Studies in History and Philosophy of Biological and Biomedical Sciences 45 (2014) 105–109

Contents lists available at ScienceDirect

Studies in History and Philosophy of Biological and Biomedical Sciences

journal homepage: www.elsevier.com/locate/shpsc

Environmental philosophy: Response to critics

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ARTICLE INFO

Article history:
Available online 21 November 2013

Keywords:
Biodiversity
Conservation biology
Ecological restoration
Historical fidelity
Naturalism
Decision theory

ABSTRACT

The following piece is a response to the critiques from Frank, Garson, and Odenbaugh. The issues at stake are: the definition of biodiversity and its normativity, historical fidelity in ecological restoration, naturalism in environmental ethics, and the role of decision theory. The normativity of the concept of biodiversity in conservation biology is defended. Historical fidelity is criticized as an operative goal for ecological restoration. It is pointed out that the analysis requires only minimal assumptions about ethics. Decision theory is presented as a tool, not a domain-limiting necessary requirement for environmental philosophy.

The reviews have raised several pertinent issues. Let me begin by noting several areas of agreement. Odenbaugh is correct that my views are imbued with a background naturalism. I suggest that the practice of philosophy should maintain continuity with the sciences. Besides the implications for environmental ethics, my naturalism has two further consequences. First, it leads to an attention to the details of individual environmental sciences such as conservation biology and restoration ecology. Second, I do not believe that conceptual analyses that entirely ignore the historical context of the sciences can prove fully adequate—this will be relevant to some of Frank's claims below. However, I agree with Frank that though the book tried to be comprehensive, it left out many issues. In particular, environmental aesthetics deserves much philosophical scrutiny—including attention to empirical questions (often ignored by philosophical aesthetics) such as what (if anything) the experience of nature contributes to psychological development and well-being (as, for instance, provocatively claimed by Louv, 2005). The epistemology of climate change models, whether we have any reason to act on them in the face of potentially debilitating uncertainties (Lloyd, 2010), also deserves sustained attention—but that is a project that needs several book-length treatments by itself. The chapter on justice and equity merely touches the relevant issues—but I did warn the reader that it claims no more.

To justify these omissions, let me note that the book was intended to provide an integrative view of environmental philosophy and, to invoke a metaphor, I did not want to lose the forest among the trees. It will be interesting to see whether or, perhaps, how much, detailed future attention to these other issues will require modification of the general framework. Finally, all four of us agree that there is much more to environmental philosophy than environmental ethics: in particular, the history and philosophy of science is critical to environmental philosophy. This broadening of perspective was the primary motivation for the book. This said, let me turn to four areas in which there is disagreement between me and my critics: (i) biodiversity and conservation biology; (ii) ecological restoration (iii) ethics; and (iv) decision theory.

1. Biodiversity and conservation biology

Most of the issues that divide my critics and me concern biodiversity and its conservation. The first issue is one which Frank raises: whether the characterization of biodiversity as the goal of conservation biology is circular insofar as it would make it impossible to question whether conservation biologists are using a correct definition. The point, though, is that this characterization was never intended as a definition. Rather, it was intended to

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http://dx.doi.org/10.1016/j.shpsc.2013.10.011

indicate where we should look to explicate adequacy conditions for definitions of biodiversity as a prelude to producing a definition itself. (This is part of my naturalism.) While such a contextual exploration may mostly be necessary because of the recent vintage of "biodiversity," there is still an interesting parallel to the concept of health and medical practice. In situations where the concept of health is unclear, for instance, in definitions of mental health, we may want to examine professional practice, for instance, psychiatric classification schemes, to guide the formulation of adequacy conditions and definitions. This does not prevent us from coming back to question the aptness of a particular definition of health used by practitioners.

A second, related, question raised by both Frank and Odenbaugh is the sense in which a conventional element enters into biodiversity definitions. Now, I do not restrict conventional content only to cases in which all options are equally good. Rather, I contextualize conventions to an explicit set of constraints (for instance, adequacy conditions) and hold that a conventional element enters into a definition if there remains epistemic discretion (Ben-Menahem, 2006), that is, the constraints do not fully specify a definition. The permitted choices are now deliberated upon using criteria that were not among the constraints—and some definitions may well be preferable to others on the basis of these new criteria. Now, given that a concept of biodiversity must be operationalizable (that is, used in the field to guide conservation policy), the adequacy conditions I lay down still leave a wide variety of choices available. That was what I mean by suggesting a conventional element in the definition of biodiversity.

A third issue, raised by Frank, is whether a definition of biodiversity should be normative. He gives example such as vertebrate species richness which may be of interest in biogeography, ecology, and evolution. I do not deny that there are many value-free concepts of ecological diversity (including richness, if we allow that it is a diversity concept [which is questionable (Sarkar, 2005)]). There has been explicit discussion of many of these non-normative diversity concepts since the 1950s. Yet, as I have previously pointed out in a discussion of the history of ecological diversity (Sarkar, 2007), this tradition was ignored when definitions of biodiversity were debated and adopted during the founding of conservation biology as an organized discipline in the late 1980s. There are many formal and conceptual connections between the two sets of definitions (Sarkar, 2010; Sarkar, Pappas, Garson, Aggarwal, & Cameron, 2004). Those involved in establishing the new discipline of conservation biology self-consciously chose to construct what they believed to be a new framework and ignored the earlier discussions of ecological diversity. They also insisted that conservation biology was a goal-oriented discipline with an irreducible normative component (Soulé, 1986). The term "biodiversity" came slightly later (see Takacs, 1996); it was never explicitly defined. My original explication of "biodiversity" (Sarkar, 2002) attempted to make sense of common usage—and the only way to do so, or so I claimed, was to pay attention to its history and note the normative role played by the term. (Other philosophers had already made the same point about the normativity of "biodiversity" though, perhaps, not quite so strongly [Callicott, Crowder, & Mumford, 1999; Norton, 2003].) The discussion in this book amplifies these earlier analyses and builds on the explicit treatment of biodiversity-related norms introduced by Sarkar (2008).

It is also true that, once "biodiversity" became a buzzword and began to generate grant money, other disciplines, particularly taxonomy, jumped on the bandwagon—but the term was unknown in these fields until after the formation of conservation biology around 1985 and the introduction of the term in 1986 (Sarkar, 2005); moreover the co-option of "biodiversity" does not prove that these disciplines were now employing a concept not already available in the ecological repertoire prior to the 1980s (see below for more on this point). As noted earlier, definitions of biodiversity in the context of its conservation are normative because of the normative nature of conservation biology which has been emphasized by its practitioners throughout its history. This is where there is basic disagreement between my treatment and that of Maclaurin and Sterelny (2008): unlike them I require that biodiversity concepts be contextualized to conservation biology in practice and to no other biological discipline, and I emphasize the normative role of the concept of biodiversity in conservation practice. To the extent that I can interpret their rather non-specific characterizations, Maclaurin and Sterelny are in agreement with me that there is no single categorical concept of biodiversity. This is an area in which my naturalistic approach to conceptual analysis pays dividends, in clearly facilitating a sharp distinction between ecological diversity and biodiversity. Finally, I should note that neither Frank nor Maclaurin or Sterelny provide any reason to suppose that any use of "biodiversity" outside conservation biology goes beyond well-known (non-normative) ecological measures long available within theoretical ecology. In particular, though this claim must remain partly conjectural in the absence of systematic analysis of the literature, the uses of "biodiversity" outside conservation biology have only very rarely gone beyond richness. (Maclaurin and Sterelny explicitly emphasize the importance of richness in their analysis of biodiversity.) Thus there is ample reason to believe that there is no interesting (or otherwise non-trivial) non-normative concept of biodiversity which is not a mere relabeling of some existing ecological measure of diversity.

Turning to conservation biology, Odenbaugh seems to question whether the emphasis on systematic conservation planning is justified. My claim in the book was that most of the concerns of conservation biology fall within systematic conservation planning. With the exception of the SLOSS debate, the topics he mentions (genetics of inbreeding, demographic stochasticity, habitat fragmentation, and metapopulation structure) are relevant to Stage 9 of systematic conservation planning ("Assess biodiversity constituent and selected area vulnerabilities"). I ignore the SLOSS debate because it was recognized by all sides to be futile by the mid-1980s (Soulé & Simberloff, 1986). However, there are areas of conservation biology that are outside systematic conservation planning, for instance, management of zoo or botanical garden populations. It is unclear that these raise novel conceptual issues. But I ignore issues such as the ethics of zoos—as Frank also pointed out, the book leaves out many topics.

My final point responds to Odenbaugh's questions about the conceptual structure of conservation biology. I agree with him that the centrality of theoretical work on algorithms in conservation biology (which has been a major part of the discipline's history [Sarkar, 2012]), may suggest a pragmatic or instrumentalist interpretation of science. Moreover, traditional philosophy of science, with its emphases on theories, models, and experiments does not seem to cope very well with late twentieth-century disciplines, not only conservation biology and restoration ecology, but also operations research, computer science, and so on. Categories such as confirmation and explanation may require radical reinterpretation, if they are applicable at all. All this may well be interesting and novel, and may lead to innovative developments in philosophy of science.

But we should not ignore a more deflationary possibility: the "theoretical" innovations in conservation biology and these other late twentieth-century sciences may simply be formal work: the design and analysis of algorithms, data structures, *etc.*—applied mathematics that may not have much deep philosophical significance about the nature of (empirical) scientific theories, scientific explanation, confirmation, and so on. Much more philosophical work needs to be done on these fields and, I hope, is forthcoming.