Please cite published version:

Hazelwood, C. "The species category as a scientific kind." ***Synthese*** **198.Suppl 12 (2021): 3027-3040.**

1. Introduction

Marc Ereshefsky's project of eliminative pluralism is simply stated in two theses: 1) In light of the myriad mechanisms of speciation legitimated by scientific practice, we ought to be pluralistic realists about species taxa, and 2) as there is no unifying feature among all species taxa, we ought to doubt the existence of the species category (Ereshefsky, 1992, 1998, 2001, 2009).[1] If there are a number of ways to legitimate species taxa, then what it means to be a species refers to various, often incompatible, criteria. For some organisms, it will mean being reproductively isolated from other populations and producing viable offspring. For others, it will mean constituting the same monophyletic lineage. For others still, it will mean occupying the same ecological niche. Thus, the species category—the rank to which all species taxa are supposed to belong—is heterogeneous. On this view, the only truly common thing among the different species taxa is that *they are species taxa*. Ereshefsky finds this to be an unhelpful unifying

[1] A bit needs to be said here about my working definitions, as there is some discrepancy in the literature about how words like "taxa," "concept," and "category" are used. By "species taxa," I am referring to recognized species such as *Drosophila melanogaster* or *Homo sapiens*. By "species category," I am referring to the taxonomic rank of "species," which is separate from "subspecies" or "genus." By "species concept," which I'll incorporate later, I am referring to the theoretical criteria scientists use to delineate species taxa. For example, two species taxa, species *x* and species *y*, are delineated by a biological species concept because they are reproductively isolated populations that cannot interbreed.

feature—one that is theoretically useless and ontologically toothless. That is, Ereshefsky advocates antirealism about the species category.

In the following sections, I will suggest a revised approach to realism about the species category that relies on a shift from "theory-focused to "practice-centered" philosophy of science (Waters, 2014). It will rest on a conception of the category as a natural kind, but this in itself is a controversial suggestion. It is controversial primarily because the standard for natural-kindhood is a subject of significant debate. To be clear, I will argue that one promising strategy for being a realist about the species category is to reframe it as a natural kind after the practice turn. I will do this by situating the species category within a recent account of natural kinds proposed by Marc Ereshefsky and Thomas Reydon called "scientific kinds." Scientific kinds highlight ontological boundaries (i.e., they say something about the way the world is divided). Most importantly, they recognize boundaries drawn from the lab and the field, not only from the armchair. The point of this exercise is to situate the species category within an account of natural kinds that is largely sensitive to scientific practice—taxonomic practices, in this case. This, I argue, will be necessary to save the species category.

2. Eliminative Pluralism

We can better understand Ereshefsky's argument for eliminative pluralism in an example. Imagine a large population of beetles. Imagine, too, that you are a naturalist in the field, and you have just happened upon this new population of beetles. To your knowledge, it has never been scientifically classified. It is your task to try and determine how many species are represented in this population. Is it all one species? Are multiple species coexisting? There are a number of ways you might set out to determine the number of species present: you might look at the morphological variation, the mating patterns, and the ecological niches that the beetles occupy. I'll address each method in turn.

There is a substantial amount of phenotypic variation among these beetles; let's say some have horns while others don't, and some have vibrant colors and patterns while others don't. You might initially be tempted to categorize the beetles into different species based on their horn lengths and coloration patterns. However, that would be an unwise first step, given that polymorphisms (multiple discrete phenotypes), sexual dimorphisms (sex-specific phenotypes), and reactive norms (continuums of phenotypes) permeate single species throughout nature. So you decide to move on and have a look at mating patterns.

These beetles are sexual reproducers. You notice immediately that some phenotypes (e.g., no horns) will mate with certain phenotypes (e.g., bright coloration) and not other phenotypes (e.g., no coloration). You take a representative sample of the beetles back to the lab and determine that the hornless beetles are not able to produce viable offspring with the colorless beetles. However, the same hornless beetles are able to produce numerous viable offspring with the colored beetles. From this evidence, you might determine that you have at least two distinct species: the hornless and colored beetles are part of the same species, while the colorless beetles are part of another. The basis of this determination comes from the biological species concept (BSC), which states that species are populations of interbreeding organisms who are reproductively isolated from other populations of interbreeding organisms (Mayr, 1982).

But your determination that this population represents two distinct species is complicated by further evidence from the field. When you return the beetles to their natural habitat, you notice that the hornless and the colorless beetles are occupying the same ecological niche and the colored beetles occupy another niche entirely. By "occupying the same ecological niche," I am referring to the specific environment the beetles inhabit, the food they eat, how they acquire that food, etc. In other words, you notice that the hornless and colorless beetles are subject to the same ecological selection pressures.

One of your colleagues feels quite adamant that, in line with the BSC, the populations of reproductively isolated beetles ought to be grouped together in the same species. After all, she suggests, sexual reproduction explains the morphological diversity within each species, and the BSC affords us the ability to draw clear boundaries. However, another colleague feels equally inclined to draw the species boundaries based on the ecological niches occupied in the population. After all, she suggests, the hornless and colorless beetles are evolving in response to the same selection pressures in similar ways and, as such, are a cohesive evolutionary unit.

This quarreling among your colleagues sends you spiraling into an episode of doubt. You recognize that you are presented with two legitimate species concepts, namely the BSC and an ecological species concept, and both are applicable to this population of beetles. That is, you recognize that each species concept carves this population into legitimate units of selection—they are evolving together in an important way, either via reproductive isolation or ecological isolation. Presented with an overabundance of legitimate species concepts, you begin to doubt if there even *is* such a thing as “species.” Is “species” simply an arbitrary construct imagined by scientists to help us do science? The pessimism is tempting. But then you begin to entertain a provocative idea: perhaps there is more than one way to be considered a “species.” If both species concepts are legitimate, then perhaps this large population of beetles will actually reflect different species boundaries (i.e., hornless and colored versus hornless and colorless) depending on which criteria you use.

The latter conclusion—that there is more than one way to be a species—is referred to as “species pluralism.” It is the position that species are real and are natural kinds. On this view, species boundaries are given by nature, but there are multiple kinds of species boundaries. Species pluralists such as Ereshefsky view biological practice as revealing legitimate mechanisms by which species as evolutionary units (i.e., cohorts acted upon by natural selection) maintain their homeostasis. Species pluralists take these empirically derived mechanisms to reveal the ways in which subpopulations are able to cluster together and progress as units of evolution. Meanwhile, the species pluralist is allowed to embrace variation among populations of individuals by rejecting essentialism and supporting the notion that any one individual may belong to a *number* of different species, depending on what demarcation criteria are being considered at the time. For example, one organism might interbreed with one population while sharing an ecological niche with another, thus responding to similar selection pressures and evolving in tandem with both subpopulations, just as our hornless beetle has done.

Furthermore, Ereshefsky takes species pluralism to mean that there is no such thing as a category “species.” Much like our doubtful entomologist from before, Ereshefsky recognizes multiple legitimate ways of thinking about populations as species.

However, as there is no *unifying* characteristic among those legitimate species concepts, the taxonomic category "species" is not a legitimate ontological category. Unless we can demonstrate that the species category itself is a natural kind, then perhaps it is unsalvageable.

I frame this discussion in terms of natural kinds because it mirrors Ereshefsky's language in (1998): "Relative to other accounts of natural kinds, the standard used here for the existence of the species category is fairly lax." He says that, "Compared to essentialist and cluster accounts of natural kinds it is relatively weak" (ibid.). Ereshefsky is not opposed to characterizing the species category as a natural kind as long as we apply a suitable account of natural kinds. But his criterion for such an account "assumes that a category exists only if its members share some commonality that generally distinguishes them from entities outside the category" (ibid.). Thus, given the analysis in the previous paragraph, the species category cannot be a natural kind. I am suggesting that there is another way to think about natural kinds—one that supports Ereshefsky's turn to scientific practice while rejecting his conclusion that it undermines the ontological import of the species category. My treatment of natural kinds is largely influenced by an edited volume from Catherine Kendig, who says that, "the past discussions of natural kinds have often answered these questions in a way that is unresponsive to, or has actively avoided, discussions of the empirical use of natural kinds…The natural kinds of a particular discipline are those entities, events, mechanisms, processes, relationships, and concepts that delimit investigation within it" (2016). The "empirical use" of natural kinds will become especially relevant in section 5, where I discuss the role of the species category *in practice.*

The next section begins the task of introducing a "practice-centered" approach to saving species, as indicated by the section's heading. The heading gestures at a legitimate consequence of the theses advanced in this paper. I'll argue that the species category is ontologically salvageable. However, it's not salvageable in the sense that Ereshefsky would prefer. The point of introducing a practice-centered approach to saving species is to encourage an evaluation of our standards for realism about a theoretical concept. The practice-centered approach abandons a traditional view that our metaphysics of science ought to be read right off our core theories, highly abstract and generalizable. Instead, the approach employs what some philosophers have called a "toolbox" view of theoretical concepts. The upshot of this approach is that we ought not derive metaphysical conclusions from theoretical concepts outside the context of scientific practice. Instead, because practitioners do science with a multiplicity of epistemic aims, a "toolbox" of theoretical concepts or causal principles will emerge (Waters, 2014; Cartwright et al., 1995; Cartwright, 1999; Wimsatt, 2007). As I'll argue in section 6, the toolbox view enables a pluralistic realism about theoretical concepts depending on how they are used in scientific practice. My treatment will reframe the species category as one such tool in the toolbox. As a result, it is a realist commitment to a theoretical concept (viz., the species category) that is saved.[2]

3. Saving Species

I take Ereshefsky to be denouncing a very particular conception of the species category—one that is metaphysical in nature, and abstracted away from scientific

[2] Thanks to Daniel Weiskopf for helping to clarify this point.

practice. Consider Ereshefsky in (1998): “Though ‘species’ has outlived its theoretical life, practical considerations keep it alive. What those considerations are and how they outweigh theoretical ones deserves further study. Pragmatics aside, there is still an ontological problem concerning the reality of the species category.” John Wilkins has more optimistically suggested that it is up to our best scientific practices to determine the viability of the species category, “not philosophers to legislate against a category so fundamental to biological practice” (2003). As the category is of operational value to science, it is not the job of philosophers to cast it out from the armchair.

Meanwhile, the practice turn has inspired a reconceptualization of natural kinds themselves. The practice turn in philosophy of science can be understood as “a turn of our attention to scientific action” instead of theory alone (Kendig, 2016). The transition reflects what Ken Waters has referred to as a “shift from theory-focused to practice-centered philosophy of science” (Waters, 2013). This project of practice-centered scientific metaphysics is derived from a long line of intellectual ancestors (see Hacking, 1983; Dupré, 1995; Cartwright, 1999; Ross et al., 2014; Ladyman & Ross, 2007; Wimsatt, 2007), but its central claim is that science is our most promising methodology for metaphysics, and that scientific practice in particular will most reliably reflect constraints from the natural world. To put it in Waters’ language: “The basic strategy underlying scientific metaphysics…is based on the idea that the world provides constraints on scientific inquiry, and that philosophers can inform metaphysics by investigating those constraints” (2017).

The approach is held in contrast to a tradition of scientific metaphysics that has sought to identify a fundamental “general structure” of the world. Waters has argued that no such general structure exists (2014, 2017). When we abstract scientific theories away from practice, we present them as if they tell us about the way the world is in a privileged sense. Instead, Waters suggests, theories ought to be understood in the context of practice (i.e., how a theory is instantiated in experimental investigation; Waters, 2017).

Waters illustrates this thesis by recounting the interplay between theory and practice in the classical gene concept. Should one approach the "transmission theory" of classical genetics (i.e., transmission of genes relies on understanding of chromosomal mechanics, differences in genes cause differences in phenotypes, etc.) outside the context of genetic experimental investigation, one runs the risk of drawing metaphysical conclusions about the classical gene concept that are not reflected in practice. In practice, geneticists used the classical gene concept as a kind of heuristic—a "blunt conceptual tool that works well in investigative and explanatory contexts in which precision is not available or useful" (Waters, 2017). The concept was "grounded" in practice—not theory—where artificial breeding dynamics were established in order to track distinctive inheritance patterns.

Waters' suggestion is that looking to how theoretical concepts are used in practice (instead of how they are abstractly understood) will foster a more reliable scientific metaphysics. He suggests this for multiple reasons. First, the world presumably places constraints on our metaphysics. What we can know about the world is surely constrained by how the world is, and this is more reliably reflected in scientific practice than in abstract theorizing. Second, looking to scientific practice allows us to derive knowledge beyond the "explanatory range of its core theory." Practice (understood as the manipulative and investigative activities of experimentation) enables the practitioner to garner knowledge through an "investigative matrix, which consists of an assimilation of core theory, concrete knowledge, procedural know-how, and strategic approaches" (Waters, 2014). The transmission theory of classical genetics, for example, only aims to explain how phenotypes are inherited from one generation onto the next. In the context of experimental genetic practice, however, the transmission theory was used to investigate a much broader set of processes than is explained by the core theory.

As the discussion of natural kinds (such as genes and species) adopts a deeper appreciation for their use in scientific practice, it will serve us to frame our discussion of the species category as a natural kind in those same terms. In the following section, I reconstruct a recent account of natural kinds that aims to develop those terms, namely, the terms of suitable ontological categories as they are revealed and co-created in practice.

4. Scientific Kinds

Marc Ereshefsky and Thomas Reydon offer an account of natural kinds in terms of scientific practice and name these kinds, quite aptly, "scientific kinds" (2014). Scientific kinds are legitimate natural kinds, but are pragmatically oriented in the sense that they reject strict requirements imposed by armchair theorizing and instead embrace classifications that are operationally fruitful and experimentally fecund. The project takes aim at Homeostatic Property Cluster Theory (HPC hereafter), which has nearly dominated the landscape in philosophy of science.

HPC kinds are groups of individuals whose properties tend to cluster together in virtue of some common underlying causal mechanism(s). The properties, however, are neither necessary nor sufficient for kind membership. One individual may possess all of the same properties as another individual of the kind, or they may possess only some in common. Nonetheless, their belonging to the same kind is not threatened by a high degree of variation. This is the most influential anti-essentialist account of kinds that, according to Andrea Scarantino, has been proposed, "[to] accommodate variation, borderline cases, and the lack of exceptionless and universal generalizations in the special sciences" (Scarantino, 2012).

HPC Theory allows us to hold that species are natural kinds while maintaining that pre-Darwinian essentialist conceptions of species do not exist. The way Boyd defends his brand of species realism is through an adoption of an "accommodation thesis," which suggests that our natural kinds ought to allow us to, at least more often than not, project hypotheses and make inferences about the kind's members. Successful species concepts will, generally speaking, afford us these abilities.

Advocates for HPC Theory suggest that biological natural kinds are "*intrinsically heterogeneous* [i.e., possess a high degree of variation] in that the individuals they subsume do not simply differ from one another in the properties they possess, but do so *by nature* or *intrinsically as things of that kind*" (Wilson et al., 2007). In biological populations, variation is the rule and not the exception. However, advocates for HPC Theory suggest that abandoning the conceptual notion that biological populations are natural kinds is an inappropriate move. It is inappropriate because the explanatory significance of a natural kind—i.e., its ability to produce successful inferences and generalizations—can still be realized by heterogeneous populations. Instead, advocates suggest, we must "refit our philosophy to our biology," and embrace a framework of natural kinds that is sensitive to the “intrinsic heterogeneity” of population thinking (ibid.). HPC Theory is an appropriate solution as it recognizes the important *causal* role that variation plays in a biological population: “One reason for this is that biological kinds are typically individuated by several causally entwined features” (ibid.).

Ereshefsky and Reydon charge HPC Theory on two counts: HPC Theory endorses kinds that our scientific classifications reject, and HPC Theory neglects kinds that our scientific classifications recognize. They advance an account of natural kinds that responds to this “mismatch between HPC kinds and the kinds of science” and, in doing so, assert that “an adequate account of natural kinds should accurately track the classifications of successful science” (Ereshefsky & Reydon, 2014). The authors suggest that HPC Theory fails to recognize three proper varieties of scientific kinds: non-causal kinds, functional kinds, and heterostatic kinds. In what remains of this section, I’ll describe their accounts of non-causal and heterostatic kinds. For the purposes of relevance and space, however, I won’t say any more about functional kinds.

One class of scientific kinds neglected by HPC Theory is non-causal kinds. The authors state that, “Clearly HPC Theory requires that natural kinds are groups of entities sustained by causal mechanisms” (ibid.). This is true of HPC Theory as defined by its second thesis, i.e., there is some underlying homeostatic causal mechanism(s) which instantiate(s) the kind’s features. This is overly restrictive for identifying proper scientific kinds, the authors argue, as there are numerous kinds recognized by scientists even as “no set of causal homeostatic mechanisms is posited as part of their ontology” (ibid.).

The authors cite the *Phylo-Phenetic Species Concept* (PPSC hereafter) as an example, which relies on pragmatic genetic and phenetic analysis to establish the parameters that define microbial species.[3] There is no assumption of underlying causal mechanisms in this concept, yet the kinds they reveal are taken as legitimate microbial species. Here we may invoke the pluralistic realist's claim: the kinds revealed by the PPSC are perfectly good species[4]. This is an ontological claim, not merely an epistemological claim.

Another class of scientific kinds neglected by HPC Theory is heterostatic kinds. While HPC Theory does a fine job of picking out a kind in virtue of its *similarities*, it struggles to identify scientific kinds with persistent *differences*. Heterogeneity is a common feature among many scientific kinds, and this heterogeneity is not successfully accounted for by HPC Theory. Many species taxa, for example, demonstrate polymorphism among members of differing life stages or sexes. While HPC Theory recognizes heterogeneity, it implicitly seeks to connect its causal mechanisms to a shared cluster of properties by virtue of conditional laws, i.e., "if the organism is male it is

---

[3] "A phylo-phenetic species is '*a monophyletic and genomically coherent cluster of individual organisms that show a high degree of overall similarity with respect to many independent characteristics, and is diagnosable by a discriminative phenotypic property*'" (Rosselló-Mora & Amann, 2001). The reader might wonder why Ereshefsky and Reydon have called the PPSC non-causal when its species are, by definition, monophyletic. The reason is because its monophyletic component reflects a correlation, not causation: "Although DNA-DNA similarity results cannot be regarded as a result of cladistic analysis, they reflect a very tight genealogical relationship among strains that share high similarity values" (ibid.). While members of the same phylo-phenetic species are assumed to derive from the same lineage, this is incidental and not necessary; the species is clustered by a pragmatic threshold of genetic similarity in order to pick out "stable" kinds.

[4] Thanks to an anonymous reviewer for pointing out that one may simply reject the pluralistic realist's claim. It may be that species revealed by the PPSC are not legitimate HPC kinds and are therefore not natural kinds. Fair enough. I suspect, however, that a monistic view about HPC might further compel one to accept Wilson, Barker, and Brigandt's conclusion (2007) that the species category is a HPC kind. This treatment of the species category as a *scientific kind* is aimed at those who are unsatisfied with the theoretical constraints of accounts such as HPC.

capable of possessing feature *a*, *x*, or *z*." But this is nonsense, because "no biological mechanism underwrites such a conditional…Clearly, such conditionals are outside the realm of science" (ibid.). Thus, in spite of its attention to similarity and shared properties, HPC Theory neglects perfectly good scientific kinds that are heterogeneous and heterostatic.

This overly restrictive nature of HPC Theory highlights the distinction in theory and practice. À la Waters' analysis of the gene concept, we see that theoretical concepts aren't always going to be totally realized in practice. HPC Theory offers an account of kinds that does not adequately reflect the kinds recognized in scientific practice, as the kinds recognized in practice will, at times, deviate from abstract theoretical considerations in order to advance the practice. Consider the PPSC once more: it utilizes a pragmatic threshold of 70% genetic and morphological similarity to organize microbial populations into species. The threshold may not highlight any causally privileged boundaries in the world. Instead, the threshold picks out "stable kinds" that allow microbiologists to categorize organisms, even if in a heuristic fashion (Ereshefsky, pers. comm.). The upshot, when one extrapolates from these examples, is that accounts of kinds that are only sensitive to theoretical considerations (e.g., "species are homeostatic property clusters with underlying causal mechanisms) will not always adequately reflect the kinds recognized in scientific practice. Furthermore, if we are compelled to embrace a scientific metaphysics that is "practice-centered" instead of "theory-focused" (and I think we have good reason to be), then we might expect that the constraints of the world on scientific practice will more reliably inform our metaphysics than abstract theoretical content alone. Scientific kinds are therefore sensitive to the shift toward practice-centered metaphysics in a way that HPC Theory is not.

5. The Species Category as a Scientific Kind

If we take the implications of Ereshefsky's and Reydon's proposal seriously, it well may be that the species category is a scientific kind. In the previous section I listed the descriptive components to scientific kinds. In the following section, I'll discuss how the species category fares in light of these components.

In one sense, the species category is non-causal. In fact, Ereshefsky accurately demonstrates this in his critique of (Wilson et al., 2007), who suggest that the species category is a HPC kind. For example, it is incorrect to say that most species share the proposed causally basic features such as gene flow and interbreeding, as most of life on earth is microbial and therefore asexual. Further, it is problematic to assume being a "unit of evolution" is a causally basic feature shared by most species, as this can refer to a number of patterns whose incompatibilities or ambiguities got us into this mess in the first place (Ereshefsky 2010a, 2010b). A lack of clear causal explanation precludes the species category from being a HPC kind. This is no problem for scientific kinds.

In another sense, the species category is heterostatic. Just as a species taxon may be riddled with various polymorphic phenotypes, the species category is riddled with different species concepts. To use Kitcher's language, "There are many such relations which could be used to delimit species taxa. However, there is no unique relation which is privileged in that the species taxa it generates will answer to the needs of all biologists and will be applicable to all groups of organisms" (1984). The heterogeneous species category will find no respite in essentialist accounts or in HPC Theory. This is no problem for scientific kinds.

As I have described the account thus far, one may worry (with just cause) that scientific kinds will endorse an “anything goes” kind of metaphysics: whatever the scientist says about how the world is divided *really is* how the world is divided. An account of scientific kinds advocates for metaphysical recognition of not only those boundaries delineated by our best theories (e.g., the biological species concept), but by the boundaries drawn up in practice through pragmatic or heuristic frameworks (Ereshefsky & Reydon, 2014). For example, the PPSC holds that bacteria are members of the same species if they meet a particular threshold of morphological and genetic similarity. This is a practice-centered metaphysical claim endorsed by the authors; as a consequence, we are asked to consider the ontological status of a phylo-phenetic species as being on par with that of a biological species. If this is the case, then Kitcher’s skepticism—“species are those groups of organisms which are recognized as species by competent taxonomists”—is not far off (Kitcher, 1984).[5]

In an attempt to alleviate these worries about a practice-centered approach, Ereshefsky and Reydon propose a normative component in their account of scientific kinds. They expect scientific kinds to be progressive when compared to rival classificatory frameworks. We ought to evaluate relevant concepts and identify the more progressive concepts lest we slide into Dupréan promiscuity (Dupré, 1995). For example,

[5] Philip Kitcher’s quip originally comes from British ichthyologist Charles Tate Regan: a species is “a community or a number of related communities whose distinctive morphological characteristics are, in the opinion of a competent systematist, sufficiently definite to entitle it, or them, to a specific name” (1926). Thanks to an anonymous reviewer for this clarification.

according to the questions relevant to scientific practice, whales are mammals and not fish. From the perspective of scientific practice, this is the more progressive way to describe the world. Ereshefsky and Reydon offer a comparison between the morphological species concept and the biological species concept to demonstrate how one can be progressive relative to the other. Consider the following from their account:

> A classificatory program is progressive if it provides principles that produce additional classifications or extend existing classifications (relative to competing classificatory programs) and those classifications are empirically successful.[6] An example of a progressive classificatory program is the Biological Species Concept when compared to its principal rival of the first half of the twentieth century, the Morphological Species Concept. Supporters of the Biological Species Concept believe that sexual compatibility and the ability to produce fertile offspring are better indicators of evolutionary unity than morphology. A famous case confirming this belief is the discovery that fruit flies thought to belong to a single species because of their morphological similarity in fact belong to two evolutionary units (species), *Drosophila persimilis* and *Drosophila pseudoobscura*…The Biological Species Concept was, and continues to be, better at detecting evolutionary units than the Morphological Species Concept. (982)

It is at this juncture that the eliminative pluralist will take issue with my treatment of the species category as a scientific kind. Take Ereshefsky's groundbreaking paper on the matter (1992): he argues that, in the absence of a unifying feature among all species taxa, the species category is theoretically useless, and therefore ought to be splintered into "biospecies," "phylospecies," and "ecospecies." The eliminative pluralist may argue that the concept-specific classificatory program Ereshefsky has developed is far more progressive than my unified category. In other words, one may argue that, at least in this case, splitting is more progressive than lumping.

[6] By producing additional classifications or extending existing classifications, Ereshefsky and Reydon mean that a particular program is progressive when it is "able to construct more stable and readily identifiable classifications" (Ereshefsky & Reydon, 2014). "Producing additional classifications" is sometimes referred to as "splitting," while "extending existing classifications" is referred to as "lumping."

But is this demonstrable in scientific practice? If we are to really consider the species category as a scientific kind, we have to consider its operational merits in practice, not its theoretical merits alone. So the eliminative pluralist will emerge the victor if they can demonstrate that the species category does not produce additional classifications or extend existing ones. Adrian Currie, in one instance, argues that this is not the case. In his recent paper (2016), Currie mounts a defense of the species category based on the *indifference* to species concepts in paleobiology. When establishing new species, paleobiologists use a set of criteria that are entirely indifferent to the “non-equivalent specifications” that delimit one species concept from another. This indifference, Currie argues, “is not simply due to paleobiologists ‘making do’ with impoverished evidence, but because of the nature of their inquiry” (ibid.). More specifically, paleobiologists “delineate species on explanatory grounds” instead of guiding their taxonomic efforts by one species concept or another. In any particular case, they might be inclined to consider models of ontogenic development, phenotype variation, pathology, sexual dimorphism, overlap in strata, fossil reconstruction, ecology, social strategy, and large-scale macro-evolutionary patterns. That is to say, their explanatory pursuits range across a myriad of species concepts. No one species concept motivates taxonomic practices in paleobiology; instead, the species category itself does a significant amount of explanatory heavy lifting.

If these insights into paleobiological practice are representative of the species category’s practical utility, then it is demonstrably progressive when compared to the “splitting” approach. It is progressive because it “provides principles that produce additional classifications or extend existing classifications (relative to competing classificatory programs)" (Ereshefsky & Reydon, 2014). Whereas the biological species concept is progressive because it revealed that one fruit fly species was in reality two separate species, the species *category* is progressive in paleobiology for a similar reason. Currie demonstrates this with a case study of the "Toroceratops" hypothesis.

The "Toroceratops" hypothesis in paleobiological taxonomy proposes that *Triceratops* and *Torosaurus*, traditionally thought to be two distinct taxa, are actually two different life stages of the same organism.[7] In other words, the "Toroceratops" hypothesis would aim to extend existing classifications to encompass both *Triceratops* and *Torosaurus* in the same taxon. But where does the species category itself come into play? As Currie states, this debate (as well as many debates about taxonomy in paleobiology) will be settled on explanatory grounds: "Specifically, do interspecies or intraspecies processes best explain the range of variation across specimens? That is, taxonomic decisions in paleobiology are made on the bases of which processes best explain morphological differences" (Currie, 2016). Paleobiologists are, at least sometimes, indifferent to which species concept is used. The species category itself is doing explanatory heavy lifting in paleobiological practice. In this case, the species category is progressive to species eliminativism as it advances the field by extending existing classifications.

At this point, it may not be clear that realism about the species category is actually progressive when compared to an eliminativist approach. If species delineations are made on the basis of amalgams of species concepts in paleobiology, one may reasonably ask

[7] For a more careful reading of the "Toroceratops" hypothesis, see Currie, A., 2016, "The Mystery of the *Triceratops*'s Mother: How to be a Realist about the Species Category," *Erkenntnis*, 81: 795–816.

why we wouldn't instead acknowledge a plurality of species concepts that are capable of being combined and that don't exist in virtue of some unified species category. But the most relevant conclusion of Currie's analysis is not simply that paleobiologists use a plurality of species concepts. Rather, the most relevant conclusion is that a plurality of species concepts are employed because paleobiologists are indifferent to which concept is used on two grounds: 1) "…species delineation in paleobiology requires controlling for contrast-cases which would be problematic no matter which concept is employed," and 2) "[the investigative concerns of paleobiologists] tend to operate at a coarser grain than that provided by species concepts" (Currie, 2016). Therefore, while the investigative concerns of paleobiologists "are informed by species-level systematics and are *prima facie* about species," "picking a particular species concept will not change the paleobiological method" (ibid.). Because the particular species concept chosen will not change the paleobiological method, i.e., concept pluralism does not obviate the need for thinking about species at the category level in paleobiology, and because taxonomic decisions are made with the category level in mind, we can say that species realism is more progressive than eliminativism in paleobiological practice.[8]

[8] The conclusions provided in this analysis are reminiscent of Ingo Brigandt's treatment of the species category as an "investigative kind concept" (Brigandt, 2003). Brigandt's account also makes the case that category realism is progressive to species eliminativism, although this paper does not engage it directly. In response to the eliminativist, Brigandt reminds us that, "there is also overlap with respect to the mechanisms that bring about the units called species…And because of this overlap and continuous transition between different evolutionary mechanisms, it is not obvious what counts as a unique and separate factor" (ibid.). This provides another sense in which privileging one particular species concept is not so helpful, and not so easily done.

6. Implications for a "Practice-centered" Conception of Species

The example of the "Toroceratops" hypothesis does a nice job of situating the species category within a "practice-centered" account of metaphysics as it is proposed by Ken Waters (2014, 2017) and others. Such an account calls for us to abandon notions of "fundamental" metaphysical categories like "species" or "genes." It is that notion of species—the theoretical notion that all species are defined as *x*—with which Ereshefsky's project of eliminative pluralism takes issue. I agree with him: if we set out to find some metaphysically unifying characteristic of every single species taxa in the species category *aside from the fact that they are all species,* we will no doubt be searching in vain. Nevertheless, its lack of theoretical unity does not entail that the species category is practically useless. On the contrary, debates surrounding the "Toroceratops" hypothesis, and other paleobiological taxonomies, rely on the notion of a species category—and not species concepts—to delineate taxa. A "practice-centered" metaphysics will, in my view, privilege the practical usefulness of the species category over its theoretical uselessness. Moreover, a "practice-centered" metaphysics allows us to make a realist claim about the species category—not only a pragmatic one![9] Consider Waters in (2017):

> We should also be realists about the central theory [of a discipline], but again we should separate our realism from a fundamentalist interpretation of the theory. Scientific metaphysics should proceed from an analysis of the form that practice [of a discipline] takes, not from an analysis of its core theoretical concepts removed from the context of that practice. Simply put, metaphysics should be practice centered, not theory focused. But practice-centered metaphysics does not ignore theory, and it does not adopt an anti-realist attitude toward those theories. Hence, this metaphysics is not based on antirealism. (102)

In other words, "fundamentalism, not realism, is the problem" (ibid.; Cartwright 1999).

---

[9] This paper relies on a notion of realism originating in Boyd (1991) and modified in Wimsatt (2007) and Waters (2010, 2014, 2017).

We may be realists about the species category, but we ought not expect any one definition of the species category to be “common to everything, always, everywhere” (Waters, 2017). And we may be realists about the species category (when situated in a practice-centered account) because scientific practice is sensitive to the constraints of the real world. There are merely pragmatic considerations in practice, yes. For example, we may measure things that are easily measured versus measuring things that are not easily measured. However, that there are technical limitations to scientific practices does not entail an antirealist metaphysics about the knowledge those practices produce. A practice-centered approach affords us the ability to be realists about species without being realists about any “fundamental” conception of species. When we look to how theoretical concepts about species are used in practice (and seemingly mature and successful practices, no less), we see that Ereshefksy is indeed correct: there are practical considerations that keep the category alive. Yet it is those same practical considerations that vindicate realism about the category as well. He is wrong insofar as he suggests the practical considerations about species only reflect a pragmatic characterization of the species category and not a metaphysical characterization of the species category. A practice-centered approach to scientific metaphysics denies this implicit dichotomy between the pragmatic and the real.

Finally, it is in this sense that the species category may be thought of as a scientific kind, because Ereshefsky’s and Reydon’s account does not demand that scientific kinds are cohesive with a unified theoretical conception of species. On the contrary, scientific kinds privilege those classifications that are useful in practice. Here the species category passes muster.

7. Conclusion

The purpose of resituating the species category as a scientific kind is motivated by a shift in the dialogue about natural kinds. Philosophers of science are increasingly compelled by the notion that natural kinds need not carve nature at its joints—i.e., they need not reveal some “fundamental” structures that pervade the fabric of the universe. Instead, perfectly legitimate natural kinds can be tools in our theoretical toolbox: “those categories and classifications that fit the knowledge-seeking questions we ask and aim to answer” (Kendig, 2016). But if the reader has interpreted my use of toolbox theorizing as an endorsement of pragmatism or instrumentalism, then perhaps they will view my treatment as being incompatible with realism. On the contrary, a shift of our attention from theory to scientific practice is motivated by an assumption that practice most reliably reflects constraints from the natural world. Successful practices line up better with the world than unsuccessful practices. I have denied the charge that investigative pragmatism and theoretical realism are incompatible, though Waters says it best in (2010):

> Both fundamentalist and toolbox theorist are realists, but their metaphysical pretensions and methodologies differ. While fundamentalists seek the universally correct theoretical account for each natural kind…regardless of explanatory interests *about those natural kinds*, toolbox theorists seek true theoretical accounts that best address particular interests.

This is the exact thinking that motivates species pluralism. I see no reason why it shouldn’t motivate species category realism as well.

References

Boyd, R. (1991). Realism, anti-foundationalism, and the enthusiasm for natural kinds. Philosophical Studies, 61, 12

Brigandt, I. (2003). Species pluralism does not imply species eliminativism. Philos. Sci. 70, 1305–1316.

Cartwright, N. D. (1999). *The Dappled World: A Study of the Boundaries of Science*. Cambridge: Cambridge University Press.

Cartwright, N., Shomar, T. and Suárez, M. (1995). "The Tool Box of Science", in: W. Herfel, W. Krajewski, I. Niiniluoto and R. Wojcicki, eds. *Theories and Models in Scientific Processes* Amsterdam: Rodopi, pp. 137–149.

Currie, A. (2016). "The Mystery of the *Triceratops*'s Mother: How to be a Realist about the Species Category", *Erkenntnis*, 81: 795–816.

de Queiroz, K. (2005). Different species problems and their resolution. *BioEssays, 27*, 1263–1269.

Dupré, J. (1993). The Disorder of Things: Metaphysical Foundations of the Disunity of Science. Cambridge, MA: Harvard University Press.

Ereshefsky, M. (1992). ''Eliminative Pluralism'', Philosophy of Science 59: 671–690.

—— (1998). ''Species Pluralism and Anti-Realism'', Philosophy of Science 65: 103-120.

—— (2010a). "Microbiology and the Species Problem", *Biology and Philosophy*, 25: 67–79.

—— 2010). "Darwin's Solution to the Species Problem", *Synthese*, 175: 405–425.

Ereshefsky, M., & Reydon, T. A. (2014). "Scientific kinds", *Philosophical Studies*, 1-18.

Hacking, I. (1983). *Representing and Intervening.* Cambridge: Cambridge University Press.

Kendig, C. (2014). Towards a multidimensional metaconception of species. Ratio, 27(2):155–172.

Kendig, C. (2016). *Natural Kinds and Classification in Scientific Practice*. New York: Routledge.

Kitcher, P. (1984). ''Species'', Philosophy of Science 51: 308–333.

Ladyman, James, and Don Ross, with David Spurett, and John Collier. (2007). *Everything Must Go: Metaphysics Naturalized.* Oxford: Oxford University Press.

Mayr, E. (1982). *The growth of biological thought: diversity, evolution and inheritance*. Harvard. The rise of the biological species concept, p. 270–285

Ross, D., Ladyman, J., and Kincaid, H., eds. (2013). *Scientific Metaphysics.* Oxford: Oxford University Press.

Rosselló-Mora, R., & Amann, R. (2001). The species concept for prokaryotes. FEMS Microbiology Reviews, 25, 39–67.

Scarantino, A. (2012). How to define emotions scientifically. *Emotion Review, 4,* 358–368.

Tate Regan, C. (1926).'Organic evolution'. *Report of the Brit. Assoc. Advanc. Sci*. 1925: 75–86.

Waters, C. Kenneth. (2010). "Okasha's Unintended Argument for Toolbox Theorizing", *Philosophy and Phenomenological Research*, 82 (1): 232-240.

—— (2014). "Shifting Attention From Theory to Practice in Philosophy of Biology." In New Directions in the Philosophy of Science, edited by M. C. Galavotti, D. Dieks, W. J. Gonzalez, S. Hartmann, T. Uebel, and M. Weber, 121– 39. Berlin: Springer International Publishing.

—— (2017). "No General Structure." In *Metaphysics and the Philosophy of Science: New Essays*, edited by Matthew Slater and Zanja Yudell, 81–108. Oxford: Oxford UP.

Wilkins, J., (2003). "How to be a chaste species pluralist-realist", *Biology and Philosophy*, 18: 621–638.

Wilson, R. A., Barker, N. J., & Brigandt, I. (2007). When traditional essentialism fails: Biological natural kinds. *Philosophical Topics,* 35, 189–215.

Wimsatt, W. (2007). *Re-Engineering Philosophy for Limited Beings: Piecewise Approximations to Reality*. Cambridge: Harvard University Press.