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vol.2, No.2, pp.187-199 ISSN 1805-3602

Carnap and 'Ecosystem'

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Received for publication: 10 March 2013.
Accepted for publication: 11 April 2013.

Abstract

How one defines the term 'ecosystem' is of central importance in arriving at good policy decisions regarding ecosystem management, so guidelines are needed on how to adequately introduce the term 'ecosystem' in scientific and policy discourse. My goal in this paper is to outline how one might approach this matter from the perspective of Rudolf Carnap's 'principle of tolerance'. I begin by outlining two interpretations of what Carnap means by being tolerant in introducing a scientific term – what I call 'conditional' and 'absolute' interpretations – and then apply these interpretations to the case of introducing the term 'ecosystem'. Specifically, I reconstruct the development of the ecosystem concept, starting from notions of a biotic community proposed in the mid-19th century, and working up to Eugene Odum's present-day authoritative definition of an 'ecosystem'. Reflection on this developmental history of the ecosystem concept reveals a number of empirical obstacles in arriving at an adequate definition of 'ecosystem', obstacles that have led some ecosystem scientists to resort to pragmatic approaches in defining ecosystems. What I show is that this presumed reliance on pragmatics is best handled if one interprets the introduction of the term 'ecosystem' along the lines of a conditional approach to Carnapian tolerance.

Keywords: ecosystem, Rudolf Carnap, pragmatics, principle of tolerance, Arthur Tansley, Eugene Odum.

Introduction

One of Rudolf Carnap's more notable philosophical contributions is his 'principle of tolerance', versions of which recur throughout his career. Perhaps the most iconic statement of tolerance occurs *The Logical Syntax of Language* (Carnap, 1934/1937) where Carnap says, "in logic, there are no morals" (p. 52). This liberality in choosing systems of logic extends in Carnap's subsequent "Empiricism, Semantic and Ontology" (Carnap, 1956) to a liberality concerning the choice of linguistic frameworks. We might call this Carnap's 'mature' version of the principle of tolerance: it is the version of tolerance expressed in Carnap's "Intellectual Biography", where he suggests that "everyone is free to use the language most suited to his purpose" (Carnap, 1963, p.18). On this approach, alternative philosophical viewpoints are answers to 'external questions' regarding which linguistic framework to use (as opposed to 'internal' questions resolvable solely by reference to the language system itself). Philosophies, in effect, are different linguistic 'proposals' which possess varying degrees of practical value, and it is the estimation of such value that motivates the proposers of language systems to adopt some framework, or not.

The philosophical question we face in this paper is how to best understand this latter (mature) version of Carnap's principle of tolerance. Specifically we ask what tolerance prescribes when it comes to the introduction of a new term into a language system. There are two readings of tolerance we will consider here, which we call 'absolute tolerance'

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(AT) and 'conditional tolerance' (CT). AT and CT provide guidance on 'term introduction': they tell us how to approach the introduction of a new theoretical term into a language system. Here is AT:

One should always be 'tolerant' in deciding whether or not to introduce a term 'X' in our vocabulary. We simply stipulate its use and incorporate it directly into our theorizing and empirical research. There are no empirical constraints on such a stipulation; the introduction of a term always has a pragmatic dimension; and introduced terms are descriptive of the world, if that is the intent of the introducer (though perhaps falsely).

For many philosophers, this is how one should understand Carnap's use of the principle of tolerance in "Empiricism, Semantics and Ontology" and in the "Intellectual Biography". The circumstances motivating the introduction of a term are said to be determined solely on pragmatic grounds and not at all governed by empirical factors. As such absolute tolerance is prescribed both as regards one's choice of terms and as regards the parameters of their use, since there are no prior limitations on what counts as an appropriate pragmatic factor that can influence one's choice, and empirical factors are deemed irrelevant for this choice (some even argue that the use of empirical facts is prohibited with term introduction since such facts are meaningless in advance of the term's incorporation into the language system, a matter I discuss below). Additionally, when AT says that the term is 'descriptive', the suggestion is that the term should be taken literally: the proposer of the term means to describe the real world and not some fictional world (that is, we are concerned only with non-fictional languages, such as the language of science). For AT, it is not thought that the inevitable and exclusively pragmatic dimensions of term introduction pose an obstacle to the objectivity of term use (an assumption, however, that we shall contest later on.). CT, by comparison, stipulates the following:

If term 'X' has no cognitive status other than as a 'proposal' to use language in a certain way (i.e., it lacks empirical constraints and is only a logical, philosophical term), then we should be 'tolerant' in deciding whether or not to introduce this term in our vocabulary. Our choice to use this term will inevitably be motivated by practical considerations, and the term is not descriptive of the world (regardless of the intent of the introducer).

That is, tolerance is 'conditional': one can be tolerant as regards the introduction of a term if this term lacks empirical import, but where the term does have empirical implications one should let those implications guide term introduction (perhaps to the point of deciding not to introduce the term). Moreover, only in the latter case is a term descriptive (i.e., to be taken literally). Where empirical constraints are lacking, and pragmatic factors take hold in term introduction, one can regard the term as (strictly speaking) fictional. People can still use the term and even be passionate about the entities that the term purportedly refers to. At occasional junctures, however, they may need to be reminded that the term doesn't actually describe, or even have the capability of describing, anything in the world.

To assist in understanding these two forms of tolerance I provide Table 1.

Table 1. A comparison of the conditional (CT) and absolute (AT) readings of Carnap's principle of tolerance.

	CT	AT
Do empirical facts guide term introduction?	Yes	No
Are pragmatic factors relevant to term introduction?	Sometimes	Always
Should we be tolerant with term introduction?	Sometimes	Always
Are defined terms descriptive of the world?	Sometimes (if the relevant empirical criteria are satisfied)	Always (whenever so intended)

At this stage there is the interesting exegetical question of which version of tolerance was advocated by Carnap. The received view on this issue has been that Carnap advocates AT; I argue, contra the received view, that he is allied to CT (Hudson, 2010). One of the key determinants that bears on the resolution of this matter is how a term, in advance of its introduction in a language system, can receive the benefit of empirical evidence either for or against its use. Thomas Ricketts (1994, 2007) and Michael

Friedman (2001) don't find much sense in such a possibility – they represent the orthodox view that terms acquire cognitive status and have empirical constraints on their introduction only within a linguistic framework – and so they vouch for a reading of tolerance along the lines of AT as a way of (charitably) reading Carnap. My view, on the other hand, argues for the feasibility of thinking about empirical evidence as independent of a language system, an approach I ascribe to Carnap on the basis of his behaviourist reading of observation reports during the 1930's (see Hudson, 2010). In this paper, we leave this exegetical issue aside and simply assume as prima facie feasible both the CT and the mainstream AT approaches; those keen to be more philosophically reassured of the viability of CT versus AT, or who have an interest in the relevant exegesis with regard to Carnap, are encouraged to consult the writings of Ricketts, Friedman and myself. If it helps, one can think of the CT approach modestly where empirical evidence is nevertheless expressed in a language prior to the term being introduced (which is in effect to grant Ricketts' and Friedman's point about there not being any pure extra-linguistic perspective) – the language is just not one that already contains the term of interest. Independently of adopting this interpretive convenience, the matter we are concerned with in this paper still stands: should one view the introduction of a term in a language system as empirically guided, as with CT, or as independent of empirical guidance, as with AT?

Methodology

My plan is to argue on behalf of CT as against AT, using as an illustrative case study the recent introduction of the term 'ecosystem' in environmental science. The case is an interesting one for at least two reasons. First of all, 'ecosystem' is a scientific term and so should be amenable to a Carnapian treatment, given Carnap's stated bias towards the scientific world-view. Secondly, it is a term fraught with strong practical, even political dimensions, and so it will be incumbent on us to pronounce on the role of these dimensions in introducing this term. It is the impact of these practical dimensions that turns out to be the key factor in deciding between CT and AT. Since the introduction of any term in a language system will naturally have some practical bearing, the argument I am providing for CT with respect to the term 'ecosystem' can be analogously made out with regard to the introduction of any term, though maybe less vividly than in the case of ecosystems.

The boundary problem

A relatively standard definition for 'ecosystem' is not hard to find. The 1993 Convention on Biological Diversity defines an 'ecosystem' as "a dynamic complex of plant, animal and micro-organism communities and their non-living environment interacting as a functional unit" (Article 2). The United States Environmental Protection Agency (EPA) provides a fact sheet, "Climate Change and Ecosystems" (published April 2010), according to which:

> an ecosystem is an interdependent system of plants, animals, and microorganisms interacting with one another and with their physical environment. An ecosystem can be as large as the Mojave Desert or as small as a local pond. Ecosystems provide people with food, goods, medicines, and many other products. They also play a vital role in nutrient cycling, water purification, and climate moderation.

These definitions, and other contemporary ones, share certain features. In particular, ecosystems involve 'interacting parts', where some parts are biotic (such as plants and animals) and some are abiotic (such as the sun's rays and soil). What the EPA definition adds is, first of all, a statement of how vague the boundaries of ecosystems are, with sizes ranging over many orders of magnitude. Secondly, the EPA is explicit regarding the practical dimensions of ecosystems, of how ecosystems can serve human interests. Ecosystems are thus unusual among scientific entities for the vagueness of their boundaries and their inherent, practical benefits for human welfare. How did environmental science get to the point of viewing ecosystems in this way?

As Frank Golley tells part of the story in *A History of the Ecosystem Concept in Ecology* (Golley, 1993), environmental scientists in their early researches into the existence of biotic communities (i.e., communities including plants and animals but excluding abiotic components) based their identifications on observable features of the environment. For instance, the botanist Anton Kerner in visiting the Danube Basin in the mid-19th century remarks:

> whenever the reign of nature is not disturbed by human interference the different plant-species join together in communities, each of which has a characteristic form, and con-

> stitutes a feature in the landscape of which it is a part. These communities are distributed and grouped together in a great variety of ways and, like the lines on a man's face, they give a particular impress to the land where they grow (Kerner, 1897, p. 885, as cited in Golley, 1993, p. 17).

Just as the lines of a man's face are visibly apparent, Kerner takes plant communities to also be readily identifiable on the basis of (naked-eye) observation, an attitude Golley takes to be characteristic of ecologists at this time who, as he comments, "walked or rode across a community and observed the presence, absence, and abundance of various organisms and interpreted those patterns" (p. 19). A more quantitative, empirical method to identifying biotic communities was adopted later on by Roscoe Pound and Frederic Clements (1897) in investigating the phytogeography of Nebraska. Using square quadrants to quantify the occurrence of plant species in pre-set areas, they observe that "the vegetation of the Earth's surface is arranged into groups of definite constitution and of more or less definite limits. Such a group is a plant formation." (Pound & Clements, 1897, pp. 313-314; as cited in Golley, 1993, p.19). A different sort of empirical investigation was taken up by Charles Elton during the 1920's based on observations made on Spitsbergen, the arctic island north of Norway. As Paul Colinvaux describes Elton's work in *Why Big Fierce Animals Are Rare* (1978; see chapter 3), Elton noticed on Spitsbergen and in other geographic areas that animal communities had hierarchical structures composed of numerous and proportionately smaller primary consumers (e.g., rodents and small birds) along with larger (by an order of magnitude) and much less numerous secondary consumers (which are predators of the primary consumers). This structure has since become known as the Eltonian pyramid and, at least as Colinvaux describes Elton's work, becoming aware of the pyramid is a matter of a relatively uncomplicated observation.

Such empirical methods, however, face difficulties when attempts are made to make them more precise. Notably, a problem arises when an attempt is made to specify the boundaries of biotic communities. This problem, which following Golley we call the 'boundary problem', is acknowledged by Pound and Clements (1897), who comment:

> [a plant formation] can rarely have definite limits, therefore, [it] must be bounded on every side by a more or less extensive belt in which the features of two adjacent formations are confused. As in the case of species, it often becomes necessary to establish arbitrary limits, within which the preponderance of characteristics must be adopted as the mark of delimitation (Pound & Clements, 1897, p. 315; as cited in Golley, 1993, p. 21).

Expressions of the boundary problem multiply in the subsequent literature. The botanist Henry Gleason recounts in both his works (1926) and (1939), on the basis of his observations of plant associations, that over time the constitution of such associations varies to such a degree that it is impossible to consider "any such area of vegetation as a definitely organized unit" (Gleason, 1939, p. 51). He notes a similar variability through space: he provides a number of examples describing how in tracing the path of a forest one finds a series of imperceptible changes that can lead one to think of it as the same forest from one end to the other – a conclusion that becomes obviously erroneous when one objectively compares the two endpoints and notices their extraordinary differences. For Gleason this imprecision regarding how to count plant associations – i.e., there is no principled way to decide whether the extended forest he describes is one community or a series of smaller ones – leads him to be skeptical about their existence altogether (as "definitely organized units"). His response, alternatively, is to regard as real only the individuals (and individual species) that make up these associations, a view he calls 'individualism'.

Jay Odenbaugh (2007) cites other examples where ecologists have performed empirical research that defies the view that biotic communities have identifiable boundaries (and thus count as real objects). For instance, he describes the work of Robert Whittaker during the 1950's and 1960's involving water and temperature gradient analyses performed on mountainside vegetation (Odenbaugh, 2007, pp. 633-637). Roughly, Whittaker found that the composition of plant communities as one ascends a mountainside (and thus moves to dryer, cooler regions) changes subtly and, just as with Gleason's forests, one can't distinguish any boundaries separating one community from the next (or even tell whether we are dealing with one community or a number of them). Odenburgh further recounts Margaret Davis' work during the late 1970's and early 1980's mapping ancient tree species migrations by means of the radiocarbon dating of fossilized tree pollen. Davis discovered that forests didn't seem to maintain their constitutional integrity over time; the constituent species moved independently in different directions, easily abandoning any community structure the forest might initially have had. Overall, then, if we take an empirical approach to identifying biotic communities, we fail to find any empirical markers for their existence and so seem forced to deny their reality. All we are left with is the reality of the individuals that purportedly constituted them.

Our assessment here might be different if we adopted a holistic approach to biotic communities, such as subsequently espoused by Frederic Clements in his (1916), and later vigorously defended by John Phillips in a series of three papers published in 1934 and 1935. On the Clements/Phillips approach, biotic communities are 'organisms' (Clements, 1916, p. 36), or as Phillips calls them, 'complex organisms' (Phillips, 1935, p. 489). These organisms undergo 'ecological succession' leading to a climax state; that is, a biotic community will undergo gradual changes in composition leading to an equilibrium, climax point where no more fundamental change occurs (such as with a 'mature' animal or plant). And just like any other viable organism, Clements notes:

> each climax formation is able to reproduce itself, repeating with essential fidelity the stages of its development. The life history of a formation is a complex but definite process, comparable in its chief features with the life history of an individual plant (1916, p. 36).

A key feature of these 'organisms' is their possession of certain special 'emergent' properties, properties possessed only by the whole organism and by none of its parts. As Phillips sees them, complex (ecological) organisms have emergent properties just as gunpowder has the emergent property of explosiveness: one could not predict (or have expected) the emergent properties of a complex organism just as one could not predict (or have expected) explosiveness to have been the result of combing sulphur, charcoal and saltpetre (Phillips, 1935, p. 491). There is naturally a lot more to say here regarding the role of emergent properties in ecological understanding. For now we can point to the value of such properties in underwriting an empirical criterion for the identification of biotic communities and in thus resolving the boundary problem. If biotic communities have special empirically-discernable, emergent properties not shared by their constituent members, then these properties could mark the existence of such communities, bypassing the problem of identifying their spatial and temporal boundaries. In fact, with the notion that the membership of a biotic community changes in a predictable and repeatable way as we move to a climax state, we are able to explain away some of the difficulties in specifying where one biotic community stops and another begins. For example, with what appears to be two communities with an unclear boundary, we might actually have a single community succeeding towards an equilibrium state.

The holistic approach, as formulated by Phillips, drew the attention of the ecologist Arthur Tansley who found it particularly objectionable. Tansley's interest was likely piqued by Phillips' portrayal of him as a holist. Phillips asserts that, according to Tansley,

> Gleason's reasons against the unit of vegetation being an organic entity are not convincing, that obviously the phenomena of vegetation depend completely upon those of the individual but that this statement applies equally to the human community which everyone would agree must be considered an organic entity: units of vegetation possess some of the characters of organisms (Phillips, 1935, p. 494, alluding to Tansley, 1920).

In response, Tansley (1935) sharply distances himself from Phillips' holism. He complains that Phillips on behalf of holism presents no "scientific arguments" (p. 285), that "Phillips' articles remind [Tansley] irresistibly of the exposition of a creed – of a closed system of religious or philosophical dogma" (p. 285). Tansley further expresses doubt about Phillips' invocation of the emergent properties of a whole as properties one could not have predicted given an awareness of the properties of the whole's component parts. In an argument that we will see repeated later on by more recent ecologists, Tansley complains:

> who will be so bold as to say that [a] new entity, for example the molecule of water and its qualities, would be unpredictable, if we really understood all the properties of hydrogen and oxygen atoms and the forces brought into play by their union? Unpredictable by us with our present knowledge, yes; but theoretically unpredictable, surely not (1935, pp. 297-298).

Nonetheless, Tansley does not completely reject Phillips' viewpoint. His more considered response is to present a new notion – an extension of the notion of a biotic community – that becomes the working concept for all subsequent ecological thinking, the notion of an 'ecosystem':

> the more fundamental conception is, as it seems to me, the whole *system* (in the sense of physics), including not only the organism-complex, but also the whole complex of physical factors forming what we call the environment of the biome – the habitat factors in the widest sense. Though the organisms may claim our primary interest, when we are trying to think fundamentally we cannot separate them from their special environment, with which they form one physical system (Tansley, 1935, p. 299, his italics).

The key elements to Tansley's notion of an ecosystem are its reference to a 'system' (in its most general sense, a set of interacting parts), and its incorporation of abiotic, inorganic factors, thus making an ecosystem more than just a biotic community. One of the first applications of Tansley's approach occurs in the work of Raymond Lindeman who performed detailed empirical research on a dying lake (Cedar Bog Lake in Minnesota) during the late 1930's and early 1940's with the goal of describing the trophic-dynamic structure of the ecosystem instantiated in this lake. Lindeman's work involved describing the lake's food-chain: identifying its primary producers as well as its primary and secondary consumers (the ecosystem's 'trophic-levels'), determining the relative abundances of producers and consumers, assessing how nutrients are processed at the each trophic level, and so on. To a degree, Lindeman took himself to be furthering both qualitatively and quantitatively the observations underlying the Eltonian pyramid (see Lindeman, 1941, p. 638 and 1942, p. 408). For example, two of his main contributions were to provide rigorous empirical justifications for the both the decreased productivity and the increased efficiency of higher trophic levels (see 1942, p. 415).

Following Lindeman, the ecosystem concept received its modern development through the work of Eugene Odum who found it useful to think of trophic levels abstractly, making it unnecessary to refer to the particular organism that occupies a trophic level (Golley, 1993, p. 77). Odum also sought to utilize the first and second laws of thermodynamics in ecological thinking (Golley 1993, p. 81), from which one can explain, for example, the decreased productivity of higher trophic levels by reference to the dissipation of energy as useless heat, as predicted by the second law. Still, even with Odum's developments (which we have only sketched), the notion of an ecosystem has not escaped the boundary problem. Consider the following passage from the 5th and latest edition of Odum's definitive textbook on ecology, *Fundamentals of Ecology*, co-written Gary Barrett:

> ecosystems are open systems. . . . The boundary for the system can be arbitrary (whatever is convenient or of interest) . . .; or it can be natural, such as the shore of a lake, where the lake is to be the system (Odum & Barrett, 2005, p. 19).

It appears on one side that, for Odum, the boundaries of an ecosystem can be a natural occurrence, discovered objectively by inspection of the world and not delimited by 'convenience or interest', and that on the other they can be artificially created by humans, firmly delimited by 'convenience or interest'. This is in fact an odd dichotomy. Consider an artificially created lake constructed to look exactly like a real lake. Given that they are empirically indiscernible, is there any reason for an ecologist to treat them any differently? Does it matter that the artificial lake is identified by its 'interest or convenience' and the natural lake isn't, given that the natural lake, by virtue of its indistinguishability from the artificial lake, has the same interest or convenience? Or again, what is a natural boundary? If the shore of a lake is a natural boundary, why isn't 10 feet in from the shore a natural boundary as well?

Given these sorts of problems, it's not clear that Odum and Barrett have, to this extent, solved the boundary problem – and in fact the current, general consensus is that there is no definitive, empirical solution to be found. On Victor Marín's view:

> ecosystems ecology is conceptually based on the [general systems theory], and . . . there is nothing there, at least from first principles, that will lead us to an objective way to create boundaries in ecological systems that we want to manage (Marín, 1997, p. 103).

Similarly, in considering the ecology of ecosystem services from a complex adaptive systems approach, J. B. Ruhl, Steven Kraft and Christopher Lant acknowledge that "there are a number of scientifically useful ways of describing ecosystem boundaries, . . . all [with] limitations", and in the end they pass on the question of what is the correct view of ecosystems boundaries to simply examining "which method . . . best serve[s] ecosystem service management policy" (2007, p. 22). Overall, the task of providing objective, empirical criteria for identifying ecosystems has been largely set aside by ecologists in deference to pragmatic determinations, as clearly illustrated by the U.S. Environmental Protection Agency fact sheet cited above. The prevailing view is that ecosystems come in various shapes and sizes and with various sorts of constitutions; accordingly, depending on our 'convenience or interest', on our 'ecosystem management policy', or on whichever practical factors bear on our thinking, certain spatial and temporal regions are thereby arbitrarily designated as ecosystems.

The significance of a pragmatic solution to the boundary problem

The resort to pragmatic factors in delimiting ecosystems can have a negative impact on ecosystem science. In simple terms, just because defining 'ecosystem' in a certain way has certain practical benefits doesn't mean that there is a real thing that is an ecosystem, so defined. But worse than that, given that people often possess divergent value systems, it follows that there will be a plethora of conflicting opinions on what can count as an ecosystem, many of which will be mistaken if only a few are compatible. What that means is that we will be compelled to accept an anti-realism concerning ecosystems: if what counts as an ecosystem is so subjective and so subject to interpersonal debate, then there are good grounds to be skeptical about their objective reality. Clearly this fact is troublesome if we aspire to construct public policy with respect to ecosystems – our policy might dramatically affect people's lives by reference to something that does not even exist (a worry adamantly expressed by Fitzsimmons (1999, pp. 24-25, p. 145).

It is at this stage that the distinction we drew above between AT and CT can play an informative role. Recall that according to AT there are inevitably pragmatic factors involved in the introduction of a term in a language system, independently of any attempt to provide empirical criteria for the use of this term. Moreover, with AT, we should always be tolerant both as regards what pragmatic factors are emphasized and how introduced terms are used. However the introduction of such pragmatic factors works out, introduced terms can be intended as descriptive of the world, irrespective of the pragmatic factors that (ubiquitously) play a role. Of course, we just noted that these pragmatic factors are especially troublesome where we are considering the term 'ecosystem' since, on this topic, people's practical interests diverge greatly, resulting in profound disagreements about what to count as an ecosystem. The effect of this disagreement is to lead one to an anti-realism about ecosystems – an anti-realism that is inescapable on AT since the pragmatic factors in term introduction are themselves inescapable. The situation, however, is fundamentally different when we adopt the perspective of CT. On this approach, before pragmatic factors become relevant, we have the initial task of trying to locate empirical criteria to govern the use of introduced terms. Should this search for empirical factors succeed, then a term such as 'ecosystem' can be said to be descriptive of the world; it will be empirically determined whether or not an ecosystem exists at a certain place or time, and will no longer be a matter of pragmatic choice. Conversely, where the search for such empirical criteria fails, at that point pragmatic features become relevant and play a decisive role in introducing the term 'ecosystem'. However, when this happens – when empirical criteria for term introduction fail – it is not contended on the CT approach that the term 'ecosystem' is descriptive of the world. Rather, 'ecosystem' now has the cognitive status of a 'proposal' to use language in a certain way. It is, as it were, a logical or philosophical term delineating a perspective on ecology, one motivated by practical considerations. As such we can be tolerant in deciding whether we introduce this term in our vocabulary, and if we do introduce it, in deciding how to use the term. That tolerance, in any case, does not land us in an anti-realism since in the absence of empirical criteria governing the introduction of the term 'ecosystem' we are not supposed to understand this term as descriptive of the world.

In brief, then, CT provides an effective solution to the seemingly inevitable spectre of anti-realism that confronts terms like 'ecosystem', terms that have their use profoundly influenced by practical interests. It relegates as 'philosophical', a matter of 'logical form', terms that lack definitive empirical criteria for their use, but is nevertheless tolerant about the use of such terms, even so far as they are impacted by pragmatic interests, so long as their non-descriptive nature is kept firmly in mind. With AT, conversely, term introduction is irrevocably impacted by pragmatic factors and there is no prior recourse to empirical factors to help maintain objectivity. Where the introduced term is claimed to be descriptive of the world, this leads at least to a form of relativism: as term introduction is always practically motivated, one's description of the world is practically motivated as well, leading to a picture of the world that is resolutely dependent on one's individual interests. Of course, as we have seen, the situation is even worse if there is a diversity of pragmatic interests at play, as there characteristically is with the term 'ecosystem', for then there will be differing conceptions of the world without any objective way to arbitrate between these conceptions, a scenario that lands us in anti-realism.

But does AT necessarily lead us to relativism and anti-realism? One might resist this outcome by suggesting that once a term is introduced (tolerantly, pragmatically guided and descriptive) its use is nevertheless constrained by empirical factors and objectivity is restored. For instance one might introduce the term 'ecosystem' as referring to a holistic entity composed of interacting biotic and non-biotic components that possesses emergent properties, without delineating beforehand (as with CT) specific empirical conditions under which an emergent property, and so an ecosystem, can be found. One would simply stipulate that ecosystems have this feature, and perhaps motivate this stipulation on the grounds of its inherent practical value. Subsequently, empirical data might appear that impact this definition. For instance, there might be a geographic region that people describe as an ecosystem, but that lacks any identifiable emergent properties. Or, a set of interacting biotic and non-biotic components with emergent properties might be found which people resist calling an 'ecosystem'. In either of these circumstances one might be drawn to question the original term introduction, despite the practical benefits of this an introduction, thus dispelling the worry that term introduction according to AT lacks empirical constraints. In this way, one might assert, AT avoids the spectre of relativism, or worse anti-realism: assuming empirical facts are themselves objective, pragmatic preconceptions that guide term introduction can be refuted, and those preconceptions that survive this testing regime have to some degree a claim to being objectively true, thus halting the slide to anti-realism.

Of course the key element to this approach is that the relevant empirical facts are themselves objective, which is a somewhat dubious possibility if the terms that ground the expression of these facts are themselves tolerantly and pragmatically introduced via AT. To be sure, the objectivity of the empirical criteria that guide term introduction on CT is itself a difficult matter to assess – but at least with CT there is a concentrated effort to eschew pragmatic influences, as compared to AT where pragmatic factors are given full sway. For example, suppose it turns out that pragmatic influences are irrevocable with term introduction, and that there is no way to be completely objective. In such a case, the prescription with CT is that such a term is non-descriptive, for pragmatic dimensions are to be introduced only if empirical criteria fail, and if empirical criteria fail the term is deemed non-descriptive. In this way CT averts the slide to relativism and anti-realism. With AT, on the other hand, the irrevocability of pragmatic factors in term introduction is in fact the norm and we are lead, as per usual, to relativism and (especially where there's a multiplicity of pragmatic interests) to anti-realism. But let us set aside the issue of the objectivity of empirical data and allow that empirical facts have an objective character with both AT and CT. And once more let us consider the case where there are untoward empirical facts that challenge an introduced term such as 'ecosystem'. How might the introducer of a term, if she is intent on preserving its legitimacy (again, with 'ecosystem', as referring to a holistic entity with interacting biotic and non-biotic components and possessing unique emergent properties) respond to this challenge? With CT there is not much to be done if the term is intended to be descriptive and the untoward empirical facts are strong enough, for empirical criteria alone will not support the use of the term. With AT, conversely, pragmatic factors are allowed to weigh in and, if they are strong enough, could counterbalance the contrary negative evidence. AT provides no prohibition on such a strategy for it views term introduction is an outright tolerant and pragmatic affair, with no empirical accountability. If it is convenient to rule out contrary empirical evidence, then one can simply view the matter that way. On AT, should empirical obstacles arise for an introduced term and one is nevertheless committed to this term and its applicability, then one must of course cope with these empirical obstacles and perhaps judge that the term is illegitimate. Alternatively, though, one could just as well for purely pragmatic reasons adjust one's world view and language system by incorporating new guidelines for term introduction that expressly discount this contrary evidence. It is because AT allows such innovations that one worries about the slide to anti-realism.

To summarize then, there is no easy solution for AT if we want to halt the progression to relativism as well as the prospect of anti-realism. Still, there are further philosophical problems to be addressed here, such as illuminating the nature of the empirical data that can serve the role allotted to it in CT.

In Hudson (2010) I describe and defend a Carnapian approach to this problem which involves a behaviourist reading of empirical data. My task now, though, is to defend CT along different lines. I propose to show how an analysis of term introduction along the lines of CT better fits the views of both a philosopher of ecology (Jay Odenbaugh) and a key ecologist (Eugene Odum), thus underlining the interpretive viability of CT over AT.

Odenbaugh and Odum – meeting the empirical challenge

We discussed earlier the challenges encountered by Robert Whittaker and Margaret Davis in locating empirical criteria for the boundaries of plant communities. As Odenbaugh (2007) recounts, Whittaker struggled to empirically locate on the basis of moisture and temperature gradients where one plant community stops and another starts; and Davis failed to find empirical indicators demonstrating that ancient forests maintained their constitutional integrity through the migration of their constituent tree species. But from this position of empirical imprecision Odenbaugh doesn't resort to introducing pragmatic considerations as a way of resolving this imprecision. Primarily he regards the conclusion that plant communities are empirically ill-defined as hastily drawn on the basis of minimal evidence; for him such a conclusion needs to be supported by more than Whittaker's gradient and Davis' ancient forest migration evidence. In service of showing that the existence of coherent plant communities is empirically defensible, Odenbaugh suggests that such communities be viewed as 'smaller' and 'more ephemeral' than usually understood (2007, pp. 637-638). He doesn't suggest any principled reason for such a suggestion – though we might suspect that such a change makes the sort of interdependence needed for community existence easier to maintain. Nevertheless, whether or not this approach works in creating coherent and identifiable plant communities is for him an empirical question, and "as such must be left to the empirical investigations of ecologists" (p. 639). As regards ecosystems Odenbaugh recognizes that, here too, there is empirical imprecision, and as a way of illustrating this imprecision he quotes a passage from Fitzsimmons (1999) highlighting the widely different ways in which different government agencies and NGOs identify ecosystems (Odenbaugh, 2007, p. 640, citing Fitzsimmons, 1999, p. 3). Odenbaugh's initial response to this imprecision is to suggest a pluralism about ecosystems – perhaps the divergent views of all these organizations on what counts as an 'ecosystem' are one and all correct. His more considered response, however, if we want to "demonstrate the nature and existence of ecosystems" (p. 640), is to 'operationalize' the notion of an ecosystem. On behalf of this approach he suggests the example of watersheds. He comments:

> a watershed is an area of land that drains downslope to the lowest point. Watershed boundaries follow major ridgelines around channels and meet at the bottom, where water flows out of the watershed into streams, rivers, or lakes. The nutrient and energetic flows have differential rates inside and outside the drainage basin. Given the existence of watersheds, I would argue that at some ecosystems have objective boundaries (2007, p. 640).

The empirical criterion for 'ecosystem' being suggested, if one looks closely at this passage, is not so much the watershed boundary but a certain rate of nutrient and energy flow, one supported by the existence of a watershed. Nevertheless, the important point for us is this: in responding to the boundary (or imprecision) problem, Odenbaugh's suggestion is not to succumb to the temptation of pragmatism (an option vigorously derided by Fitzsimmons) but rather to identify empirical criteria that will work in restoring the needed empirical precision for ecosystems. In other words, Odenbaugh tacitly endorses CT as the methodology that governs how we should approach term introduction.

Some success in establishing empirical criteria for the identification of ecosystems may be found in the work of Eugene Odum. Earlier on we saw some of the problems Odum and Barrett encountered identifying non-artificial boundaries to ecosystems. Odum's approach, however, is in fact decidedly holistic. He provocatively comments, "the old folk wisdom about 'the forest being more than just a collection of trees' is . . . the first working principle for ecology" (1977, p. 1289), a statement repeated verbatim in his 2005 textbook (Odum & Barrett, 2005, p. 8). To illustrate this emergentist phenomenon Odum and Barrett (2005, p. 7) use the oft-cited example of the emergent properties of water given the combination of oxygen and hydrogen. Such emergence is analyzed by them in terms of the unpredictability of water's properties given the properties of its molecular components. Of course the unpredictability criterion is troublesome, as recognized long ago in Tansley (1935), and recognized more recently in the ecological literature by Edson, Foin and Knapp (1981). Nevertheless Odum provides a striking example of how an ecosystem (or a biotic community) can possess properties that, at least, are very surprising given the properties of the ecosystem's components, an example based on research he performed with his brother Harold Odum on a coral reef at Enewetak Atoll in the Pacific Ocean in 1954 (see Odum H. & Odum E., 1955). The Odums found that this reef maintained a high rate of primary productivity despite the low nutrient content of the surrounding ocean (Odum H., & Odum E., 1955, p. 319, Odum, 1977, p. 1290, and Odum & Barrett, 2005, pp. 7-8; see also Johannes *et al.* 1972). As Odum (1977) describes the result:

> the inflow of nutrients and animal food from surrounding ocean waters was inadequate to support the reef if corals and other major components were functioning as independent populations (p. 1290).

For the Odums, this (emergent) empirical phenomenon serves as a sign that the reef is an ecosystem involving symbiotic interactions between constituent plant and animal communities, an indicator that doesn't require a preliminary drawing of boundaries but, instead, a 'reading off' of these boundaries given the physical dimensions of the reef wherever these symbiotic interactions are occurring.

What we see then is that, for the Odums, as for Odenbaugh, the solution to the boundary problem is not found in pragmatics but in the identification of useful empirical criteria that serve to demarcate biotic communities and ecosystems. In this sense then they are tacit advocates of CT, of the view that, at least with regard to the terms 'biotic community' and 'ecosystem', empirical criteria govern term introduction without prior recourse to pragmatic considerations. Overall, then, they are a part of a tradition of ecological thinking, expressed by Kerner, Pound and Clements, Gleason, Tansley, Lindeman, Whittaker, Davis, and no doubt others, who at least saw the point in starting out purely empirically in identifying ecological entities, and who, if they eventually resort to pragmatics, would do so as a response to the failure of the pure empirical approach.

Consider, conversely, if ecologists were advocates of AT. Their first step then, in introducing a term such as 'ecosystem', would be to state their pragmatic preferences right from the top. For example, suppose an ecologist has an interest in ensuring his property rights against eco-activists. He might be worried that, if the government adopts a certain definition of what constitutes an ecosystem, his property rights would be abridged. Thus, in the interest of protecting these rights he vouches for a certain way of defining 'ecosystem' that serves this interest. This strategy, obviously, strikes many as anti-scientific and empirically irresponsible: but it is precisely the strategy that is warranted by AT in so far as it expresses 'absolute tolerance' on what pragmatic interests can motivate term introduction. As such, it is hard to believe that Carnap would have actually approved such a reading of tolerance, especially considering that he thinks his views to be "closer to that of physicists and those philosophers who are in contact with scientific work" (1963, pp. 17-18). At this stage the natural response for those who wish to ascribe to Carnap an 'AT' interpretation, one that is consistent with his empiricist scruples, is to point out that defining a term in a certain way is no guarantee that the world will cooperate. For instance, the above property owner may define 'ecosystem' however he likes, but empirical facts can be very unpredictable and may not support his practical interests in other ways. Yet what stops the property owner in such a case from returning to his initial term introduction and adjusting his language system in a way that arbitrarily preserves his conception of an ecosystem and suits his pragmatic interests? Of course where tolerance is understood 'absolutely', there is no prohibition here. But again, the AT proponent one might respond: the property owner can redefine 'ecosystem' as he likes and pragmatically coordinate its success, but that is no guarantee that there are ecosystems such as he conceives of them. His view of ecosystems can be practically valuable and empirically confirmed through careful ad hoc adjustments, but may not reflect ecological reality. This, however, is to put a point on the basic problem with AT: it not only embraces a fundamentally relativistic view of science, it leads to an anti-realism as well, for the picture of the world generated by being absolutely tolerant and pragmatic about term introduction is one for which there is little hope of accuracy.

Conclusions

In this paper I have attempted to justify CT as the preferred approach to term introduction by showing 1) that it contains the key to averting the anti-realism and relativism endemic to those sciences with a strong pragmatic component (such as ecosystem science), and 2) that the history of the ecosystem concept as portrayed in the work of ecologists has a pattern that can be effectively characterized in terms of CT: traditionally, ecologists have sought to begin with empirical criteria that will constrain their use of the term 'ecosystem', and in not finding suitable empirical criteria have resorted to pragmatics in introducing this term. No doubt there will be philosophers who will regard the approach I have taken here as regressively positivistic (and who may regard the ecologists themselves as regressively positivistic, if my portrayal of them is accurate). Criticism of this sort would likely focus on the adequacy of CT as regards to the task of setting empirical criteria for term introduction. Along these lines let us look at two sorts of critiques of CT.

First of all, with CT, one attempts to locate (non-pragmatic) empirical criteria that will govern the introduction of a term, criteria which will ensure that introduced terms are capable of being descriptive of the world. They will empirically determine whether or not an ecosystem exists at a certain place or time, an issue that will not just be a matter of pragmatic choice. However, within these empirical confines there is a lot of flexibility. For instance, consider two rudimentary definitions of the term 'biotic community': (a) the set of all individuals of all species within a delimited area, and (b) the set of all individuals of all species within a delimited area in which 'emergent properties' are produced by the interactions of constituent individuals. Assuming there are such things as emergent properties, and that we have a precise empirical characterization of an emergent property, which definition should an ecologist adopt? Both definitions introduce empirical conditions for the use of the term, and there are many other alternative, empirical criteria here that we could have adopted. So how do we adjudicate between the various the empirical criteria that can be used to govern the introduction of the term 'biotic community'?

The key to responding to these concerns is to note that the empirical criteria that play a role in term-introduction are not meant to be fixed in stone. As a science develops, scientists are free to return to and revise these criteria later on. In fact, this is the sort of revision we see suggested by Odenburgh. Robert Whittaker, recall, was working with a set of empirical determinants on biotic communities: he examined the possibility that such communities could be tracked by the use of moisture and temperature gradients. And, as we saw, this approach failed due to the boundary problem; we were not left with identifiable boundaries between one community and the next. What this suggests is that Whittaker's presumed set of empirical criteria needs to be discarded, and as an alternative Odenburgh proposes empirical criteria (never fully elaborated by him) that would entail 'smaller' and 'more ephemeral' biotic communities than usually thought. But Odenburgh recognizes that even this revised approach might fail, dependent on further empirical investigation. This is how we should expect the process of term introduction to go: not as an activity where the definitions of terms are empirically settled one-and-for-all, but as a process in which the empirical criteria governing term use alters over time.

It is the character of CT that such revisions would be motivated empirically and that, if not, the descriptive nature of the relevant definitions would be lost. By comparison, with AT revisions could be made for purely pragmatic reasons, which is the source of our concern with AT.

The second concern with CT is whether it is feasible to have empirical criteria for term introduction that are indeed free of pragmatic influences, and related to this, whether it is even desirable to have criteria 'pragmatically sanitized' in this fashion. In response, it is worthwhile noting that CT is not entirely free of pragmatic factors. Such factors will inevitably motivate the decision to introduce some term rather than another, and more generally will impel someone to investigate a particular subject matter. It is, for example, usually a pragmatic issue that motivates people to study environmental science in the first place (such as concerns about global warming or species extinctions), and a further pragmatic matter that leads them to investigate ecosystems as a whole rather than just the individual species that make them up (for example, one might harbour the belief that the best way to save a species is to preserve the ecosystem of which it is a part). But these sorts of pragmatic issues, as important and ubiquitous as they are, need not influence the content of an introduced term (as Cole, 1992 notes): one might think it valuable to think of the natural world as constituted by ecosystems, but that doesn't determine what an ecosystem is, just as one might think that red is the best colour, but that, too, doesn't determine what red is. As regards the content of a term, there is nevertheless always some element of pragmatism concerning the scope of the application of the term: whereas pragmatic influences can do little to affect whether one takes a stop sign to be red in broad daylight, they might having a bearing on whether a stop sign is red in the dark, or when illuminated with a bright green light. But here we are considering ecosystems in their most basic, natural manifestations (e.g., naturally occurring lakes, rivers and forests), and as we saw above, in defining such ecosystems, ecologists readily follow the two-step approach recommended by CT, to first seek an empirical determination for 'ecosystem' and failing that to resort to a more pragmatic approach.

References

Carnap R., [1934] 1937. Logische Syntax der Sprache. Springer. Wien. English translation by Amethe Smeaton, The Logical Syntax of Language, Open Court.

Carnap R., 1956. Empiricism, Semantic and Ontology, reprinted and revised in Meaning and Necessity: A Study in Semantics and Modal Logic, 2nd edition. University of Chicago Press, Chicago. pp. 205-221.

Carnap R., 1963. Intellectual Autobiography, in P. A. Schilpp (ed.), The Philosophy of Rudolf Carnap. Open Court, LaSalle, pp. 3-84.

Clements F., 1916. Plant Succession: An Analysis of the Development of Vegetation. Washington D.C.: Carnegie Institution of Washington, as partially reprinted in Keller and Golley (2000), pp. 35-41.

Cole S., 1992. Making Science: Between Nature and Society. Harvard University Press, Cambridge.

Colinvaux P., 1978. Why Big Fierce Animals Are Rare: An Ecologist's Perspective.Princeton University Press, Princeton.

Edson M., Foin T., & C. Knapp, 1981. 'Emergent properties' and ecological research. The American Naturalist, 118: 593-596.

Golley F., 1993. A History of the Ecosystem Concept in Ecology. Yale University Press, New Haven.

Fitzsimmons A., 1999. Defending Illusions: Federal Protection of Ecosystems. Rowman & Littlefield Publishers, Oxford.

Friedman M., 2001. Tolerance and Analyticity in Carnap's Philosophy of Mathematics In J.Floyd and S. Shieh (eds.), Future Pasts; The Analytic Tradition in Twentieth-Century Philosophy. Oxford University Press, Oxford. pp. 223-255.

Gleason H., 1926. The individualistic concept of the Plant Association, Bulletin of the Torrey Botanical Club. 53: 7-26

Gleason H., 1939. The individualistic concept of the Plant Association. American Midland Naturalist 21: 92-110, reprinted in Keller and Golley (2000), pp. 42-54.

Hudson R., 2010. Carnap, the principle of tolerance, and empiricism. Philosophy of Science 77: 341-358.

Johannes R. et al., 1972. The metabolism of some Coral Reef communities: A team study of nutrient and energy flux at Eniwetok. Bioscience 22: 541-543.

Keller D., & F. Golley, 2000. Science of Synthesis: An Introduction to the Philosophy of Ecology. University of Georgia Press, Atlanta.

Kerner A., 1897. Natural History of Plants: Their Forms, Growth, Reproduction and Distribution. Translated by F. Oliver. Blackie, London.

Lindeman R., 1941. Seasonal food-cycle dynamics in a Senescent Lake. American Midland Naturalist 26: 636-673.

Lindeman R., 1942. The trophic-dynamic aspect of ecology. Ecology 23: 399-417.

Marín V., 1997. General system theory and the ecosystem concept. Bulletin of the Ecological Society of America. 78: 102-104.

Odenbaugh J., 2007. Seeing the forest and the trees: Realism about communities and Ecosystems. Philosophy of Science, 74: 628-641.

Odum E.,& G. Barrett, 2005. Fundamentals of Ecology, 5th edition. Thomson Brooks/Cole. Belmont, CA.

Odum E., 1977. The emergence of ecology as a new integrative discipline. Science 195: 1289-1293.

Odum H., & E. Odum, 1955. Trophic structure and productivity of a windward Coral Reef Community on Eniwetok Atoll. Ecological Monographs 25: 291-320.

Phillips J., 1935. Succession, development, the climax, and the complex Organism: An analysis of concepts. Part III. The complex organism: Conclusions. Journal of Ecology 23: 488-508.

Pound R., & F. Clements, 1897. The Phytogeography of Nebraska, 2nd edition. University of Nebraska, Lincoln.

Ricketts T., 1994. Carnap's Principle of Tolerance, Empiricism, and Conventionalism. In P. Clark and B. Hale (eds.), Reading Putnam. Blackwell Publishing, Cambridge. pp. 176-200.

Ricketts T., 2007. Tolerance and Logicism: Logical Syntax and the Philosophy of Mathematics. In M. Friedman and R. Creath (eds.), The Cambridge Companion to Carnap, Cambridge University Press, Cambridge. pp. 200-225.

Ruhl J., S. Kraft, & C. Lant, 2007. The Law and Policy of Ecosystem Services. Island Press. Washington.

Tansley A., 1920. The classification of vegetation and the concept of development. Journal of Ecology 8: 118-44.

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

United States Environmental Protection Agency, 2010. Climate Change and Ecosystems, http://epa.gov/climatechange/Downloads/Climate_Change_Ecosystems.pdf, accessed April 17, 2013.