

8 Natural Kinds and Biological Realisms

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The species category does not exist.

—Ereshefsky (1998, 113)

Taxa of higher rank than species do not exist in the same sense as do species.

—Eldredge and Cracraft (1980, 327)

To the cladist true believer, there is no such thing as a reptile.

—Sterelny and Griffiths (1999, 197)

1 Introduction

There are a number of “realism” issues in biology, issues about what “exists,” what is “real,” what is “objective.”¹ In general, realism issues tend to be confused and the biological ones are no exception. We shall see that the interesting realism issues in biology are best seen as ones over which kinds “carve nature at its joints”—which kinds are “natural kinds”—and that seeing them as “realism” issues has caused unclarity and confusion.²

I shall start with the issue that arises out of the debate between “species monists” who think that there is just one good “species concept”—one good account of what it is to be a species—and “species pluralists” who think that there are many. To that end, in section 2 I summarize the various species concepts, and in section 3 the motivation for pluralism. In section 4, I describe realism in general, particularly “realism about the external world.” Against this background I turn, in section 5, to the issue in question, focusing on the apparent clash between Marc Ereshefsky’s “pluralistic antirealism” (1998) and Philip Kitcher’s “pluralistic realism” (1984). In section 6 I consider some “realism” issues about genera and higher categories in the Linnaean hierarchy.

2 Species Concepts

Species pluralism arises out of the controversy over which species concept is correct. "The species problem is one of the oldest controversies in natural history" (O'Hara 1993, 231); it is "one of the thorniest issues in theoretical biology" (Kitcher 2003, xii).³ There are around two dozen species concepts and "at least seven well-accepted ones" (Ereshefsky 1998, 103). Samir Okasha (2002) places them in "four broad categories":

1. *Phenetic* concepts On this sort of view, organisms are grouped into species on the basis of overall similarity of phenotypic traits. This is thought by its proponents to have the advantage of being fully "operational." Okasha says that phenetic concepts are "the least popular" (2002, 199), and this is hardly surprising since they arise from the "philosophical attitude . . . of empiricism" (Sokal and Crovello 1970, 29). "Phenetic taxonomists have often wanted to segregate taxonomy from theory" (Sterelny and Griffiths 1999, 196).
2. *Biological Species* concepts (BSC) The most famous example of a BSC is due to Ernst Mayr. He defined species as "groups of interbreeding natural populations that are reproductively isolated from other such groups" (Mayr 1969, 26). Kim Sterelny and Paul Griffiths remark that "If the received view has a received species concept," it is BSC (1999, 188).
3. *Ecological Niche* concepts According to these concepts, a species occupies a certain ecological niche. "A species is a lineage . . . which occupies an adaptive zone minimally different from that of any other lineage in its range and evolves separately from all lineages outside its range" (van Valen 1976, 70). Okasha puts the view succinctly: species "exploit the same set of environmental resources and habitats" (2002, 200).
4. *Phylogenetic-Cladistic* concepts (P-CC) On this view we "identify species in terms of evolutionary history . . . [with] particular chunks of the genealogical nexus . . . Species come into existence when an existing lineage splits into two . . . and go extinct when the lineage divides, or when all members of the species die" (Okasha 2002, 200). Sterelny and Griffiths claim that "something like a consensus has emerged in favor of a *cladistic* conception of systematics" (1999, 194).

The various species concepts are answers to what Mayr (1982, 253–254) calls the "species category" problem: they are telling us what it is it for a kind to be a species rather than a subspecies (variety), genus or what have you. In claiming this I am not being controversial. However, my claim about the following questions is controversial: Do these species concepts

tell us anything about what Mayr calls the “species taxon” problem? Do they tell us about what it is for an organism to be a member of a kind that happens to be a species? The answer to the “first-level” taxon problem tells us why Fido is a dog; an answer to the “second-level” category problem tells us why dogs are a species but poodles are not. Now it is common to think that species concepts not only answer the category problem but also the taxon problem (Dupré 1981; Sterelny and Griffiths 1999; Wilson 1999; Okasha 2002). I have argued elsewhere (Devitt 2008) that this common thought is wrong. But the main point to make here is that theories about the species category are one thing, theories about species taxa are another. In particular, realism about the *category* is one thing and realism about *taxa* is another.⁴

3 Species Pluralism

As I have noted, controversy has raged over which of the many species concepts is right. In the face of this controversy some have argued that we should abandon the monist idea that just one concept *is* right, and hence that there is just one species category. Rather we should adopt the pluralist idea that *many* of the concepts are right and hence that there are many species categories. According to Kitcher, many concepts “can be motivated by their utility for pursuing a particular type of biological inquiry” (1984, 118). Kyle Stanford puts the point thus: “certain explanatory demands are *inextricably bound* to certain species concepts” (1995, 72). And there are many different, but equally legitimate, types of biological inquiry and explanatory demands: “we have independent and legitimate explanatory interests in biology which require distinct concepts of species” (*ibid.*, 76).⁵

In brief, there are a range of cases that force our attention to “the diversity of biological interests” (Kitcher 1984, 125), motivating different ways to classify organisms into species. And these ways will often lead to different classifications. Thus Ereshefsky claims that the BSC interbreeding and the P-CC phylogenetic approaches to species “carve the tree of life in different ways. Many interbreeding species fail to be phylogenetic species, and many phylogenetic species fail to be interbreeding species” (1998, 105).

I have doubts about the extreme form of pluralism urged by Kitcher, doubts that I will air in section 5. Still, the case for some form of pluralism strikes me as strong. But what bearing does this monism-pluralism issue have on biological “realism”? To assess this, we need to be clear about what

biological realism is. And to get clear about that it will help to start by considering realism in general.

4 Realism in General

The background issue that is most relevant is often known as “realism about the external world,” concerned initially with the observable entities of common sense but spreading to scientific entities, both observable and unobservable. Let us attend only to scientific entities. What is realism about these entities? It has two dimensions, one committed to the *existence* of entities, the other to their *mind-independence*. We can capture the doctrine well enough as follows:

Realism: Entities of most scientific kinds exist mind-independently.

I have argued the case for this doctrine at length in *Realism and Truth* (1997) and subsequent papers (1999, 2001, 2002, 2005).

This doctrine, Realism, needs to be kept quite distinct from another traditionally called “realism.” This other realism is about “universals,” abstract entities like kinds, properties or sets. Insofar as this realism arises from the “one-over-many problem,” I follow Quine in thinking it arises from a pseudo-problem (Devitt 1980; Devitt and Sterelny 1999, 277–279). Furthermore, I lean toward the view that there are *no* good reasons for realism about universals—that is, I lean toward nominalism—but I shall not try to argue the matter. Indeed, it seems to me best to discuss the issues of realism in biology without any commitment on this vexed, millennia-old, metaphysical issue.⁶ I shall continue to talk of kinds as if they existed but remain neutral on whether this is a mere manner of speaking that can be paraphrased away or whether it amounts to a real commitment.

One further aspect of realism debates looms large in biology. Philosophers have been concerned not only with whether the posits of science exist mind-independently but also with whether these posits are “appropriately special” rather than somewhat arbitrary. In particular there has been a concern about whether our scientific posits “carve nature at its joints,” about whether there is something in the nature of the world that, in some sense, *determines* our categorization of it. I take this to be a concern about whether the kind of entity posited by a theory plays a causally significant role, whether it is partly *because* an entity is of that kind that it has the characteristics and behavior that it has. Theories need to posit such so-called “natural” kinds if the theories are to be genuinely explanatory. And Realists are likely to take it for granted that their

paradigm entities—for example, cats and planets—are indeed of natural kinds.⁷

This admittedly vague idea of naturalness is what Kitcher has in mind in saying that “natural kinds are the sets that one picks out in giving explanations” (1984, 132 n.16). Similarly, Richard Boyd says that “the naturalness (and the ‘reality’) of natural kinds consists solely in the contribution that reference to them makes to [the accommodation between conceptual and classificatory practices and causal structures]” (1999, 141). There are two things we should note about definitions along these lines. First, they are insufficient: being a motor car, a hammer or a paperweight are causal-explanatory kinds. We need somehow to capture the idea that natural kinds are causal-explanatory “in science.” Second, explanatory significance, and hence naturalness, comes in degrees: positing some kinds may be very explanatory, positing others only somewhat explanatory, positing still others not explanatory at all.

This provides one reason for not using the term ‘realism’ to label the issue of naturalness: *existence, unlike naturalness, does not come in degrees*. But a more important reason is that the requirement that a kind be natural is *quite distinct from* the requirement that entities of that kind exist objectively and mind-independently. As we shall see in the next section, it is easy to name kinds of entities that are more arbitrary than causal-explanatory, and yet those entities still exist mind-independently. And if there were kinds of entities that were mind-dependent in, say, a Kantian way, those kinds could still be causal-explanatory. So I shall keep the issues distinct. But they have not been kept distinct in biology, with some unfortunate consequences that will soon become apparent.

I shall start with Ereshefsky’s antirealism about species and his apparent disagreement with Kitcher.

5 Ereshefsky versus Kitcher

In section 2, we noted the importance of distinguishing realism issues at two levels to do with species: at the first level we are concerned with species taxa—for example, tigers—and at the second level we are concerned with the species category itself. Ereshefsky is concerned with the latter. He claims that “the species category does not exist,” but he “does not call into question the reality of those lineages we call ‘species’” (1998, 113, 104).

Ereshefsky’s argument, the “Heterogeneity Argument,” starts from species pluralism: “a number of species concepts should be accepted as legitimate” (1998, 103). He takes this pluralism to imply antirealism and

thus sees himself as being at odds with pluralistic realists like Kitcher (1984) and John Dupré (1993). Ereshefsky rightly points out that “species pluralism implies that the world contains different types of species” (1998, 111). He goes on:

What do these different types of species have in common that renders them species? If species taxa lack a common unifying feature, then we have reason to doubt the existence of the species category. (*Ibid.*)

Ereshefsky finds a suggestion in the literature for what the common feature might be, a suggestion with two components: (i) a similarity in the “process that renders species taxa cohesive entities”; (ii) a similarity of “structure.” He argues that neither component can do the job (*ibid.*, 111–113), and concludes that “what is left as the common feature of species taxa is the term ‘species’” (*ibid.*, 113).⁸ Hence he draws his conclusion that the species category does not exist.⁹

What could he mean by this conclusion? He certainly does not mean that tigers and the like do not exist: we have noted already that he does not reject first-level taxa realism. And he seems to accept that there are “different types of species,” each one captured by a different species concept; for example, “interbreeding species,” “phylogenetic species,” and “ecological species,” or more briefly, “biospecies,” “phylospecies,” and “ecospecies” (*ibid.*, 115, 117). So he accepts that there exist organisms that are members of a biospecies, organisms that are members of a phylospecies, organisms that are members of an ecospecies, and perhaps organisms that are members of other types of species. But then, since biospecies, phylospecies, ecospecies, and perhaps others are all types of *species*, all of these organisms are members of a species. Indeed to be a member of a species simply *is* to be a member of a biospecies, phylospecies, ecospecies or perhaps other types of species. So what entity could Ereshefsky be denying the existence of?

His response to a “realist rejoinder” points to the answer. In this response, he denies that there is any “distinctive commonality” among biospecies, phylospecies, and ecospecies. He is not satisfied with a disjunctive species category of the sort just illustrated. “A disjunctive definition of the species category would not tell us why various taxa are species . . . disjunctive definitions lack ontological import” (1998, 115). What he finds lacking in the species category is explanatory significance or anything close to that; the category is too close to being arbitrary. But how could this lack be a lack of *existence*? I take it that Ereshefsky must be thinking of the species category as an abstract entity, a “universal,” and

must be presupposing a sort of *selective* realism about such entities. Whereas an *unselective* realist is committed, roughly, to there being a universal for every predicate, a selective realist is committed to there being one for some but not all predicates.¹⁰ And Ereshefsky is committed to there being one for a predicate where the objects it applies to share a distinctive commonality, but not for any other predicates. According to his pluralism, species lack that commonality. So the species category, a universal, does not exist.

Now, it would be better if we could capture Ereshefsky's position on biology without commitment to a heavy-duty, highly controversial, metaphysical thesis of selective realism about universals. And we can. We can capture it as a rejection of the view that the species category is a "natural" kind. And this rejection is not based on denying the *existence* of anything but on denying the *explanatory significance* of kinds being species.

To see that non-naturalness does not arise from nonexistence, it is helpful to note that entities of many kinds exist, even exist mind-independently, where their being members of those kinds is of little or no explanatory significance. Consider cousins, for example: these include first cousins, second cousins, fifth cousins thrice removed, and so on. The explanatory significance of being a cousin is surely close to zero, and we get even closer to zero if we consider step-cousins. And there is nothing to stop us naming a *totally arbitrary and non-natural* kind: thus we could call anything that is either an acid, a river or a bachelor a "grugru." Yet despite non-naturalness and explanatory insignificance, cousins, step-cousins, and grugrus really exist, and do so mind-independently.

So the best metaphysical message to take from biological pluralism is not that the species category does not exist or that it is not "real." Nor is the best message that species do not correspond "to something in the *objective structure of nature*" (Kitcher 1984, 128), are not "an objective feature of the living world" (Sterelny and Griffiths 1999, 180). For, according to pluralism to be a species is to be a biospecies, phylospecies, ecospecies or perhaps other type of species—and there is nothing subjective or otherwise mind-dependent about that.

To think clearly about realism issues it is vital to distinguish sharply two sorts of freedom, a freedom we have and a freedom we do not (Devitt 1997, 245). The freedom we *do* have is to choose to name any kind we like, whether for explanatory reasons or frivolous reasons or no reason at all; naming kinds is a subjective matter. The freedom we *do not* have is to choose whether something is a member of a kind, whatever our reason for naming that kind in the first place; kind-membership is an objective

matter. We have chosen to name cats for very good explanatory reasons and grugrus for no good reason at all, but grugrus exist as objectively and mind-independently as cats—and they existed long before I named them “grugrus.” My naming them “grugrus” didn’t make them grugrus any more than people naming cats “cats” made them cats. It is common to talk as if, in doing science, we impose our concepts to “carve up reality.” But this is not literally so: we choose our concepts in an attempt to discover the causally significant features of a nature that is already “carved up.” The importance of distinguishing theory-making from world-making could hardly be exaggerated.¹¹

In sum, Ereshefsky is best seen as rejecting the view that the species category is a natural kind because that category is not explanatory. He is best seen as rejecting this because that is what his argument from pluralism supports without the baggage of selective realism about universals.

I noted that Ereshefsky sees himself in disagreement with Kitcher and Dupré who, he claims, “suggest a realistic interpretation of species pluralism: various species concepts provide equally real classifications of the organic world” (Ereshefsky 1998, 103). Is there an actual disagreement, and if so what is it? I shall only consider Kitcher and shall set aside any possible disagreement arising solely from Ereshefsky’s selective realism about universals.

Kitcher does, of course, *call* his position “pluralistic realism,” but what exactly is “realist” about his position? First of all he sets aside as “trivially true” a realism about the species category (and taxa) that is like the doctrine I have called simply Realism, requiring only mind-independent existence. He is interested in a stronger realism:

Pluralistic realism rests on the idea that our objective interests may be diverse, that we may be objectively correct in pursuing biological inquiries which demand different forms of explanation, so that the patterning of nature generated in different areas of biology may cross-classify the constituents of nature. (Kitcher 1984, 128)

Kitcher clearly thinks that *various species categories*—biospecies, phylospecies, ecospecies, and the like—are explanatory and hence natural kinds. Setting aside for a moment a disagreement about the variety of species categories, I assume that Ereshefsky would agree (1998, 117). But, of course, the view that the various species categories are natural kinds is not the view that *the species category itself* is natural. The latter view requires that it be explanatory significant that some kinds are *species*. Yet, according to Ereshefsky’s argument, pluralism has the consequence that the species category is disjunctive and not explanatory. So if Kitcher’s pluralistic

realism is about the species category itself, and not merely about various species categories, then Ereshefsky is indeed in disagreement with him. Is Kitcher's realism about the species category itself? He certainly does not *argue* that the species category is explanatory. And the message I take from "the moral for philosophy of science" that he draws at the end of his paper is that he thinks that the category is *not* explanatory: 'species' refers not to a natural kind but to a "heterogeneous collection" of natural kinds (1984, 129). If I am right about this then Kitcher's pluralistic "realism" is not, in this respect, different from Ereshefsky's pluralistic "antirealism," and so they seem not to be in disagreement after all.

My suggestion is that the appearance of disagreement here between Ereshefsky and Kitcher has arisen from confusingly describing an issue about natural kinds as an issue about realism.

However, Ereshefsky and Kitcher do differ in their pluralisms and hence in the variety of species categories that they are prepared to be "realist" about. Kitcher's pluralism is radical. He follows Mayr in drawing a distinction—a very important one in my view (Devitt 2008)—between two types of explanation in biology, ones that Kitcher calls "structural" and "historical." Structural explanations seek to "explain the properties of organisms by means of underlying structures and mechanisms"; the historical seek to "identify the evolutionary forces that have shaped the morphology, behavior, ecology, and distribution of past and present organisms" (1984, 121). Kitcher claims that these two types "generate different schemes for classifying organisms" (*ibid.*, 122). He finds variations within the two types, which leads him to posit nine different taxonomies *and hence nine distinct species categories* (*ibid.*, 124).

Ereshefsky's pluralism is more conservative. He is prepared to accept only species categories that are justified by historically motivated taxonomies, for example, the categories of biospecies, phylospecies, and ecospecies. His form of pluralism

assumes that all species taxa are genealogical entities. To assume otherwise places species outside of the domain of evolutionary biology. The explanatory backbone of evolutionary theory is the assumption that organisms are connected by genealogy. (Ereshefsky 1998, 107)

However, this is not really an *argument* against Kitcher's radicalism. For Ereshefsky is attending only to the explanatory needs of evolutionary biology, whereas Kitcher is emphasizing that there are *other* explanatory concerns in biology: there are the concerns of *structural* explanations. And Kitcher thinks that these explanations motivate taxonomies, and hence

species categories, that are different from those motivated by historical explanations.

Is there an argument against Kitcher's radicalism? I think so. Suppose that we go along with him about the nine different taxonomies: what about the inference to nine different categories? If the taxa picked out by a certain taxonomy are to justify a certain species category, *S*, it has to be *explanatorily significant* that the taxa are *S*. Now given the role of species in theories of evolution it is plausible to think that a *historically* motivated taxonomy *will* justify a species category: as Ereshefsky puts it, "species taxa are the paradigmatic units in which descent with modification occurs" (1998, 107).¹² But why suppose that a *structurally* motivated taxonomy will justify a category? That is, even if structural explanations demand certain taxonomies, what significance is there for such explanations that some taxon is a *species*? Consider Kitcher's example:

A biologist may be concerned to understand how, in a particular group of bivalve mollusks, the hinge always comes to a particular form. The explanation that is sought will describe the developmental process of hinge formation, tracing the final morphology to a sequence of tissue or cellular interactions, perhaps even identifying the stages in ontogeny at which different genes are expressed. (Kitcher 1984, 121)

It is hard to see how it makes any difference to the structural explanation we seek whether that group of mollusks is a species, subspecies, genus or whatever. Our interest in structural explanations may demand that we group those mollusks together in our taxonomy but it does not seem to demand that we assign them to any particular category. So I am inclined to think that only historically motivated taxonomies can justify species categories and that Ereshefsky is right to be conservative.

I shall return to this disconnect between taxonomies and categories in discussing the higher categories in the next section.

6 The Higher Categories

The issues that come up for species can come up for any of the higher categories ("ranks") in the Linnaean hierarchy, "the tree of life": genera, families, orders, classes, phyla, and kingdoms. Thus, consider the kinds we call "genera." At the first level, there is the Realist issue of whether *Canis* and other genera exist mind-independently. Then there is the further issue of whether it is explanatorily significant that they are *Canis*: the issue of whether *Canis* is a natural kind. At the second level, we are concerned with the analogous pair of issues about the *category* of being a genus; in particular,

with whether it is explanatorily significant that *Canis* and the like are *genera*, and hence with whether being a genus is a natural kind.

Let us start with the first level. No issue came up at this level for species and that is surely appropriate.¹³ We should all hold the Realist view that dogs and the like exist mind-independently. And being a dog is surely a natural kind if anything is: it features in historical explanations because being a dog is part of the evolutionary story; it features in structural explanations because a lot of the morphology, physiology, and behavior of Fido is explained by his being a dog.¹⁴ And one would have thought that there should similarly be no first-level issue with the higher taxa. Once again we should be Realist and we should take the higher taxa as natural kinds. Thus, whether or not being a genus is a natural kind surely being a *Canis* is: it features in historical evolutionary explanations and structural explanations, just as being a dog (*Canis familiaris*) does. And so too, say, being a mammal. This is not to say that membership in a higher taxon is as explanatorily significant as species membership: explanatory significance comes in degrees, as we noted (sec. 4). Still, surely *some* of the morphology, physiology, and behavior of Fido are explained by his being a *Canis*.

In light of this, one wonders what to make of the view that "taxa of higher rank than species do not exist in the same sense as do species" (Eldredge and Cracraft 1980, 327). According to Ereshefsky this sort of view is part of "The Modern Synthesis." He describes that Synthesis as holding: "Higher taxa . . . are merely artifacts of evolution at the species level. So while species are real and the 'units of evolution,' higher taxa are merely aggregates and 'historical entities'" (2001, 229). Ereshefsky himself rejects this view (1991, 381), as does James Mallet: "Whether species do have a greater 'objective reality' than lower or higher taxa is either wrong or at least debatable" (1995, 296). Clearly I think that Ereshefsky and Mallet are right in their rejection. I suspect that the rejected view arises out of a confusion of a Realism issue with a naturalness issue and/or a confusion of a first-level issue with a second-level one.

Another first-level issue has arisen from the fierce controversy between those favoring "phenetic," "evolutionary," and "cladistic" taxonomies. Sterelny and Griffiths favor the cladistic taxonomy and take it to be the emerging consensus (1999, 194). Yet cladistics involves the following surprising "metaphysical" first-level claim:

real groups in nature are all, and only, *monophyletic* groups . . . groups that consist of a species and all, and only, its descendants. To the cladist true believer, there is no such thing as a reptile. 'Reptile' does not name a real group, for there is no species

that is ancestral to *all* the reptiles that is not also ancestor of the birds. (Sterelny and Griffiths 1999, 197)

Now, we should note first of all that the claim that “there is no such thing as a *reptile*” is a most unhappy way of putting the cladistic point. *Of course* there are reptiles because there are crocodiles, snakes, and lizards and to be a *reptile* is simply to be one of those. Reptiles exist, and do so mind-independently, just as much as cousins, step-cousins, and grugus. And it is not much more apt to say that reptiles are not a “real group” in nature: there is nothing interestingly *unreal* about crocodiles, snakes, and lizards. I take it that the best way to put the cladistic point is to say that being a *reptile* is *not an explanatory significant kind*. In general, the cladist thinks that non-monophyletic groups are not natural kinds: once again, the non-natural is being confusingly described as the non-real.

Is this restriction to monophyletic groups appropriate? Although Sterelny and Griffiths favor cladism, they are inclined to think that the restriction is too extreme:

there may well be sensible evolutionary hypotheses about all the nonmarine mammals. The group is not a monophyletic clade, because there is no species ancestral to all the land-breeding mammals that is not also ancestral to the whales. (1999, 198)¹⁵

This is an evolutionary reason for abandoning strict monophyly. And there are other explanatory concerns in biology that seem to demand this as well: the sorts of structural concerns that partly motivate the phenetic and evolutionary taxonomies. There seems no reason to think that only monophyletic classifications can serve those other concerns. Perhaps being a “*reptile*” is explanatorily significant in many cases.¹⁶ Doubtless *some* non-monophyletic groups that biologists have posited do not “carve nature at its joints,” but the cladists case that *none* do seems inadequate at best, reflecting attention only to historical explanations.¹⁷

Turn now to the second level. We might expect taxonomic disputes to play a role here in two ways. (A) The pluralism about the species category that we have been discussing may have repercussions for higher categories (ranks). (B) There is the possibility that the just-mentioned dispute between phenetic, evolutionary, and cladistic classifications is relevant.

I shall be very brief about (A). If anything like Kitcher’s or Ereshefsky’s species pluralism is right then it seems that we would have to have a distinct tree of life for each type of species; the species of each type are the base taxa for a distinct tree. If so then the *best* we could hope for with the higher categories is that each type of a category—for example, each type

of genus based on a type of species—is a natural kind. But turning to (B), we find that this hope seems not to be realized.

Consider (B). In discussing Kitcher's pluralistic realism, I noted that if the taxa picked out by a certain taxonomy are to justify a certain species category, *S*, it has to be *explanatorily significant* that the taxa are *S*. And using Kitcher's mollusk example, it is hard to see how it makes any difference to structural explanations whether a group is a species, subspecies, genus or whatever. This carries over to the higher categories: taxonomies may not yield explanatory categories in general. So even if we could establish that a certain taxonomy—phenetic, evolutionary, cladistic or whatever—was theoretically sound, so that classifying organisms in that way served our explanatory purposes in biology, that alone would not justify the view that any category posited by the taxonomy was a natural kind. Just because a taxonomy is right to classify a group of organisms as *Canis* and a subgroup as *Canis familiaris* does not show that there is any explanatory significance in treating the former as a genus and the latter as a species. We still need to show that the category itself does explanatory work.

Do the higher categories do any explanatory work? Cladists think not:

taxonomic ranks make little sense. . . . they do not think there will be any robust answer to the questions when should we call a monophyletic group of species a genus? a family? an order? Only monophyletic groups should be called anything, for only they are well-defined chunks of the tree. But only silence greets the question are the chimps plus humans a genus? (Sterelny and Griffiths 1999, 201)

So, on the cladistic picture, all categories (ranks) above species must be abandoned. Ereshefsky agrees although, as we have noted, he abandons the species category as well. He rightly points out that if a certain category is to be acceptable, the taxa of that category must be “comparable” and draws attention to reasons for thinking that this condition is not met (1999, 299). Brent Mishler claims that “practicing systematists know that groups given the same rank across biology are not comparable in any way” (1999, 310–311). In a lengthier critique of the Linnaean hierarchy, Ereshefsky mentions the drive to introduce more ranks, leading to a hierarchy in flux and to disagreements about the rank of certain taxa (2001, 215, 226). The signs are that, although the higher categories may have some pragmatic value, they are doing no explanatory work: they are not natural kinds.

In sum, at the first level I have found nothing against the view that the higher taxa, even some that are not monophyletic, are natural kinds. The

second level is different: the signs are that the higher categories are not natural kinds and so the Linnaean hierarchy must be abandoned. Finally, we should note that abandoning the Linnaean hierarchy is not abandoning a hierarchy altogether, it is not abandoning a tree of life. It is abandoning the labeling of categorical ranks in that tree (Ereshefsky 1999, 299; Mishler 1999, 311). But in light of (A) and (B), we may have to accept that there is more than one correct *uncategorized* tree of life, each reflecting legitimate explanatory concerns.

7 Conclusions

My aim in this essay has been to examine certain issues in biology to show that they are really issues over which kinds are causal-explanatory and hence “carve nature at its joints.” The issues are over which kinds are “natural,” not over the mind-independent existence of anything. So describing the issues as being about “realism”—as about what “exists,” is “real,” or is “objective”—has led to unclarity and confusion. Throughout my discussion I have emphasized the importance of distinguishing between first-level issues about *taxa* and second-level issues about *categories*.

Species pluralism is the view that there are several equally good accounts of what it is to be a species. Ereshefsky presents his argument from species pluralism to antirealism as an argument against the *existence* of the species category. I have argued that it is better seen as an argument against the *explanatory significance* of that category, hence an argument against that category being a natural kind. Not surprisingly, Ereshefsky sees his “pluralistic antirealism” as opposed to Kitcher’s “pluralistic realism.” Yet on close inspection, the two positions are similar: they agree that the species category itself is not explanatory but that various types of that category are explanatory. However, they differ in that Kitcher’s pluralism is more radical: Kitcher thinks that structural explanations in biology justify some species categories whereas Ereshefsky thinks that only historical evolutionary explanations can do so. I presented an argument that Ereshefsky is right: even if structural explanations motivate taxonomies they do not seem to show that a species category plays an explanatory role.

Finally, I considered antirealism about the higher categories. At the first level, despite the urging of cladists, there seems to be no good reason to suppose that only monophyletic higher taxa are natural kinds. However, at the second level, the signs are that the higher categories are not natural kinds: they seem to do no explanatory work. If they don’t, then the

Linnaean hierarchy must be abandoned. Furthermore, the case for various taxonomies suggests that we may have to accept more than one uncategorized tree of life.

Notes

1. This essay draws heavily on Devitt 2009a.
2. Elliott Sober (1980, 203) struggles with what Ernst Mayr means by *real*. David Hull draws attention to the unclarity of these “realism” issues (1999, 25–26).
3. Although, interestingly enough, an issue that Darwin himself was skeptical about: he talks of “the vain search for the undiscovered and undiscoverable essence of the term species” (1859/2004, 381).
4. Mayr’s distinction is established but often overlooked—for example, in Dupré 1981; Griffiths 1999; and Sterelny 1999. In his brief discussion of Stanford and Ereshefsky on realism, Kevin De Queiroz (1999, 74–75) does distinguish the taxa issue from the category issue but he does so against a background of a definition of realism that applies only to taxa. That definition also takes realism to be a commitment only to mind-independent existence, ignoring the far greater importance of explanatory significance in this debate.
5. As Hull emphasizes, many biologists require that a species concept be not only theoretically explanatory but also easily applicable: it must be “operational.” Hull rightly points out that “the philosophical arguments against operationism are decisive,” but then goes on to be surprisingly tolerant of an “operational criteria for theoretical concepts” (1997, 371).
6. And even more from commitment on the equally vexed issue of the nature of properties (supposing there are any).
7. We should acknowledge the popular view that species are *individuals* rather than kinds (Ghiselin 1974; Hull 1978). On that view, the species of tigers is an individual constituted by the fusion of all tigers and any particular tiger is a *part* of that individual rather than a member of the tiger kind. Adjusting my discussion of biological realisms to take account of this view would have no significant effect, so I shall continue to write as if species are kinds.
8. Richard Boyd claims that the types of species are unified by “an especially close homeostatic relation between the classificatory practices” (1999, 171); see also Wilson 1999, 203–204.
9. In another paper, Ereshefsky gives a related reason for doubting the existence of the species category: the failure of attempts to distinguish species from the higher taxa, in particular the failure to do so in terms of the processes of speciation and interbreeding (1999, 269).

10. Thus, David Armstrong (1978) is a selective realist, holding that empty predicates, negative predicates, and most pertinently, disjunctive predicates have no corresponding universal. He thinks that some predicates apply to the world in virtue of many universals. Most importantly, he looks to science to tell us which properties there are.

11. It is easy even for staunch realists to slip into loose ways of talking that suggest world-making. Thus Kornblith says that when we “group objects together under a single heading on the basis of a number of easily observable characteristics . . . we thereby create a nominal kind” (1993, 41). But we don’t! We create a *concept* that picks out a kind that may or may not be “real” in Locke’s terms, but which has its members independently of our creation. And Boyd, talking of kinds with nominal essences, says that their “boundaries” are “purely matters of convention” (1999, 142). But they aren’t! Our naming a kind picked out by a certain set of descriptions is conventional, but the boundary of the kind thus picked out is not.

12. Despite this, I do have my doubts that *any* species category is really explanatorily significant.

13. Stanford (1995) seems to disagree; see Ereshefsky 1998 and Devitt 2009a for criticisms.

14. It is perhaps worth mentioning that being a member of a certain subspecies or variety is also explanatorily significant: Fido’s being a pit bull explains a lot about his morphology, physiology, and behavior. Cf. Joel Cracraft’s description of concern about “the ontological status of subspecies” (1983, 100).

15. Boyd is also skeptical of monophyly (1999, 182).

16. It is also surely explanatorily significant that something is a predator or a parasite. Consider this, for example: “The Lotke-Volterra equations . . . describe the interactions of predator and prey populations” (Sober 1980, 202). But these are not the sort of classifications that concern the cladists. (Thanks to Marc Ereshefsky.)

17. Mishler concludes his summary of the argument for monophyly with this remarkably inadequate claim: “Because the most effective and natural classification systems are those that ‘capture’ the entities resulting from processes that generate the things being classified, the general biological classification system should be used to reflect the tree of life” (1999, 309–310). It is probably the case that the classification systems in *all* sciences “capture” entities resulting from processes—entities don’t come from nothing!—but it doesn’t follow that they should be classified to reflect those processes.

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