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Discussion

Differentiating and defusing theoretical Ecology's criticisms: A rejoinder to Sagoff's reply to Donhauser (2016)



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ABSTRACT

In a (2016) paper in this journal, I defuse allegations that theoretical ecological research is problematic because it relies on teleological metaphysical assumptions. Mark Sagoff offers a formal reply. In it, he concedes that I succeeded in establishing that ecologists abandoned robust teleological views long ago and that they use teleological characterizations as metaphors that aid in developing mechanistic explanations of ecological phenomena. Yet, he contends that I did not give enduring criticisms of theoretical ecology a fair shake in my paper. He says this is because enduring criticisms center on concerns about the nature of ecological networks and forces, the instrumentality of ecological laws and theoretical models, and the relation between theoretical and empirical methods in ecology that that paper does not broach. Below I set apart the distinct criticisms Sagoff presents in his commentary and respond to each in turn.

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Mark Sagoff pursues a practically-motivated approach to environmental philosophy that I greatly appreciate. In several works, he critically analyzes the foundations of theoretical methods in ecology, and questions whether they can be usefully applied for public policy and resource management decision-making (Sagoff, 1997; 2000; 2003; 2013; 2016). Such analysis is crucially important because leading environmental advisory organizations have cast ecology as an objective guide for significant policy and management decisions.¹

In some recent papers, I examine how different inferential methods in theoretical ecology work and how they can (and cannot) be usefully applied for practical decision-making. In my (2016), "Theoretical Ecology as Etiological from the Start," I dispel claims that theoretical ecological research is unscientific and of no value for practical decision-making because it is supposed to rely on teleological thinking. Therein, I spotlight comments made by Sagoff (2013) as expressions of the criticisms I had in mind.

I was pleased to see Sagoff's formal response, and sincerely appreciate his endorsement of my positive arguments in my recent paper. In a kind fashion, he says: "Donhauser admirably succeeds in showing that several founders of ecological theory, including Lindeman (1942) and Hutchinson (1948), had 'efficient' rather than

'final' causality in mind." So, although he lodges complaints later on, Sagoff confirms that I succeed in showing that ecologists abandoned robust teleological views long ago, and use teleological characterizations of ecological phenomena as metaphors that aid in developing mechanistic accounts of the underlying causes of observable ecological "network-level" dynamics. He also takes no issue with my operational account of the role of teleological metaphors in certain reasoning processes commonly employed in ecology after Hutchinson (1948) (pp. 71–2). Moreover, Sagoff does nothing to undermine my contention that modern ecologists generally embrace a thoroughgoing instrumentalist and metaphysic-neutral view on the nature of ecological networks and properties (pp. 68–9; 74). In fact, most of his reply concerns not what I argue in my paper but what it left him wanting me to say about the nature of ecological networks, so-called ecological "forces," and theoretical models of such things.

Sagoff's negative appraisal is that I have been uncharitable in my treatment of criticisms of theoretical ecology. According to him, this is because enduring criticisms center on interrelated concerns about the existential status of ecological networks and forces, the instrumentality of ecological laws and models derived therefrom, and the relation between theoretical and empirical methods in ecology that not discussed in that paper. Yet, despite his initial comments about the ways I succeed in the paper, Sagoff's interpretation and critique of my paper show that he and I see theoretical ecology and its criticisms very differently.

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¹ See Donhauser, 2016a and Donhauser, 2016b and the sources cited therein for more discussion of ways in which ecology has been bestowed this guiding role.

Sagoff gerrymanders the scope of my brief and narrowly focused comments about criticisms of ecology, and claims that I should have addressed several issues my paper was never meant to address. Though he frames it as a response to my claim that methods in modern theoretical ecology have always been underwritten by an etiologial, rather than a teleological, understanding of ecological networks, he uses his commentary primarily as a vehicle for endorsing several independent criticisms of theoretical ecology that he runs together and that are outside the explicit scope of my paper. Unfortunately, the criticisms Sagoff defends in his reply, and his comparatively few criticisms of claims I make in my paper, are founded on mistaken assumptions about the conceptual foundations and basic workings of theoretical methods in ecology. Accordingly, I see this opportunity to respond Sagoff as an opportunity to push forward understanding of ecology's conceptual foundations and practical value.

My rejoinder to Sagoff will progress in two main steps. First, in §1, I assess Sagoff's critique of my paper, and identify problems with its set up, scope, and framing that largely belie his commentary. In §2, I then systematically defuse several independent criticisms Sagoff endorses in his commentary.

1. Problems with Sagoff criticism of Donhauser (2016)

1.1. A bait-and-switch

Sagoff defends criticisms of theoretical ecology he says I unfairly brush aside via presenting what he frames as counterpoints to my contention that 'theoretical ecology has been etiologial from the start.' In my (2016), I show that in "the first, and [...] archetypical, project in modern theoretical ecology" and key works directly preceding it, seminal ecologists explicitly abandoned robust teleological commitments and use "teleological metaphors to aid in generating component-to-component accounts of the mechanics of ecological network-level dynamics" (p. 73). In response, Sagoff claims that theoretical ecology has *never been* etiologial. However, he does not then establish this or even deny my point. He instead uses 'etiologial' in a way inconsistent with my carefully restricted usage, and uses the slogan that 'ecology has never been etiologial' to loosely pull together several independent criticisms that are tangential to the considerations of my paper.

This begins happening as he specifies that by saying 'theoretical ecology has never been etiologial' he means: "[ecologists have] not presented empirical evidence of the causal forces it theorizes, such as density dependence, competitive exclusion, Lotka-Volterra predator-prey relations, the logistic relation of species abundance to resource limits, and the like" (p. 2). Sagoff brings in further tangential concerns later, but even here, at his commentary's start, he has conceded my main point and shifted to discussing concerns about "ecological forces" and the relation between theoretical and empirical work in ecology that are outside the scope my paper. After this brief clarification on the first page, Sagoff (literally) does not mention etiology, teleology, or the distinction between "top-down" and "bottom-up" thinking anywhere else in his commentary. He instead launches into a discussion of old criticisms of theoretical ecology that see it as divorced from empirical work, which he ties to independent concerns about ecological forces, regularities, and the existential status of ecological entities.

He buttresses his worry about an alleged disconnect between theoretical and empirical ecology by citing Hall (1988), who once complained that ecological principles and models are often taught, and presented in textbooks, as if they are "a priori true" even though they have known exceptions (p. 2; cf. Haskell, 1940). Sagoff attaches this arguably anachronistic brand of critique lodged by Hall to the abovementioned independent issues by then pointing to

an oft-cited paper by Lawton (1999); who argues that there are no ecological community-level regularities in nature because contingent local factors are so various and diverse that such regularities just do not occur. Sagoff then abruptly brings in worries about the nature of ecological networks, properties, and kinds, saying: "many of the kinds of [network-level] properties theoreticians posit and mathematicians model are observable in principle; the critics contend, however, that these patterns or regularities are not observed in fact" (p. 1). He relates this loosely associated bundle of independent concerns back to my paper in an ad hoc fashion; by concluding that my recent demonstration that 'modern ecologists do not rely on assumptions about teleological causes of ecological network-level properties' is inconsequential to enduring criticisms of theoretical ecology. According to him, this is because undermining enduring criticisms requires defusing the aforementioned concerns by establishing that ecological network-level properties, the "forces" that produce them, and regularities and laws applying to them are observed in nature.

1.2. Misjudging the scope

My (2016) paper's main point is that ecologists' use of teleological language is metaphorical and a conceptual device for helping to fill out the "bottom-up," efficient, causal picture they presuppose. Yet, Sagoff's commentary focuses on topics orthogonal to the questions of whether ecologists accept a robust teleology and "top-down" causes—*while he also explicitly agrees that they do not*. What's more, he unfairly flags my comments about criticisms of theoretical ecology as uncharitable and inconsequential.

Sagoff mistakenly believes that I respond to a much broader class of criticisms of ecology than I do in my paper. He remarks that my paper's introduction, "helpfully and accurately cit[es] a list of authors who have questioned whether the entities described in ecological theory exist in any meaningful sense at all" and others who "have argued that theoretical ecological research is empirically unfounded (even empirically unfindable)" (p. 68). Yet, I cite those criticisms early on to emphasize the juxtaposition between the unfettered embrace of ecology in policy circles and the wariness of it expressed by some academics. I do not purport to respond to the whole class of enduring criticisms, and my project is not even presented as a response to criticisms. It is presented as a historical project that provides insights into the heuristic roles of teleological metaphors in certain sorts of theoretical research, and which has ramifications for certain misinterpretations of work in modern ecology. The explicit aim is not 'defusing criticisms of ecology' but 'providing insights into the workings of certain inferential processes in ecology' through examination of key moments in theoretical ecology's genesis. I do say that my arguments have ramifications for *some* criticisms, but those comments are restricted to criticisms that concern teleological language used in ecology.

As I say at the top of my paper, in the final section, "I critically respond to opposing literalist construals of teleological characterizations in ecology—including enduring arguments according to which theoretical ecological research is unscientific and of no value for practical decision-making because it is supposed to rely on teleological, 'magical,' thinking" (p. 68). I accomplish this by providing textual and conceptual support for my prior contention that robust teleological views are not and have not been embraced as a mainstream convention within ecology since Hutchison (1948) at least. *Nowhere* do I say that my arguments in the paper have ramifications for criticisms of ecology centering on concerns other than ecologists' ostensible appeals to teleological causes. So, since Sagoff agrees that I succeed in establishing that modern ecologists do not embrace robust teleological views, he must concede that

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