Pessimism about Ecosystem Ecology: A Reply to Sagoff

Jay Odenbaugh
Department of Philosophy
Lewis and Clark College
0615 SW Palatine Road
Portland, Oregon
97219

1. Introduction. Mark Sagoff (2003) has articulated a set of important criticisms of theoretical ecosystem ecology in his essay "The Plaza and the Pendulum: Two Concepts of Ecological Science".[1] The criticisms are as follows: If theoretical ecosystem ecology is to be a successful part of ecological science, then

- It must clearly and correctly define the objects of study and in particular the concept of an *ecosystem*,
- Mathematical models in ecosystem ecology must be rigorously tested,
- Insofar as ecosystems are structural and functional units, we must determine the purported causes of this structure and function, and
- Ecosystem theory must help solve pressing environmental problems.

Sagoff argues that ecosystem ecology has not satisfied these conditions. He does not claim that *in principle* ecologists could not. However, the burden-of-proof is thus placed on the scientists and their philosophical friends. Moreover, *if* ecologists must choose between using a bottom-up case study method or a top-down theoretical method and *if* the top-down theoretical method is unsuccessful, then it appears that the only serviceable method is the bottom-up method.

These criticisms are not new of course though Sagoff has presented them in an incisive fashion. Each one of these issues has a venerable history.[2] Moreover, they are in

[1] By "theoretical ecology" or "mathematical ecology", I believe that Sagoff means "theoretical ecosystem ecology". He is not critical in particular of population or community ecology (Sagoff, personal communication) in this essay though he may have reservations about those areas as well.
[2] For debates over nature of ecosystems and ecosystem see Golley 1993, Hagen 1993; for debates over the nature of mathematical theory and its evaluation see Kingsland 1985; for an account of ecosystem ecology and environmental politics see Bocking 1997.

accord with the work of E. D. McCoy and Kristin Shrader-Frechette in their book *Method in Ecology* and R. H. Peters in his *A Critique for Ecology*. Though I believe these issues are important scientifically and philosophically, we can turn back, or at least blunt, Sagoff's criticisms. In this essay, I focus on the first three criticisms. In the first section, I argue that the distinction between the *bottom-up* and *top-down* methods rests on a false dichotomy. In the preceding sections, I argue that ecologists have the resources to respond to the other important objections.

2. Two Kinds of Research or a False Dichotomy? Sagoff claims that ecological science pursues two different kinds of inquiry, *bottom-up* and *top-down* (2003, 531). *Bottom-up* inquiry is experimental and observational research concerning local populations (and maybe communities) by uncovering the causal processes at work. *Top-down* inquiry uses mathematical theory to account for the general structure and function of large-scale systems. Thus, we have a set of contrasts:

- Inductive vs. deductive
- Observational vs. theoretical
- Small-scale vs. large-scale systems

Sagoff treats the methods as mutually exclusive.[3] He writes,

> How can one tell whether the ecological goings-on in a lake, forest, or estuary exhibit the kinds of patterns or processes that warrant a theoretical top-down mathematical approach as contrasted with a case-based bottom-up inductive inquiry? (2003, 531).

Likewise,

[3] Sagoff claims that bottom-up inquiry is inductive and top-down inquiry is deductive. However, any method of inquiry will uses *both* modes of inference. For example, theoreticians can make predictions by deducing claims from their models. However, they do this using parameter estimates and this comes from samples of data.

> No one knows where the future of ecology lies. If the sites that ecologists study are like the swirl of activity at the Luxembourg Gardens, the properties of which are historically contingent, ecology may mature into an inductive and experimental science like medicine… On the other hand, if ecosystems, once defined and delimited, turn out to be regulated systems like Foucault's pendulum, ecology may become a mathematical and deductive science like physics (2003, 548).

Ecologists' research does not however fit Sagoff's dichotomy. They bring experiment, observation, and theory *all* to bear on a given system.

For example, consider the work of ecologists like Gene Likens and Herbert Bormann at Hubbard Brook ecosystem in New Hampshire (see Bocking 1997, 116-50). Hubbard Brook is a second-growth hardwood ecosystem which has a relatively impermeable rock beneath soil. The area is divided into several watersheds where all the runoff goes to several streams.[4] This has allowed ecologists to experiment on different watersheds and provide detailed observational and experimental studies of the flow of nutrients in this ecosystem.

Nonetheless, theoreticians Jim Janak Daniel Botkin, and Jim Wallis devised mathematical models like JABOWA (*Ja*nak, *Bo*tkin, and *Wa*llis) to determine how the forest would change with time as the result of many different factors. In effect, the model simulated the growth of individual trees in different species using information from tree competition, reproduction and morality, and physical variables like soil moisture, light, and nutrients. Here we have experiment, observation, and theory all being used together.

[4] Incidentally, a common way of characterizing the boundaries of an ecosystem is through the notion of a watershed. If we use a watershed to define an ecosystem, then we characterize the system in terms of chemical cycling.

Granted, it is different individuals who carried out the different tasks; nonetheless, each set of methods was used on the same ecological system.[5]

There is a division of labor in ecology. There are those ecologists whose bread and butter is field experiment and natural history. Others are trained for work in the lab. Likewise, there are those who are mathematically knowledgeable and whose home is in front of a computer screen. We should not suppose that differences in scientific training and talent indicate a fundamental difference in methods. What is the future of ecology? It most certainly lies at the *intersection* of theory, experiment and observation (Karieva 1989).[6]

3. What in the World is an Ecosystem? Sagoff correctly notes that concept *ecosystem* is a difficult to define and offers two challenges to ecosystem ecology. First, "[N]o theory can be tested unless it defines the class of objects the behavior of which it seeks to understand" (2003, 535).[7] If we cannot define 'ecosystem' in a satisfactory way, then we cannot properly evaluate claims made about ecosystem structure and function. Second, even if we can define 'ecosystem' in a satisfactory way, we still must provide identity

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[5] It is also interesting to note that it was Likens and Bormann who brought the three to Hubbard Brook. Bormann knew of Botkin at Yale and Likens knew that IBM was interested in computer modeling. So, they brought in Janak and Wallis as well from IBM. Here we have experimentalists searching for theoreticians so that they can both work on the same system (Bocking 1997, 136-7).

[6] Sagoff also makes note of the fact that in many mathematical ecology textbooks, the authors shy away from presenting data for or against the theories discussed. This gives the appearance that models cannot or least have not been examined with real data. However, this is to make too much out of pedagogical purposes. Theoreticians recognize that if they were to include these details, the text would be enormously large. Likewise, they recognize that most of their students learn about model testing in their biostatistics classes. I emphatically agree that students should learn the models, data, and techniques of model selection. However, this is a bit much too expect in one text and in one course. For an excellent introduction to model selection see *The Ecological Detective*.

[7] Incidentally, I am not convinced that this is true. If we can reliably recognize the purported entity, then we may make claims that we can test without any definition much less a correct definition. I believe there are examples of this in biology concerning the concepts of *species* and *gene*.

conditions for ecosystems undergoing change – when do we have the same ecosystem or a distinct one? Sagoff believes that ecologists have carried out neither task.

With regard to the first point, Sagoff claims ecosystem definitions are either *over*-inclusive or *under*-inclusive (2003, 537–8). If we define 'ecosystem' in an over-inclusive manner, then there will be objects that are counted as ecosystems by our definition that are not ecosystems. For example, suppose we define an ecosystem as

> [A] spatially explicit unit of the Earth that includes all the organism, along with all components of the abiotic environment within its boundaries (2003, 537).[8]

It is thus clear *any* set of biotic and abiotic components are or can be an ecosystem. If we define 'ecosystem' in an under-inclusive manner, then we stipulate that ecosystems have certain properties and thus it becomes a matter of definition and not discovery whether they have a certain structure of function. Consider the following definition offered by Eugene Odum:

> [An ecosystem] is a unit of biological organization made up of all of the organisms in a given area (that is, 'community') interacting with the physical environment so that a flow of energy leads to characteristic trophic structure and material cycles within the system (1969, 262).

Now consider the following question "Do ecosystems have a characteristic trophic structure?" The answer is trivially *yes, by definition!*

I am in agreement with the first point. Many ecologists define 'ecosystem' (and other concepts) in ways that are far too broad. However, the second point I think is flawed. One of the important lessons that Hilary Putnam and Saul Kripke have taught philosophers is that definitions in science are often *empirical hypotheses*. The claim that "water is H_20" is claim about the essence, constitution, or nature of a substance or kind *water*. The question "Is water made of H_20?" has a trivial answer just in case water is constituted by H_20 and one knows this. However, the claim that a given liquid sample is water is not trivial nor is the claim that it consists in H_20. The same is true in the case of ecosystems. The definition given by Odum, if correct, renders the question "Do ecosystems have a characteristic trophic structure?" trivially true. However, the claim that a group of biotic and abiotic components is an ecosystem is not trivial nor is the claim that they exhibit a particular trophic structure or a particular material flow.

Another important consideration raised by Putnam and Kripke is that one can refer to a natural kind by a definition that is incorrect. This is the point of causal theories of reference. Thus, I explicitly deny that one must correctly define a natural kind before one can offer and evaluate hypotheses about that kind. *Even if* the definitions of ecologists of 'ecosystem' are all deficient it does not follow that they are not referring to ecosystems nor that their theories cannot be tested. If one believes that a theory cannot be tested until its key concepts have been correctly defined, then evolutionary theory has begun to be tested only since we provided a correct definition of 'species'. Given the debate over the nature of species, it appears that evolutionary theory has never been tested.

Nonetheless, Sagoff raises the crucial questions: Are there ecosystems and if so, what is the correct definition of them? We offer definitions of these entities and evaluate them empirically. We then revise them. Moreover, we do the same with respect to account of the identity conditions of ecosystems and the nature the causes of ecosystem

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[8] This is a definition reportedly offered from Gene Likens.

structure and function. These are in my mind substantive metaphysical and conceptual issues and ecologists and philosophers should be working on them.[9]

4. Ecosystem Ecology, the Promiscuous Science? Sagoff offers the following criticism of mathematical theory as it is developed in ecosystem ecology:

> Theories of ecosystem structure and function confront a second problem simply because there are so many of them. The abundance of untested mathematical theory threatens to turn ecology into formal rather than an empirical science… (2003, 536).

He also writes,

> Other sciences tend to replace one theory with another, for example, the phlogiston with the oxidative theory of combustion. Ecology, in contrast, seeks to add one theory to another (2003, 541).

Sagoff is offering the following argument. Ecosystem ecology consists in a large number of mathematical models. If it consists in such a large number of models, then testing these models is well nigh impossible. Hence, much of ecosystem theory is untestable. Let me consider these issues in turn.

First, I *agree* and *disagree* with Sagoff that ecosystem theory consists in a large number of mathematical models. How so? There are a large number of models; however, there are a few *types* of mathematical models in use in ecosystem ecology. Generally speaking, there are two sorts of models – compartment models and individual-based models.

[9] Sagoff also raises the following worry about ecosystems (2003, 540). Are we or are we not parts of ecosystems? If you answer "no", then there are very few ecosystems on a planet given our causal influence as a species. If you answer "yes", then it appears that one of most accessible places to test ecological theory is not "in the wild but in intensely managed ones" (2003, 540). I would agree that humans are a part of ecosystems – we are one species among other – though our ecological impact singles us out. This is not because we are metaphysically special but rather because we are powerful drivers of ecological change. Likewise, I would also agree that much of our theory testing *should* occur in human-managed ecosystems…the laboratory. This is what makes the existence of ecological research stations like Hubbard Brook so desirable.

As a very simple introduction, a compartment model has each form of nutrient or energy as a distinct compartment X_i (say ammonia) and each compartment has inflows and outflows designated by J_i.[10] We can represent a compartment with the following illustration:

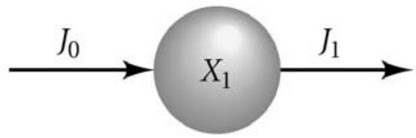

Figure 1 A simple compartment model (from Ricklefs and Miller 1999)

This can be represented with a differential equation as follows:

$$\frac{dX_i}{dt} = J_1 - J_0$$

So, for example, one might model the global water cycle as a two-compartment model using two compartments – vapor and liquid. One would need to determine the inflows and outflows and how those are a function of the other elements in the system. As another example, consider the following study of nitrogen transformation in the water column at the Bay of Quinte, Lake Ontario. Nitrogen in the ecosystem follows the path

> (ammonia → nitrate) → particulate nitrogen (organism + detritus) → dissolved organic nitrogen (DON) → (ammonia → nitrate)

We can represent this with a graphical model (without the corresponding differential equations) of the following form where g_i represents the rate at which material in compartment X_i is transferred to the next compartment:

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[10] Compartment models can have compartments that are populations and species as well.

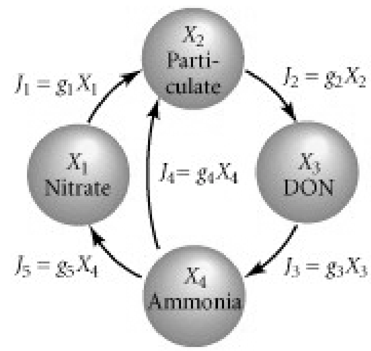

Figure 2 Compartment model of nitrogen transformation at Lake Ontario (from Ricklefs and Miller 1999)

These models can be very complicated as one might guess (see Shugart 1993, 84 – 93 for more details).

Individual-based models (IBM) form the second type of ecosystem model. Traditional models in ecology like the Lotka-Volterra models are *state variable* models. They assume that the differences between individual organisms are unimportant and can be treated as homogeneous. Thus, these are *mean field models* – the individuals all have the same properties and so we can simply average over those individuals. However, many ecologists have come to recognize that this is far too simplistic. Modelers have since tried their hand at a very different approach. IBM take the life history of each organism and integrates this information so that one can simulate the behavior of the community or ecosystem (Horn, Shugart, and Urban 1997).

One common type of model is a "gap forest model" of which JABOWA was an early instance. These models simulate forest dynamics by exploring the establishment, annual growth in height, and mortality of every tree in "the zone of influence" of a single dominant tree in the canopy[11]. These models have been successfully verified and validated in many instances. A model is *verified* if its predictions fit a given set of data where the parameters of the model are estimated from the data. A model is *validated* if its predictions fit a new set of observations that are independent of the data used to estimate the parameter of the model. Here is list of some successes of forest gap models.

Some successful tests and applications of forest IBM based on the dynamics of individual trees and gaps. See Shugart (1984) for details and references.

Verification = fitting of parameters to match data

Models can be made to represent these known features of forests:

1. Forestry yield tables for loblolly pine in Arkansas.
2. Succession of forest types at middle altitudes in Australian Alps and Smoky Mountains.
3. Response to clear-cut in Arkansas wetlands.
4. Forest types changing in response to flood frequency in Arkansas and Mississippi.
5. Structure and composition of forests in New Hampshire, Tennessee, Puerto Rico, and flood plain of Mississippi River.
6. Age-specific structure in a subtropical forest.
7. Arkansas upland forests based on 1859 survey.

Validation = tests with independently gathered data

8. Response of Eucalyptus forests to fire.
9. Effects of chestnut blight on forest dynamics in southern Applachians.
10. Yield tables for *Eucalyptus delegatensis* in New South Wales.
11. Average annual increment in diameter at four sites in Tennessee.
12. Distribution of tree diameters in Arkansas upland and Puerto Rican rain forest.
13. Elevational gradients of forest composition and structure in New Hampshire and Australian Alps.
14. Effects of hurricanes on diversity of Puerto Rican rain forest.

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[11] I am ignoring landscape models simply because they concern several ecosystems at once.

Thus, I disagree that in the relevant sense, there are a large number of ecosystem models. There are few types with many different instances tailored to particular sites, but this is nothing approaching the list Sagoff provides including "game theory, hierarchy theory, chaos theory, statistical mechanics, network theory, probability theory, and even the theory of oscillators" (2003, 541).[12] Likewise, I believe that many of these models have been applied and tested and many have done very well indeed.

5. Conclusion. It should be apparent that I am optimistic about the role of ecological theory in accounting for the dynamics of ecosystems and their components. These models are different and complement one another. They can be tested and have done well. Moreover, their success can be had even when we do not have an adequate ontological account of what ecosystems are. Thus, my optimism extends to using ecosystem models to help us understand environmental problems. As far as I can ascertain, there is no *philosophical* obstacle to such applications. Nonetheless, Sagoff has drawn our attention to important issues in the foundation of ecosystem ecology. It would behoove us to take them seriously.[13] My optimism is tempered because science is difficult and success is not guaranteed.

Bibliography

Bocking, S. (1997) *Ecologists and Environmental Politics*.
Brown, J. and P. Rotherty. (1993) *Models in Biology*: *Mathematics*, *Statistics*, *and Computing*. New York: Wiley.

[12] It is also important to note the difference between *borrowing* concepts or assumptions from other areas and those areas providing an *ecosystem* model. At best, ecosystem modelers use concepts from various branches of mathematics like any good applied mathematician; however, this does not mean that those areas provide distinct theories for scientists.
[13] It is important to take these criticisms seriously since many critics of ecosystem management such as Chase (2001) and Fritzimmons (1999) have made them crucial to the substance of their views.

Chase, A. (2001) *In a Dark Wood*. Transaction Publications.

Fitzsimmons, A. (1999) *Defending Illusions*. Rowman and Littlefield Press.

Golley, F. (1993) *A History of the Ecosystem Concept in Ecology: More Than the Sum of the Parts*. New Haven: Yale University Press.

Hilborn, R. and M. Mangel. (1997). *The Ecological Detective*: *Confronting Models with Data*. Princeton: Princeton University Press.

Horn, H., Shugart, H. and D. Urban (1989) "Simulators as Models of Forest Dynamics" in *Perspectives in Ecological Theory* (eds.) J. Roughgarden, R. May, and S. Levin. Princeton: Princeton University Press.

Karieva, P. (1989) "Renewing the Dialogue between Theory and Experiment in Population Ecology", in *Perspectives in Ecological Theory* (eds.) J. Roughgarden, R. May, and S. Levin. Princeton: Princeton University Press.

Odum, H. (1969) "The Strategy of Ecosystem Development," *Science* 164: 262 – 270.

Peters, R. H. (1994) *A Critique for Ecology*. Cambridge: Cambridge University Press.

Sagoff, M. (2003) "The Plaza and the Pendulum" *Biology and Philosophy* 18: 529 – 552.

Shrader-Frechette, K. and McCoy, E. D. (1993) *Method in Ecology*. Cambrige: Cambridge University Press.

Shugart, H. (1984) *A Theory of Forest Dynamics: The Ecological Implications of Forest Succession Models*. Springer-Verlag.