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Biodiversity is a chimera, and chimeras aren't real

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Abstract A recent article by Burch-Brown and Archer (Biol Philos, 2017) provides compelling arguments that biodiversity is either a natural kind or a pragmatically-valid scientific entity. I call into question three of these arguments. The first argument contends that biodiversity is a Homeostatic Property Cluster (HPC). I respond that there is no plausible homeostatic mechanism that would make biodiversity an HPC natural kind. The second argument proposes that biodiversity is a multiply-realizable functional kind. I respond that there is no shared function to ground this account. The final, and strongest, argument, is that biodiversity is an ineliminable explanans and explanandum in various subdisciplines of biology. I argue that once we look at the details of the relevant research, not only does biodiversity in a broad sense not function in explanatory roles, but we must eliminate biodiversity in favor of more specific concepts in order to make sense of the leading explanations in contemporary ecology and conservation science.

Keywords Biodiversity · Natural kinds · Ecology · Conservation · Philosophy of biology · Environmental ethics

This journal has recently published two excellent articles defending *biodiversity realism*, which holds that biodiversity is not merely a useful fiction or shorthand, but real in some sense. It can take various forms, seeing biodiversity as a natural kind, natural property or quality (Maclaurin 2016), or as a pragmatically-valid scientific entity that doesn't necessarily carve nature at a joint. In one of the two papers on the topic (the other being Lean 2017), Burch-Brown and Archer (2017) defend

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biodiversity realism from the specific threat of *biodiversity eliminativism*. They focus on eliminativism as outlined in recent work by Santana (2014, 2016), who takes the position that biodiversity is neither a unitary natural entity nor justified by appeal to the normative goals of conservation biology. Similar arguments have also been made by Maier (2012), who doesn't deny its natural existence but calls into question its normative and scientific utility, and Morar et al. (2015), who worry about both its ontology and its usefulness. In the face of these arguments for eliminativism, the recent papers make strong cases for realism. The ball is now in the eliminativist's court, and what follows is my attempt to pass it back to the realists.

I'll focus on some arguments that I think eliminativists have yet to provide a good rejoinder to. Burch-Brown and Archer propose two ways that biodiversity might be a natural kind: it could be a homeostatic property cluster, or a multiply-realized functional kind. I'll address both possibilities. They also argue that since biodiversity functions both as explanans and explanandum in multiple branches of biology, we must take it seriously as a scientific concept which picks out a causally-relevant real-world entity. Focusing on these arguments means ignoring other interesting arguments, like Burch-Brown and Archer's appeal to the normative value of biodiversity, and Lean's attempt to show that an explication of biodiversity in terms of phylogenetic diversity evades standard worries about the concept. But these latter types of argument are the subject of ongoing debate in the literature, and it's less pressing that I address them here.

Before responding to Burch-Brown and Archer's arguments, we need to reconstruct the earlier moves in the debate. Standard definitions of biodiversity are, to use Burch-Brown and Archer's term, *multidimensional* in that they encompass variation in multiple forms at multiple levels of organization (2017). Multidimensional biodiversity includes not only the number of different species (species richness) and their relative abundance, but also genetic diversity, phenotypic diversity, differences in evolutionary history such as phylogenetic diversity, diversity at community and ecosystem levels (e.g. ecosystem diversity, functional diversity, and trophic network diversity), and differences in the diversity represented between areas (complementarity and β-diversity), among other things. On the multidimensional account, these are all dimensions of biodiversity, which is an overarching and unitary entity.

Santana argues that "each plausible facet of biodiversity comes significantly apart" both empirically and conceptually from "the other facets" (2014: 768), and thus the "supposed correlations between these properties are not tight enough to warrant treating and measuring them as a bundle" (2014: 761). For example, Darwin's Finches "form a rich number of species but are not very diverse in the intuitive sense, given their morphological similarity" (2014: 768), nor do they exhibit high phylogenetic diversity, since their evolutionary radiation was relatively recent. Cases of divergence such as these, Santana argues, make it hard to argue that quantities such as species richness, phenotypic diversity, and phylogenetic diversity are all dimensions of the same overarching property (or entity or concept).

Burch-Brown and Archer astutely situate this argument in a tradition of philosophical eliminativism, prominent especially in philosophy of mind and cognitive science. The general schema of this eliminativist argument runs as follows:

(1) *x* is a higher-level concept decomposable into distinct lower-level concepts *w, y, z,* etc.

(2) The objects picked out by lower-level concepts *w, y, z* etc. don't tend to share either

a. A relevant cluster of properties, or
b. A high degree of observable correlations

(3) To support robust inductive generalizations across the group, a grouping must either share

  a. A relevant cluster of properties, or
  b. A high degree of observable correlations

(4) [From (2) and (3)] The grouping picked out by lower-level concepts *w, y, z* etc. doesn't support robust inductive generalizations across the group

(5) To be a natural kind, natural quality, or pragmatically-valid scientific kind, a grouping needs to support robust inductive generalizations across the group

(6) Therefore, [from (1), (4), and (5)], *x* cannot be a natural kind, natural quality, or pragmatically-valid scientific kind

As instances of this argument schema, Burch-Brown and Archer cite Griffiths (1997, 2004) on emotion and Machery (2009) on concepts. Machery, for instance, identifies the psychological *concept* as the higher-level concept *x,* and *exemplars, prototypes*, and *theories* as the *w, y,* and *z* into which it can be decomposed. But since exemplars, prototypes, and theories are ontologically and functionally distinct, and don't support robust generalizations across the set of all three, they can't form a natural kind together. Instead, Machery argues, we should adopt the "Heterogeneity Hypothesis" which eliminates *concept* from technical psychological discourse in favor of the more specific categories like exemplar and prototype.

Santana (2014) instantiates the same argument schema in the case of biodiversity. For Santana, *x* is *biodiversity*, and *w, y, z,* etc. are species richness, ecosystem diversity, genetic diversity, etc. Since the eliminativist argument schema is valid, the realist needs to show which premise or premises don't obtain in the case of biodiversity. Burch-Brown and Archer raise several possibilities, which I'll address in turn.

First, they raise the possibility that (2) might not obtain because "the dimensions of diversity do tend to be reasonably strongly clustered, and that there are underlying mechanisms for this clustering" (2017). The idea is that biodiversity might be a Homeostatic Property Cluster (HPC) in the sense of Boyd (1999). The virtue of the HPC account of kinds is that it doesn't pick out any single property or set of properties as essential for kind membership, and thus tends to accommodate biological kinds better than alternatives. Instead, it requires the presence of a mechanism which maintains similarity among kind members. Reproductive barriers, for instance, ensure that members of sexually-reproducing biological species tend to be more similar to each other than to other organisms. Burch-Brown and Archer suggest that we might find a similar HPC account of biodiversity, which would undermine the eliminativist's premise (2). They don't develop this suggestion at all, however, presumably because it is implausible that there is any mechanism maintaining homeostasis across the different dimensions of biodiversity. Primarily this is because different dimensions of biodiversity are not even properties of the same types of entities. Some dimensions, like species richness, are properties of ecosystems and communities, some, like phenotypic and genetic diversity, can be properties of species and populations, and some, like ecosystem diversity and complementarity are properties of groups of ecosystems. It would be a strange mechanism indeed that worked on objects at all these different levels of organization. Moreover, factors which we know play a role in promoting some types of diversity we also know don't play a role in others. Species richness increases as latitude decreases, for instance (Hillebrand 2004), but whatever correlate of latitude causes this doesn't seem to affect every dimension of diversity. Genetic diversity within species, for instance, is probably not affected, and even species richness within certain taxa, such as certain avian and planktonic groups, turns out to have the opposite relationship with latitude (ibid). Because of the heterogeneity between the dimensions of biodiversity, other mechanisms which promote or inhibit diversity will be similarly limited in scope. Natural selection, to give another plausible candidate, can certainly promote speciation and thus species richness. But under most selective regimes it inhibits genetic diversity; mutation is the primary driver of genetic diversity, and the vast majority of genetic diversity created by mutation is quickly pruned away by selection. And selection's effect on trait diversity is mixed, since it acts as a promotor by sustaining novel adaptations but also as an inhibitor through convergent evolution. Natural selection, like the causal correlate(s) of latitude, is not a mechanism which acts in a unitary way to maintain clustering among the different dimensions of diversity. Given the disparity between these dimensions, any other candidate mechanism will probably also affect the different dimensions of biodiversity in varied and misaligned ways. For this reason, it is implausible that there is any homeostatic mechanism which would make biodiversity an HPC.

Perhaps recognizing this, Burch-Brown and Archer suggest a "different strategy" which involves positing that "the property of biodiversity is a natural kind which can be multiply realized" (2017). Here, again, the inspiration is from debates in philosophy of mind. Against reductionism and eliminativism the functionalist argues that mental states which play similar functional roles might have disparate physical substrates (Putnam 1967, 1980). Pain, for instance, might be functionally similar in humans and some extraterrestrial or evolutionarily distant species, despite fundamental physiological differences between us and them. This is no reason to claim that pain doesn't exist; it just entails that pain isn't equivalent to its physical realization in human bodies. The eliminativist thus must argue that not only do *x, y, z,* etc. not share a relevant cluster of physical properties, but they don't share equivalent functional roles. If they do share functional roles, then they will possess observable correlations or a relevant cluster of functional properties, and premise (2) of the argument scheme will be false. To illustrate: supporting concept eliminativism requires Machery to argue that not only are prototypes, exemplars, and theories realized differently in the brain, but that they often play different functional roles in human cognitive architecture. The realist, conversely, will want to demonstrate that they fill similar functional roles.

When it comes to biodiversity, as Burch-Brown and Archer acknowledge, "the challenge for this strategy is to show that systems that are biodiverse in different ways really do share something important in common" (2017). They don't provide any possible examples of such a commonality, however, presumably because the disparity between the different dimensions of diversity makes it unlikely that they share any functional role. Functional diversity, for instance, clearly plays a role in perpetuating biogeochemical cycles, but, given functional redundancy in many systems, species richness does not (e.g. Beare et al. 1995). It's hard to imagine what functional role all the disparate dimensions of biodiversity might share. As far as I can tell, the leading possibility is that all dimensions of biodiversity provide ecosystem services (Millennium Ecosystem Assessment 2005). But 'provides ecosystem services' isn't a compelling functional role for several reasons. First, *ecosystem services* is itself a messy, heterogeneous concept (Mace et al. 2012; Polasky et al. 2015). It includes everything from clean water, pollination of crops, and fun trails to hike on, to stress relief, transformative religious experiences, and entrepreneurial opportunities. Appeal to ecosystem services is thus unlikely to sway the eliminativist, who will be disposed to see ecosystem services, like biodiversity, as a convenient lumping together of disparate entities rather than a clean scientific kind. Even putting that aside, however, 'provides ecosystem services' is a function too abstract and general to ground a useful functional kind. Consider an analogy with 'keeps the author of this paper alive.' The set of things which contribute to that abstract and general function includes most types of cell in my body, the nutrients I eat, the employment that allows me to purchase those nutrients, the social institutions which ensure that the money I earn can be exchanged for nutrients, and so on. It would be ludicrous to say that we should treat my t-cells, the soymilk in my fridge, and the Federal Reserve as all members of a well-defined natural or scientific kind merely on the basis that they all contribute to a broadly-defined functional process. Certainly there aren't many robust inductive generalizations that could be made across that kind. Likewise, the list of things which plays the abstract functional role of provisioning ecosystem services will include much more than the various dimensions of biodiversity—unless cumulonimbus clouds and solar radiation and beautiful rock formations are all counted as part of biodiversity—and thus not justify treating biodiversity as a kind. Moreover, the function is specified in too general a way to warrant defining a scientific kind on its basis. In sum, claiming that biodiversity is a multiply-realizable functional kind is unlikely to succeed as a strategy for the realist.

Given the implausibility of seeing biodiversity as either an HPC or a multiply-realizable functional kind, Burch-Brown and Archer wisely spend more time developing an argument from the role biodiversity plays in scientific explanation. As I read it, this argument targets premise (5) of the eliminativist schema, contending that the "multidimensionalist view does not depend on the dimensions of diversity correlating with one another," since "there are other conditions under which a higher-level concept like biodiversity is valuable" (2017). Specifically, in the context of explanation a more abstract, higher-level concept may be more useful than a collection of lower-level concepts, "whose relationships would otherwise be too complex to capture easily" (ibid). Given the sheer complexity of ecological interactions, biodiversity is a pragmatically-justified scientific concept because it is a tractable explanans and explanandum, while the collection of its individual dimensions isn't. In other words, the eliminativist argument falls short because it demands utility in the domain of prediction—c.f. "robust inductive generalizations"—but concepts can also be useful in the domains of explanation and understanding.

I must concede this point, and so grant that the eliminativist argument schema is incomplete as I've presented it. Here's a revision of the argument, instantiated for biodiversity, and modified to account for the utility of explanation:

(1) *Biodiversity* is a higher-level concept decomposable into distinct lower-level concepts *species richness, ecosystem diversity, genetic diversity,* and others [henceforth "SEGO"]

(2) The objects picked out by SEGO don't tend to share either

  a. A relevant cluster of properties, or
  b. A high degree of observable correlations

(3) To support robust inductive generalizations across the group, a grouping must either share

  a. A relevant cluster of properties, or
  b. A high degree of observable correlations

(4) [From (2) and (3)] The grouping picked out by SEGO doesn't support robust inductive generalizations across the group

(5) To be a natural kind or natural quality a grouping needs to support robust inductive generalizations across the group

(6) Therefore, [from (1), (4), and (5)], biodiversity cannot be a natural kind or natural quality

(7) To be a valid scientific kind a concept needs to either

  a. Be a natural kind or natural quality
  b. Or function ineliminably as an explanandum or explanans

(8) Biodiversity doesn't function ineliminably as an explanandum or explanans

(9) Therefore, [from (6), (7), and (8)], biodiversity isn't a valid scientific kind.

Under this restructuring of the argument, the disagreement comes down to premise (8). Burch-Brown and Archer's argument contends, contra eliminativism, that biodiversity does function ineliminably in explanatory roles.

Following Burch-Brown and Archer, I'll address each explanatory role independently. First, let's examine their contention than biodiversity functions as an explanandum. The sheer fact of life's variety, they argue, is one which many branches of biology try to explain. Darwin and Wallace, for instance, took as the goal of evolutionary theory to explain not only adaptiveness, but also to explain what we would now call biodiversity. As 19th Century naturalists collected specimens, they were astounded by the differences in form among those specimens, even among similar creatures, by which "they were constantly led to think upon the 'why' and the 'how' of all this wonderful variety in nature" (Wallace, quoted in Burch-Brown and Archer 2017). Diversity has continued to be a target of explanation not only in evolutionary theory, but other branches of biology, like ecology. Theories of biogeography, for instance, are in part aimed at explaining diversity within and between ecosystems.

I'm happy to grant that, speaking loosely, biodiversity is a key explanandum in biological science. But only speaking loosely: speaking strictly, biodiversity isn't an *ineliminable* explanandum.[1] On the contrary, in scientific practice it is in effect eliminated in favor of more specific alternatives, with talk of "biodiversity" merely functioning as a convenient shorthand for some subset of the dimensions of biodiversity. Wallace's curiosity isn't about biodiversity of every sort, but specifically about the "almost infinite number of its specific forms, the endless modifications of structure, shape, colour, and surface-markings that distinguish them from each other, and their innumerable adaptations to diverse environments" (quoted in Burch-Brown and Archer 2017). It is phenotypic diversity in particular that Wallace thinks we need evolutionary theory to explain. Likewise, theories of biogeography don't purport to explain biodiversity in all its various dimensions, but only particular kinds of biodiversity. MacArthur and Wilson's seminal work on the topic (1963, 2001), for instance, models only species richness, and is explicitly not about relative abundance, trait diversity, phylogenetic diversity, or many other dimensions of biodiversity. Other work, of course, can try to explain these other dimensions. Hubbel's Unified Neutral Theory (2001), for instance, is about species abundances as well as richness, but also explicitly abstracts away from genetic and phenotypic diversity within functionally-similar groups. Additionally, in Hubbel's theory functional diversity is taken as a given, and thus cannot be an explanandum of his theory. This isn't an indictment of the theory, merely an observation that particular theories of biogeography, just like particular models of evolution, purport to explain only a

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[1] An anonymous reviewer worries that this is too weak, since my claim here is merely that biodiversity is eliminable, which doesn't logically entail that biodiversity should necessarily be eliminated. The logical point is apt, and worth calling attention to, but the weak claim is sufficient in the context of my broader argument. The weak claim is enough to establish premise 8 of the revised eliminativist argument, which is what I take to the be point of contention between Burch-Brown and Archer and me. The reason premise 8 need not be stronger is because the appeal to practical explanatory utility is a last ditch effort to rehabilitate a scientific kind which has failed to carve nature at its joints. But such a last-ditch justification falls apart if there is an equally practical alternative which better serves the same explanatory ends, and as I argue in much of the rest of the paper, eliminativism provides just such an alternative to biodiversity.

limited subset of the dimensions of biodiversity rather than biodiversity in general. In other words, when biologists say that “biodiversity” is the explanandum of their theory, this is mere loose talk. What the biologist really means is that her theory is meant to explain species richness, or genetic diversity, or phylogenetic diversity, and so on, but not all the above. Consequently, biodiversity isn’t ineliminable as an explanandum, it’s just a useful linguistic shorthand that is in practice eliminated in favor of more specific concepts.

We have good reason for this elimination and reduction to more specific contexts as well. Evolution and ecology explain the different aspects of biodiversity in different ways. The geographical and evolutionary factors that explain speciation, for instance, are not the same factors which explain ecosystem diversity. For example, distance between populations can be the primary explanatory factor for their speciation, but the primary factor explaining why one equatorial ecosystem is a rainforest and another is desert will not be the distance between them, but climatic and geological features. Because each dimension of biodiversity has a different explanation, we actually understand biodiversity better when we eliminate it in favor of lower-level concepts rather than treating it as a unified explanandum, contrary to what Burch-Brown and Archer argue.

Burch-Brown and Archer also contend that biodiversity is an important explanans in contemporary ecology and conservation biology. It plays a key role, they argue, in explaining ecosystem stability and ecosystem functioning. They cite recent reviews by ecologists that have stressed this (Balvanera et al. 2006; Hooper et al. 2005), leaning especially on Cardinale et al. (2012), which is comprehensive and authored by many of the leading researchers on the relationship between biodiversity and ecosystem function. Cardinale et al. (2012) list six claims that they see as being supported by the balance of evidence from the last two decades of research, including that “biodiversity increases the stability of ecosystem functions through time” and that “diverse communities are more productive”. Moreover, the research they are drawing on uses measures of different dimensions of biodiversity—functional measures as well as measures of species and genetic diversity—so as Burch-Brown and Archer read it, “it is not just diversity in any given dimension but also overall heterogeneity that is of ecological importance” (2017).

When we look at the empirical research on which these claims are based, however, it is diversity in specific dimensions, and not diversity in general, which is causally and explanatorily relevant. My claim here isn’t merely that the relevant ecological experiments use only one or two dimensions of biodiversity as a measured variable. There are good operational reasons for doing so[2] even if biodiversity more broadly is what the experimenter is after. I’m making the stronger claim that in the experiments and models used by ecologists only a subset of dimensions of biodiversity plausibly plays any explanatory role. For example: why are diverse communities

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[2] Mainly that biodiversity in the multidimensional sense isn’t objectively measurable. Even some prominent biodiversity proponents acknowledge that “there will always be some way of comparing (say) one wetland to another that will count the first as the more diverse, and another procedure that will reverse the result” (Maclaurin and Sterelny 2008: 133).

more productive, where *productivity* is the rate of biomass production? According to Cardinale et al. they are more productive because "they contain key species that have a large influence on productivity, and differences in functional traits among organisms increase total resource capture" (2012). They aren't appealing to biodiversity in all its multiple dimensions as an explanans, but only appealing to two dimensions—species and functional diversity. They know they are warranted in doing so because the research on the relationship between diversity and productivity primarily highlights those two dimensions. The research they cite on the relationship between biodiversity and productivity (Loreau and Hector 2001; Cardinale et al. 2011) tackles the specific question of which mechanisms explain the relationship. One of the two mechanisms the literature explores is *complementarity*,[3] which is when different species (or members of different functional groups) efficiently partition resources or positively reinforce each other. "One common form of complementarity in plant communities," to use an example from Loreau and Hector, "arises between legumes, which have the ability to fix atmospheric nitrogen, and other plants, which have access only to soil nitrogen" (2001: 72). Both because the legumes use different resources than the other plants, and because they produce resources useful to the other plants, their presence increases the productivity of the community. The other mechanism studied in the literature is the effect of "single, highly productive species" (Cardinale et al. 2012), or what Loreau and Hector call the *selection effect*. The idea behind the selection effect is that the more species you throw into a competitive community, the more likely it is that the community becomes dominated by a species which is highly productive in the community's environment. On the assumption that "the biomass of a mixture of species is equal to the monoculture biomass of the most productive of the component species," productivity will sometimes increase and never decrease with an increase in species richness (ibid).

The literature debates the relative explanatory importance of complementarity and the selection effect,[4] but for present purposes what matters is that neither explanation even purports to be an explanation in terms of biodiversity in all its dimensions. Both are specific in the mechanisms they propose, and in both cases those mechanisms are specific to species and functional diversity, not biodiversity in every dimension. Take the legume example: it is implausible that functional diversity in that case is meant as an operational measure for biodiversity more broadly, because the functional diversity itself fully explains the effect. We don't need to appeal to other sorts of differences, such as the phylogenetic diversity between legumes and other plants, in order to explain how they complement each other as part of the nitrogen cycle. Nor is it clear that that phylogenetic distance (or genetic diversity, etc.) would add anything to the explanation, since in this case it is just an accidental

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[3] Note that in this context the ecologist's sense of the term is used, as defined in this sentence, and not the conservationist's, for whom complementarity is a dimension of biodiversity between communities or ecosystems.

[4] Of the cited reviews, for instance, Loreau and Hector (2001) lean heavily towards complementarity, because they find that the selection effect varies in strength from system to system, and is even often negative. The more recent Cardinale et al. (2012), on the other hand, argues that complementarity and the selection effect are each 50% responsible for the biodiversity-productivity link.

correlate of the actually-explanatory functional diversity. Ditto for cases of selection effect. It's true that "the vast majority of experimental manipulations and available data" on biodiversity and productivity "consider species richness as the primary aspect of diversity" and so our current explanations may be missing out on other explanatorily-relevant causal factors at other levels of organization (Cardinale et al. 2011). But that's just it: to the extent that other dimensions of biodiversity play a role in explaining productivity, they will do so through independent causal factors, not through some unitary mechanism involving all the different dimensions of biodiversity. Keep in mind that only some units of biodiversity (genes, species) are plausible units of natural selection as well, and others (most phylogenetic groups, ecosystems) aren't. So, it isn't possible that natural selection can be the mechanism by which biodiversity in all its dimensions promotes productivity. In other words, it isn't possible that the selection effect is an effect of biodiversity in general rather than of a few individual dimensions of biodiversity. The same sort of problem arises in the case of complementarity. if we want to understand what makes some communities more productive than others, we need explanans at the level of individual dimensions of diversity, and not biodiversity in the multidimensional sense.

Given the potency of the argument that biodiversity functions as an ineliminable explanans, it's worth running through another counterexample. This time we'll dip into the most venerable segment of biodiversity-ecosystem function research: diversity-stability. The *diversity-stability hypothesis* states that more biodiverse ecosystems are more stable. Over the decades, ecologists have seesawed about whether the diversity-stability hypothesis is true (Justus 2008), but at present there is a tendency, though not necessarily a consensus, among biodiversity-ecosystem function researchers to think that it holds. Some explanations of a correlation between biodiversity and stability propose a common cause, such as nutrient availability, that promotes both properties, but many explanations assign biodiversity a causal role in promoting stability. The possibility that biodiversity in a multidimensional sense does ineliminably play this explanatory role is excellent fodder for the realist, as Burch-Brown and Archer realize.

Unfortunately for the realist, the right answer to the question, "Is the diversity-stability hypothesis true?" is "It's complicated," and the chief complication is that the hypothesis is framed in termed of higher-level umbrella concepts in need of reduction and elimination. Let's start with *stability*. In a review in *Nature*, McCann (2000: 230) identifies six different concepts of stability ecologists may be interested in. A system possessing *equilibrium stability* returns to an initial state after a small perturbation. A population possesses more *general stability* the further the floor of its population fluctuations is from zero. A less *variable* system or community exhibits less statistical variance in population densities. *Equilibrium resilience* is the ability of a system to return to an initial state rapidly after a perturbation, whereas *general resilience* is the rate at which the system settles to any solution of the dynamic system, whether or not it is an equilibrium or close to the initial state. Finally, *resistance* is the ability of a system to avoid changing in the face of a disturbance. Justus

(2008) identifies another stability concept not covered by McCann, but of interest to ecologists: *tolerance*, which is the degree to which a system can be changed and still return to something like its initial state.[5] We have, therefore, at least seven different types of stability, and possession of one is largely independent of possession of the others. To give an extreme example, the Moon is very stable in the sense of not being variable (its population densities are always close to zero) and having high equilibrium stability (if we introduce a new species to the Moon, the Moon will quickly return to its initial state when that species dies, or flies back to Earth). But it has very low general stability, since all its population densities are zero. To give a more serious example, ecosystems dependent on periodic wildfires, such as the forests of Western North America (Agee 1998), might have high variability due to the fires, low resistance given the way fire transforms the landscape, and high resilience since the members of the community have adapted to recover quickly from fires and thus succession to a terminal state is rapid. All this goes to show that "stability" isn't a unitary concept. Instead, stability is a collection of intuitively, but not theoretically or empirically, similar properties. Consequently, there can be no unitary relationship between biodiversity and stability. Instead we can only probe how biodiversity could explain each individual aspect of stability.

As the reader should expect by now, once we zoom into the level of individual types of stability, biodiversity in the broad multidimensional sense isn't the best fit for explaining stability differentials between systems. Instead, sometimes individual dimensions of diversity are the best explanans, and sometimes factors other than diversity are the best explanans. Consider the most influential empirical work on diversity-stability, Tilman's work with grass communities (Tilman and Downing 1994; Tilman et al. 1998, 2006). Tilman and colleagues regularly found that more diverse grassland communities were more stable—speaking loosely. Strictly speaking, what they found was "that greater numbers of plant species led to greater temporal stability of ecosystem annual aboveground plant production" (2006: 629). In other words, they found a relationship between species richness and productivity. You need only jump back three or four pages to see my extended argument for why that relationship can't be understood as biodiversity in the multidimensional sense explaining productivity. Given the messiness that is "stability," the same reasons pop up twice over for not construing the relationship to be one of biodiversity explaining stability in any strict sense.

The realist will object that I'm taking operationalizations of biodiversity (species richness) and stability (productivity) as the end goal of the experiments and not as observables from which we infer the relative quantities of the higher-level concepts. For the realist's objection to obtain, however, the diversity-stability link would have to work through implausible mechanisms. Let's consider the leading proposals for why diversity explains stability. McCann (2000) identifies four primary theories. The first two draw on the fact that different species "respond differentially to variable background processes" (2000: 230). Given that fact, a more species-rich system

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[5] The definitions in this paragraph are glosses on more technical, mathematical definitions given by McCann and Justus.

is more likely to have species which respond to changing conditions differentially—some will do better and others do worse in the new conditions—meaning that the total biomass will be more stable under this *averaging effect*. Similar is the *Negative-covariance effect*, since species whose abundances covary negatively, such as competitors, will respond to changes in a way that balances out the overall biomass. A cold snap which kills snakes, for instance, will lead to an increase in the population of foxes who compete for the same prey, and vice versa, meaning that a community with both snakes and foxes will have a more stable biomass than one with only one or the other. Among the leading explanations of diversity-stability is the *insurance effect*: diversity provides redundancy, and redundancy buffers against cascading changes. Consider the fate of the Saguaro cactus, which is pollinated by bees. If colony collapse disorder kills off the bees, will the cacti also go extinct? No, because bats and doves also pollinate the Saguaros. Having a diverse array of pollinators in the Sonoran Desert buffers the Saguaros against being part of an extinction cascade if any one pollinator species dies off. That means the Sonoran Desert, an ecosystem anchored by the Saguaros, is more stable that it would be with a less diverse set of pollinators. Finally, McCann identifies a *weak-interaction effect*, the theory that more diverse ecosystems have more plentiful weak interactions between species. Strong interactions are, ironically, a weak link in ecosystem stability. On one hand, they are prone to turn into runaway consumption, and on the other their loss can require major adjustments from the system. But a plenitude of weak interactions "serve to limit energy flow in a potentially strong consumer-resource interaction" and also "may drive spatial variability in community structure," thus making the ecosystem more stable, at least in some senses of *stable* (McCann 2000: 232). There are, in sum, a few different (and non-exclusive) theoretical ways in which diversity could help promote and explain stability.

On consideration, however, none of these four theories implicates multidimensional biodiversity in that explanatory role. All four effects work mostly at the level of species diversity. Biodiversity at some other levels might participate in the effects more weakly. Genetic diversity within species, for instance, might also provide an averaging effect, but given the complex relationship between genetic difference and fitness under changing conditions, it would probably be a weaker effect than the species averaging effect. On the other hand, genetic diversity would provide little to no negative-covariance effect, since a reduction in the abundance of one gene won't typically entail an increase in abundance of another, and even if it did, it wouldn't further entail an increase in biomass (or whatever other measure of stability) elsewhere. Another example: most varieties of functional diversity will increase the weak interaction effect, since interactions are a type of function. But functional diversity does nothing for most versions of the insurance effect, since insurance is a form of functional redundancy, not functional diversity. Similar issues will apply to any dimension of biodiversity within ecosystems or lower levels of organization. Each dimension might help explain stability in some senses but not others, through some mechanisms but not others. Other dimensions of biodiversity play little to no role in explaining stability. Dimensions of biodiversity between ecosystems, like ecosystem diversity and β-diversity, can explain little about the stability of individual ecosystems because they are properties of the wrong sort of object.

The best explanations of ecosystem stability will thus be explanations in terms of specific dimensions of diversity, specific causal mechanisms, and specific types of stability, since no general relationship holds. Recall that Burch-Brown and Archer state that we need the higher-level multidimensional biodiversity concept to explain the workings of complex systems like ecosystems "whose relationships would otherwise be too complex to capture easily" (2017). The opposite it true. If we keep our lens zoomed out at the general level of multilevel biodiversity and stability, we should expect decades of seesawing on the relationship, because there is no unitary relationship. But if we tackle the complexity of ecosystems by zooming into examine specific dimensions of diversity, as the best experimental and theoretical work does, we have compelling and understandable explanations. "Biodiverse systems are more stable" resists consistent mathematicalization and is hard to test empirically. Compare: "More species rich communities are more tolerant of disturbances because it is more likely that the ecosystem functions performed by one species can be substituted for by another if the first species undergoes a population reduction." Now that's a good explanation! And part of what makes it good is that we've eliminated the umbrella concepts *biodiversity* and *stability* in favor of more specific concepts, which allows us to cut through the complexity of ecosystems to focus on one relevant mechanism or process. Contrary to what Burch-Brown and Archer suggest, the complexity of ecosystems is a point in favor of eliminativism, not against it.

I see one avenue of possible response for the realist. Grant me my conclusion that the best explanations of stability in ecological theory will eliminate biodiversity. In the conservation context we may still want to retain biodiversity as a concept, because we care about ecosystem stability, and if enough dimensions of biodiversity individually promote different dimensions of stability, we'll want to conserve biodiversity broadly. I'm all in favor of conservation, but appealing to stability as a reason to be biodiversity realists falls short because "[d]iversity is rarely a primary driver of stability, but is instead a secondary driver, itself subject to the same anthropogenic drivers affecting stability" (Ives and Carpenter 2007: 61–62). We know what's driving biodiversity loss: pollution, overharvesting, habitat destruction and fragmentation, climate change, and human-facilitated invasions. We know what's driving ecosystem destabilization: pollution, overharvesting, habitat destruction and fragmentation, climate change, and human-facilitated invasions. It thus makes little sense to treat biodiversity itself as the best site for causal intervention in favor of promoting ecosystem stability. Sure, to the extent diversity-stability obtains we might want more biodiverse ecosystems. But if we're really worried about destabilizing our environments, the thing to do isn't to worry specifically about biodiversity losses. The most effective strategy would be to fight pollution, overharvesting, habitat destruction and fragmentation, climate change, and human-facilitated invasions. As a means of promoting stability, trying to manipulate biodiversity levels is like trying to keep a drunk driver from crashing by having a passenger put a hand on the steering wheel to hold it steady. Yes, there's a correlation and even a causal relationship between the drunk driver's erratic movements and the probability of crashing. But if you really want to prevent a crash, kick the intoxicated person out of the driver's seat. Yes, there's a correlation and even a causal relationship between diversity and stability, but if you really want to prevent a crash, you have change the primary driver: anthropogenic environmental degradation. This is not to say that we don't need an understanding of ecological factors and the ability to take measurements to use in prioritizing our conservation response. My point is merely that such understanding and said measurements won't require invoking biodiversity in the broad sense. For example, when we worry about desertification, "let's increase/maintain the biodiversity of at-risk areas" isn't nearly as helpful as identifying and addressing the specific industrial and agricultural practices leading to vegetation loss, recognizing and altering the contributing hydrological practices, and discovering and protecting the specific species and communities which play a primary role in soil pedogenesis. It's that sort of focus on the specifics of human-ecological interaction, rather than on using measures of broad-sense[6] biodiversity to set priorities, that will optimize conservation success. The practical utility of broad-sense biodiversity conservation as a means to promote stability is thus not a good reason to resist eliminativism.

The debate between the biodiversity realist and the biodiversity eliminativist isn't about our scientific and ethical goals. Both share the motivation to understand ecology and to protect our environment. The eliminativist, however, recognizes that the multidimensional biodiversity concept impedes those goals, since those goals are best met by zooming into a finer-grained understanding of ecosystems and communities. Biodiversity isn't a natural kind, even on permissive functionalist or HPC accounts. Nor is it at the right level of specificity to serve as a good explanans or explanandum. Most work in the relevant sciences already tacitly acknowledges this by engaging with only a limited number of dimensions of biodiversity. The eliminativist is arguing that we can only gain clarity and understanding by making this tacit acknowledgement explicit.

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[6] Specific dimensions of biodiversity, on the other hand, will usually play a role in a project of this type. We may use descriptions and measures of functional diversity in our attempts to conserve pedogenesis, for instance, or want to promote grass species richness to create resistance to overgrazing.

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