

A Philosophy for Biodiversity?

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I. Introduction.

Sahotra Sarkar's *Biodiversity and Environmental Philosophy* is a welcome addition to the fields of environmental philosophy and the philosophy of science. First, his book has a rigorous and careful discussion of why we should preserve biodiversity. This is all the more important since much of environmental ethics has rested on normative claims which are unclear in meaning, appear unjustified at best and unjustifiable at worst, and are politically ineffective. Second, Sarkar is at home in the science of conservation biology and offers important analyses of methodological issues in both ecology and conservation biology. Third, his book does not sustain what might be thought of as a "North American bias" but takes seriously environmental issues and perspectives from many different places like Australia and India.

In this commentary, I raise worries and open questions that can be divided into four sections: (a) those concerning moral philosophy, (b) those concerning ecology and in particular whether we are in an extinction crisis, (c) how biodiversity is defined as a concept, and (d) what the aim and structure of conservation biology is. Ultimately, though my analysis is critical at some junctures, its purpose is to repay and hopefully improve on Sarkar's rewarding analysis. Let me now turn from praise to criticism.

II. Values

Here is what I take to be Sarkar's argument for preserving biodiversity.

1. If an object or process has positive transformative value, then we should preserve that object or process.
2. Biodiversity has positive transformative value both directly and indirectly.
3. Hence, we should preserve biodiversity.

There are several preliminary issues to consider. First, what is Sarkar's account of transformative value? Second, what is the distinction between *direct* and *indirect* transformative value?

In order to proceed, we must first define the notion of *demand value* (2005, 77).

(*D*) An object has demand value just in case it satisfies some felt preference of an individual.

Now we can define transformative value of an object as "the ability of that object to alter demand values" (2005, 77). More precisely,

(*T*) An object has transformative value if it would alter a felt preference into some other felt preference.

Likewise, we also have the notions of direct and indirect transformative value (2005, 82 – 86).

(*T_D*) An object has *direct* transformative value if it would alter a felt preference into some other felt preference both of which directly concern biodiversity.

(*T_I*) An object has *indirect* transformative value if it would alter a felt preference into some other felt preference both of which indirectly concern biodiversity.

Sarkar raises two worries concerning the argument (2005, 95 – 98):

Directionality worry: Not all transformative values are positive – some are in fact negative. Some experiences of biodiversity in fact have negative transformative value.

Boundary worry: It does not appear that we can determine what objects and only those objects have transformative value.

His response to these worries is (2005, 98 – 103)

Response (DW): The indirect transformative value that biodiversity has – how it indirectly contributes to the sciences in particular – is mostly positive.

Response (BW): Draw a distinction between incidental and systematic transformative value. Biodiversity has systematic transformative value.

Here an object has *incidental* transformative value if it transforms demand values for only a few individuals and there is no systematic rationale for extending that transformation for other individuals or other objects. An object has *systematic* transformative value if it transforms demand values for more than a few individuals and there is a systematic rationale for extending the transformation for other individuals and objects.

The argument above is obviously valid though I think there are reasons for being suspicious of the first and second premise. First let me “unpack” the first premise. Note that not every transformation is such that we want to promote the transformative value of that object. Bryan Norton – the first philosopher to explicitly provide an account of the notion of transformative value – reminds us of “bad apples” (1987, 11). So, we need to revise our account to mark the distinction between positive and negative transformative value and Sarkar recognizes this. We can define positive and negative transformative value as:

(T^+) An object has positive transformative value if it would alter a felt preference into some morally superior felt preference.

(T^-) An object has negative transformative value if it would alter a felt preference into some morally inferior felt preference.

But now the question is what makes a felt preference morally superior to some other felt preference? Norton’s original answer occurs through distinguishing between *felt* and *considered preferences*. Felt preferences are simply those desires or wants which can be

satisfied by some specifiable experience of that individual or some state of affairs. A considered preference is any preference that can be similarly satisfied by some specifiable experience or state of affairs and is one an individual would have after careful reflection. What is careful reflection? Norton writes that careful reflection

...is taken to include a judgment that the desire or need is consistent with a rationally adopted worldview, which in turn includes a set of fully defended scientific theories, an attendant metaphysical framework interpreting those theories, and a set of rationally developed, fully defended aesthetic and moral ideals (1987, 9; see Sarkar 2005, 100 for discussion).

So, we can now we can more clearly express (T^+) as follows:

(T^+) An object has positive transformative value if it would alter a felt preference into some considered preference.

Likewise, we can revise the first premise as,

If an object or process would alter a felt preference into some considered preference, then we should preserve that object or process.

Now that we have clarified (1), we must turn to (2). If there is some objective standard against which the satisfaction of preferences are determined to be morally valuable or not, then judgments of transformative value are dependent on such considerations. Moreover, if we antecedently disagree that the preferences that result from transformation by biodiversity are morally superior ones, then claims of transformative value will be ineffective since they depend just on this point. That is, all parties could agree on the transformation but disagree as to whether the transformation is positive or negative. In environmental debates, there is an enormous amount of debate over what preferences are considered. If the value of biodiversity fundamentally rests on reshaping what it is to be human, biomimicry, the demise of western religions, and similar considerations then claims of transformative appear to be unpersuasive (2005, 83 – 86). Thus, claims of transformative value are effective only if we are already agree on what demand values

themselves are considered. So, in conclusion, for us to determine whether (2) is true, we must have already settled the debate over whether environmentalist preferences are considered or not. Hence, transformative value attributions will be ineffective unless we have resolved these controversial moral matters.

It is also worth noting that Sarkar is very critical of existing accounts of intrinsic value for environmental objects, and in my view, rightfully so (2005, 45 – 74). However, once one introduces the notion of “careful reflection” and “considered preferences”, then it appears that what are called “dispositional” accounts of value might avoid the worries that plague earlier accounts of intrinsic value. For example, suppose that we can articulate and defend the following:

Something is intrinsically valuable just in case it would be valued under ideal conditions for its own sake.

To add some details, consider an adapted version of David Lewis’ (1989) account value.

Something is intrinsically valuable just in case one would desire to desire it for its own sake under full imaginative acquaintance.

On Lewis’ account what is desired are propositions (or properties) and so one might have idealized second-order desires concerning propositions like, “Monarch butterflies and their migratory behavior continue existing”. Thus, with much work, there may be space for a defensible account of intrinsic value.

Now let me turn to issues concerning ecology and whether we are in an extinction crisis.

III. Species Area Curves and Extinction Crises.

Sarkar correctly recognizes that current extinction rates pose a problem only if there is a “crisis” which is occurring or will occur as the result. However, one must properly characterize what such a crisis is before one can proceed. He notes, “It is

impossible to say anything sensible about the latter question unless the term ‘crisis’ is precisely defined” (2005, 134–135). Sarkar adopts the convention that there is such a crisis if extinction rates are approximately those of any of the past mass extinctions. More precisely this amounts to the claim that there is a biodiversity crisis only if the extinction rate is at least twice the background rate. First, I suggest that this notion misses something crucially important. The notion of a *crisis* connotes that something of value is at risk of being lost and Sarkar’s notion does not entail this. That is, Sarkar’s definition is purely descriptive whereas it appears that an adequate notion of “crisis” would have a normative element as well. However, I will place that worry to the side.

There are three general approaches to projecting extinction rates. First, there is the approach which utilizes species area models which we will consider. Second, there is an approach which examines how well-studied taxa move through the categories *vulnerable*, *endangered*, *probably extinct*, and *certified extinction* on the International Union for the Conservation of Nature and Natural Resources (IUCN) “red lists”. Third, there is an approach which uses IUCN data to estimate the probability of extinction as a function of time. Nevertheless, if we are to determine that we are in a biodiversity crisis, then we must determine three rates: the background extinction rate, the current extinction rate, and the projected extinction rate. If the projected rate is twice that of the background rate, then we have reason to believe we are in an extinction crisis. Let me now present one of the most common arguments for believing that we are in a biodiversity crisis which depends on the species area relation and of which Sarkar is skeptical (2005, 120 – 121; 142 – 144).

The species area relation has quite the legacy in ecology. Ecologists have long recognized that as area increases, so does the number of species in that area. The biogeographer Phillip Darlington surmised that with every tenfold increase in area, the number of species doubles. Michael Rosenzweig writes, “You will find more species if you sample a larger area. That rule has more evidence to support it than any other about species diversity” (1995, 8). Ecologist Nicholas Gotelli claims that the species-area relationship is one of the few laws in ecology (1995, 172).

One species-area model representing this relation that fits many data sets is the power function,

$$S = cA^z \quad (1)$$

where c is a fitted constant, and z is a parameter that generally has values specific to the type of area under consideration.

Ecologists Robert May, John Lawton, and Nigel Stork in their essay “Assessing Extinction Rates” make an argument for an extinction crisis based on the following approximation. We can provide a linear approximation to the species area curve at the point $(S_{\text{original}}, A_{\text{original}})$ by following equation.

$$\Delta S = z \Delta A \quad (2)$$

where ΔS is the proportional reduction in species richness and ΔA is the proportional reduction in area. Their argument applies this model (2) to global losses of species with a conservative value of $z = 0.25$. Rates of tropical deforestation have been claimed to be in the range of 0.8 – 2% per year. If $\Delta A = 0.8\%$, then our minimal projection of $\Delta S = 0.2\%$. If $\Delta A = 0.2\%$, then our maximal prediction of $\Delta S = 0.5\%$. So, between 0.2% and 0.5% of species will go extinct per year. May, Lawton, and Stork suppose conservatively that

there are 5 million species globally. Thus, our model and estimates project a minimal loss of 10,000 species per year and projects a maximal loss of 25,000 species per year. This is equivalent to an average species' lifespan between 200 and 500 years. Finally, this is also equivalent to losing approximately 1 to 3 species per hour.

Sarkar makes the following claims about the background rate of extinction, “Then the background extinction rate, as estimated from the fossil record is about 1 species per year... Probably no one would be surprised if this estimate is off by a factor of 10 or even 100” (2005, 139). Thus, it appears even with the uncertainty mentioned – even if the background rate was off by a factor of 100 and the low projection by May, Lawton, and Stork is right – we are in an extinction crisis.

Why would one be skeptical of the above argument? It is crucial to note that there are a variety of uncertainties involved in our species-area argument. First, our model is idealized since we are assuming that loss of species is only a result of loss of area and thus we are ignoring many important causal factors related to extinction like exotic species, disease, overhunting, habitat fragmentation, edge effects, and habitat diversity. Second, we do not know exactly how much habitat we are losing per year. At best, we are losing between somewhere between 10 and 15 million hectares of closed tropical rainforest per year (and tropical rainforest contain approximately $\frac{1}{2}$ of the species that exist). Third, we do not know how many species currently exist within an order of magnitude. There may be as many as 5 to 30 million extant species given our best evidence and our very poor knowledge of taxa other than mammals, birds, and some insects.

Nevertheless, Sarkar believes that this type of argument is very problematic for other reasons: “The trouble is that the species-area curve is notoriously unreliable and there is little reason to trust these estimates” (2005, 143) and “...except for the ecological truism that species richness increases with area, there is little value that the species-area curve and the theory of island biogeography contribute to reserve design” (2005, 123). First, he correctly notes that there are data sets which the above the species area power function does not fit well (2005, 122 – 123). Nevertheless, there are many, many data sets at a variety of spatial scales from areas in provinces, archipelagos, and across provinces for which the power law model does fit well. Second, the mechanism which explains the species area relationship is very much debated (McCoy and Connors 2001, 397 – 399). For example, is it that larger areas per se can support more species? Is it that larger areas contain more habitats and greater number of habitats supports more species? Is it that larger areas are more likely to receive colonists than smaller areas? Finally, do larger areas contain a greater number of resources and thus larger areas support more species? Though the mechanism or mechanisms generating species area relationships – be it area per se, habitat diversity, passive sampling, or resource concentration – is of great importance the species area model can be silent on this since it represents this pattern independent of the mechanisms involved. Even if the species area relation does not explain why species diversity increases with area this does not invalidate the pattern itself. Third, some have argued that the since the equilibrium model of island biogeography has been seriously criticized as well as the SLOSS debates that resulted from it, we should be similarly skeptical of the species area model above. However, the species area relationship and the curves that model it are distinct from the theory of island

biogeography. The latter attempted to explain such relationships by examining how rates of immigration to islands from the mainland and rates of extinction on islands lead to steady state diversities. However, again we should not confuse a purported explanation of a pattern with the pattern itself whether it is Preston's attempted derivation from the lognormal distribution (1948), MacArthur and Wilson's equilibrium model (1967), Harte, Kinzig, and Green's self-similarity hypothesis (1999), or Hubbell's neutral theory (2001). Let me now consider a few additional points.

First, as more research has been done on species area relationships the above argument actually appears "optimistic" (Rosenzweig 2003, 199). Note that the species area model – contrary to some critics – applies to any type of area not just islands; it is the z -value which pertains to the specific type of area. The z -values as measured empirically actually depend on the scale at which one is considering. "Sample area" species area relationships (SPARs) have a z -value between 0.1 – 0.2, "archipelagic" SPARs have z -values between 0.25 – 0.55, and "interprovincial" SPARs have z -values between 0.6 – 1.0 (Rosenzweig 2003, 195 – 196). So, let us just consider the proportion of species remaining as determined by the proportion of area remaining. That is, given S' and S are the new and old number of species respectively and A' and A are the new and old amount of area respectively, then:

$$\left(\frac{S'}{S}\right) = \left(\frac{cA'^z}{cA^z}\right) = \left(\frac{A'}{A}\right)^z$$

If we suppose the Earth is like an archipelago as May, Lawton, and Stork do, then we could argue for example that a reduction to 2% area with a $z = 0.25$ would leave 37% of

the species remaining. However, in looking at global losses of species we should not be looking at archipelagos but provinces. As Michael Rosenzweig notes,

A biogeographical province is a region whose species have evolved within it, rather than immigrating from somewhere else. Although the concept is merely an ideal – every place has at least a few species that arrived as immigrants – it is close to true in many places, such as different continents or well-separated periods in the history of life. (2003, 195 – 196).

Thus, for provinces their steady state derives from their rate of speciation and rate of extinction. He goes on,

The world of nature reserves is not an island but a shrunken province. Its source pool is the past. Species that become extinct in it cannot immigrate from the past to recolonize the world of the future. So, like any evolutionarily independent providence, our miniaturized natural world must seek its steady state along the interprovincial SPAR, not the island SPAR. (Rosenzweig 2003, 200)

The appropriate z -value is then between 0.6 – 1.0. Given the model above, then we have for a reduction to 2% area and a $z = 0.8$ and so 4% of species remaining. Given more accurate considerations of z -values and our species area model, it looks like matters could be far, far worse.

Second, we should distinguish between skepticism concerning the species-area *relationship* and species-area *curves*. The former is the claim that is a positive monotonic relation between area and species diversity and he certainly does not doubt this. Rather it is the functional form that the species area relation is supposed to take that is suspect. There are a variety of mathematical forms consistent with the species area relation and no one of which is fits every data set well (see Figure 1).

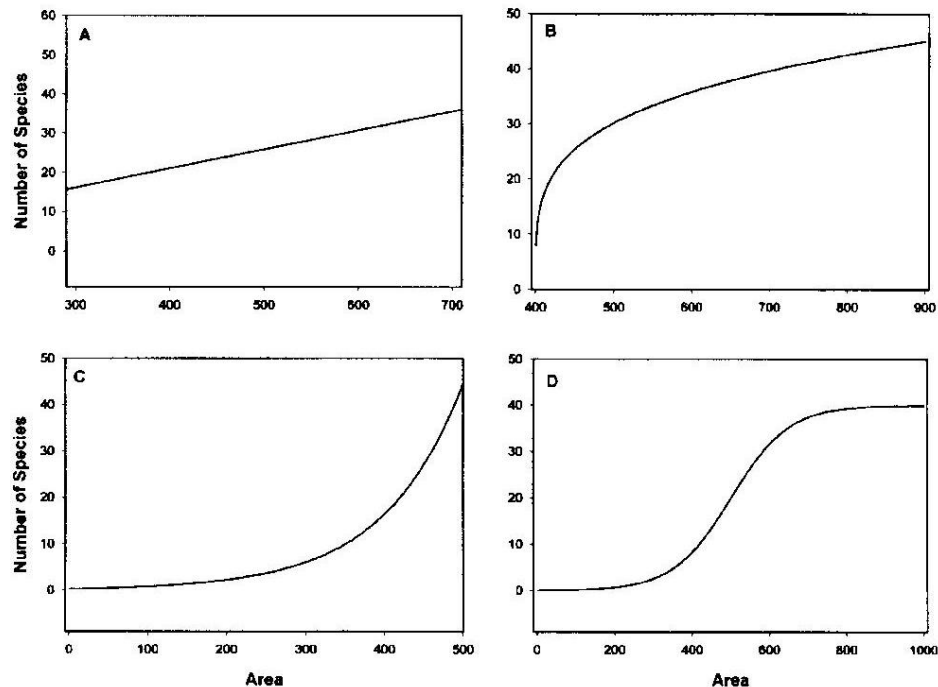


Figure 1. Shapes of species-area curves encountered in empirical studies: (A) linear species-area curve; (B) concave-downward species-area curve (linearized by the log-log transformation); (C) concave-upward species-area curve (linearized by a semilogarithmic transformation, area log-transformed); (D) sigmoid species-area curve (usually not transformed). Linear curves are encountered at intermediate spatial scales, concave downward curves at larger spatial scales, concave upward curves at small spatial scales, and sigmoid curves when a wide range of spatial scales is studied (from Connor and McCoy 2001, 402).

Nevertheless, even if we replaced the power law with some other functional form we would get qualitatively similar results. As biologist W. V. Reid writes, “...it can be argued that the exact rate of extinction is not terribly important given that current extinction rates greatly exceed background rates” (1993, 55). Daniel Simberloff writes, “Nevertheless, it is a worthwhile exercise to use the species-area relationship to attempt a first guess at how many extinctions deforestation will generate in tropical forests” (1993, 78).

Thus, though we do not know how many species we are losing per year, we have good reason to believe we are losing more than twice that of the background rate of

extinction Sarkar's arguments notwithstanding. Hence, we have good reason to believe that we are in an extinction crisis as Sarkar has characterized it.

IV. Defining 'Biodiversity'

Informally, biodiversity is characterized as variation at all biological levels—from the gene to the individual to the population to species to the ecosystem. However, this proposal is impractical. As Sarkar writes,

Conserving biodiversity, and construing the term intuitively to refer to all the biological diversity that there is, at every level of both hierarchies amounts to say that “biodiversity” refers to all biological entities. “Biodiversity” in effect becomes all of biology. Conservation would be an impractical proposal if biodiversity were construed in this way: everything biological would become a goal of conservation. (2005, 180)

Customarily, we then characterize biodiversity as variation in genes, species and ecosystems (2005, 180). This is a better proposal since through these three targets, we capture much of the variation of interest. Sarkar argues that important objects of conservation are lost if we then suppose we can conserve everything of interest by preserving biodiversity in this sense. For example, the behavior of migratory of some monarch butterflies would not be preserved under this account (2005, 180 – 182). However, if we aim to preserve species and for at least for instrumental reasons their populations and properties, then surely we should preserve populations with exceptional migratory behaviors. Nevertheless, I will let this point pass.

Sarkar attempts to define biodiversity *implicitly* rather than *explicitly*. To define a concept *explicitly* we formulate a set of necessary and sufficient conditions for the satisfaction of the concept. For an implicit definition, the concept is defined by a set of axioms, rules, or a procedure in which the concept appears—in effect, it is defined by its “role” in set of propositions or as the “output” of a procedure. He writes, “...biodiversity

should be (implicitly) operationally defined as what is being optimized by the place prioritization procedures that prioritize all places on the basis of their biodiversity content using true surrogates” (2005, 182). What is a place prioritization procedure? Abstractly, a place prioritization begins with a list of “cells” or places and a list of surrogates that must be represented at a certain “target” in a conservation area network. Ideally, we then would like to select the smallest set of cells such that every one of our surrogates meets its target or select those cells subject to a given size which maximizes the number of surrogates over and above the target. In order to see more concretely how this works, we must make distinctions between biodiversity, true surrogates (target variables), and estimator surrogates (estimator variables) (2005, 168 – 170). True surrogates consist in species, characters or traits, life-zones, or environmental parameters which represent “general biodiversity”. Estimator surrogates consist in environmental parameter composition, soil-type composition, dominant vegetation composition, subsets of species composition, and subsets of genus or other higher taxon composition. So, biodiversity is preserved by preserving true surrogates through preserving estimator surrogates. Very roughly then, a place prioritization procedure optimizes the number of estimator and hopefully true surrogates with respect to the least number of places and what is so optimized is biodiversity.

I have worries about this way of characterizing biodiversity. This concept of *biodiversity* incurs the problems associated with all operationalized concepts. First, the concept will be defined as what a place prioritization procedure maximizes – if the procedure cannot or should not be used due to a lack of data concerning surrogates of choice or the estimator surrogates do not reliably represent true

surrogates and thus “general biodiversity” for example, then the concept with respect to a given set of places will be undefined or will incorrectly represent “general biodiversity”. Surely, how much biodiversity a place contains is not simply determined by our measurement procedures? Second, different place prioritization procedures (with possibly different optimal outcomes) will define a different biodiversity concept by definition. However, this seems to multiply concepts beyond necessity – surely different place prioritization procedures do not define different biodiversity concepts. Third, when one explicates a concept, the explicatum and the explicandum must be sufficiently similar for the analysis to be of value. It is not clear that the concepts are sufficiently similar in this case for two reasons. Sarkar notes that what he has characterized is a *relational* concept: a place has more, equal, or less biodiversity content than another place. The concept we started with is a *categorical* concept and is not relativised in this way. Similarly, the concept he has articulated certainly may be of crucial use to conservation biologists, but the concept as formulated by Rosen and others appears to be left behind. As science studier David Takacs writes,

Biodiversity is the rallying cry currently used by biologists to draw attention to this crisis and to encapsulate the Earth’s myriad species and biological processes, as well as a host of values ascribed to the natural world (1996, 9).

In what way is this concept Takacs mentions “the same” as the one Sarkar defines?

V. The Aim and Structure of Conservation Biology – An Open Question.

Conservation biology is often characterized by its practitioners as a “crisis discipline” (Soulé 1985). That is, it has normative foundations at its core; specifically, it has the norm *preserve biodiversity* at its center. In this, it is more similar to say medicine than it is to ecology or evolutionary biology (Odenbaugh 2003). However, it is clear that conservation biology draws much from ecology, genetics, biogeography,

microeconomics, policy analysis, systematics, and ultimately even disciplines like philosophy. Sarkar argues that conservation biology consists in “a consensus framework for adaptive management”—more specifically, it consists in the systematic conservation planning through the “adaptive management of landscapes” (2005, 151 – 159). I have misgivings that we can construe all of conservation biology as essentially systematic conservation planning. Some of this “meta-discipline” concerns issues like overexploitation and maximum sustainable yields; policy issues like “debt for nature” swaps and international environmental law and trade agreements; restoration of degraded wetlands, prairies, and tropical rainforest; changing habitat requirements of species given sea level rise, warmer waters, and anthropogenic climate change more generally; and the deleterious effects of exotic or invasive species. These issues do not fit neatly into systematic conservation planning though they are arguably key topics in conservation biology

I will not attempt to describe the rich structure that this suggested consensus framework possesses (Sarkar does a far better job than I could). I also wholeheartedly agree with Sarkar that philosophers ought to investigate the aims and structure of conservation biology; nevertheless I would like to close this essay with an open question for philosophers who continue the project initiated by Sarkar. It is unclear how and in what way (if at all), the aims and structure of conservation biology maps onto any of the favored units of analysis that has concerned philosophers of science. Is conservation biology a theory, a collection of models, a research program, a set of procedures, or a set of norms? As rich as the structure is that Sarkar describes, one is left wondering how conservation biology can expand our understanding of the sciences and what ways it is

consonant with what concepts philosophers have already labored over (2005, xi - xii; 218).

VI. Conclusion.

Though I have found a variety of problems with Sarkar's analysis of environmental ethics and conservation biology his work repays close analysis. I believe that his text will be an excellent foray for students and professionals involved both academically and on the ground with preserving biodiversity.

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