvious boundaries or stable memberships, as their constituent populations not only change but also move in and out of areas, and in and out of relationships with other populations. Communities are consequently interesting to philosophers interested in ontology — in what kinds of things exist — as unusual scientific objects. But others with interests in communities, including ecologists, conservationists, policy-makers, land-managers, environmental philosophers, and philosophers of science, have an interest in whether these unusual features make communities unreal. Familiar objects have identifiable boundaries, for example, and if communities do not, maybe they are not objects. Maybe they do not exist at all. The question this possibility suggests, of what criteria there might be for identifying communities, and for determining whether such communities exist at all, has long been discussed by ecologists. This essay addresses this question as it has recently been taken up by philosophers of science [Shrader-Frechette and McCoy, 1993; Shrader-Frechette and McCoy, 1994; Sterelny, 2006; Odenbaugh, 2007], by examining answers to it which appeared a century ago and which have framed the continuing discussion.

Plant ecologists struggled openly and vigorously through the early twentieth century with the definitions, and then with the legitimacy, of their basic units. Though this discussion continues, a conversation about the discipline's foundations prospered from the 1910s to 1950s with a rough continuity of participants

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and issues. As those decades advanced, plant ecologists paid increasing attention [page 50] to the roles of animals in ecological communities, but animal ecology developed largely independently (as described by Gregg Mitman, for instance [Mitman, 1988; Mitman, 1992]). Focusing on vegetation as the basis for defining ecological units even when animals were integrated, plant ecologists asked how portions of it can be demarcated from their surroundings for analysis. Current ecology generally calls distinguishable, multi-species groupings “communities,” though in the early twentieth century, depending on their scales, on the more-specific properties attributed to them, and on the preferences of individual scientists, they were variously called associations, societies, facies, and formations. Except where noted, I will use “community” in the general, contemporary sense which includes all these multi-species groupings.

Disregarding for now the particular connotations of these various kinds of groupings, whether any groupings exist at all in such a way that they can be substantively differentiated from their surroundings as communities is of interest for several projects in philosophy of science. First, working towards understanding the ecological usage of “community” parallels philosophical attempts to define “chance,” “time,” “species,” and “gene” in other sciences. That is, some philosophers have hoped to make a contribution to science itself by clarifying terms or pointing to their ambiguities. Examining communities (sometimes independently of the term “community”) contributes to science in so far as biology texts sometimes muddle this part of their field’s development, potentially confusing current discussion — see section 5, below. Second, some philosophers have used scientific fields’ terminology to assess them. Scientific realists in philosophy of science have linked the question of whether theoretical progress has occurred in a particular scientific field to the question of whether the entities postulated by its theories really exist [Leplin, 1984]. Ian Hacking worried, for instance, that because gravitational lenses postulated by theoretical astrophysics had never been observed, that field might therefore not be on the right track [Hacking, 1989]; a few years later, the predicted gravitational lenses were observed, and their existence bolstered confidence that astrophysics had made progress towards revealing nature. Whether or not entities’ status is the best standard for progress in ecology, it is useful to ask whether ecology meets it, and how the discipline has fared against it over the course of its development. Third, others have recommended that philosophy and science can work together to produce a richer understanding of nature than either one can generate independently, a project Peter Godfrey-Smith has recently called developing a “philosophy of nature” [Godfrey-Smith, 2009].

But also, beyond philosophy, policy-makers, land managers, and conservationists have an interest in whether communities of some sort can be reliably distinguished from their surroundings. And so, environmental philosophers consequently inherit the question of communities’ existence as they examine conservation’s and policy’s foundations, and environmental ethicists face it (whether they recognize it or not) as they consider what kinds of things humans have duties towards, or duties to preserve. If some areas of vegetation can persist autonomously as units [page 51] better than other groupings can, or are more stable or real or integrated than others, or consist of parts which are especially fragile when dissociated, they may be better candidates for protection than other areas. Casually speaking, they might be more ecologically-sound or ecologically-significant than areas of remnant vegetation in landscapes modified by humans. Conversely, if ecology *cannot* recognize any robust groupings larger than single-species populations — where "robust" means that they can be reliably identified on the basis of well-grounded criteria — attempts to protect them come to seem misguided.

Both epistemological and environmental projects have an interest in the status of ecological communities, but environmental philosophy has special reason for concern, in that if calling a group of organisms a community is akin to *arbitrarily* drawing a line around them on a map, a number of its projects are jeopardized. So, without entirely resolving it in this essay, I consider the recognition of and challenge to communities as they appeared in the early twentieth-century debate where they were first, influentially asserted. Then I assess the implications of that debate for our current philosophical discussion of ecology. Specifically, I discuss how two scientists' theories have come to frame a debate about communities widely reported by biology textbooks and histories, and reproduced by philosophers. I argue that accounts of this dispute — the Clements/Gleason debate — have inaccurately radicalized the views of the scientists whose names are attached to it. Worse, these fanciful, radicalized positions are untenable in themselves in a way that infects their enduring currency in debates, whether their authors are treated as allies or antagonists.

Philosophical discussions taking impoverished concepts as starting points risk being unproductive at best and muddled at worst, while enjoying a superficial patina of biological respectability. So, after unyoking the scientists' positions from the standard versions of them, I argue that a class of arguments against community-preservation is thereby undermined. Then, after stepping back to consider how and why inaccurate versions of scientific positions could have such longevity and cachet, I consider how their untenability affects current philosophy of ecology. I argue that, as an example, Jay Odenbaugh's recent appeal to Clements and Gleason in arguing for realism about communities is diminished by his problematic versions of them. Yet, observing the significant common ground between them clarifies the way forward.

1 CLEMENTS, GLEASON, AND PRESERVABILITY

Among episodes in the history of ecology that have drawn attention from beyond that discipline, the debate about plant community structure during the early and middle twentieth century has attracted a remarkably diverse and persistent congregation of commenters. Much of the credit for this goes to the episode itself, for its distinctive features. First, it was set off by what was arguably the first body of general theory in ecology, a theory of plant succession — of the development of vegetation through time — advanced by Frederic E. Clements and others. Second, this theory touched off especially vigorous criticism, even condemnation, immediately on publication. [page 52] The main justifications offered for its rejection did not arise over a long series of papers generating accumulating anomalies, but instead were already in print within a year of the theory's kernel publication, Clements's comprehensive 1916 volume *Plant Succession: An Analysis of the Development of Vegetation* [Clements, 1916]. By the following year, October 1917, ecologist Henry A. Gleason had published "The Structure and Development of the Plant Association," which introduced what would remain for some time the core objections to Clements's theory [Gleason, 1917]. Third, these criticisms, both Gleason's and others', have appeared to some to have produced one of ecology's most visible paradigm shifts, one close to, though not exactly fitting, the mold developed in Thomas Kuhn's *Structure of Scientific Revolutions* [Kuhn, 1962]. Fourth, this conspicuous shift of opinion away from Clements's theory has seemed to others less-attracted to Kuhn's flirtations with incommensurability and relativism to be a laudable instance of progress already appearing in the early decades of a young, diverse science with few unequivocal instances of theoretical progress. Accounts of it often serve the point: 'see how wrong ecology used to be, and how much it has since learned?' Fifth, and of most interest to this essay, the scientific debate (including the position eclipsed by opposition) set up terms for subsequent debates in community ecology, terms persisting here and there to the present. Though there certainly have been other motivations, these features especially have attracted commentators and retellings.

To situate the two ecologists who are the episode's protagonists: Clements and Gleason each performed his formative research in the Midwestern United States, near the historical boundary between eastern forests and Midwestern plains. Clements began to develop his ideas about plant ecology while studying at the University of Nebraska as both an undergraduate and graduate student under Charles E. Bessey, leader of the discipline-shaping Botanical Seminar. After earning his PhD in 1898, he remained on the faculty there until 1907, when he left to become the chairman of the Botany Department at the University of Minnesota. After completing his most influential theoretical work, he was employed by the Carnegie Institution, traveling and researching around the United States, until 1942. Gleason left the Midwest after undergraduate and masters work at the University of Illinois, to earn his PhD at Columbia University in New York in 1906, but then held faculty positions at Illinois and the University of Michigan until 1919. He finally returned to the East Coast to spend the rest of his career at the New York Botanical Garden studying plant taxonomy more than ecology. Histories of these figures and their scientific and cultural contexts have been produced from a wide variety of perspectives by both biologists [Phillips, 1931; McIntosh, 1975; Tobey, 1981; Hagen, 1988; Nicolson, 1990; Hagen, 1992; Worster, 1994; Barbour, 1995; Nicolson and McIntosh, 2002] and historians [Malin, 1947; Hagen, 1988; Hagen, 1992; Worster, 1994; Kingsland, 2005].

For philosophy of ecology, one noteworthy aspect of these ecologists' historical situation — and a theme well-developed in the works just mentioned — is that the context of the putative waning of Clementsian ecology and the ecological ascendance of Gleasonian ecology was the Dust Bowl disaster in the Great Plains of the American Midwest. [page 53] As crops on land recently converted from prairie to cultivation were obliterated by drought and wind in the early 1930s, it became easier to interpret vegetation as directed more by disturbances than by orderly processes. The variability of habitat stood out more vividly than its stability, in a way that cast doubt on ecology which seemed to presuppose long-term stability of habitats. The Dust Bowl context itself has been offered as at least as plausible an explanation of the shift of favor as the theories' predictive and explanatory strengths and weaknesses. Whatever the appropriate sociological explanation, understanding the changes in ecological science through this period requires realizing that this was not an episode of theory-change in a constant context. Circumstances Clements's theories were designed to help understand themselves shifted in a way which made the theory appear extraneous or false. Since the scientific concepts developed during this period retain a life in philosophy of science, we should consider how the concepts' scientific careers were influenced by context.[1] The ecological severity of the circumstances in which Clements's theory was partly abandoned by ecologists and the degree to which its central concepts have endured in the ecological literature[2] and in natural history together suggest that the theory did not wane in popularity solely on account of its wrongness *per se*. That external rather than strictly internal (or evidential) factors promoted its demise has contributed to its enduring relevance as something other than a discarded falsehood.

Also noteworthy in this historical situation of the two ecologists is that early in the debate, Gleason moved to an institution where his research became only indirectly ecological, so that the ascendance of Gleasonianism included only limited ascendance of Gleason as a professional ecologist. Moreover and at first glance surprisingly, Clements never published a response to his nominal antagonist. For both these reasons, to the extent there was a Clements–Gleason debate, it was only fractionally a debate between Clements and Gleason.

Yet as their positions have been taken to frame a debate, the positions have been offered as contraries, in the following way. Clements's theory has traditionally been tied to two related claims: (1) that vegetation develops in any given area in a way comparable to, or literally identical with, the development of an individual organism; and (2) that the development of vegetation in an area necessarily culminates in a particular type of vegetation, called that area's "climax," which is determined by its climate. Gleason and his ecological theory have correspondingly been associated with the rejection of these claims, and identified with an alternative he called the "Individualistic Concept of Ecology." The positive core of the [page 54]

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[1] One need not accept much of Kuhn's framework for scientific change, for instance, to agree with him that the acceptance and rejection of scientific theories can be and has been influenced by external circumstances. That influence has been accepted even by accounts of theory-change trying to defend its potential rationality much more than Kuhn did [Kitcher, 1993, for instance].

[2] To mention scattered well-cited examples of their endurance: identifying climax communities in intestinal fauna and ocean-floor cyanobacteria, and determining spider abundance at various forest-succession stages [Bultman *et al.*, 1982; Reid *et al.*, 2000; Hooper, 2004].

individualistic theory has usually been summarized as the view that individual plants disperse and establish themselves independently of others, so that plant communities are merely unstructured aggregates of independent plants. Such unstructured Gleasonian communities would have dynamics rather unlike those of the integrated collectives attributed to Clements which develop as units towards definable climaxes.

As I remarked, an attraction of this episode for commentators has been the tension between these two apparently-opposite theories. Their neat opposition supports a framework for a narrative of conflict and theory-replacement in early plant ecology. But even more beguiling have been their vivid images, their provocative similes and metaphors. Tossing up fodder for poetic imagination, the debate sets the idea of a collective social organism against the idea of disaggregated, dissociated individuals — images which evoked in the subsequent decades the antagonisms of communism and capitalism, totalitarianism and democracy. Through the Cold War period of the middle twentieth century, such similes resonated with popular ambitions and popular fears burgeoning beyond the scientific discussion [Mitman, 1995]. Within scientific ecology and at its boundaries, these images set up a pointed question about the nature of nature. They frame the possibilities for what can be an object of ecological inquiry. Further, the opposing icons of collectives of organisms and dissociated, free individuals crystallize opposing answers to the question of whether communities are structured, organized entities or whether they are randomly-assembled aggregates of individuals. They have provoked curiosity within science and beyond it about whether nature and the nominal objects of ecology are essentially functions of order or of chaos.

Yet, the possibility that nature at the ecological scale might be chaotic has serious practical implications. When conservationists, politicians, and ethicists aim to preserve communities or endorse preserving them, they assume that communities are more than arbitrarily-identified fictions. If ecology demonstrates that communities are mere fictions, a *modus tollens* inference is licensed, concluding that community-preservation efforts are futile or misguided. This inference depends on an underlying conditional claim, that:

> Communities can be preserved only if they are real, orderly entities and not chaotic (not, that is, unreal fictions imposed on real chaos).

At this level of generality, the conditional claim expresses a sensible view. A thing cannot be preserved as such if it does not exist. Nor can it be meaningfully preserved if it has arbitrary boundaries and negligible structure. In so far as communities are collections, in this case *some things* might be preserved, but not *something.* Imagine, as an image of a worst-case scenario, trying to preserve a liter of the sea *in situ.* Tracking the individual components, one finds they rapidly dissipate and mix with others; tracking the location, one finds that it rapidly changes as particles arrive and depart. In the absence of ecological communities, one could identify the organisms in an area and attempt to keep them there or within some dynamic limits. Or, one could avoid interfering with an area, come [page 55] what may. But those projects are not what those seeking to preserve communities normally understand themselves as doing. They take themselves to be preserving *something.* So, as it relates to conservation practice and advocacy, the claim is sensible at this level of generality.

Problems begin when this reasonable claim is made more specific by affixing it to scientific positions, to draw further conclusions. Arguments employing the claim have instantiated the positions it mentions with Clements's and Gleason's theories, so that the claim becomes:

> Communities can be preserved only if they are Clementsian, not Gleasonian.

Drawing inferences via this conditional requires that the ecologists' assertions of order and chaos deny each other — that their ecological theories have communities either existing in an orderly way or being fictions imposed on chaos, and that these claims negate one another, or are mutually exclusive. Read in a straightforward way, the concepts do negate one another: clearly-bounded functionally-structured superorganisms in which individuals are controlled by their systems are not unbounded, unstructured collections of causally-unrelated individuals. As Jay Odenbaugh suggests, some purposes may be served by engaging with these concepts in abstraction, whether or not they reflect the views of any scientists [Odenbaugh, 2007, 629]. I will argue for the inaccuracy of aligning these polar concepts with those scientists, but also that more is at stake than historical accuracy in determining the scientific legitimacy of these concepts.

What is at stake appears when we notice how the conditional claim employing the concepts is used to draw further conclusions. Donald Worster uses it to complain about contemporary ecology, lamenting its inadequacies for supporting conservation efforts [Worster, 1990]. When ecologists approach nature assuming it is Gleasonian, he worries, they fail to produce science which can support conservation.[3] J. Baird Callicott identifies "residual traces of the early twentieth-century Clementsian super-organism paradigm" in Aldo Leopold's defense of his land ethic, and finds a broader commitment to community stability in the environmental ethics tradition following him [Callicott, 1996, 358]. Aligning "the insidious challenge to nature conservation posed by poststructuralists," with a scientific challenge to communities, as revealed by Worster's "exposé" of a "deconstructive siege of nature," Callicott worries about the consequences for community-preservation if nature is "chaotic, changing unpredictably, and disturbance ('perturbation') by wind, flood, fire, pestilence, not freedom from disruption is nature's normal state" [353–355]. Leopold's 's commitment to community-preservation is undermined by normalizing disturbance, and Worster and Callicott are each alarmed by the threat this poses to conservation. Then, with opposite sympathies, Allan Fitzsimmons argues against the existence of ecosystems in a broad sense incor-

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[3] Worster is actually concerned, in this well-cited article, with two kinds of disordered ecology: Gleasonian and systems theory on the model of Odum. Only the first, community-ecology branch is at issue here.

porating the equilibrium assumptions of Clementsian “organicism” [Fitzsimmons, 1999, 143]. He reasons that because there are no ecosystems (in a broad sense incorporating communities),[4] we should not attempt to preserve any such thing. Treating ecosystems-science as a Kuhnian paradigm (with the associated implications for anti-realism), he offers that “we know that the boundaries of ecosystems are guesswork and rarely represent real features on the landscape. We know that the landscape is in constant flux so that the ecosystems depicted by researchers constantly change in space and time in poorly understood ways, turning ideas such as ecosystem stability and sustainability into oxymorons” [161]. If so, he reasons, conservation policies are flawed because they attempt to preserve illusions glommed onto real chaos from questionable motives. [page 56]

However, while the first conditional is reasonable, the second is mistaken. A mythology has grown up around the early ecologists and their concepts, mistakenly aligning them with order and chaos.[5] If, as I will argue, this is a mistake, the second claim is not an instance of the first. The correctness or coherence of Clements’s or Gleason’s ecology in particular do not have the general implications for conservation they have been taken to have. Obviously, one upshot of this argument is that these challenges to community-preservation from classical ecology do not go through so straightforwardly. Towards arguing against the second conditional claim and rejecting accepting it as an instance of the first, I begin by positioning the historical debate between these theories, and then observe how the debate has been reconstructed, comparing that to the scientists’ research.

2 THE PROSPECT OF SCIENTIFIC ECOLOGY

Already in the first decade of the twentieth century, C. E. Moss describes the terminology for classifying vegetation inherited from the nineteenth as being in disarray: “the subject of ecological plant geography has suffered and still suffers very considerably from a lack of uniformity in the use of its principal terms” [Moss, 1910, 18]. His 1910 survey focuses specifically on discrepancies in the usages of “formation,” “association,” and “society” — three terms for different kinds of groupings of plants which would be, in our current usage, different kinds of communities. Moss observes the variety of usages among German botanists through the nineteenth century (Schouw, Griesbach, Hult, Kerner, Drude, Flahault, Schimper,

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[4] He defends this claim, for instance, by using Shrader-Frechette and McCoy’s argument against community stability to undermine ecosystem stability. If his general argument runs that *ecosystems* can be preserved only if they are *Tansleyan*, but in fact they are not, still many of his sub-arguments apply equally well to communities.

[5] An earlier essay [Eliot, 2007] also argued that the Clements/Gleason debate should be understood differently than it often has been. That article focused on the assumptions shaping science-interpretation, and specifically on whether it has been reasonable to interpret the Clements/Gleason debate as having been about the assertion of an ecological law of succession and the denial of that law. The analysis here works alongside that one by focusing instead on the metaphysics of Clements’s and Gleason’s and other ecologists’ positions, which is to say, on their arguments concerning communities as things.

Warming), following them up to the adoption of similar terminology by British [page 57] and American ecologists at the beginning of the twentieth (Cowles, Moss, Clements). His verdict is that ecologists have not settled on a shared set of terms, but moreover that their problem is not just finding the right words; it is that there are substantive disagreements about what the words should refer to, and about how these terms should be related to one another. Moss indicates optimism for improvements in the uniformity of usage. But, concerned that terms for subdivisions of plant associations ("plant societies" and "facies") have already been used in multiple senses, he concedes that "in fact, so many terms have been used by ecologists and plant geographers with so many different significations that it would appear to be impossible to find any term to which the above objection does not apply" [48].

Moss's solution for terminological disorder is causal investigation. Though in 1910 he can be aware only of Frederic Clements's earliest work produced by that time (1899–1907), his optimism lands on a concrete project in Clements's approach. More adamantly than others, Clements had begun to argue that formations, the largest-scale groupings of species, should not be identified in the field primarily by physiognomic criteria — that is, on the basis of the *appearances* of vegetation — but recognized instead on the basis of common *habitats*. Extending back at least to the German *Naturphilosophie* tradition in botany, physiognomic approaches to vegetation sought to identify the character of landscapes just as one might identify people's characters by visually scanning their faces. As Moss describes, Clements responded to this tradition by arguing in his early work that areas of vegetation should be differentiated by their differing causes. "Habitat" thus becomes a way of referring to these causes. This approach can work at the various scales at which there are common conditions. "Formations" are the units of vegetation at the largest scale in space and time, and consequently reflect the widest range of habitat conditions, the most inclusive scope of similarity. Their appearance at any time is normally accordingly diverse. Formations are comprised of "associations" which may be recognized empirically, and aligned with more temporary causes. At both levels, units' boundaries are determined by the extent of action of causes. Moss endorses this initiative to investigate causes as a way out of the conceptual morass.

Crucially in Moss's estimation, Clements's causal analysis would render ecological classification more *scientific* by aligning ecological units with identifiable causes that can be investigated experimentally. The result would be concepts and corresponding units reflecting reality better than do the subjective impressions of naturalists:

> Although many earlier writers regarded the formation and the habitat as vitally connected, it is to Clements (1905) that ecologists owe the most emphatic expression of this view. Clements (1905:292) stated unequivocally that 'the connection between formation and habitat is so close that any application of the term to a division greater or smaller than the habitat is both illogical and unfortunate. As effect and cause,

> it is inevitable that the unit of the vegetative covering, the formation, should correspond to the unit of the earth's surface, the habitat.' This view, as has been shown, was by no means new; but no one had previously stated with sufficient emphasis and in general terms what must be regarded as the foundation of the modern treatment of vegetation. The concept is much more stimulating and much more scientific than a merely physiognomical view of the formation; and this latter view, useful enough in the early days of plant geography, has now been quite outgrown. It is no longer possible to regard a forest as a 'formation,' nor even a coniferous forest. Such complex pieces of vegetation must be resolved into separate associations, and the latter rearranged into formations on a basis which shall commend itself to those who search after real affinities and underlying causes. The rearrangement of associations into formations will not be accomplished at once, except in the case of well-marked habitats. Where the habitats are less sharply defined, much exact and quantitative experimental work remains to be done; and here again Clements, in his *Research Methods in Ecology*, has performed useful and pioneer work. Until much work of this character has been performed, until certain habitats have been more closely investigated, ecologists and plant-geographers must be content to refer certain communities simply to their associations, rather than hastily build up formations on flimsy foundations. [Moss, 1910, 33] [page 58]

What is most notable here is why Clements's theory seems progressive to Moss in 1910. It seems to him an advance because it attempts to ground the differentiation of communities on their distinct causal backgrounds rather than on common appearances, and to identify causal backgrounds through "exact and quantitative experimental work." Investigating causes this way is "more scientific," and yields more accurate units. It offers a more scientific response to an existing demand: botanists and geographers had announced the need for sound nomenclature at international congresses because their fields require discussing areas of vegetation, and they want to establish their work as scientific at a time when many other fields of biology had been professionalizing. Clements's contribution is simply to offer a means for remodeling botany as scientific, by pushing its investigation of causes.

It is problematically artificial to claim a sharp boundary between natural history and science, either in the historical development of ecological knowledge or in the practice of contemporary researchers, but even so, descriptive natural history adds something recognizably different as it engages with the investigation of causes. Causal investigation can help us understand unlike things as instances of single kinds for substantive reasons, which to say, understand how they participate in the causal structure of the world. Recognizing the participation of individual things in the causal structure of the world is in turn a large part of what it means to gain scientific understanding. So, if descriptive classification can also be scientific, causal investigation unites it with theory in a way that helps it further understanding. Just as in systematics, where understanding of evolution by natural selection (and later genetics) restructured how organisms are arranged [page 59] into species and higher taxa, so in ecology classificatory optimism arose about the same time.

In fact, this parallel is drawn explicitly by F. F. Blackman and Arthur Tansley in their early review of Clements's *Research Methods* [Blackman and Tansley, 1905; Clements, 1905]. Taxonomists at the beginning of the twentieth century had faced the same kind of conceptual disarray ecologists now were facing. And they, too, had earned some optimism about gradually revealing objective edges for their groupings through a combination of new theory and emerging experimental techniques developed to assess theories' applicability to nature. Beginning with the problem of identifying entity-boundaries in ecology, Blackman and Tansley land alongside Moss on Clements's work as a basis for optimism. Here they discuss how to probe the boundaries of communities as a problem paralleling one in taxonomy. In both cases, units grade into one another. Yet, in both disciplines it is possible through investigation to discern the actions of different causes which objectively distinguish the units. Referring first to vegetation, they write:

> If you get a gradual and continuous change of one or more factors in passing away from a given spot characterised by a definite assemblage of plant-forms you may pass through a region which shows a continuous change in vegetation structure and composition till you arrive at quite another definite assemblage. At what point is "the final test" to be applied? The difficulty here seems to be fairly comparable with the difficulty of delimiting species in taxonomy. Critical study will in very many cases enable us satisfactorily to delimit formations which at first present bewildering difficulties. The same is true of species. There may be cases in which the difficulties are so great that there is still room, after the best investigation we can give, for difference of opinion as to whether the assemblages in dispute shall be "split" or "lumped"; which means that the subjective element cannot at present be entirely eliminated. The same is true of species. Meanwhile we are convinced that both species and formations have a real objective existence, though widespread doubt exists in both cases, especially among those who have not given attention to their actual study. The real differentiating factors in the two cases are probably of entirely different nature and in both cases we are far from having explored them to the bottom. Nevertheless we have full confidence that finality in these provinces will be reached in the course of future work. The work of Jordan, of De Vries, and of the Mendelians seems to furnish a beginning in one province, while Dr. Clements's researches constitute an important advance in the other. [Blackman and Tansley, 1905, 250–251]

Noticing Blackman and Tansley's early optimism about defending communities' objective existence invites curiosity about what happened to ecology's optimism for this new "more scientific" approach. One kind of answer to this question is defended in Ronald Tobey's history. In *Saving the Prairies*, he describes how a [page 60] Clementsian paradigm (in Kuhn's sense) eroded and collapsed though a combination of scientific, contextual, and internal sociological factors. Along with Tobey, a number of historians and historically-oriented biologists have analyzed this shift in similar ways, including those discussed below in section 5.

For philosophy of ecology, however, a further question emerges when we observe that the eclipse of Clements's ecology involved not only a theory-shift away from a theory deemed worse or less-true, but also decades of claims that Clementsian ecology is *unscientific*. Ecologist Daniel Botkin, for instance, remarks that the Clementsian approach to communities will soon seem "silly" as an explanation of nature, and that it "by the 1940s had been completely dismissed in the United States, where it remained a historical curiosity, useful in explaining to students of ecology why it is an inappropriate perception." For Botkin, Clements's account of communities was not merely mistaken, but moreover "quickly dismissed when proposed in the scientific age," because it was not only wrong, but also deficient as science [Botkin, 1990, 98–99]. It is one thing to be wrong; it is another to be unscientific. How did conventional wisdom switch from treating Clementsian ecology as scientifically progressive to unscientific? Then, how does its ontology — the units and entities it employs — contribute to or impede its success as science?

From Moss's early review all the way through to the present, one finds arguments both (a) that Clements's theory was scientifically progressive, while Gleason's undermined ecology's goals, and (b) that Clements's theory was unscientific, while Gleason's was scientifically progressive. I suggest this feature of the debate is not unrelated to the other feature I noted, the prominence of its similes in discussions of both theories from their early reception to the present. Commentators have sometimes taken the scientists' similes to *be* their positions, and even where commentary has been more sophisticated, it has usually understood the positions as what one *should* be committed to if one is committed to a certain simile, rather than what the scientists themselves actually did commit to.

3 ORDER AND CHAOS

Donald Worster, positioning Clements's and Gleason's theories as opposites in an influential article, has labeled them "the ecology of order and chaos." On the order side is of course Clements's structured climax sere (the sequence of vegetation leading up to a climax state). Most of Clements's 1936 essay, his most widely reprinted piece of writing, is devoted to the structure of the climax sere, and this emphasis on its structure has suggested that the sere itself imposes a causal structure on the organisms within it. That impression derives from an abundance of terminology. Though I will not explain its full structure here, the climax sere has numerous parts, fostering the impression that these parts, and consequently vegetation itself, are structured by an overarching organizing-principle. In discussing later developments of terminology, Clements writes that "the climax group now comprises the following units, viz. association, consociation, faciation, lociation, [page 61] society, and clan," and these are only some of the components of the climax [Clements, 1916, 272].[6] As Clements worked out how to explain the complexities of vegetation, his theory became increasingly laden with vocabulary he deployed to handle variation. It contains names for vegetation at multiple scales, from whole regions to tiny clusters of a few plants, and names especially for describing variation due to particular classes of causes. Clements's prodigious collection of terms, whose coinage from Latin and Greek roots he displays special relish in detailing, has struck few readers, scientific or lay, as palatable. For most published respondents, the unsavory volume of the coined vocabulary has led to a sense of the whole theory as an undigestible effort to force an imaginary order onto disorderly nature. Botanist Neil Stevens, for example, approvingly cites this damning review of Clements and Shelford's *Bioecology* in 1950:

> This book is a fine example of an important and already difficult subject discussed in an abstruse, involved, pompous and thoroughly tiresome manner. Simple things are made complex, and complex things made well-nigh incomprehensible.... Nor is the mounting use of coined words helpful in elucidating the text. One is led almost to believe that ecology, as understood in the Clements-Shelford biome, is the occupation of thinking up new names for old things. [Stevens, 1950, 112][7]

Beyond the sheer volume of terms *per se*, Clements's coinages have led to a number of further commitments being attributed to him, typically as part of repudiating them. Most commonly, the baroque order the terms and their relations suggest have often been thought to imply deterministic orderliness. They have been thought to cumulatively describe a system in which areas' climates are tied to their climax vegetation in lawful associations — despite the *prima facie* implausibility of doing so — as if Clements were naming the mechanical parts of a landscape-factory. Or, they have been thought to correspond to the myriad anatomical parts of complex organisms, with the connotation of governed, purpose-driven development. Furthermore, the vocabulary has been considered ontologically overeager, in that each of the newly-coined vegetation terms have been thought to reflect discrete units of vegetation, so that Clements is thought to recognize entities everywhere, including where they do not exist. Though this assumption of existence applies also to his various concepts like subclimax, disclimax, and serclimax, the impression of Clements's ontological naïvete has especially derived from the impression that his stages of plant succession are supposed to be temporally discrete, and that his areas of vegetation are supposed to have sharp boundaries. Nascent suspicion of ontological naïveté is clinched by Clements's attraction to the organism-simile, which seems to treat vegetation units as existing as discretely as individual organ- [page 62]

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[6] Confusingly, towards the end of his career, Clements came to use this term to refer not only to the end-point of seral sequences but also to vegetation units or formations as a whole, in order to identify the units with their best-adapted species.

[7] I have been unable to identify the original source of this review, and it may be unpublished except in quotation by Stevens.

isms.

Gleason's terms and phrases — here metaphors — are as evocative of chaos as Clements's "organisms" are of order. To mention one later example:

> Suffice it then to repeat that on every spot of ground the environment is continually in a state of flux, and that the time-period in which a certain environmental complex is operative is seized on by the particular kinds of places which can use it. The vegetation of every spot of ground is therefore also continually in a state of flux, showing constant variations in the kinds of species present, in the number of individuals of each, and in the vigor and reproductive capacity of the plants. [Gleason, 1939, 99]

Gleason's description of both environments and the vegetation occupying them as in continuous flux, like his remarks elsewhere that the co-occurrence of organisms in a particular place is a matter of mere "coincidence," evoke the opposite of Clementsian orderliness. Where Clements appears holistic, Gleason appears reductionistic — at least where holism means that vegetation can only be understood as whole entities clearly distinguishable from their surroundings, while reductionism means that no such whole exists as a real thing, and that only the plants which are its supposed components do. Consequently, while Clements laid out explanatory theory and a research program for ecology, Gleason's ideas may entail that community ecology is impossible. Communities seem not to exist beyond the dynamics of individual organisms, and those dynamics are themselves chaotic or random [Gleason, 1926, 16]. Gleason writes that "the distribution of species is primarily a matter of chance, depending on the accidents of dispersal" [Gleason, 1925, 74]. And a year later he offers "that careful quantitative study of certain associations from 1911 to 1923 produced the unexpected information that the distribution of species and individuals within a community followed the mathematical laws of probability and chance" [Gleason, 1926, 16]. This phrase, "follow[ing] the mathematical laws of probability and chance," demands clarification about what the laws of chance are and what they might apply to, since it cannot mean that plants have equal likelihoods of appearing anywhere.[8] But if vegetation is best described by randomness, that outcome leaves precious few research avenues for community ecologists.

Beyond its political resonance, this language of order and organization found in Clements and the opposing flux and continuum found in Gleason set up this scientific debate as an iteration of a very old philosophical discussion. From the beginning of Western philosophy, the fragmentary remains of presocratic philosopher Heraclitus's writings offer provocative images of nature as in flux, in part or wholly. Heraclitus's remark that "upon those that step in the same rivers, different and different waters flow," or colloquially, "you can't step in the same river twice," [page 63]

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[8] Obviously, in the Sonoran Desert, one's odds of finding a saguaro are rather different than one's odds of finding water lilies, for both contingent reasons having to do with past dispersal and necessary reasons having to do with the dynamics of physiologies and environments.

offers the best-known image of a portion of nature being dynamic to such a degree that its identity is compromised [Kirk *et al.*, 1983, 195]. Its implicit suggestion is that dynamic entities, those with shifting properties, not only become different in shifting their features from moment to moment, but also through these shifts become different things entirely. Yet this image, like Descartes's famous example of a ball of wax which retains its identity despite radical changes in all its properties, depicts a problem different than that which ecologists face in recognizing communities. Ecologists' problem with communities is closer to that hinted at by another of Heraclitus's fragments about flux. This fragment proposes an implication of bringing various things together in a group, as communities do by definition: "Things taken together are wholes and not wholes, something which is being brought together and brought apart, which is in tune and out of tune; out of all things there comes a unity, and out of a unity all things" [Kirk *et al.*, 1983, 190]. That is, once one starts uniting one thing with another, one finds that all its neighbors can be united with it, and one is left with a unity which excludes nothing. Once one starts thinking of one's siblings and parents as united with oneself to form a family, one finds that "family" can include third cousins and in-laws' siblings' spouses, too.

Yet, the relatedness and consequent potential unity of everything with everything else, as illustrated by Heraclitus's fragments, is not normally a problem. It does not derail our ordinary thought or everyday activities. One can easily delineate one's immediate family for whom taxes must be paid from the larger portion of one's family one wants to invite to a reunion, and this latter group from the whole of humanity (who are in the end all family, too). Similarly, differentiating in our perception of the world around us the signals reflecting the presence of entities from those signals reflecting the space around them — being able, that is, to recognize the edges of ordinary objects — is not normally a practical problem. It is a philosophically-loaded problem for neuropsychology [Marr, 1982, for instance]. But, at least when watching ourselves, we do not set coffee mugs on thin air instead of our desktops, despite that the information our senses gather about the world around us is continuous and constantly fluctuating. We are not fooled by the changing colors and shapes of things into seeing the world as an undifferentiated mishmash, like a pointillist painting viewed from too close. William James evocatively imagines that a baby, confronted with its own new sensations, "assailed by eyes, ears, nose, skin, and entrails at once, feels it all as one great blooming, buzzing confusion" [James, 1890, 488]. But James makes this famous remark only on the way to analyzing how children learn *not* to experience the world as an undifferentiated continuum. They learn quickly how to individuate things from their surroundings — chairs from floors and food from spoons. Kuhn revives this phrase from James to present an image of what our experience of the world would be like in the absence of a paradigm that structures that experience for us [Kuhn, 1962, 113]. But even if learning strongly influences how we perceive, our ordinary perception can succeed when supported by a store of previous experiences amounting to considerably less than a scientific theory or theoretical [page 64] paradigm [Richeimer, 2000, 388–391].

Ecological objects are different. We do not normally learn as children to distinguish ecological communities from their surroundings, because their boundaries are rarely obvious, and may never appear at all to laypeople. That is, differentiating communities from their surroundings often requires expertise, and perhaps also a theoretical framework like Kuhn has in mind. But if individual plants are in flux, and if when they appear together it is merely a matter of coincidence, theories permitting groups to be distinguished from one another as units are *prima facie* suspicious. We should mistrust what passes for expertise about their dynamics. We should suspect that supposed experts perceive order where it does not exist.

Wondering whether expertise about ecological communities is possible, we can find Plato's response to Heraclitus handy. While making a broader argument in *Theaetetus* against the idea that to perceive something is to know it, Socrates urges that if one adopts Heraclitus's position that everything is in flux, one can no longer count on one's language. If not only the properties but also the boundaries and identities of certain things are unstable and consequently indeterminate, those are not the sort of things which can anchor the meanings of our words. For two beings to communicate (which is implicitly to say, communicate *meaningfully*) about desks, there must be some actual or possible thing which is distinguishable from its surroundings as a desk [Burnyeat, 1990]. Since the expression of the position itself that everything is in flux depends on the usability of the words it is couched in, this is damning. Relatedly, in *Sophist*, Plato's Visitor from Elea raises the alternative specter, the problem which arises if words pick out only unique things (and perhaps only as they exist statically at a single point in time). Ignoring the relatedness of things, we find that "to dissociate each thing from everything else is to destroy totally everything there is to say" [Plato, 1993, *259e*]. That is, thought and speech generally, and scientific description and explanation in particular, require that the things they refer to be able to be disentangled fairly reliably from their spatiotemporal surroundings as repeated instances of the same kind of thing.

Tailoring Plato's point to fit ecological objects: as far science goes, chaos, in the sense of complete disorder, is a nonstarter. Science cannot gain a foothold in a [page 65] completely disordered world. One might object that scientists have recognized and developed a mathematical account of chaos. But such "chaos theory" discusses a different kind of chaos than disorder. That other chaos is what Stephen Kellert in a philosophical account defines as "the qualitative study of unstable aperiodic behavior in deterministic nonlinear dynamical systems" [Kellert, 1993, 2]. Like any other science, applying chaos theory requires, trivially, that there be systems to which it may be appropriately applied. Whether systems may be singled out for substantive description, and what kinds of systems those are, are precisely what is in question in the ecological discussion. Mathematical chaos moreover explains how there might be order in apparently disordered systems. It is therefore not what is at issue in Worster's usage of "chaos," for instance. Far from it being a nonstarter, ecologists have found mathematical chaos useful in ecological analysis, for instance of population cycling.[9]

So, for present purposes I mean by "chaotic," for a system, that it is entirely disordered. If a system is entirely disordered, any predictive science concerning it is stymied, except that one could banally predict disorder. But more significantly, if attributing this sort of disorder entails that every arbitrarily-bounded unit works equally well for characterization at some level of description, then there are no entities a scientific discipline can call its own at that level of description. Without a substantive level of description of its own, a discipline can be folded into others which do have unique domains about which substantive claims may be made, without any loss of information or understanding. Applying this idea to ecology, if ecology is to be a partly-autonomous discipline in the way that naming it and working on it implies, research in ecology requires employing *some* units. It requires them in so far as it needs to discuss parts of the universe in isolation from other parts. Furthermore, it needs for there to be some units which are better suited for theorizing than others are. Otherwise, ecology cannot uniquely contribute to understanding.

In a nominally-Gleasonian spirit one could offer individual organisms as the units for ecology and reject any units larger than individuals. But this move is not independently open even to the ecological reductionist: if a reductionist wishes to understand the features and dynamics of an ecological system as a function of its components, she still presupposes that a *system* may be identified in some way or other. This system could in principle be the entire world, or the collection of all living things. But for pragmatic reasons, her understanding anything requires that some parts of nature be substantively isolatable from the rest of the universe as systems (even if those systems' dynamics are still entirely a function of their parts). A first pragmatic reason is that sometimes we want explanations of the dynamics of particular systems like lakes and forests. If all ecology can tell us is that the dynamics of a forest depend on everything else in the world or universe, ecology is not worth pursuing. That is because we can rarely acquire very much data about any given system (especially without enormous expense), so that if our understanding of particular dynamics depends on data about all phenomena, we will not reach understanding of those particulars. A second pragmatic reason is that if the entire world or universe could only be understood as such, we would be unlikely to reach such understanding without understanding something about the dynamics of particular parts first. So, even if systems' dynamics are entirely a function of their parts, ecology needs systems as units.

Yet, this argument risks begging the question. Perhaps there are no parts of the living world which may be distinguished from their surroundings enough that they support better predictions or explanations than other parts. Perhaps ecology is not a science because it cannot be. There are at least two good reasons not to accept this alternative. First, ecology has predictive successes which are demonstrably not just lucky guesses, and some of its models and descriptions have proved useful. Second, what understanding we have of the features of organisms from elsewhere [page 66]

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[9] See discussion in [Pool, 1989], and, as an example, [Tilman and Wedin, 1991]).

in biology is largely due to our recognizing the action (or process or mechanism) of natural selection. That natural selection works, that it has successfully produced complex forms, entails that there have been patterns of success and failure in the struggle for survival among individuals and populations. Those patterns are ecological patterns. So while ecological prediction and even description may be difficult, its domain is not patternless, not entirely random. And significantly, the action of natural selection suggests that some of those patterns are ecological in that they involve relationships among populations, not just individuals.

Accordingly, when ecologist Michael Barbour writes, criticizing Clementsian ecology, "if one wishes to recognize associations, perhaps on the basis of the presence of certain dominant species, one can do so and even draw lines on maps; but this activity must be recognized as arbitrary, subjective, and a gross simplification of nature," we should not read his argument as rebutting *all* ecological entities as they appear in scientific discussion [Barbour, 1995, 237].[10] Even a gross simplification is not necessarily a falsehood when it serves a descriptive function. In examining Barbour's own research one immediately recognizes it as not at all defeatist about the whole enterprise of ecology, and moreover open to the possibility that ecology might offer substantive claims about parts of nature in isolation from the entire universe. That Barbour discusses systems does not prove that communities exist. Rather, it deflects criticisms like Barbour's from the position that community ecology and its systems stand or fall together, where that ecology's systems stand means that it has ability to discuss real patterns in partly-isolatable portions of nature. Natural selection suggests that there are real patterns, and empirical successes suggest that ecology, however nascent, sometimes stands.

If all is not lost to chaos, then, the difficult remaining problem becomes how to draw lines which are not entirely "arbitrary, subjective, and a gross simplification of nature." What should guide delimiting systems, and what degree of confidence should we place in the entities they delimit? What kind of characteristics, that is, does a community or other ecological system need to have in order to persuade us of the reasonableness of describing it in at least provisional, partial isolation? If figuring out how this works for ordinary objects provides puzzles for philosophers of language but poses no problem for ordinary thought, disentangling ecological objects from their surroundings in this way produces a real, practical problem for ecologists, and raises interesting philosophical questions about ontology, too.

I have mentioned the workaday and even moral consequences of this practical problem. In Donald Worster's inference, the shift from a Clementsian to a Gleasonian ecology produced a crisis for anyone wanting or needing to employ ecological units, most of all environmentalists and conservationists. In "The Ecology of Order and Chaos," Worster's chief concern is with the idea that during the latter half of the twentieth century, ecology experienced a paradigm shift from a Clementsian framework to a Gleasonian one, a shift away from finding order in [page 67]

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[10] Barbour's objection here can be read as tailored to association-types rather than community tokens, but types are not bounded by "lines on maps" — community tokens are.

nature towards regarding nature as in flux.[11] The pathology of this shift lies its crippling conservation or preservation; if scientists do not treat communities as real things, the justification for conserving them is injured:

> There is a clear reason for that outcome, I will argue, and it has to do with drastic changes in the ideas that ecologists hold about the structure and function of the natural world. In [mid-twentieth-century environmentalist Paul] Sears's day ecology was basically a study of equilibrium, harmony, and order; it had been so from its beginnings. Today, however, in many circles of scientific research, it has become a study of disturbance, disharmony, and chaos, and coincidentally or not, conservation is often not even a remote concern.[12] [Worster, 1990, 3]

One might justifiably balk at criticizing science for not offering up an ontology or account of nature supporting applying one's own value-system to the world. Even if one accepts, with Helen Longino for instance, that values from the context of science can constrain scientific reasoning, it is wrongheaded to criticize scientific results for not fortifying our values. Yet, if the concept of order or chaos is assumed by theories pre-theoretically or pre-empirically, it becomes reasonable to criticize it, and even for laypeople to do so. Longino argues that this even shores up science's objectivity [Longino, 2004]. So, there is a reasonable interpretation of Worster's complaint.

Though Worster ends up resigning himself in the essay's final sentence to accepting the theoretical complexity needed to describe nature, it is only after lamenting at length that ecology has lost something if it moves away from equilibrium assumptions. Confidence in order has been lost, and Worster understands "order" to have several components. Orderly systems, he believes, have equilibria, are "perfectly predictable" [13–14], and have holistic dynamics, especially "emergent collectivity" [8]. Worster takes Gleason to have aimed to demolish ecology's confidence in meeting all three of these standards. For Gleason, indicates Worster,

> there is no such thing . . . as balance or equilibrium or steady-state. Each and every plant association is nothing but a temporary gathering of strangers, a clustering of species unrelated to one another, here for a brief while today, on their way somewhere else tomorrow. [8–9]

Yet, equilibrium, emergent collectivity, and predictability are quite different features of systems. Why should one suppose that orderliness of communities involves these particular commitments? Must it? And what is the degree of orderliness [page 68] one needs to suppose in order to understand the dynamics of systems? If one

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[11] Worster treats Eugene Odum's early systems ecology along with Clements on the side of order, but since systems ecology, shifting focus to abiotic components and ignoring species boundaries raises distinct issues, it can be understood historically as a separate tradition.

[12] "Coincidentally or not" offers Worster a rhetorical hedge here; the essay overall suggests that he does not regard this as coincidental, and the essay would not be worth discussing absent its implication of that relationship.

recognizes with Plato that to discuss nature one needs to suppose it orderly to some degree, the question becomes whether equilibrium, emergent collectivity, and perfect predictability need to be supposed to discuss systems, or something else, perhaps something less. What motivates treating deterministic holism as the main alternative to disorder?

To be discussable, a thing minimally needs to be at least in principle reliably identifiable and roughly distinguishable from its context. Meeting that minimal criterion means already resisting Heraclitus's image of the un-ignorable unity of all things. At least some communities, like those of terrestrial organisms on very remote islands or aquatic organisms in very isolated ponds, meet it easily. No ecologist I am aware of, including Gleason, denies that some communities meet this criterion of orderliness — being roughly distinguishable in such a way that a layperson could find their boundaries. But then, to be the sort of thing scientists can successfully theorize about, a thing's dynamics also need to be patterned or regular or organized to some degree — there must be repetition of phenomena for instance. As I argue in the next section, even Gleason agrees that communities have this sort of order. (If regularity is denied by part of ecology, it is not in Gleasonian ecology, that is.) So, a first step away from chaos requires some minimal distinguishability and regularity. But a thing may be orderly in this way, having regular internal dynamics, and yet be very difficult to engage predictively, much less predict perfectly. Even perfect predictability, though, is a long step from having holistic dynamics.

So, preservationists face the question question what kind of order communities need to have to be preservable, beyond just being describable. If communities have real equilibria or emergent properties, those would contribute significantly to the case that they are appropriate objects for conservation. But need they have? That is less clear. Whenever natural selection is operating in communities, at least that force opposes their equilibria. Large-scale climate shifts may work against equilibria, too. But, we do preserve communities with degrees of success whether or not they are in fact equilibrial or have emergent properties. We should determine what and how they need to be for this preservation to be realistic. With both questions in mind — what is required for recognizing and then for preserving communities — I return to the nominally opposite views to find their common ground on this point, along the way considering how they came to be regarded as opposites, and then how that matters to what communities are.

4 CLEMENTS'S AND GLEASON'S ONTOLOGIES

The legend of order and chaos, as expressed by Worster and repeated by others, is supported by the legend of Clements and Gleason, and the former unravels with the latter. By "the legend of Clements and Gleason" I mean (a) the two claims traditionally attributed to Clements — that communities are like organisms, and they develop according to a simple, deterministic law, (b) Gleason's rejection of [page 69] these claims in his "Individualistic view," and (c) the broader narratives their scientific claims have been embedded in, which further the impression of that (a) and (b) are polarized claims. Having discussed what is at state in order and chaos, (c), I turn now to comparing the specific content of Clements's and Gleason's views with the legend about (a) and (b).

Clements's putative law

A foundation of the legend is the law which is supposed to be the backbone of Clementsian order. Eliot [2007] argued that in his mature theory Clements does not assert a deterministic law of vegetation, that "climates beget climaxes." Even so, laws of vegetation *have* been asserted. In 1825, Adolphe Dureau de la Malle claims that the improved success of crops when they are rotated reflects a general law of nature:

> The alternance or alternative succession in the reproduction of plants, especially when one forces them to live in societies, is a general law of nature, a condition essential to their conservation and development. This law applies equally to trees, shrubs, and undershrubs, controls the vegetation of social plants, of artificial and natural prairies, of annual, biennial, or perennial species living socially or even isolated. This theory, the basis of all good agriculture, and reduced to a fact by the proved success of the rotation of crops, is a fundamental law imposed upon vegetation. [Dureau de la Malle, 1825; Clements, 1916]

This "law" is a statement of the idea that often plants modify external conditions in a way that makes those conditions more suitable for plants other than themselves. In other words, Dureau de la Malle has identified in a very general way the chief mechanism driving successional change, the dynamic between plants' physiologies and their habitats. This "law" does not claim that certain plants are everywhere followed by certain other plants. While identifying a facilitation relationship, it does not ground precise predictions. Clements, too, in the appendix of his earliest book, lists several "laws of succession" [Clements, 1904]. But none of them express constant conjunction, much less nomic expectability or necessitation or counterfactual dependence in the way philosophers of science have typically expected scientific laws to. "Law" is being used in a different sense here than any of the usual philosophical ones, to refer to a combination of local mechanisms which do not operate in isolation, and to some non-predictive, non-universal regularities.

So while Clements occasionally makes leading claims which seem to indicate that a simple law is about to be unveiled — like that the relation between habitat and plant "is precisely the relation that exists between cause and effect," and "the essential connection between the habitat and the plant is seen to be absolute" — such comments are significantly tempered by reminders that, for instance, "the habitat is the sum of all the forces or factors present in a given area" [Clements, 1905, 17,18]. Consequently, while one might usefully remark that it is true, in a [page 70] law-like way, that habitat controls vegetation, this is not yet to commit to anything Gleason would disagree with.

The question of whether Clements's theory can be distinguished from Gleason's through his commitment to ecological laws turns on how Clementsian explanations of vegetation actually proceed, and what they invoke. This most-general law-like association suggests a methodological starting-point rather than (assuming that particular habitats and particular vegetation-types are filled in) the autonomous explanatory apparatus it has been made out to be. It suggests an idealized sequence of development of idealized vegetation best adapted to an area's climate. But it does so in order to help explain actual instances of vegetation (which, he recognizes rarely match idealized conditions) by appeal to departures from idealized conditions. What seems to be a law is the basis of a framework for incorporating diverse local causal factors and using them to explain. Indeed, a 1906 *American Naturalist* review of Clements's 1905 *Research Methods* emphasizes that its significance lay in shifting vegetation science *away* from naïve generalizations and towards investigation of specific causal factors: "This work should do much towards establishing ecology and experimental plant evolution upon a firmer basis by pointing out the need and the method of making absolute determinations of factors, instead of the inaccurate generalizations so often recorded" [Allen, 1906, 805].

Later, in Clements's major theoretical work of 1916, *Plant Succession*, the phrase "law of succession" appears twice, with two different meanings. In one instance, Clements writes, "to this fact," that in open associations immigration is inhibited by present occupation, "may be traced the fundamental law of succession that the number of stages is determined largely by the increasing difficulty of invasion as the area becomes stabilized" [Clements, 1916, 77–78]. This is to say that increasing occupation of an area makes invasion by new plants increasingly difficult and that this impediment to invasion affects how many immigrants one actually detects. The statement is a causal generalization, but as what it explains is the stages of vegetation in idealized sequences, it does not yet explain what the theory is designed to explain — what vegetation appears in an area. It is certainly not a climate-begets-climax law.

At another point, while speculating about the history of vegetation, Clements mentions "the basic law of succession that life-forms mark the concomitant development of the habitat and formation stage by stage, and that this development is reflected in the structure of the vegetation" [494]. This is closer to a climate-begets-climax law, but importantly different; it suggests that changes in vegetation track *shifts* in habitat, and it opens a discussion of a range of sources of habitat dynamics. Recognizing this association between changing habitat and changing vegetation helps interpret this next statement, which is closest of all to causal law:

> The habitat is the basic cause, and the community, with its species or floristic, and its phyads and ecads, or physiognomy, the effect. But the effect in its turn modifies the cause, which then produces new effects, and so on until the climax formation is reached. A study of the whole [page 71] process is indispensable to a complete understanding of formations. [123]

An obvious, reasonable interpretation of such claims is that they express a law like Newton's *force* = *mass* × *acceleration*, where if one provides a set of circumstances, one can derive an outcome, and thereby predict and explain that outcome. That is, one could construe these statements as functioning as laws in a hypothetico-deductive system, in Carl Hempel's sense [Hempel, 1966]. Philosophers have produced various other interpretations of scientific laws, too (see, e.g. discussion in [Weinert, 1995; Psillos, 2002]). But whatever conception of laws philosophers favor themselves — empirical regularities, or casual regularities, or inference rules, or unifying axioms of deductive systems or something else — the usual motivation for identifying laws is that they license a *modus ponens* form of explanatory reasoning, deriving some y from some 'if x then y' and some information x. The initial acceleration of a baseball, for instance, can be derived from its mass and the force applied to it, using the $F = MA$ law. Whatever particular conception of law is in use, invoking a law to produce explanations typically requires that what is explained corresponds at least roughly to some such derived y, and that the circumstances employed to explain correspond at least roughly to some such x or xs.

Unlike law-based theories in this most general sense, Clements's explanatory framework associates climates and climaxes through a kind of idealized association *not* normally realized. The association between climate and climax is that the climax is comprised of the species which most successfully outcompete other species in an area over the course of long-term competition, while the climate is the area's long-term average habitat characteristics. The connection between the two lies in the physiological adaptation of the former to the latter, as a consequence of evolution. Yet this framework explains the *changes* in vegetation in terms of *changes* in habitat. It does so by appealing to the ways actual habitat characteristics *deviate* from average conditions, dynamically. Because habitats, as they affect the survival of particular kinds of plants, depart from average conditions in extremely many ways, the idealized association between average conditions and climax vegetation offers a methodological starting point for empirically investigating the effects of certain kinds of deviations *as* deviations. Conceiving habitat conditions as deviations from an idealization is what allows their incorporation into explanatory structure at all.

Unlike a simple climate-begets-climax law, Clements's theory thus attempts to offer explanations by drawing on a whole variety of causes rather than a single one like "climate." It enumerates four general classes of causes as producing vegetation: initial causes, ecesic causes, reactions, and stabilizing causes. Initial causes are those instigating succession, such as a clear-cut or forest fire; they create the possibility of succession by eliminating current vegetation. The particular character of the initial causes affects which plants are able to establish by determining initial habitat features. Ecesic causes are the characteristics of plants affecting their establishment and growth, including the ranges of conditions in which they [page 72] survive and their adaptations for dispersal and immigration. Reactive causes are the ways in which plants themselves affect habitats for other plants. Stabilizing causes are the features or activities of plants adjusting habitat characteristics in such a way that habitats become unfavorable to new immigrants of other species (e.g. by reducing nutrients or light). Reactive and stabilizing causes are distinguished by what they benefit or harm. If a plant's changing its habitat benefits the plant itself, this change is called stabilizing; otherwise, it is reactive. After initial causes instigate succession, its dynamics are a function of reactive and ecesic causes, until — in stable habitats, anyway — stabilizing causes explain the persistence of particular species.

The myriad causes in these four classes are non-synonymous with 'climate' and can give rise to a range of outcomes, depending on the particulars of habitat and available species. Which is to say, Clements's theory is neither a 'monoclimax' theory, expecting a single outcome of succession, nor a monocausal theory treating vegetation as arising from a single cause. Whether or not 'climate begets climax' should count as a law is partly a function of how inclusive we are willing to be with the term 'law.' But Clements's explanatory strategy is enough different from the way laws are employed, I suggest, that we miss something about its approach to explanation when we call this part of the theory a law in the usual sense connecting one kind of cause to one kind of outcome. Indeed, the accounts recounted above demonstrate that treating the generalization this way has contributed to misunderstandings of the theory as a whole as deterministic.[13]

Clements's loose organism

If, comparing the explanatory and predictive resources employed by Clementsian ecology with those appearing in standard accounts of its simple law, we find them richer than that caricature, attention to the Clementsian account of communities reveals a comparably more complex treatment of them, as well. Fostering confusion, Clements makes claims nurturing the conclusion that he believes in ecological communities every bit as discretely-bounded and functionally-integrated as human bodies. The best-known is the provocative sentence, "as an organism, the formation arises, grows, matures, and dies" [Clements, 1916, 16]. Odenbaugh has taken this to suggest a close resemblance to a multicellular organism:

> a community may be a tightly integrated group of species that bear various causal relations among their component species. The community forms an individual, as if it were a multicellular organism. This is a Clementsian community: a group of species that strongly interact with one another [Odenbaugh, 2006, 217]

How strongly, then, does Clements intend the comparison to organisms? We can answer this by appraising what work he puts it to. The comparison to organisms encompasses four similarities which are of at least heuristic value in analyzing [page 73] units of vegetation. First, it suggests that unlike vague entities such as seas or clouds, vegetation units have boundaries. Clearly, not all organisms have clean

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[13] And see [Clements, 1916; Eliot, 2007] for further details.

boundaries,[14] but it is a general characteristic of organisms that they exhibit edges between themselves and their surroundings. Second, organisms exhibit patterns of growth and change which are predictable at least in a general way. Third, these roughly predictable patterns can be explained by evolutionary adaptation, or at least by descent with modification. Finally, their component parts demonstrate interdependence acquired through historical adaptation or accommodation to one another.

But Clements never suggests that these similarities between ecological units and individual organisms rise to the level of homologies. Where historical adaptation has produced similar features in unrelated entities (sometimes even by response to similar environments), like producing wings separately in birds, bats, and insects, these are analogous features. Clements's claims to similarity are never stronger than this. "As an organism, the formation arises, grows, matures, and dies" might be read as suggesting that plant formations are themselves organisms. The "as" here can be read as suggesting that formations *are* organisms (that is, it can be read in the "*qua*" sense of "as," meaning, "with respect to its being . . . ") or as marking a comparative simile. Evidence appears already four sentences later for the latter interpretation, when Clements writes that "the life history of a formation," (here sounding literal), "is a complex but definite process, comparable in its chief features with the life-history of an individual plant." If formations *compare* to individuals in a few chief features, they clearly are not *literally* individuals themselves. And in the next paragraph, Clements remarks that vegetation units differ from individuals in that they are capable of altering their habitats, whereas individuals acting alone cannot do much to alter their habitats (at least at the scale of influencing other populations) [Clements, 1916, 124-125].

Moreover, there are other features Clements attributes to units of vegetation which are not even analogous, much less homologous, to individual organisms. Foremost is the climax concept — the idea that units develop towards a mature state which has the capacity to endure indefinitely unless interfered with. Individual organisms cannot do so, in so far as they always, unless terminated sooner, advance through developmental stages to senescence and death. Though some can persist a very long time, none can endure stably for indefinitely long. Nor in many cases are their later features predictable from before they are born or produced. Given these disanalogies, it is then even more noteworthy that appeal to the organismic analogy offers no resources for explaining community dynamics other than that they can be understood as consequences of adaptive histories. This assertion that evolutionary processes produced each of them hardly rises to the level of homology; it is exactly the relationship shared by evolutionary analogs!

So, if Clements does not intend the organismic metaphor in a strong or literal sense (as I suggest he cannot, given what he attributes to it), why does he open his discussion of vegetation with it in his 1916 book, and remark again on its [page 74] significance elsewhere? Part of the answer lies in what he holds a comparison to organisms involves. Joel Hagen, perhaps uniquely attentive to Clements's use of

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[14] See the interesting discussion of this point, at length, in [Wilson, 1999].

the simile, points out that Clements must have had in mind for comparison a quite simple organism:

> It most certainly was not an organism in the same sense as a vertebrate animal or even a higher plant. What Clements seemed to have in mind as models for the community-organism were much simpler plants and animals, perhaps what we would refer to today as protists. [Hagen, 1992, 22–23]

Suggesting that Clements's implicit object of comparison is simple living structures, Hagen further remarks that such a comparison would have been "quite unremarkable" to biologists at the beginning of the twentieth century. It flags some basic similarities: continuity and growth in size through time, relationships among components, and a physiology, in the sense that the whole adapts to changing circumstances by modifying its components.[15] I depart from Hagen in asserting that while Clements extends physiology to populations, his wholes do not themselves have causal agency. Causal agency explicitly, exclusively lies with plants and habitats. But, following Henry Cowles's research on the Indiana Dunes, Clements attempted to represent vegetation as dynamic, rather than static. Treating vegetation as a consequence of the physiological interactions of constituent plants encouraged him to extend the concept of physiology to cover populations, and then the causal interactions among populations as their competition for resources changes the overall shape of the whole over time. While I do not take physiology to connote holism, nonetheless, as Hagen explains, the simile would have conveyed to his contemporaries that Clements was examining vegetation physiologically and as a dynamic unit able to shift in response to circumstances.

Still, while accepting that the comparison to organisms is not literal and that it is not to complex organisms, one might respond that Clements expresses certain other commitments by invoking it. Even protists distinguish themselves from disordered aggregates in two ways: functional integration and reasonably clear boundaries. Their functional integration is a matter of their exhibiting repeated, complex internal dynamics, and parts whose presence and structure can be explained by their functions in serving the fitness of their organisms (and the structures from which they have evolved). Does not Clements mean by invoking the organism simile that each individual species serves the development of the community towards climax in just this way? Then, while they don't exhibit perfectly sharp boundaries, every organism exchanges matter and energy with its surroundings. Both of these features have been attributed to Clementsian communities.

A first reason to conclude that Clements does not attribute this kind of thick functional integration to the relationship between individuals and communities (or, "seres," specifically, which is to say, communities developing over time) is that he does not believe that communities in particular areas necessarily have certain [page 75] species-components. Each area, as defined by environmental conditions that are similar to some degree, has species which have evolved to be adapted to its normal

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[15] See [Hagen, 1992] for further, useful discussion.

conditions, and these species often alter local conditions (e.g. by changing the nutrient profile of soil and creating shade) in ways facilitating the entrenchment of other species well adapted to those conditions. But this process is in no way inevitable: "In the case of invasion," Clements writes, "it is obvious that the failure of the dominants of a particular stage to reach the area would produce striking disturbances in development. Likewise, the appearance of alien dominants or potential climax species would profoundly affect the usual life-history" [Clements, 1916, 33]. Lest one imagine the theory assumes that invasions and disturbances are rare in nature, consider Clements's comment that "unlikeness and variation are universally present in vegetation" [Clements, 1907, 289], and further that a primary reason Clements coined his much-lamented, expansive collection of terms for different kinds of vegetation is that each of them reflects variations on idealized, normal vegetation. Each such variation reflects the action of a different kind disturbing cause, and thus departure from idealized normal vegetation.

There is a second reason to conclude that Clements understands vegetation to have rather less functional integration than even rather simple organisms like protists have. Animals exhibit centralized control, with a central nervous system issuing signals to bodies' components and receiving signals back from them, while seres clearly do not exhibit anything comparable. But even in much simpler organisms, components directly serve one another. In paramecia, vacuoles transport nutrients to lysosomes, fusing with them to accomplish digestion. These components of the organism interact directly. In plant successions, on Clements's view, *all* interactions among plants are indirect. All of them are accomplished through an intermediary, usually a resource. It is by adjusting the water content of soil or the amount of sunlight other organisms encounter in those other media that one plant affects another. Clements calls this driving force of succession "reaction on" an external medium. But since that is the nature of interactions among component species, any two organisms or species are completely intersubstitutable if they can survive in the same conditions and react on conditions in the same way. Expressed casually, a shade-loving fern does not care whether its shade is produced by an oak or a jacaranda, and Clements asserts no other way in which any one plant cares, so to speak, about other plants' identities beyond how they affect habitat. This is not to deny that there are much more specialized and fragile interactions in nature between mutually-adapted species, only to note that Clements's theory never asserts them among plants. His strongest statements of causal relationships do not therefore connote them. It is easy to imagine, if one starts with the organism simile, that Clements *would* assert such direct interactions, but that is to be misled by the metaphor's possible connotations. For him, internal community dynamics are exclusively indirect and identity-independent. This commitment is entirely missed by Richard Levins and Richard Lewontin, for instance, who assert that for Clements, "the behavior of the parts [of a plant community] is wholly subordinated to this abstract principle," and that "Clementsian idealism sees the [page 76] community as the only causal reality, with the behaviors of individual species populations as the direct consequence of the community's mysterious organizing forces" [Levins and Lewontin, 1985, 135].

Yet, the organism comparison implies a degree of functional integration, even if it is loose. I suggest that understanding areas of vegetation as functionally integrated serves as a cornerstone of Clements's strategy for explaining their dynamics. Its implication is that the idealized sere has a defined endpoint, the climax, and that successions are end-directed in this sense. The emergence and entrenchment of climax vegetation require a causal, developmental sequence leading up to it. One can reason towards the idealized developmental sequence for an area only by considering the causal influence plants have on one another through reaction. Since the idealized sequences are essential to this explanatory approach, and identifying causal connections among plants is essential for establishing sequences, explaining requires identifying actual or potential causal connections. As they produce sequences, they are also trivially ends-directed, and functional in that sense. As discussed, Clementsian explanations frequently employ these idealized sequences to explain vegetation departing from them, and in these cases, too, they invoke the causal contributions of plants themselves alongside the causal contributions of environments. In sum, using this approach to explaining vegetation trivially requires discussing causal relationships among plants, and in the idealized sequences with defined end-points, vegetation is treated as aiming towards an abstract end-point. So, vegetation explained this way is treated as trivially functionally integrated. The functional integration is trivial in the sense that while it is necessary for constructing explanations this way, it does not involve the claims that vegetation itself has these functions or end-points, that idealized endpoints ought to emerge, that they necessarily do emerge, that communities are causes, or that causation is top-down.

However, recognizing the functional or organizational looseness of the Clementsian community (and seral sequence) does not help with, and even amplifies, the problem of communities' boundaries. On this point, Clements can seem elusive. On the one hand, he sometimes characterizes formations as potentially "continental in extent" [Clements, 1936, 253], but elsewhere, especially in his work on indicators, he isolates areas of developing vegetation as small as "the north side of a rock" [Weaver and Clements, 1938, 373]. In practice, at any rate, he identifies areas of vegetation at many, nested scales, not taking their boundaries to be fixed, rigid, or definite. While some organisms are nested, as our intestinal fauna are nested inside us, that communities can be identified at such a range of scales suggests a further disanalogy with organisms. Individual plants participate in communities to the degree that they causally contribute to communities, or are produced by common causes. The strength of plants' and habitats' causal contributions can be used to identify better and worse boundaries for dividing up communities.

Building on this idea, Clements navigates the problem of community-boundaries [page 77] by rejecting that communities have sharp boundaries, even as they are bounded. He adopts a particular term for the phenomenon of gradation between communities at their edges: "zonation." Zonation is the blending of one population into another at its edges, a phenomenon often taken to have been used by Robert H. Whittaker to refute Clements after he observed it in the Great Smoky Mountains and Siskyou Mountains [Ricklefs, 1997, 507–510, for a textbook example]. What keeps community boundaries from becoming arbitrary where blending occurs is that zonation aligns with particular causes as multiple causes work simultaneously. For instance, soil salinity-levels may correlate with the abundance of one species along one gradient, while soil acidity is correlated with the abundance of another, along a different gradient [Weaver and Clements, 1938, 226–233]. There is order along these boundaries, but it is order tied to underlying causes, not the order of discrete objects and sharp edges. It is considerably different than the sort of order imagined by those starting with higher animals or plants as models for the organism simile. Further inferences about sharp boundaries drawn from the simile are false, and Clementsian ecology in no way denies the kind of visual disorderliness one observes in overlapping populations like Whittaker identifies.

Gleason's order

If Clements's communities turn out, on examination, to be compatible with a much greater degree of observed disorder than is imagined by commentators inferring his theory from his simile, Gleason's explanatory strategies similarly involve a much greater degree of order than is usually imagined. I argue this point in three steps. First, prior to his move away from views resembling Clements's, Gleason's early work supposes a great deal of order in plant succession. Much of the terminology he uses is Clementsian, and his explanatory strategies align with Clements's. As a second step, a few years later in 1917 and 1926, reacting to Clements's major publication of 1916, Gleason offers two sets of criticisms of Clements's approach. But in examining these objections while keeping an eye on the obvious, earlier affinity with Clements they arise from, we find Gleason objecting to fewer Clementsian ideas than is usually supposed. We should not automatically assume that Gleason's views shifted where there is no evidence of him abandoning his earlier views. This recognition attributes a burden of proof to anyone claiming that Gleason moved towards radical views opposite Clements's — a burden which I do not think any evidence bears. As a third step, I note the degree of community order and organization Gleason assumes in his later ecological writings, which further undermines the attribution to him of radical views of the sort inferred from his individualistic concept alone.

Early in his career, three years after his 1906 PhD, Gleason's analysis of vegetation conveys all the orderliness of mature Clementsian ecology. Here, he analyzes the ecology of the Midwestern prairie as a Clementsian sere:

> Within every complex of related plant associations, there are one or more definite orders of succession, leading from pioneer to climax associations. [page 78] The steps in the succession follow each other in a regular series and constitute what may be called a normal succession. [Gleason, 1909, 269–270]

At this early stage of Gleason's career, he treats succession as having a normal order, proceeding from associations of colonizing, pioneer species to associations of entrenched climax species. Associations are structured and bounded. Prairie remnants "still existing along our railroad tracks give only a faint idea of the normal structure of the prairie vegetation" [269]. Not only do these remnant communities have structure, they are recognized as resembling the normal structure for vegetation in that area. Then, as he observes the prairie and the forest butting up against each other in Illinois, he remarks on a "tension zone between the two associations" [270]. That is, the associations are identifiable, and competing. But of course, this sort of analysis precedes Gleason's famous reaction eight years later to Clements's *Plant Succession.*

Even in 1917, right alongside his famous criticisms, Gleason makes some fairly strong gestures towards the nominally-Clementsian pole. For instance, he announces a commitment to the "actual existence," as a matter of observable fact, "of definite units of vegetation" with self-maintaining structure:

> Of the actual existence of definite units of vegetation there is no doubt. That these units have describable structure, that they appear, maintain themselves, and eventually disappear are observable facts. That to each of these phenomena a definite or apparent cause may be assigned is evidenced by almost any piece of recent ecological literature. But the great mass of ecological facts revealed by observation and experiment may be classified in different ways, and from them general principles may be derived which differ widely in their meaning or even in their intelligibility. [Gleason, 1917, 464]

The qualification accompanying this declaration of allegiance to units is that there are different ways to make sense of ecological observations. And this idea points towards the further conclusion that Clementsian ecology offers just one way of doing so. Gleason's particular resistance to Clements's way of assembling ecological facts into ecological units crystalizes in two objections:

1. the units of vegetation are dissimilar to organisms;
2. Clements should not enlarge the unit of vegetation to include a climax and the stages leading up to it. [463]

These dismissals do not necessarily reject the causal story about what produces vegetation, but focus rather on the language used to describe it. Of course, the content we assign to these complaints turns on the degree and kind of difference Gleason actually takes there to be between vegetation and organisms, considered below. The second objection explicitly concerns terminology; units of vegetation are not exactly like organisms, and the seres Gleason recognizes in his early work [page 79] should not be treated as fundamental units for description. The primary unit should be the association (or community, in our terms), rather than the sequence of associations called a sere. Gleason's third and fourth objections function similarly:

3. new terminology like Clements's is not needed for describing succession;
4. Clements excludes apparent exceptions to his generalizations by definition. [Gleason, 1917, 463]

The third objection is obviously terminological, decrying Clements's terminological enthusiasm noted earlier. Gleason would later refer in passing to Clements as "an enterprising classicist," and he came to dislike Clements's approach of understanding associations in relation to normal types, where the various kinds of departure were what drew Clements to classifications requiring nomenclature. [Gleason, 1936b, 41]. The fourth objection concerns Clements's putative law, unpacked above. It reflects a methodological departure, resisting Clements's use of an unfalsifiable idealization as part of his explanations. Neither of these are yet objections to communities' organization, boundaries, or functional integration.

In a second critical essay of 1926, Gleason restates the first of these objections more adamantly, and adds two further criticisms directed at Clements's strategy of associating particular environments with particular vegetation types. In short, he claims that,

5. similar and homogeneous environments exhibit varied vegetation;
6. associations under same name occur in different environments. [Gleason, 1926, 17]

Taking the first of these objections first, Gleason in 1917 argues that even if units of vegetation are comparable to organisms, they are merely *like* organisms in these respects. They are not themselves literally organisms. So, the question is, What kinds of similarities are there?

> Various analogies may easily be drawn between a unit of vegetation and an organism, but these analogies are always more apparent than real, and never rise to the rank of homologies. For example, it is obvious that an association may appear on a new area, develop to maturity, and finally disappear, but these phenomena are nowise comparable to the life history of an individual. A spore of *Rhizopus*, for example, given the proper environment, will grow to maturity and reproduce without the presence of any living organism. The first pioneer species of an association, on the other hand, will merely reproduce themselves, and maturity of the association will never be reached unless its other species are also present in a neighboring area. Similar exceptions may be taken to all other analogies between the individual and the association, designed to demonstrate the organic entity of the latter. [465]

Gleason's argument here is that while there are similarities between vegetation [page 80] units and individual organisms, there are also dissimilarities, like the possibility of developing to maturity in the absence of other individuals. Plant associations require immigration of their components, while individuals can develop on their own, apart from other organisms. Consequently, associations cannot be organismic. This is indeed a disanalogy, though not a deep one. Notably, an individual plant cannot mature in the absence of components it can assimilate, either. The difference is just that, as a matter of scale, these components need not be organisms themselves in the case of the individual.

Gleason's use of this particular dissimilarity to object to Clements's analogy rises to the level of rejecting Clements's theory only if (a) Clements intends vegetation units to *be* organisms, rather than be comparable to them, and (b) treating units of vegetation as organisms makes a difference to explanations and predictions for vegetation. Otherwise, it works not so much a rejection of Clements's theory as a reining in of Clements's excesses in comparing seres to organisms. Yet, this move risks begging the question of how rhetoric can be distinguished from theories. In so far as the simile is offered as part of its author's conceptualization and presentation of the theory, it is questionable to disentangle them. But, Clements's comparison to organisms does not appear among his theory's resources for explanation and prediction, and it does not determine the properties of ecological units to which it has been applied. Consequently, I propose that Gleason be read, at least on this one point, as meaningfully criticizing the usefulness of the comparison. But to the degree this comparison can be disentangled from explanatory and predictive resources and ontological commitments, it is not yet in itself a rejection of the *theory*.

Much less attention has been devoted in the literature to the other five of these objections Gleason raises to Clements. Beyond the rejection of the climax, objection (2) concerns the enlargement of the unit of vegetation. Gleason's other objections, as I enumerated them, include a denial of the need for new terms to describe succession, arguing that Clements excludes some objections by definition, and complaining that Clements's theory attends inadequately to the consequences of variation. Each of these differences, like Clements's statements which suggest that climaxes obtain deterministically, arise from methodological differences between them over how best to represent causes and disturbing conditions. Clements needs more terms for describing succession than does Gleason, for instance, in part because he enlarges the unit of vegetation; and he does this latter as part of a quite different strategy for handling the complexity which *both* of them recognize in successional systems. As Gleason comments, "the great mass of ecological facts revealed by observation and experiment may be classified in different ways," and Clements adopts a physiologically-normal sequence as starting point for general theory organizing them, while Gleason leans towards probabilism (without developing general theory very far beyond leading suggestions) [Gleason, 1917, 464]. Among the observations differently organized into explanatory theory are the content of Gleason's objections (5) and (6). Each ecologist has a way of making sense of these observations that depends on how he assigns normalcy and variation to [page 81] particular states of affairs. In Clements's case, a physiologically-adapted sequence of vegetation is normal and variations from it are explained as departures due to environmental variation from average. In the probabilistic theory Gleason gestures towards (but does not develop), any probabilities would similarly need to be assigned to habitats chosen as normal. A move away from climax sequences does not alleviate the problem of explaining variation, even for a theory assuming radical individualism. The difficulty of handling variation is merely relegated to the project he does not explore — assigning probabilities. Probabilities can only be assigned theoretically in relation to ranges of conditions treated as normal.

To now reach the third step of the case, the extent of order recognized by the later Gleason, who by this point was increasingly working on taxonomy rather than ecology, is apparent in his ecological papers of 1936. In them he reveals an acceptance of the causal basis of Clementsian order. The strongest basis for Clementsian order lies in reactive causes. Recall that what structure a community has for Clements is produced by the action of four kinds of causes — initial, ecesic, reaction, and stabilizing. Initial and ecesic causes refer simply to environmental conditions creating openings for plants to establish, and the physiological characteristics of plants permitting them to establish, respectively. The action of initial and ecesic causes involves nothing not captured by Gleason's comment, for instance, that "all phenomena of succession depend on the ability of the individual plant to maintain itself and to reproduce its kind" [Gleason, 1927, 325]. Reactive causes are those covering the relationships among individual plants — Gleason's supposed denial of which offers the basis for the view that considers vegetation less orderly. Reactions are specifically, for Clements, those influences of individual plants on their environments that change their environments and thus the environments of other resident or immigrant plants. (In the special case where reactive causes favor the plants responsible themselves, like when a plant's offspring survive well in its own shade, reactive causes are called stabilizing causes, the fourth class.) Reaction is the force knitting the Clementsian community together as a unit. It is the class of causation supposedly missing in the Gleasonian Individualistic Community, or it is supposed to have vanishingly weak effects therein. Yet, Gleason points to the same phenomenon:

> Nevertheless, these plants [of different species] have definitely an influence on each other. To select perfectly obvious examples, it is clear that the larger plant affects the light and, though its leaf-fall, the soil environment of the smaller, while the latter intercepts rainwater and reduces the light for seedings of the larger one. The two plants have intersecting spheres of influence; each interferes with the environment of the other.... Intensifying the influence of either plant within its sphere has a direct effect on the life and well-being of the other. It may act either favorably or unfavorably. [Gleason, 1936a, 444–445]

That is the essence of reaction, of the sum of causal influence Clements holds is exerted among plants in an association or community. And indeed, Gleason uses [page 82] even the terminology of reaction in a different paper from the same year:

> It is probable that every species of plant, no matter whether its individuals are large or small, abundant or few, reacts on its environment

> in a manner peculiar to itself.... It also seems probable that the joint reaction of the whole population is one of the most important factors in maintaining the uniformity and the equilibrium, and therefore the identity of the association. [Gleason, 1936b, 44–45]

Moreover, here the reactions are part of the causal structure contributing to uniformities and equilibria of vegetation. As reactions function this way, all the classes of causation recognized by Clements are thereby asserted by Gleason, along with their putative effects. If so, the ecologists' differences on communities do not lie in their understandings of causal structure.

To understand the relationship between their theories, consider how you and I might analyze the disappearance of a sand castle on the beach.[16] Looking at one sand castle, we each notice it lightly eroded by the breeze, and then, because it was built especially close to the rising tide, the moment when its foundations are first degraded by a trickle lapping its base, before a large wave suddenly reduces it to a undifferentiated lump and it steadily thereafter declines to flatness under light foamy washes. Imagine that, asked separately by children what happened to their creation, we tell nearly identical stories, narratives involving the same series of causes and changes: roughly, breeze, location, base-erosion, splash, fade. Now further imagine that we offer similarly identical narratives to the children who built their castle up closer to the dunes, which lost its towers almost immediately to the high winds there, but otherwise remained intact surprisingly long into the evening before being trampled by teenagers. Imagine that if our particular stories about these particular castles differ slightly, they do not differ much. We recognize the same series of major causes and effects. But now imagine that we are asked for our theories of sand-castle disappearance. Our approaches for producing general theory at this level are underdetermined by our understandings of particular cases. Our sharing causal understanding is consistent with our adopting very different methods for assembling those classes of causes into generally-applicable theories. You propose to model disappearance as a function of location on the beach relative to water down low and wind up high. I build my model on the idea that disappearance has a range of probabilities aligned with time scales.[17] These approaches are starting points. We each still have to figure out what to say about teenagers. You may regard my approach as hopeless, and vice-versa. But to consider our understandings as opposite, to imagine that we disagree about how sand castles disappear, is to grossly underestimate our shared familiarity with those systems.

Yet, one might object that when Clements's and Gleason's theories are described [page 83] as opposite, what is meant is not differences in the analysis brought particular cases, but differences at the level of generalized theory. Clements's and Gleason's general models of communities are indeed very different. Gleason himself pointed

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[16] This example is original, but a similar one is used to different effect by [Jackson and Pettit, 1992].

[17] These modeling strategies are not supposed to capture features of Clements's and Gleason's, but simply contribute to the point that such strategies are underdetermined by causal understanding.

to the theories' relationship in remarking how the "great mass of ecological facts . . . may be classified in different ways" [Gleason, 1917, 464]. But though they raise occasional differences in emphasis like the degree of influence of water, the ecologists' differences do not lie in causal understanding. Nor do they lie in how they treat the nature of communities as things. Consider the robustness of communities in Gleason's account, and the resemblance to Clements's loose organisms:

> Since the first recognition of the plant community, irrespective of the name applied to it, its cause, or its scope, and continuing to the present day, the individual plant community has always been a geographic unit. It occupies space and has boundaries. Moreover, it exhibits uniformity of structure within the area. Extent, boundary, uniformity: these are the *sine qua non* of every community. [Gleason, 1936a, 447]

This combination of factors — extent, boundaries, uniformity — makes the community, to the degree it possesses them, an appropriate entity for a single explanation. "We must admit," Gleason writes, "that a *stand* of vegetation is a concrete entity" [Gleason, 1936a, 450]. His understanding of vegetation has it very far from chaos.[18]

5 POLARIZING NARRATIVES

This recognition produces a mystery, however: if Clements's and Gleason's understandings of the causal structure of communities are quite close, then, how did the legend of order and chaos become affixed to them? The purpose of this section is to develop an error theory, as J. L. Mackie has used the term, to mean an explanation for the persistence of mistaken ideas [Mackie, 1977]. We typically acquire our understanding of these scientists' theories from historical accounts written by historians and ecologists. My main error theory is that those have tended to emphasize intellectual inheritance of concepts and usage of terminology, at the expense of considering practices or methodologies, especially representational, predictive, and explanatory strategies. While absorption in language is no sign of bankrupt historiography *per se*, in this case alternate modes of historiography focusing on practice or theory-structure have been rare enough to distort our understanding. [page 84] Linguistic opposition has become theoretical opposition, beyond mere difference. To observe this, I begin with Worster's own history of ecology volume, and then

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[18] Malcolm Nicolson [Nicolson, 1990] and Nicolson and McIntosh [Nicolson and McIntosh, 2002] have developed the best existing analyses of Gleason's views and methodology, especially of Gleason's use of mathematics and how he understood the chanciness he occasionally invoked. That they do not revisit Clements leaves them repeating a stronger opposition between the two than I have argued exists. But Nicolson's is the one account I am familiar with that understands Gleason as not having been committed to radical, disordered individualism. I discovered this work on Gleason after developing the account presented here, and it is a welcome complement, as useful for further inquiry into Gleason as Hagen's articles [Hagen, 1988; Hagen, 1992] are for further inquiry into Clements, despite the differences I have with each author's account.

remark on two other historical accounts and on textbooks. (After developing this error theory, I resume the main line of argument in section 6.)

Worster's moral and political narrative

Worster develops the historical narrative behind the ecologies of order and chaos in *Nature's Economy: A History of Ecological Ideas* [1977, and in a revised edition as of 1994], which lends significant portions of two chapters to Clements and his ideas, and to Gleason only a brief appearance, on a few pages. In his account, Worster refers to Clements's science as "dynamic ecology," and associates dynamic ecology with the thesis that "the climax or adult stage [of a plant association] is the direct offspring of the climate"[19] [Worster, 1994, 295]. Following tradition, Worster takes Clements's association of climate with successional development to be coupled with a faith in a fairly strong determinism by climate or the character of a climax community in any place. And he cements that association by pointing to Clements's organismic simile:

> Just as physical maturation into adulthood is programmed into the genes of the child or seed, so the climax community marches toward an automatic, predetermined fate. Only in freakish circumstances does the process bog down at a subclimax level of development, a kind of arrested adolescence. [Worster, 1994, 211–212]

Read as an assertion about communities more generally, this sentence has Worster assigning to Clements's theory of vegetation the view that only in exceptional circumstances does vegetation fail to achieve climax, and likening the development of a community to the physical growth of a child, furthermore understanding the results of each as inevitable. For Worster, the view that climaxes are determined follows from treating plant communities as complex organisms. Worster writes, "undoubtedly the explanation for Clements' emphasis on the sere and its climax lies in his underlying, almost metaphysical faith that the development of vegetation must resemble the growth process of an individual plant or animal organism" [211]. Worster traces this metaphysical faith back to Clements's interest in Herbert Spencer's *Principles of Biology*, which treats society as a "social organism."

In Worster's estimation, Gleason's criticism is thus aimed at Clements's metaphysical faith in social organisms. It takes specific form in objecting to Clements's organismic analogy, and then by extension, to his assertion that successions culminate in climaxes. [page 85] Against Clements's organismic concept, Worster suggests Gleason offered the "'individualistic' view of nature" (beyond, apparently a thesis about

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[19] This, however, represents a category mistake, in that while Clements refers to his *branch of science* or approach to ecology as "dynamic ecology," meaning the study of ecological dynamics (and in his case, of changes in vegetation), this branch is distinguishable from any particular thesis advanced as part of his work in that area. McIntosh, for example, describes appropriate usage [McIntosh, 1985, 76–77]. Of course, Clements also appears to have had the ambition to synthesize previous work in this area into a foundational theory, and so he does consider his ideas foundational to this branch of inquiry. But Gleason's work is, if anything is, dynamic ecology, too.

vegetation). He rejected “rigidity” and a “formal concept of ecological dynamics” and “precise succession” for a looser account; “organized being[s]” for “haphazard, imperfect, and shifting organization;” and “carefully orchestrated” succession for “accidental groupings.” “More important,” Worster maintains, “Gleason’s ‘individualistic’ view of nature suggested that the climax was a haphazard, imperfect, and shifting organization — one that man need not worry overly much about disturbing” [Worster, 1994, 238–239].

In Worster’s broader narrative, the backers of order and chaos are motivated by their politics, by their views of conservation and technology more than vegetation per se. He reaches to find motivations for their views in divided politics, so that their positions not only have implications for conservation, but also have roots in different degrees of enthusiasm for it. Worster treats Gleason’s central claims as rejections of the claims he understands as the core of Clements’s theory — that plant associations advance towards climaxes, and do so as a feature of resembling organisms. Then, treating Gleason primarily as a critic, he ascribes to him the motive of rejecting what Worster thinks are “the anti-technology implications in the climax ideal”:

> There were a number of scientists, too, who found the anti-technology implications in the climax ideal hard to accept. From this objection, as much as from any purely scientific quarrel with Clements, there emerged in the thirties an ‘anti-climax’ party. Earliest to join issue with Clements on this point was Henry Gleason of the University of Michigan. [Worster, 1994, 238]

This first inspiration for objecting stands two steps removed from Clements’s theory of succession. It is one step removed because it arises from treating the climax sere as an ideal state, where “ideal” means something close to ‘desirable from a human perspective,’ as opposed to ‘physiologically ideal,’ which is clearly Clements’s usage [Clements, 1907; Eliot, 2007]. In so far as Clements expresses the implications of his theory for conservation, the achievement of a climax sere in any given area becomes a normative state of affairs — the appropriate and best outcome of vegetative development. In the theory of vegetative succession, however, the climax sere is merely a normal ending-point, in the same way protein-folding has a normal end-point, where the normative connotations borne by “normal” do not involve a claim about what ought to be, and consequently fail to bear in any direct way on technology.

Worster’s account of Gleason’s motive is further removed from Clements’s descriptive theory in that it concerns a potential *implication* of that additional claim about climax seres. Worster treats the climax sere as a normatively-ideal state of vegetation (a state, that is, which *ought* to arise), by appealing to Clements’s discussions of conservationism in non-scientific writing. He remarks that this understanding of “Clements’s doctrine of the climax as a natural ideal was by now firmly lodged in the national imagination”[Worster, 1994, 237]. From this assumption, Worster argues that the Dust Bowl of 1934 came to be understood [page 86] "in the American mind" as a negative consequence of permitting technology (tractors, combines, plows, etc.) to interfere with the climax communities of the prairie. Gleason is then supposed to have advanced his theory of vegetative succession as a reaction to this inferred consequence of an apparent implication of Clements's theory read (problematically) as normative [237–239].

However, Gleason's objections to Clements's theory do not mention this distant implication Worster considers his central stimulus. For support, Worster reaches to cite poet and journalist Archibald MacLeish as drawing these implications, in an article for *Fortune* on the American grasslands, though MacLeish refers there to neither scientist [Worster, 1994, 454, fn. 23]. MacLeish does conclude that agriculture must conform to local environmental conditions — especially soil — or it cannot succeed, landing on the poetic synecdoche that the plow can produce disaster [MacLeish, 1935]. But if nothing in Gleason contradicts this idea, and it does not appear in the articles Worster cites when referring to anti-technology implications, Gleason also mentions no objections to Clements other than to his descriptive theory of vegetation. While every scientific disagreement is embedded in the human world of scientists' concerns with reputation and ambition, support for the terms of this quarrel being not "purely scientific" is missing.

Tobey's intellectual-inheritance narrative

While Worster sketches the Clements and Gleason debate as a dispute over the politics of conservation and technology played out as a scientific debate over the metaphysics of vegetation, historian Ronald Tobey's *Saving the Prairies: The Life Cycle of the Founding School of American Plant Ecology, 1895-1955* treats the rise and fall of Clementsian vegetation-theory as a Kuhnian "microparadigm." Worster, too, renders Clements and Gleason central players in a paradigm shift, but Tobey works out the Kuhnian dynamics in much greater detail [Worster, 1990, 11]. He remarks that he takes a Kuhnian approach out of a desire to avoid the "embarrassing methodological fallacies" of conventional intellectual history, which has "isolated the major ideas of the Clementsian ecological theory and followed their development in Clements's published writings, prefaced by a reconstruction of their precursors and suffixed by their denouement in the hands of his critics" [Tobey, 1981, 6]. Though Tobey proposes that his alternative method of attending more to how many times scientists cite one another and to "the relationship between ideas and the social and material structures," can move his analysis beyond the "debilitating flaws in the history of ideas" into "the bracing wind of rigor and procedure," he directs relatively little attention to Clements's published writings, to the detriment of his account of them [6].

Tobey locates Clements's work as essentially in competition not with Gleasonian individualism but with ideas from the University of Chicago school of H. C. Cowles, [page 87] and (after denouncing conventional intellectual history) at the intersection of various intellectual traditions:

> By the end of the nineteenth century, two distinct approaches of expla-

> nation for vegetational change competed for advocates in the United States. One approach, which was to lead to Frederic Clements's mature work, *Plant Succession* (1916), was centered at the University of Nebraska and was the result of a formalization of the experience of Bessey's students by the theory of Oscar Drude and Clements's reading in sociology. [Tobey, 1981, 108]

Tobey understands Clements's account of vegetation as essentially an inheritance of diverse influences from his most important teacher at Nebraska, Charles E. Bessey. One of these legacies was Bessey's "pragmatism," closely resembling C. S. Peirce's and emphasizing direct experience with what is described over starting from known categories and assigning names. Such an approach arose in opposition to the German *Naturphilosophie* tradition unfolding from Goethe to Julian Sachs. But the origin of Clements's "organistic metaphors" is, for Tobey, Clements's possible reading of Herbert Spencer and Bessey's affinity with sociologist Lester F. Ward's liberalism. Clements apparently also drew from Bessey the "idealistic tradition" in botany giving ontological status to vegetative formations, inherited ultimately from the European Floristics tradition of Alexander von Humboldt, via Oscar Drude's plant geography. Meanwhile, he was also influenced by its opposite, a reductionist "mechanistic tradition" which denied the reality of vegetative units and was derived primarily from Darwin. Beyond these intellectual bequests from Bessey, Clements's major contemporary influence lay in competition with Cowles, whose "model was built upon a philosophical approach to vegetation quite distinct from that of the Nebraska scientists" — one derived from Danish botanist Eugenius Warming [111]. While Clements "explored [the grasslands] in terms of climatic formations," the Warming-Cowles approach did so "in terms of topographical and biological habitats"[110].

Yet, Clements's explanatory framework incorporates *both* the climatic formation and the topographical habitat. It attempts to assert an ontological framework to hang vegetation on, but does so in order to connect it, in a reductionist manner, with local, individual causes. That is, one can trace these themes in Clements's writing, but they offer little insight into scientific strategies. Tobey presents Clements's understanding of the vegetative formation as a combination of "two conceptions … — organism and population — [which] implied quite contradictory conceptions of development." The "organismic" concept "implied that development was caused by a major external cause" — climate — while the "population" concept "implied strong habitat influence on the development of vegetation," making climate "of secondary importance" [Tobey, 1981, 80]. If Clements's theory can be read as bearing traces of these ideas, this usage of "habitat" (in opposition to climate, rather than incorporating climate) bears no resemblance to Clements's own. As Tobey writes, "according to Clements, eight major physical factors controlled habitat conditions: water content of the soil, humidity of the [page 88] air, light intensity, air and soil temperature, precipitation, wind, soil class … and ground physiography" [Tobey, 1981, 72]. At least the majority of these factors are functions of climate; all are features of climate at least on some scales.

With respect to the development of the formation through time — which is to say, succession — Tobey again discovers intersecting concepts at the heart of Clements's approach: "Clements's vision was fundamentally ambiguous, expressing both an idealistic interpretation of growth and its contrary, a mechanistic model of change." But then also, "Clements's conception of change was typological . . . by providing a schematic growth in terms of distinct categories or types of being, with succession as the change of one type into another" [80]. Clements may again be read as heir to these traditions, but to do so means focusing misleadingly on his language. His "vision" is ambiguous only because rendered as a conflict of inherited concepts. Absent this story of inheritance, the confusion falls away.

So, beyond just mentioning the concepts Clements incorporates, Tobey also attributes some empirical claims to his theory by calling it a "monocausal (climatic) theory of formations," and imparting to this one cause deterministic influence over vegetative change [Tobey, 1981, 103]. He writes, "in Clements's *Structure* . . . a formation *had* to develop as the terminal climax of succession," [105] and that "development of vegetation towards its terminal climatic formations was always progressive. It could not permanently regress or stall short of the final form" [82]. So, "an essential principle of Clementsian theory was that every succession was headed toward a climatically caused monoclimax" [104]. Neither the term nor its associated concept, "monoclimax," belong to Clements. "Monoclimax" has been used subsequently by others to mean the idea that one, and only one, particular climax-type is determined in some area. And yet, Clements never asserts this.[20]

So with respect to the metaphysics of communities, Tobey, like Worster, treats Clements's organismic simile as central to his explanation: "adoption of the organismic model was not a matter of heuristic convenience for Clements, as it would become for A. G. Tansley, who in 1931 referred to the 'quasi-organism.' For Clements, the formation was ontologically as real as the individual plant or animal" [Tobey, 1981, 81]. This is a *non sequitur*; however much the moon might be "as ontologically real as" cheese, this does not entail literalism in their comparison. Whether the organismic simile is literal or not, the passages from Clements which Tobey immediately cites as revealing Clements's realistic (as opposed to pragmatic) usage of "organism" in simile and metaphor do not indicate one usage or the other. Tobey offers:

> Hence: '[Succession] is the basic organic process of vegetation, which results in the adult or final form of this complex organism.' And: 'All the stages which precede the climax are stages of growth.' As he stated in the second sentence of *Plant Succession*, 'As an organism the formation arises, grows, matures, and dies.' [Tobey, 1981, 81–82] [page 89]

Yet, the first and second quotations employ "organism" and associated terms metaphorically, and the third employs it as simile. Leaving the question open, neither wears on its sleeve the strength with which its comparison is intended.

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[20] See the argument against this misattribution in [Eliot, 2007, 94–97].

Like Worster, Tobey finds little role for Gleason's alternative views in his account of the rise and fall of Clements's. Partly, this is because he considers Oxford ecologist Tansley Clements's more significant critic [Tobey, 1981, 155–190]. But mostly, Tobey believes that the Clementsian "microparadigm" fell not to criticism, but to the "theoretical exhaustion of [its] intended paradigm examples," in the mould of Wolfgang Stegmüller's account of theoretical decline [216–219]. Specifically, Clements and Clementsian ecologists relied most strongly on the North American grasslands as examples of succession, but the apparent climax community of the grasslands was replaced by other organisms like *Opuntia* cacti after facing the Great Drought of 1933–1941. Without access to this paradigm example, newer ecologists were not convinced of the theory built from it, and the ascendant "range management literature" of 1947–1955 ceased to cite Clements and his allies.

Tobey does briefly treat Gleason's 1917 and 1926 essays, and considers the "key proposition" of the 1917 article to be the rejection of the plant association's status as organic entity, which Tobey analyzes as untenable. But then, he attributes a radical ontology to Gleason:

> In Gleason's universe, therefore, there were only individual organisms (and, presumably, physical objects). This position was philosophically untenable, as any nineteenth-century idealistic philosopher could quickly have shown, but Gleason was no more a professional philosopher than Clements or Tansley, and whistled his tune, oblivious to the cemetery of buried doctrines similar to his. [170–171]

If this is an argument that an eliminativist metaphysics is untenable, it depends on one's assent to core ideas of German Idealism, ideas neither then nor now standard issue for scientists. Gleason *could* maintain that there are only individual objects and organisms; the more interesting question is what kind of ecology can be done on that basis. Tobey takes Gleason's theory to suffer from other metaphysical difficulties, suggesting intriguingly that Gleason's later commitment to the existence of species shares ontological problems identical to those threatening Clements and Tansley. But we do not learn more of the substance of Gleason's ecology, probably because Tobey considers it internal to the Clementsian microparadigm, if nonetheless critical of it.[21] So, Clements appears in Tobey as essentially sponsor of an organismic representation of vegetative processes; Gleason appears as essentially its co-paradigmatic detractor. Gleason's three other central criticisms from 1917 are disregarded.

Tobey's construction of the Clements/Gleason opposition thus arises from several quirks of his attention to it. He attends primarily to the authors' inheritances of terminology without heeding the terms' specific meanings and roles in their new theoretical contexts. Clements and Gleason patently employ contrasting language, but that state of affairs is something quite different from their having opposite causal understandings of the systems they both study. Tobey reveals [page 90]

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[21] "Although one internal critic, H. A. Gleason was highly cited . . . " [Tobey, 1981, 140].

little of their understandings, though one is left with the impression of familiarity with the theories via their language.

McIntosh's conflicting-concepts narrative

Ecologist Robert P. McIntosh also characterizes the theoretical differences between Clements and Gleason in his retrospective discussion of Gleason's career, "H. A. Gleason — 'Individualistic Ecologist' 1882-1975: His contributions to ecological theory." Though Gleason, rather than Clements, is therefore his central subject, McIntosh characterizes their relationship along much the same lines as Worster and Tobey do.

McIntosh treats Clements as primarily endorsing "the rather extreme position that the successional development of a community is comparable to the development of an individual organism" [McIntosh, 1975, 259]. And again, the development of vegetation within a vegetative community is deterministic, with an inescapable terminus fixed by climate: "A key element of Clements' concept of vegetation was that succession was always progressive to a single climax association under the control of the regional climate." In contrast, Gleason is treated as dissenting from this rigid determinism. McIntosh writes, "Gleason, along with W. S. Cooper and others, dissented from the rigid Clementsian concepts of succession .... Thus, he clearly came out against the monoclimax concept proposed by Clements and endorsed a much less rigid view" [255]. The insight behind Gleason's rejection of Clements's account of vegetation was that he "was, more than most of his contemporaries, impressed by the heterogeneity and variation of vegetation both in space and time" [261]. Clements, endorsing the view that vegetation must always progress towards a single climax type, along a determined path, was less sensitive to the reality of change in nature:

> Gleason wrote that as early as 1908 he became convinced that succession could be retrogressive, and that the Clementsian concept of succession, as an irreversible trend leading to the climax, was untenable. He, of course, allowed that succession was influenced by climatic change, while Clements presumed stable climatic conditions. [255]

This is much the same account of their differences that we find in Worster and Tobey, offering undefended attributions.

Even in this retrospective account of his career contributions, Gleason's theorizing appears mostly as criticism of a Clementsian ecology committed to universal, fixed determinism: "Gleason contributed little in the way of detailed studies of succession, but his consideration of succession effectively resisted too rigid a formalization, and his early ecological instincts appear sound, even conventional, by [page 91] today's hindsight" [McIntosh, 1975, 256]. Again, the criticism is of rigid formalization. The positive thesis McIntosh finds among Gleason's "avowedly heretical ideas" amounts to an individualistic explanation of vegetation [261]. McIntosh claims, "Gleason's most significant and most lasting contribution to ecology was his 'individualistic concept.' It persists in the current research literature and recent textbooks of ecology as one of the basic tenets of modern ecology, although it earned him little credit when he propounded it" [258]. The individualistic concept suggests that variable conditions, and varying sets of organisms will produce differing developmental sequences for different areas of vegetation. The individuality of each such process, arising as a consequence of the idiosyncratic activities of its individual components, creates variation in possible outcomes beyond what Clements recognized. McIntosh writes, "each area, he said, is a resultant of a unique mixture of migrants, environment, and historical sequence, and there are no grounds for recognizing one as normal and typical" [261]. This attributes, I believe, two theses to Gleason — that no association is normal to any area, and that associations do not have recognizable identities.

McIntosh does, however, offer the more novel insight that Clements's and Gleason's accounts of vegetation reflect their different modes of thinking, and that this is even perhaps their key difference. So, quoting A. O. Lovejoy's comment from a different context, he summarizes, "there are not many differences in mental habit more significant than that between the habit of thinking in discrete, well-defined class concepts and that of thinking in terms of continuity ... " [McIntosh, 1975, 270]. I think that, after characterizing Gleason as more sensitive to variation than Clements, McIntosh emphasizes their differences more in terms of habits of thought than causal or ontological structure, because he has recognized earlier in the article, if more quietly, the closer relationship than his account overall admits between Clements's and Gleason's views. For instance, he observes that Gleason, while emphasizing variation, accounts for uniformity of vegetation (where it appears) as a consequence of the actions of similar causes:

> Under the individualistic concept, [Gleason] said, the fundamental idea is 'the visible expression, through the juxtaposition of individuals, of the same or different species and either with or without mutual influence, of the result of causes in continuous operation.' He noted that similar juxtaposition of plants is simply due to the similarity in contributing causes. [255–256]

But if so, the difference between Clements and Gleason becomes much more a matter of emphasis than McIntosh concludes. And further, Gleason's account is shown to treat one and the same phenomena under different terms mostly as a consequence of constructing the boundaries of the successional process differently:

> Gleason's view of succession between vegetational provinces had its counterpart in Clements's concept of the clisere; and he differed from Clements in including interformational sequences as successional, whereas Clements regarded succession as proceeding to a climax determined by stable climatic conditions. [255–256]

[page 92]

Clements at least in this instance appears to be equally aware of variation in vegetation, but to have accounted for that variation differently. And this, again, reveals weaker difference between their accounts than McIntosh's conclusions indicate.

McIntosh presents another account of Clements a decade later, in *The Background of Ecology: Concept and Theory* [1985]. Here his depiction of Clementsian ecology is more sophisticated, to the degree that it avoids explicitly attributing “monoclimax” determinism, and treats Clements as primarily concerned with measuring and representing change. Rather than emphasizing a deterministic association of climate with climax, he notes Clements’s statement of a “‘universal law,’ that ‘all bare places give rise to new communities except those which present the most extreme conditions of water, temperature, light or soil.”’ McIntosh treats this generalization not as a deterministic law but as a *ceteris paribus* law: “In either case, ‘except’ could be followed by the phrase ‘where it does not’ with equal validity”[McIntosh, 1985, 79]. This is at least an account of the generalization closer to Clements than we find elsewhere.

But note that this generalization is not the same one that others attribute as deterministic to Clements — that being a deterministic connection between climate and climax — as it predicts for bare places *some* new community, and not any particular one (e.g. the expected climax). On this second matter, McIntosh describes the cause and effect relationship Clements asserts for habitat and climax, but immediately suggests that for Clements, “the ‘historical fact’” is also explanatorily significant, remarking even that “Clements was the most explicitly philosophical and historical thinker of the early plant ecologists” [McIntosh, 1985, 78]. McIntosh includes more about Clements, some of which follows Tobey, but to mention just one of his other observations, he emphasizes like others that “the essence of Clementsian theory of vegetation was that the plant formation was a ‘complex organism’ and, like an individual organism, it changed not in haphazard ways but by progressive development” [80]. And McIntosh thus fills out the organismic concept as an assertion of deterministic development, remarking that “later ecologists sometimes seized on *development* to avoid the presumably rigid deterministic connotations of *succession*” [80]. Interestingly, McIntosh also believes that “the entire premise of Clements’s dynamic ecology was that the ‘seral stages’ of a series of populations or groups of populations followed in sequence,” an idea which appears in textbooks as Clements’s central testable claim [82].

McIntosh’s characterization as a whole (which includes more than I have included here), is closer to Clements than most others except Hagen [1992]. More than others, however, it is not so much an integrated account of his theory as, in the style of a scientist surveying literature, a catalog of various ideas attributed to Clements. Accordingly, McIntosh does not illustrate any reasoning logically connecting the idea that seral stages following in sequence is “the entire premise” of Clementsian ecology, for instance, with the others he attributes to Clements. He does not, for instance, connect it to the suggestion that the formation is an organism, [page 93] or to Clements’s assertion of climax, or with the treatment of his quadrat method, or with other points. So while McIntosh’s account is somewhat more accurate than others, this may be in part because it does not attempt to provide a unified characterization of Clements’s approach to representing vegetation so much as a few disparate observations of it. In summary, then, while McIntosh observes that Clements includes a significant historical element in his explanations, he still describes Clementsian succession as essentially deterministic, and this as a function of its organismic structure. What is emphasized is Clements's unusual language, and speculation about what metaphysics it invokes; what is missing is Clements's reasoning behind it, such as would help make sense of the terminology's role.

Textbook narratives

Ecology textbooks have offered similar narratives. Michael Begon *et al.* treat Clements and Gleason under the heading "The Problem of Boundaries in Community Ecology," and characterize Clementsian ecology with the organismic simile: "Clements (1916) conceived of the community as a sort of *superorganism* whose member species were tightly bound together both now and in their common evolutionary history. Thus individuals, populations and communities bore a relationship to each other which resembled that between cells, tissues, and organisms." Gleason's contribution is thus of course identified in contrast as "the *individualistic* concept," remarking that he "saw the relationship of coexisting species as simply the results of similarities in their requirements and tolerances (and partly the result of chance)." They attribute to the individualistic concept the implications (rather than the justification) that community boundaries may not be distinct, and that "associations of species would be much less predictable than one would expect from the superorganism concept" [Begon *et al.*, 1990, 627]. Clements is mentioned in one other segment, in association with his "rather extreme *monoclimax* theory." They write that "Clements argued that there was only one true climax in any given climatic region," and characterize it as an extreme view by virtue of both endorsing the existence of the climax and considering a single type strongly determined [Begon *et al.*, 1990, 646, italics original].

In another popular ecology textbook, Robert Ricklefs lends significantly more space to each ecologist. He devotes a section to "the holistic concept" and "the individualistic concept" of community structure, where the former suggests that parts cannot be understood independently of the whole, and that "a community is much more than the sum of its individual parts" [Ricklefs, 1997, 500]. But interestingly, he leaves these views unattributed, though Clements and Gleason are the first scientists mentioned afterwards, three pages later. There, Ricklefs treats Clements as "the most influential advocate of the organismal viewpoint," understanding communities "as discrete units with sharp boundaries and a unique organization [sic]." Gleason, he notes, rejects these claims and suggests that a community is not "a distinct unit like an organism." Ricklefs ties, then, the [page 94] organismal view and its rejection to the open and closed views of community organization; the organismic view suggests that "the ecological limits of distribution of each species will coincide with the distribution of the community as a whole," which is to say, closed community structure. Gleason's view suggests the opposite [503]. Ricklefs also treats Clements in association with "the concept of climax as an organism," even quoting the first paragraph of Clements (1916) which mentions both the organismic simile and the climax formation, before tying Clements to "the monoclimax theory" [529–530, 538]. Finally, as I mentioned earlier, he sets up R. H. Whittaker's recognition of communities grading into one another against Clements's account of communities as having discrete boundaries.

If the positions comprising this episode have been inaccurately radicalized to polar opposition, despite Gleason supposing a kind of order and Clements recognizing a degree of disorder, I suggest the polarization has been a function of the prevailing historiographic approach taken towards the episode. This approach has paid attention to the theories' language at the most general level, then interpreted their explanatory approaches by speculating about the language's connotations and linking it to intellectual traditions which employ similar language and concepts. It has inferred explanatory approach from similes rather than causal claims. Insofar as metaphors can contribute to understanding, nothing is intrinsically wrong with such historiography, but it can seriously mislead when employed to make inferences about explanatory strategy where actual strategy is ignored. This is a general danger for science studies, one reflected in egregious conclusions drawn elsewhere, too.

But it is not just historians' attractions to language and metaphor which can foster such distortion. Philosophers approaching scientific episodes have frequently approached them with an eagerness for conceptual analysis, foregrounding terminology. If philosophers have occasionally made contributions to science by clarifying concepts and revealing confusions, their disposition for linguistic analysis has also contributed to concepts themselves being centerpieces of theories alongside laws [Hempel, 1966, is just one of many endorsing such foregrounding]. Where philosophers have repeated the polarizing narrative, it may be in part a function of this habit.

Because scientists themselves have also treated these ecologists as opposites, the preceding discussion of narratives supports an error theory. Another contributor to the mistake besides that historiographic tendency is the typical rhetorical polarization of debates in the theoretical disciplines, as repeatedly illustrated by Sharon Kingsland in *Modeling Nature* [Kingsland, 1995]. But if rhetorical and linguistic polarization in the debate has contributed to the opposition legend, it also obscures the full range of intermediate positions held during the period. Though Tobey characterizes the alliances among ecologists in terms of adherence to paradigm, ecologists continued through the early twentieth century to lament the diversity of nomenclature which reflected diverse understandings, like Moss did at its opening.

6 MULTIPLE COMMUNITIES

Now, with the error theory in hand, I turn to considering the implications for philosophy of ecology of recognizing the significant common ground between Clements and Gleason. This essay has not argued for or against their views, nor for nor [page 95] against the existence of communities, but has sought to contribute to the discussion of *criteria* for community-recognition. At varying distances in the background has been a motivation for developing such criteria: the question of whether communities are preservable. While philosopher Kim Sterelny has discussed community criteria independently of the preservation question, Jay Odenbaugh and Kristin Shrader-Frechette and Earl McCoy, at least, have brought preservation to the foreground [Shrader-Frechette and McCoy, 1993; Shrader-Frechette and McCoy, 1994; Sterelny, 2006; Odenbaugh, 2007].

Recall Worster's Clements-derived features of communities discussed in section 3: having equilibrium, being perfectly predictable, and having holistic dynamics including emergent collectivity.[22] Callicott similarly points to Leopold's Clementsian vision of communities as stable superorganisms. If they were to exist, Clementsian communities, in the various senses of the term, would be preservable. But what has been packed into being "Clementsian" for a community in the accounts just described makes them more complex than we have evidence for any community being, and more than almost any ecologist asserts.[23] In the line used as this essay's epigraph, E. Lucy Braun, prominent American botanist and president of the Ecological Society of America during the 1950s, remarked: "no serious student of succession (a process) has ever claimed that a succession is made up of 'discrete units'" [Braun, 1958].[24] Even the strongest proponent of communities as holistically-organized units, South African ecologist John Phillips, neglects to argue for sharp boundaries in space or time [Phillips, 1935b; Phillips, 1935a; Phillips, 1931]. In a series of discussions of the organism concept, Phillips argues for — or at least describes and asserts — emergent holism, a view that communities are wholes with properties independent of their parts. His argument for this lies in the unpredictability of properties of wholes from the properties of their parts:

> Very briefly and generally stated, it is the view of the authors and disciples of this concept that there is a creative synthesis and emergence of properties, structures, forms, stages or levels; such newness, springing from the interaction, interrelation, integration and organisation for qualities — whether these be inorganic, organic, or psychic — could not be predicted from the *sum* of the particular qualities or kinds of qualities concerned; integration of the qualities thus results in the de- [page 96]

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[22] Relatedly, but much more concretely and modestly, Sterelny offers as a criterion possession of "causally salient, functional properties," specifically top-down causal dynamics, such communities themselves "play a role in determining the presence, abundance, and fate of the populations out of which they are composed" [Sterelny, 2006, 216, 217].

[23] That is, there may or may not be communities with internal dynamics of the sort I have attributed to Clements's communities; it is unlikely that there are communities with the internal dynamics typically attributed to Clements's communities.

[24] One should be concerned that this not be a tautology instantiating the No True Scotsman fallacy, ruling out a class of scientists believing in discrete units as "unserious students," but it is clear in the article cited that Braun regards Clements, at least, as, though wrong, very much a "serious student of succession."

> velopment of a whole different from, unpredictable from, their mere summation. [Phillips, 1935b, 489–490]

Yet, oddly Phillips leaves the key premise that there are such unpredictable properties undefended. His three-part article is concerned with "an analysis of concepts," and examination "of the views of certain workers" rather than contributing significantly to their defense [494]. In the analysis of concepts, however, what does become clear is that this emergence is a function of holism. That is, it is not the parts of a community which work together to produce emergent phenomena; instead, wholes themselves are causally efficacious (or, at a further level of abstraction which makes sense of an explicit reference to Plato, the *holism* itself is): e.g., "it should be plain that [emergence, holism, and the complex organism] are inherently related: holism the causal factor: emergence arising from this factor: the complex organism an integration of emergents, of wholes of potential development, to a yet more efficient whole" [Phillips, 1935b, 494]. Here is Phillips's most vivid difference with Clements. For the latter, wholes are not the cause of successions. Instead, the four classes of causes linked to plants themselves, as discussed above — initial, ecesic, reactive, and stabilizing — produce successions.

Significantly, Phillips does not attempt to defend these views from empirical results, nor from their being needed to make sense of empirical results. Nor does he attempt to employ them to explain biological systems. Instead of being motivated by research, Phillips's endorsements appear to have been motivated by an antecedently adopted philosophical holism. Noting the philosophical influence on Phillips of South African politician General Jan Smuts, historian Peder Anker describes Phillips as "having fashioned himself as a follower of Smuts's philosophy of holism," and then having "transferred Smuts's theory of the evolution of personalities and wholes into the natural world" [Anker, 2001, 134]. Phillips described his "holistic attitude" as revealing in a "spiritual experience" "the greater truth that ecology is an attitude towards facts and their meaning" [Anker, 2001, 146]. The terminology does not appear accidental when Phillips refers above to advocates of community-holism as "disciples of this concept." That is, the one prominent example of an ecologist endorsing the sort of view typically attributed to Clements does not involve arguments from evidence (in contrast to the substantive methodological program Clements pursued).[25]

Consequently, if ecologists, or at least the mainstream of ecologists working towards causal theories of vegetation, did not even in this primitive period of ecology understand communities as discrete units governed from the top down, [page 97]

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[25] If it complicates the wedge just driven between Phillips and Clements somewhat to note that Clements expresses enthusiasm for Phillips's discussion of climax vegetation and the complex organism concept — "This characterization has recently been annotated and confirmed by Phillips' masterly discussion of climax and complex organism, as cited above, a treatise that should be read and digested by everyone interested in the field of dynamic ecology and its wide applications"[Clements, 1936, 262] — that wedge is robust so far as Phillips and Clements are bound by shared philosophical orientation rather than anything resembling shared causal and explanatory theory. Phillips developed none, and his holism never seriously influenced explanation in community ecology, even if Clements expressed enthusiasm for Phillips as an ally.

it is foolish to establish a criterion for conservation to meet which demands that. Such a criterion would extend past what even the scientists describing communities have thought of them as meeting. In trying to understand what parts of nature are preservable, we should not establish demanding requirements for ecological entities without good arguments for those requirements. As "community" in ecology's sense is a term developed by scientists, the question of whether one of them is preservable turns on fixing what one is or would be, for scientists.[26]

In his own conservation writings of the late 1930s, Clements notably does not accept that conservation can be undertaken only if or because communities are discrete, holistic entities [Clements, 1949a; Clements, 1949c; Clements, 1949b]. He repeatedly stresses that the relevance of his theory to conservation lies instead in its predictive power. While the theory he develops in *Plant Succession* does not make falsifiable predictions about what plants will appear where (because it recognizes the commonness of disturbance at a variety of scales and the possibility that best-adapted plants are not present), it provides a foundation for the rarely-discussed work in *Plant Indicators*. That volume develops strategies for drawing inferences from known relationships between climate/habitat and physiology, together with observed vegetation [Clements, 1920]. This work suggests how one can infer predictions about past vegetation and climatic conditions from present observations. One can use this data to make future predictions, too. For example, sowing seeds of desirable plants on dry fields will often not restore those plants to an area, as when they are late-successional species they will tend to be out-competed by early-successional plants attempting to colonize them. One can learn, on his view, by observing both disturbed and less-disturbed instances vegetation what *sequences* of species are capable of thriving in the area, physiologically. And this investigation will not always arrive at particular native species of plants, either. For landscape restoration, non-native species may even be useful for aesthetic reasons or for creating the conditions under which desirable species (sometimes non-native) grow best [Clements, 1949c, 276]. Through his preference for native species, Clements remarks that

> while a natural treatment presupposes the use of species and communities in the regional association or faciation, it also permits modification and enhancement consistent within its limits. The process of succession by which nature reclothes bare areas is to be utilized as the chief tool in landscaping, but the process is often to be hastened or telescoped to secure more rapid and varied results (Plate 70). [276]

The adjacent photographic Plate 70 brings the point home by depicting non-native "tamarisks planted along highway for ornamentation and shade." [page 98]

None of this supposes that communities are holistic entities. Highway roadsides are prime areas for ecological restoration in Clements's view, for instance,

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[26] There is more to this project than I take up here, as I have focused on early community ecologists' understanding of these groupings. While their concepts have enduring currency, as I take up below, this analysis should complement further investigation of contemporary usage.

despite amounting to artificial swaths through landscapes. These human-planted, arbitrarily located and bound spaces invite conservation, that is, subject just to the lesson that plants which cannot outcompete others in local conditions will not remain there long, and will frustrate a restorer. The sense in which communities are preservable or restorable in these spaces (as opposed to component species *per se* being preservable) arises from the facilitative and inhibitory dynamics which take place among species living in the same area. Further, under most natural circumstances late-successional species, for instance, cannot thrive without being preceded by earlier-successional organisms, so that many plants can only be preserved under conditions normally contributed by other plants. This requirement is not due to magical, holistic connections among native species: non-native species are entirely substitutable for native ones so long as they are sufficiently physiologically similar to change abiotic habitat conditions for their neighbors in similar ways. That is, for Clements, plants can be restored or preserved only when and because their dynamics are preserved. Other features traditionally attributed to holistic communities are not presumed in his conservation writings, though he employs that terminology in them at times to indicate interactions.

Stripped of scientific respectability, causal holism recedes in plausibility as a necessary criterion for community-recognition. At least, a burden of justification is thus imposed on defending that criterion. Absent such holism, we are left with causal interactions among plants and their environments as a neutral starting-point. This returns us to the question of criteria for communities. Jay Odenbaugh has recently defended realism about communities by appealing to them [Odenbaugh, 2007]. He sets out three community concepts of increasing strength, assigning them in turn to the three ecologists, Gleason, Hutchinson, and Clements. In his scheme, a Gleasonian community is “a group of species in a particular area and time” [631]. It is distinguished from stronger community concepts by not requiring that to form a community, constituent populations need to interact causally. Immediately on introducing it, however, Odenbaugh argues that the Gleasonian community concept faces an “$(n+1)$th problem.” That is, under this concept it is indeterminate for any collection of n populations (of more than one species) in an area, whether an additional population is part of the community. “Particular area and time” is loosely defined enough to include any given area and time that can be specified, and the concept offers no further criterion determining whether any additional population is part of the community. The $(n+1)$th problem becomes a difficulty for the Gleasonian community concept because it permits any given assemblage of populations to count as a community [632]. It is too inclusive.

Odenbaugh recommends that the $(n+1)$th problem is avoided by rejecting the Gleasonian concept and adopting one of two stronger community concepts. The weaker of the two alternatives is his Hutchinsonian community concept, named for ecologist G. Evelyn Hutchinson. Odenbaugh’s Hutchinsonian community is “a [page 99] group of species that at least weakly interact with one another and not others at a time and through time” [Odenbaugh, 2007, 633]. It adds weak interaction to the Gleasonian community. Then, Odenbaugh's stronger alternative is the Clementsian community. This is "a group of species that strongly interact with one another at a time and through time" [633]. Odenbaugh's Clementsian community differs from his Hutchinsonian community just in requiring "strong" rather than "weak" interactions. Unfortunately, strong and weak interactions are not further characterized. Surely causal influences come in degrees, and some are stronger than others. But since for Clements all interactions among plants are indirect and intersubstitutable, there is little conceptual space to articulate a Hutchinsonian concept weaker than that. If one takes that to be the Hutchinsonian concept, and makes Clements's stronger, one is invoking a criterion without basis in predictive and explanatory ecological theory.

Whether via the Hutchinsonian or Clementsian community, Odenbaugh takes interactions among populations (whether they are weak or strong interactions) to answer the $(n+1)$th problem. However, interactions on their own do not solve it. Few organisms on earth, if any, live without becoming benefactors or beneficiaries of habitat modification for or by organisms of other species. Thus, interactions alone do not offer a basis for differentiating particular communities from the global community of all organisms (or nearly all organisms, if there are some extremophile species, for instance, which live and die independently of others). An appeal to interactions does prevent a population having no causal interactions with others in a community from being part of it, from being a "+1" for it. Yet this appeal allows the addition of any population causally connected in space and time, and thus licenses treating as communities any of the full series generated by successively adding populations to any population, up to the global community of all organisms. That is, interactions answer the $(n+1)$th problem, but only at the cost of embracing extreme promiscuity about community-identification. A more significant cost is that such communities are not robustly preservable. Removing a single species-population from a community (assuming the community can endure for some time with the same species-mix in the absence of that species) would often leave one with a community rather than something relatively impoverished. For nearly any community of n population, its $(n-1)$ will still be a community.[27] Thus, one could continue removing populations from the community and still be left at each step with a community. This is not to suggest that single populations *can* always be removed without losing other populations, like when a community loses one of the dozens of species of migratory birds eating its small flies, leaving everything else intact. Rather, the point is that "interactions" are insufficient for solving the $(n+1)$th problem in a way that helps in contexts like conservation. After all, it is considering contexts like conservation that gives the $(n+1)$th problem its bite. [page 100]

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[27] This works unless we have picked a population which is the only bridge between two others. Since interactions include anything affecting the living conditions of other species, interactions are so ubiquitous that that is the exception rather than the rule. This is an empirical claim which could turn out false and weaken the point. But this exception applies only when there is no population which can be individually removed from a community leaving the others intact.

Though he does not adopt either one of them in particular, Odenbaugh's own view of communities does not add criteria to his Hutchinsonian or Clementsian concepts. He offers: "species populations form an ecological community just in case they exhibit community interactions, or put differently, they possess a community-level property" [636]. Then, he defines a "community-level property" entirely in terms of causal interactions, so that it becomes just another way of referring to them: a community-level property is "any causal biotic relation between two or more species" [636]. "Biotic" might do some work to restrict the class of causal interactions, but since interactions among plants are typically mediated by abiotic resources like nutrients and water, and these are included among causal interactions Odenbaugh recognizes, "biotic" adds no further restriction on causal interactions.

It is sensible to take causal interactions as a starting point for community definition, as Odenbaugh does; we attribute a status to ordinary objects like desks that we do not attribute to smoke rings, because of the strength and persistence of causal interactions among the molecules of the former. But, once we recognize that the denial of causal interactions is a position not advanced in ecology, a view or "concept" merely asserting their presence becomes banal. Responding to the point that the Clementsian and Gleasonian concepts might not have been articulated as such, Odenbaugh offers that whether or not these concepts are historically accurate, "critically engaging the stereotype serves a valuable purpose" [Odenbaugh, 2007, 629]. But recognizing interactions alone does not contribute substantially to defending robust realism for ecology or providing an ontology for ethics and conservation. Resisting unrestricted mereological composition, Odenbaugh asks, "do we really want to be ontologically committed to the existence of an object composed just of my left foot, Lewis and Clark College, and Sevilla, Spain?" [631]. No, indeed, there are not apparently any purposes for which that is an interesting object. Yet, the bare existence of interactions, without any further characterization of what kind they are, does not reveal why that object is in distinctly worse shape than the collection of, say, Drake Passage Wandering Albatrosses, anchovy populations off the coast of Peru, and the population of occasional patrons of DiFara Pizza in Brooklyn, New York (some of whom eat anchovies). These populations are joined by causal biotic interactions, and thus should form a community on Odenbaugh's standard, but the same rhetorical question applies: Do we really want to be ontologically committed to the existence of an object composed of Wandering Albatrosses, Peruvian anchovies, and DiFara Pizza patrons? The considerations invoked as weighing against the former by Odenbaugh's rhetorical question weigh equally against the latter being an object, and therefore against it counting as a community (and rightly so). Moreover, even if one were to disagree that the same considerations weigh against each, the existence of my distributed object would not be sufficient for the main purposes for which some philosophers have sought a community concept, namely defending ecology's success in relation [page 101] to scientific realism or providing a suitable ontology for environmental ethics and conservation. That is, causal, biotic interactions alone do not get us there; they are not enough to establish communities.

Particular kinds of causal interactions, however, *are* important to efforts to preserve biological units. Dependencies are basic facts of life. It is easy forget that we human beings do not survive long without environmental oxygen, or outside a narrow range of temperatures. Consequently, dependence relations are significant to conservation. The degree to which species depend on one another has been a motivation for discussing preservation of communities rather than just species (as the US Endangered Species Act of 1973 has been employed). For example, keystone predators are those on whom the persistence of a number of other species depends. The loss of wolves in an area can cause a trophic cascade in which species composition of an area is radically altered [Hebblewhite *et al.*, 2005, for instance]. So, conservationists need to attend to this particular kind of causal interaction — dependence — and may refer to organisms connected by such interactions as "communities" in this sense. Dependence relations among populations are sufficient for their becoming potential targets of conservation interest, because such dependence relations causally affect the outcomes of trying to preserve some collections of organisms rather than others.[28] Groups exhibiting obligate, non-intersubstitutable dependencies are sufficient to a further kind of conservation interest. And, if we add the desideratum that we are interested in preserving groupings which can only exist as such, dependence relations become necessary, too.

On the other hand, if our interest is, just to take an example near at hand, a Clementsian approach to long-term vegetation explanations or forecasts,[29] dependence relations are neither necessary nor sufficient for identifying communities. We must instead identify all the populations in an area with significant competitive interactions. A strategy for identifying communities that misses populations engaged in these interactions will fail to capture causally significant influences, and thus fail to capture the relevant kind of communities. Attending to dependence relations among living populations is not sufficient for picking out communities for this purpose. Furthermore, three populations competing for a common abiotic resource with no dependence relations among them count as a community under Odenbaugh's definition. Odenbaugh rightly lists exclusively-competitive interactions among the appropriate relations binding populations together with others. Dependence relations are not necessary for picking out communities for the purpose of long-term forecasting, just as they are not sufficient. So, what is necessary and sufficient for one kind of community is neither necessary nor sufficient for another kind.

The upshot of comparing these two sample projects for which one might employ community-criteria is that while communities may trivially require interactions among their constituent populations — Odenbaugh's criterion — that is not [page 102]

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[28] That is, so far, not to say that anyone should be interested in preserving such groups, only that they now have properties making them something other than random assemblages, and are thus sufficient to a certain interests in groups.

[29] This applies just as well to interest in a number of contemporary approaches like David Tilman's resource competition theory [Tilman, 1982; Tilman, 1988].

enough to support realism about communities *per se*. So, what is enough? That depends on what we want to identify communities for. 'What are the criteria for communities?' is thus not a productive question for philosophy at that level of generality. Various further interests (conservation, realism, prediction) determine different particular criteria for communities such as can fulfill those interests. Whether collections of populations comprise communities will vary depending on what we need communities to be. Especially as philosophers investigate communities in order to address these further interests, we should thus not attempt to identify what communities are *per se*.

Importantly, this is a different result than the claim that communities are fictions. Every ecologist, including Gleason, recognizes interactions among organisms, including that some require others, to survive. But how collections of interacting populations are rightly attributed a further status depends on both what kinds of interactions are in nature and what that status is. This is also a different result than that there is nothing more for philosophy to say about communities. Particular interests in communities produce puzzles about kinds and strengths of causal interactions and how best to recognize and talk about them. There is a live discussion of the possibility of top-down causation, for instance [Mikkelson, 2004; Mikkelson, in press; Sterelny, 2006], and whether any kind of holism makes sense. But these questions about the features of communities are where the action is, not the general question of whether communities exist.

7 CONCLUSION

This essay has sought to puncture the legend of Clements and Gleason, and along with it another legend it has supported, one which has in turn motivated keeping it alive — the legend of order and chaos. Clements did not treat vegetation as developing like birds and mammals do, nor as sharing many structural commonalities with organisms, like holistic, functional integration or discrete boundaries. Gleason did not treat vegetation as composed of individuals unaffected by their neighbors or difficult to group into robust collections. The ecologists agree that plants are affected by their environments and affect one another indirectly, and that those are the only kinds of causal relationships on which further theory can be built. As they recognize quite similar interactions, they depart from one another at the stage of trying to assemble the various kinds of causes into portable general theory. So, there is not a scientific basis in this debate, where it is usually located, for setting up against one another ecologies of order or chaos.

The ecologies of order and chaos, as they live in narratives, have had, as scientific episodes go, unusual power to inspire outrage and condescension. Why? I noted that one of the most interesting features of the episode is ecologists' shift from treating Clements's causal investigation as the way to render ecology more scientific to treating it as hopelessly unscientific, beyond just right or wrong. Studying [page 103] the legends of Clements and Gleason and of order and chaos reveals something interesting about historiography — about understanding the history of science. It reveals that focusing on similes, metaphors, imagery, and their potential connotations can seriously mislead us in trying to understand others' understandings. Images help scientists create theories and communicate them, but scientific investigation, understanding, and explanation have other components; investigative and explanatory methodology are especially easy to overlook. In this episode, commenters drawn to the imagery have paid little attention to methodology and causal understanding, aspects which, whether the theories are wildly mistaken or not, are straightforwardly scientific. Historiography itself has made them unscientific.

So, putting the focus instead on how they assign causes to vegetation, that we find Clements and Gleason both recognizing causal interactions provides a basis for rejecting the claim that communities can be preserved only if they are Clementsian not Gleasonian. That in turn provides a basis for rejecting the claim that communities can be preserved as such only if they have some exotic kind of order or structure. If there are reasons certain groupings cannot be preserved as such, we do not learn about them by examining the concepts advanced in this debate.

Turning to implications for philosophy of ecology's discussion of communities, that Gleason did not deny causal interactions supports Odenbaugh's strategy of identifying a modest community concept based on the sort of causal interaction every ecologist recognizes. But the problem for Odenbaugh's approach to defining community criteria is that additional criteria are needed for the particular projects which have motivated seeking them. However, community concepts with additional features making them robust enough to support certain further interests include too much to serve other interests, and vice-versa. The richest challenge for philosophical discussions of communities is therefore not disorder, but multiplicity.

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