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BIOBEHAVIOUR

Whales, fish and Alaskan bears: interest-relative taxonomy and kind pluralism in biology

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Abstract
This paper uses two case studies to explore an interest-relative view of taxonomy and how it complements kind pluralism in biology. First, I consider the ABC island bear, which can be correctly classified into more than one species. I argue that this classificatory pluralism can be explained by reference to the range of alternative explanatory interests in biology. In the second half of the paper, I pursue an interest-relative view of classification more generally. I then apply the resultant view to a second case study: whether whales are fish. I argue that this question is not one about scientific vs folk usage, as has been assumed. I also develop a new view: that *Fish* should be rejected as a category, both from the point of view of biological science, and from the point of view of folk taxonomy. Along the way, I use the interest-relative view to shed light on the circumstances under which higher taxa should be accepted as legitimate categories for biological science.

Keywords Interest-relative taxonomy · Kind pluralism · Species · Higher Taxa

1 Introduction

Imagine that you are a school photographer, and I am a school cook. You want to take a good photo of the children, and I want to cook them lunch. For your task, you need to arrange the children into groups by height, because you need to put the shortest children at the front, and the tallest ones at the back. For me, things are different. I should categorise them based on their dietary requirements. I will use groups such as meat-eater, vegetarian, vegan, child with food allergies, child with religious commitments, etc. It makes sense for me to group the children this way because it is most useful to my purpose. Needless to say, our two systems will diverge in their groupings. Suppose

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Anita and Polly are meat eaters (with no allergies or religious commitments), and that Anita is short and Polly is tall. They will fall into the same kind for me but not for you. Our kinds cross-classify each other. It is clear that neither one of us has a uniquely privileged taxonomy of the children. Your taxonomy is good for your purposes and mine is good for mine.

This paper is about how this sort of classificatory taxonomic divergence occurs in biology, and what we should do when it does. We must start with the notion of *kind pluralism.* By this, I mean all and only cases when we have more than one classification system, such that:

(1) The systems do not agree in all of their groupings
(2) For at least two of the systems, it is not the case that they should be rejected as illegitimate
(3) No one of the systems is the 'best', 'privileged' or 'objectively correct' system

The core terms in (1–3) will be explained and elaborated as we go. The photographer/cook case is a small example of kind pluralism. The case also embodies a particular view of taxonomic divergence, which we can call the interest-relative view. On this view, alternative categorisation systems are assessed relative to certain interests. Three core points make up the substance of the interest-relative view of taxonomic divergence. First, different interests are what lead to the taxonomic divergence. Second, what vindicates the alternative systems as legitimate is the fact that each taxonomy is indeed useful for that particular interest. Third, because each is legitimate for its own purpose, no one of them is objectively 'the best'. Rather, each is useful for the interest it is intended for. The interest-relative view just summarised is a general picture of how divergent taxonomies arise, and how we should judge them as legitimate.

Views similar to the interest-relative view are defended by Rorty (2000), Taylor and Vickers (2017) and Goodman (1955).[1] In philosophy of biology, a view similar to this is associated with Dupré (1993), and I have recently developed a similar view for a case in psychology Taylor (2017). The purpose of this paper is not to argue that the interest-relative view is better than any other view of taxonomy. Rather, it is to introduce an interest-relative view of taxonomy, clarify some of its relations with kind pluralism, and lend it initial plausibility by showing how it can correctly deem some taxonomies legitimate and others illegitimate. For these reasons, the interest-relative view will largely be assumed in what follows. Even opponents of the view will be interested in how the view copes with concrete case studies from biology, so this paper will still be important for the unconvinced.

In Sect. 2, I examine the various alternative concepts of 'species' within biology. I argue that the interest-relative view can make sense of this divergence in terms of the variety of *explanatory* interests within biology. I do this by way of a case study: the ABC island bear. In Sect. 3, I impose two criteria that taxonomic systems must fulfil in order to be deemed legitimate. I then briefly consider how the view relates to the issue of natural kinds (Sect. 4). Finally, I consider what the interest-relative view has to say about the issue of whether whales are fish. I argue that the *Fish* category fails to be a useful category, both for the purposes of biological science, and from the point of

[1] As Dasgupta (2018, pp. 279–280) points out, Goodman thought that linguistic history (not usefulness) determines which taxonomic practices we engage in.

view of folk taxonomy (Sect. 5). In a sense I argue that the question of whether whales are fish is a misplaced one because it assumes that *Fish* is a respectable category into which whales should either be placed or not placed. But the truth is different. *Fish* is not a legitimate category in the first place.

2 The ABC island bear

It is well-known that there are several distinct concepts of 'species' in biology (Kitcher 1984; Dupré 1981, 1993; Ereskefsky 1992, 1998, 2001; Stanford 1995; Zachos 2016; Wilkins 2018). The phylogenetic concept sorts organisms into species based on their ancestry. The ecological concept classifies organisms into species based on their being lineages that share the same ecological niche. Very roughly, an ecological niche is composed of the environmental pressures that are placed on an organism, and the properties and behaviours it has developed to cope in that environment (Ereshefsky 2001). The biological concept claims that species are relatively isolated gene pools. On this picture, species are groups of actually or potentially interbreeding populations, and these populations must be reproductively isolated from other populations (Mayr 1957). A species (on the biological species concept) will include populations of organisms that can (actually or potentially) exchange genetic material in a relatively free manner, and will exclude those populations where this kind of gene exchange is either not possible, or is highly restricted.

These are only three species concepts, and they only scratch the surface of divergence in species concepts within biology. Zachos (2016, ch. 4) and Wilkins (2018, appendix B) each list 32 species concepts, and both acknowledge that their lists aren't exhaustive.[2] With at least 32 concepts to pick from, why choose these three? The first reason is that this paper is concerned with how the interest-relative view deals with cases of kind pluralism, and the three concepts already outlined are sufficient to generate cases of kind pluralism (as we shall soon see). For this reason, they are sufficient to make the points with which this paper is concerned. Extension of the arguments to cases where more species concepts are at work will be straightforward.

I will make some further comments about the other species concepts operative in biology in order to justify setting them aside. Many of them simply aren't relevant to this paper. For example, the phylo-phenetic species concept was primarily designed for microbiology (Rosselló-Mora and Amann 2001), which is not the focus of this paper. A further important point is that it is misleading to see this long list of 32 species concepts as distinct from the three ones that I will primarily be working with. Rather, most of these alternative species concepts contain elements of the three concepts outlined above.[3] There are various ways that this occurs. The phylogenetic, biological and ecological species concepts will themselves subdivide into other subconcepts. Indeed, it is probably more accurate to say that each one represents an *approach* to biological taxonomy, rather than a single concept. As an example of this, take the biological

[2] Thanks to an anonymous referee for pushing me on this.

[3] I say that *most* of the alternative species concepts contain elements of the three mentioned above. Arguably, the pheneticist one does not (I will return to this species concept in Sect. 3).

species concept. In animal biology, interbreeding is often invoked with relation to the biological species approach, because interbreeding is the property that animals typically use to exchange genetic information. However, distinct tools are needed to apply the biological species concept to the microbiological world, because of course microbiological organisms do not interbreed (Redfield 2001). It's not even clear whether the biological species concept can apply to microbiology at all (Ereshefsky 2010). Indeed, even if we restrict our attention to animal biology, there are many different versions of the biological species concept, which differ in the extent to which they emphasise mate recognition behaviours in understanding gene flow between organism populations (Mayr 1970; Paterson 1985).

Another way that elements of the three approaches given above might filter down into other species concepts is when species concepts are developed that try to draw together the insights of other species concepts in order to form a unified one (Stamos 2003; Van Valen 1976; de Querioz 2007). Indeed, even authors who claim to reject the species category altogether typically replace it with concepts that draw heavily on one, two or all of the three approaches summarised above. For example, Pleijel (1999) aims to replace 'species' with the notion of a 'Least Inclusive Taxonomic Unit', but this concept makes crucial use of the notion of monophyly, which is an idea at the core of the phylogenetic species concept. Yet another way that we can see the influence of the phylogenetic, biological and ecological approaches is in the integrative taxonomy movement, which applies several distinct approaches to the same group of organisms, in the hope of finding convergence (Padial et al. 2010; Schlick-Steiner et al. 2010). In summary, though I do select three specific approaches from the panoply of available species concepts, they are the three approaches that most prominently breathe through the contemporary work on biological taxonomy, and so much that I say will have general application.

Consider the ABC island bear. There is a population of bears that live on the Alaskan 'ABC' islands. They are the result of successful interbreeding between female polar bears and male brown bears, and they can mate with both (Cahill et al. 2013). Because they are capable of this interbreeding, they constitute a population which can exchange genetic material with brown bears (*Ursus arctos*) and polar bears (*Ursus maritimus*). By the biological species concept, then, they are conspecific with these two groups. However, they have behavioural adaptations to their environment that are shared by brown bears, but not polar bears. So by the ecological concept, they should be classified as conspecific with brown bears, but not polar bears.

Here we have an instance of kind pluralism. I now show how an interest-relative view of biological taxonomy can explain the emergence of this kind pluralism. Amongst the range of explanatory goals in biology is assessing the extent to which a particular behaviour or phenotypic trait helps the organism in a specific setting, and thus what fitness advantage it confers. This helps us explain how it evolved. For example, perhaps we are interested in the explaining the evolution of bears that live in cold environments where there is a lot of ocean life, but very little land or plant life. If this is our interest, then we will be interested in particular traits of bears that live in such an environment. Given this interest, it is useful for us to taxonomise together those bears that live in the particular cold environment we are interested in, and which have developed a similar cluster of traits specifically to deal with them, such as white fur, the building of especially well insulated dens, or the hunting of very fatty ocean animals.[4] This restricts the grouping to one particular environmental setting, and to the particular properties that we wish to explain. This restriction is useful because it sets the explanatory target of the enquiry (we want to explain how *these particular* features help in *this particular* setting). It would not make sense for our group to include bears that live in other environments (and have different adaptations to their environments), because that is not our focus. If we included those bears in the group that requires explanation, then our explanations would be ranging over the wrong set of organisms. This grouping would of course include polar bears that live in the Arctic, and it would *exclude* ABC bears, because they do not live in such an environment, and they do not have the properties that are distinctive of animals that fulfil such an ecological niche. This would be the ecological species concept at work.

Other biologists may be interested in other explanatory goals, such as explaining how certain traits entered the population. For example, we may be interested in why ABC bears have a mitochondrial DNA profile like that of polar bears, whilst having the fur colour of brown bears (Cahill et al. 2013). For this task, we will be interested in grouping together those populations of bears that can successfully pass around genetic information with each other. Recognising these populations is essential for our explanation. For example, once we know that female polar bears engaged in breeding behaviour with male brown bears, we can explain how the ABC bears got their brown fur: they got it from the male brown bears, through the exchange of genetic material, by mating behaviour. Likewise, they have the mitochondrial DNA profile of polar bears because mitochondrial DNA is passed down the female line, from the female polar bears that they are descended from (Cahill et al. 2013). On *this* taxonomy, brown bears, ABC bears and polar bears are all grouped together inclusively. Without including both brown bears and polars into the populations of organisms that can exchange genetic information, we will not have our explanation of how polar bear mitochondrial DNA and brown fur is present in the same population of ABC bears.

In this way, the interest-relative view neatly explains this case of kind pluralism. One taxonomy (the ecological one), does not group together ABC bears with polar bears, but does group them with brown ones. On the other (the interbreeding one), ABC bears are grouped together with polar bears and brown ones. For the interest-relative view, these distinct systems arose because of distinct explanatory interests in biology. They are worthy of preservation because each one is useful for the interest that it was meant for.[5]

Notice that sometimes we will require different species concepts for different explanatory targets within the same overall project. If we are interested in how the brown fur trait got into the population, we must talk about interbreeding behaviours. When we are interested in how the brown fur became propagated within the population, and became the dominant fur colour, we will have to talk about ecological niche (and also common ancestry). The explanatory goals may not be explicitly separated, and the distinctions between them may be extremely fine-grained.

[4] There is some debate over whether polar bear fur is 'white', strictly speaking, because of its unusual radiative properties (Preciado et al. 2002), but I set this aside.

[5] The ecological and interbreeding concepts represent the clearest example of kind divergence, and are sufficient to establish my conclusions. Similar things will apply to the phylogenetic concept.

I have argued that the interest-relative view can make sense of this instance of kind pluralism in terms of a range of alternative explanatory goals within biology. Here one clarification is important. This does *not* imply that divergence in *explanatory* goals is the *only* way that kind pluralism can arise in biology. On the interest-relative view, any kind of divergence in interests can potentially give rise to kind pluralism, even interests that are nothing to do with explanation. This may raise a worry: if *any* interest can in principle give rise to kind pluralism, does the interest-relative view end up too permissive? It is to this that we now turn.[6]

3 Anything goes

As just stated, the interest-relative view is open to the possibility that a difference of interests (explanatory or not) can give rise to taxonomic divergence. Some will object that this is too liberal (e.g. Ereshefsky 2001, pp. 150–154; Ghiselin 1987, p. 146). The objection is not so much that an interest-relative view vindicates *all* possible taxonomies, but rather that it vindicates ones that we have some independent reason to think should not be vindicated.

This paper is not the place for a thorough analysis of this problem in all of its forms. Rather, I will outline two criteria that we can use to dismiss some taxonomies as illegitimate, on the interest-relative view. The aim is not to argue that these criteria can handle all of the potential problematic cases. I rather argue that these two criteria as plausible ways that the interest-relative view can dismiss at least some problematic taxonomies. The main purpose of this section is to establish these criteria, such that they can be deployed later in the paper when we consider whether whales are fish.

First, consider that the interest-relative view says that alternative systems should be accepted insofar as they fulfil a certain interest. So, the following criterion is reasonable:

(i) Taxonomic systems should be rejected as illegitimate if they fail to fulfil the interest that they are intended for.

How does this work in practice? Consider astrology. Astrology recognises the Sun and Moon as planets. Is this a legitimate classification system, by the lights of the interest-relative view? No. Astrology has a particular set of interests, which is predicting future events in humans' lives and predicting personality traits. But the grouping it produces is not useful for that interest. The system that astrology uses doesn't actually help predict events or personality traits (no grouping of celestial bodies could). So it isn't useful by its own lights, and it fails by the first criterion. It can be rejected.

One of the worries that fuels the 'anything goes' objection is that we should avoid views which legitimise pseudoscientific systems. Demonstrating how the interest-relative view can do this will help lessen this worry. However, similar points apply outside of pseudoscience as well. Consider the pheneticist school of biological taxonomy. Pheneticism taxonomises organisms into species based on their overall similarity (Sneath and Sokal 1973; Sneath 1995). The purpose of this system is to group life in a

[6] For some examples of non-epistemic interests that can result in different kind taxonomies, see Ludwig (2017).

theoretically neutral manner. The generally accepted view is that this approach failed, because the choice of which properties one picked would be theoretically driven, and thus the hope of establishing a taxonomy in a theoretically neutral way is violated (Ridley 1986; cf. Matthen 2009, p. 99). This received view of the pheneticist project is controversial (Lewens 2012), but we can say that *if* it is accurate, then it represents a good example of a serious taxonomy in biology that can be rejected for violating the first criterion.

Return again to the fact that the interest-relative view judges taxonomies by their ability to fulfil certain interests. Suppose we have two proposed taxonomies, A and B, and that A is useful for fulfilling some purpose, but B is *more* useful for fulfilling that very same purpose. Then, since A is the less useful for this purpose, it can be rejected and B can be embraced. This naturally leads to a second criterion by which to judge rival systems:

(ii) For a system to be accepted for a particular purpose, it must be the case that no rival system is better at fulfilling the purpose in question.[7]

As a potential example, consider cancer taxonomy[8]. Standardly, cancers were taxonomised by their organ of origin (perhaps along with their histological profile). But recently it has been proposed that grouping them by their epigenetic molecular profile is more useful for cancer treatment (Cortés et al. 2014, p. 70, Prat et al. 2013, p. 1). Suppose this new taxonomy really is more useful for this purpose. In this case, the former system is of course useful for treatment, but the new proposal is *more* useful, so the new one should be preferred (at least for that purpose) and the former should be rejected.[9]

Notice that these two criteria are in a sense not substantive *additions* to an interest-relative view of taxonomy. Rather, the interest-relative view says that different taxonomies flow from different interests. In this vein, judging different taxonomies by their ability to fulfil different interests (in the two ways outlined above) is a natural extension of the theory, rather than an ad hoc modification.

I will note one issue in order to set it aside. Usefulness can of course come in degrees: some groupings can be more or less useful for a particular purpose. Because the current view explains legitimacy in terms of usefulness, then legitimacy will also come in degrees. There are many interesting questions connected to spectra of legitimacy (some of which I have touched on elsewhere, Taylor 2018). However, I do not have space to discuss them here.

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[7] Dupré (1993, p. 51) also suggests something similar to the second criterion. But Dupré also claims that we can judge taxonomies based on whether they are epistemically virtuous (1993, pp. 242–243). Ereshefsky (2001, p. 162) criticises Dupré for not specifying which virtues he has in mind. We need not settle this debate, since my own proposed view does not make use of these virtue claims.

[8] Thanks to Will Davies for making me aware of this revision in cancer taxonomy

[9] Of course, in reality, it will not always be so straightforward. There may be aggregation issues. For example, suppose taxonomy X is useful for n purposes, whilst taxonomy Y is useful for n + 1 purposes. But the purposes for which X is useful may be somehow more important, along some suitably defined metric of importance (thanks to an anonymous referee for drawing my attention to this). I do not have space to address these issues fully here, but my preference would be to say that in such cases, it is indeterminate whether X should be preferred to Y. We should not worry if our view of classification has indeterminacy as part of it. On the contrary, indeterminacy arises in many ways when we examine kinds in the life sciences (e.g. Taylor 2019).

At this point, we can draw together the ideas of the paper developed so far to summarise the links between the interest-relative view of taxonomy and the kind pluralism we find in biology. The fact that different interests will deliver different groupings *explains* why it is that different systems with different groupings arise in biology (that is, it explains the phenomenon that point (1) is referring to in my definition of kind pluralism from Sect. 1). Conditions (i–ii) provide an explanation of what it *means* for two taxonomies to be 'legitimate'. That is, they are legitimate when they cannot be dismissed by either criterion. The concept of a 'legitimate' taxonomy is a core part of understanding kind pluralism, as kind pluralism is all about when different taxonomies can both be legitimate (see point (2) in the definition of kind pluralism). Finally, the two criteria (i–ii) also help to explain why we have cases where there is no objectively correct taxonomy of a group of organisms (point (3)).

4 Natural kinds

I have been talking about *kinds*, but to many, *natural* kinds are where it's at. Natural kinds (it is said) are part of the objective structure of the world, and when our systems latch onto those categories, we are carving nature at the joints (Sider 2011). Such a view may be thought to clash with the interest-relative view. But on inspection, the difference between natural kinds theory and the interest-relative view is not great.[10] In the philosophy of science, a theory of natural kinds attempts to give a general theory about what structure certain groups of entities have. This structure is then invoked in order to explain what makes certain explanatory and predictive practices in science highly successful (Magnus 2012, pp. 48–50; Boyd 1999, p. 68; Slater 2015; Franklin-Hall 2015). The core claim of any theory of natural kinds is that there are certain categories that are especially useful for scientific prediction and explanation, and that these categories refer to kinds that have a certain common structure (this common structure is spelled out differently by different theories, of course). This core claim is completely consistent with the view I have been putting forward. Whether or not the kinds that are especially useful for explanation and prediction do have a certain common structure, the main claims of this paper (that alternative taxonomies should be judged based on their usefulness) remain the same.

Of course, some theorists of natural kinds claim that we should *prefer* the categorisation practices that match up with the natural kinds in the world. But even this claim is not necessarily in tension with the interest-relative view. Suppose for the sake of argument that it is true that all categories that are particularly useful for scientific prediction and explanation have a certain structure in common, and that these are the natural kinds. If that is true then of course we should prefer the categories that latch onto natural kinds *for the purposes of scientific explanation and prediction*. There is no conflict here between my view and a theory of natural kinds.

How my account compares to theories of natural kinds can more easily be seen by way of comparison with Ereshefsky and Reydon's view (2015). Like myself, they impose a set of restrictions by which we can judge some kinds as superior to oth-

[10] Thanks to an anonymous referee for emphasising this.

ers, and like myself, these are not metaphysical criteria. They claim that kinds must be internally coherent, empirically testable and non-degenerative in Lakatos' (1970) sense. By contrast, I do not place these constraints on legitimate kinds. The constraints I place (spelled out with criteria (i–ii)) are much more liberal. Once again, the friction here is only superficial. Ereshefsky and Reydon offer their account as a way to judge the relative merits of *scientific* kinds (they sometimes say 'natural kinds'), but the interest-relative view is a way of testing the legitimacy of kinds in general, including non-scientific ones. It would be inappropriate to place constraints such as non-degeneracy and empirical testability on non-scientific taxonomies, because these are not relevant to non-scientific interests. It may be that Ereshefsky and Reydon are correct about what restrictions should be placed on *scientific* kinds, whilst the two criteria given above provide a good account of the legitimacy of kinds generally (including non-scientific ones).[11]

5 Are whales fish?

I now turn to the complex issue of whether whales are fish, and what the interest-relative view has to say about this issue. The whales/fish question is especially important to us, for several reasons. First, it is at the core of debates over how folk kinds relate to scientific kinds (Dupré 1981, 1993, 1999; Phillips 2014; Sainsbury 2014). So, it must be addressed by any view that purports to be about the legitimacy of both scientific and non-scientific kinds. Second, I have noted the worry that the interest-relative view is too permissive. In this section, I will deploy the two criteria I introduced above, and argue that they can provide us with a great deal of power when it comes to dismissing some taxonomic systems as illegitimate. Demonstrating the power of these criteria will have an emollient effect on those still concerned about whether the view entails 'anything goes'.

The whales/fish issue is usually seen as a clash between folk and scientific usage. For example, when discussing the issue, Dupré says:

> 'The general point of the discussion so far is to insist that ordinary language and science provide largely independent and often disjoint ways of classifying the biological world' (1999, p. 469)

Similarly, Ereshefsky and Reydon say:

> '[in the case of whale classification] folk biology and scientific biology provide competing classificatory programs' (2015, p. 984).

Here we clearly see Dupré, Ereshefsky and Reydon setting up the issue as one where folk and scientific groupings clash with each other. But setting up the issue like this is imprecise for several reasons. For one thing, folk linguistic usage gives us at least *two* distinct taxonomies. At one time, folk usage had it that whales are fish (Burnett

[11] Ereshefsky and Reydon (2015, p. 973) assume that scientific kinds are epistemically superior to folk kinds. They presumably mean superior for the interests of scientific explanation and prediction. Note that it may very well be that scientific kinds are the best ones *for these interests*, whilst it is still the case that scientific kinds are not superior to folk kinds in any absolute sense.

2007, ch. 2). But now, of course, folk usage has it that whales are not fish. That is what we are all taught in school, and when you ask people if whales are fish, then they will say no. So here we have one kind conflict: between modern and non-modern *folk* classification. Scientific taxonomy is also not monolithic. Scientific taxonomy used to claim that whales are indeed fish (Linneaus 1756, p. 39). So we have the non-modern scientific system to contend with, as well as the modern scientific system. The most tempting misconception of all is to assume that modern scientific taxonomy claims that whales are not fish. As I will argue below, this is incorrect.

5.1 The modern scientific use

First, terminology: I use the italicised *Fish* to refer to the proposed category, and the non-italicised word to refer to fish themselves. It is natural to think that on a modern scientific taxonomy of life, whales are not fish. After all, biology tells us that they are mammals. However, I shall now argue that a scientific taxonomy of life *does not* classify whales as non-fish. My argument for this claim is based on the claim that *Fish* is not a biological category, and that as a result science is silent on whether whales are or are not fish.

Dupré notes an idea like this, saying that 'the notion that there is a 'scientific' usage of the word 'fish' is a decidedly suspect one' (1999, p. 466). However, the reasons he gives for this claim should be resisted. He considers the proposed definition of a fish as a 'cold blooded-vertebrate with gills' (this would exclude whales, which are warm-blooded). He points out that on this definition, the category will not cover all of its intended referents. Some tuna are warm-blooded, and so would be excluded. Dupré also claims that on this definition, *Fish* would include a wide range of organisms, and that there is 'little or no biological rationale for a category containing just these groups' (1999, p. 466).

The fact that a good definition of 'fish' cannot be given which gives a set of scientifically respectable properties possessed by all and only its intended referents does not show that *Fish* is not a biological category. In an era of biological taxonomy that has rejected essentialism, the idea that different members of a single taxon must all share a set of intrinsic, scientifically respectable properties such as 'vertebrate' or 'cold-blooded' that is necessary and sufficient for membership into that taxon is now almost dead (Sober 1980, Hull 1965). Given that we are no longer essentialists, we shouldn't expect a good scientific taxon to be amenable to a definition in terms of intrinsic properties. So the failure of this particular definition of 'fish' (both in terms of capturing its intended referents, and in terms of the scientific rationale given for it) does not show that *Fish* isn't a biological category. Dupré's reasons should be rejected.

So, we should set aside Dupré's reasons to reject *Fish* as a good category for biological science. We should look to the interest-relative view for such reasons. To summarise, my argument will be this: recall that for a category to be accepted as legitimate, two criteria must be met. First, it must fulfil an interest. Second, it must be the case that no other taxonomy fulfils that same interest better. So, for *Fish* to be accepted as a legitimate category for contemporary biological science, it must fulfil these two criteria with respect to some interest from contemporary biology. I will first consider the ecological and interbreeding approaches considered above, and argue that they do not show that the *Fish* category is useful for biological science. Then, I turn to cladistics, and argue that we have good reason to think that cladistics is useful for biology, but that we cannot use the usefulness of cladistics to show that *Fish* is useful, because cladistics does not recognise *Fish.*

With the conclusion that cladistics does not recognise *Fish*, we can engage in the following reasoning. In order to be a legitimate category for biology, a grouping must fulfil some biological interest such that no other grouping is better at fulfilling that interest. So, given that cladistics is a taxonomic system that gives us groupings alternative to the *Fish* one, it follows that, in order for *Fish* to be legitimate, it must fulfil some biological interest, such that the groupings given by cladistics are not better at fulfilling that interest. This is because if the categories given to us by cladistics are better at fulfilling that interest than *Fish*, then *Fish* will be an inferior category for that interest and can be rejected. I consider a range of biological uses that *Fish* may fulfil, and argue that in all cases, either *Fish* is not useful for that purpose, or the groupings given to us by cladistics are more useful. Therefore, it is not the case that *Fish* can fulfil some biological interest and that no rival is better at fulfilling that interest, and so *Fish* is not a legitimate biological category. I will not attempt to consider every possible biological use that might be suggested for *Fish*, but rather just restrict myself to the clearest examples. Extension to other potential uses for the category will be straightforward.

This argument demonstrates some more of the power of the interest-relative view. The idea that *Fish* is not a biological category has often been mentioned, but as we shall now see, the interest-relative view can give us a principled explanation of why it is not one, which is not susceptible to the weaknesses that other arguments for the same conclusion face.

5.1.1 Interbreeding and ecology again

The first suggestion can be dealt with very quickly. Above, I discussed the ecological and interbreeding species concepts. These concepts pick out groups that are useful for biology, as we have seen. But obviously, we cannot claim that such concepts will vindicate *Fish.* Such concepts do not apply to *Fish*, because they are species concepts, and so do not apply to a proposed taxon as high-level as *Fish*. It is not the case that *Fish* represents a stable set of populations that can freely exchange genetic material with each other. It is also not true that all fish occupy the same ecological niche. If anything is going to vindicate *Fish*, then it will have to be suitable to apply to higher taxa (i.e. taxa higher than ‘species’ in the Linnean hierarchy).

5.1.2 No higher taxa!

One extreme view is that there is *no* useful way for biology to classify higher taxa, perhaps because higher taxa are unimportant for biology generally (cf. Ghiselin 1987, p. 141; Eldredge and Cracraft 1980, p. 249).[12] This is an extreme view. But even if

[12] Strictly speaking, Ghiselin and Eldredge and Cracraft argue that higher taxa are not the units of evolution, not that they are unimportant for biology. Ereshefsky (1991) replies to these arguments.

we do accept it, it is perfectly consistent with my claim that *Fish* is not a scientific category. If we claim that there is *no* useful way for biology to classify higher taxa, then (since *Fish* is a higher taxon if it is a taxon at all) of course it will follow that *Fish* is not a useful category for the purposes of biology. Thus, it will not pass the test of usefulness, and (for the interest-relative view) can be rejected.

5.1.3 Cladistics

Above, I dismissed the ecological and interbreeding concepts of species because they clearly can't apply to a taxon as high as *Fish.* Isn't the same true of the phylogenetic species concept? Well of course the phylogenetic *species* concept won't apply to *Fish*, but the general cladistics approach (that gave birth to the phylogenetic concept) is an obvious place to look for a way of classifying higher taxa in a biologically useful manner (Hennig 1950, 1966). Indeed, cladistics is by far the dominant approach to higher taxa, in both biology and philosophy (Rieppel 2005; Boyd 2010). The success of cladistics gives us good reason to think that it is a useful approach to higher taxa, at least for some biological interests. So, if *Fish* is recognised by cladistics, then this gives some good reason to think that it will be useful for biology. Maybe this can legitimate *Fish.*

But this doesn't work. At the core of cladistics is the idea of a monophyletic taxon: a taxon that contains an ancestor and all and only its descendants. These 'clades' are the groups that are recognised by cladistics. It is well known that 'fish' is not a monophyletic taxon. Rather, it is paraphyletic (it includes a common ancestor and some but not all of its descendants). The paraphyletic status of the fish category stops it from being a clade, and thus from being recognised as a scientific category by cladistics. The fact that *Fish* is not recognised by cladistics blocks us from justifying it based on the claim that cladistic taxa are useful.

I have argued that we have good reason to think that cladistics is taxonomic system that is useful for at least some of the purposes of biology, and can apply to higher taxa, and it excludes the *Fish* category. This is important for the argument that *Fish* isn't a legitimate biological category. To be a legitimate biological system, a taxonomic system must fulfil some biological interest *such that no other system fulfils that interest better.* So, in order to legitimate *Fish*, it is not enough that it fulfils some interest, it must do so *better* than the groupings of higher taxa given to us by cladistics (since these are rival groupings to the *Fish* one). Below, I consider several suggestions, and suggest that either *Fish* is useless for the purpose in question, or that the groupings given to us by cladistics are better.

Note that my argument doesn't imply that all higher taxa must be monophyletic in order to be useful for biology. We can agree with some authors that we shouldn't always be wedded to monophyly (Brummitt 2002, 2003; Mayr and Brock 2002). The claim isn't that monophyly is *always* the way. The claim is just that we have available a monophyletic taxonomy which is different from *Fish*. Thus, if *Fish* is to be vindicated, it must be the case that there is at least one biological interest such that the monophyletic taxonomy is not better at fulfilling that interest than *Fish*. The claim that there are *some* taxonomic groupings that are non-monophyletic, and which are better at fulfilling some interest than the monophyletic one, is not in tension with this claim. The claim is just that (for the interests I will examine) *Fish* is not such a category.

5.1.4 Descent and modification, uniformity, homeostatic cohesion and selection

Ereshefsky (1991) presents various ways that higher taxa may prove useful for biology. Since we are concerned with higher taxa, I take the uses illustrated in his paper as my model (to be clear, Ereshefsky is arguing for the legitimacy of higher taxa in general, not defending the *Fish* category). I argue that for all of the uses he proposes, *Fish* would either be useless, or be less useful than the monophyletic system that is its rival. Ereshefsky comments that higher taxa display a pattern of descent with modification, and that this makes them legitimate units of evolution. But when he says that higher taxa have common patterns of descent, he is defending the legitimacy of *monophyletic* higher taxa (he calls them 'lineages' but by this he means monphyletic taxa (1991, pp. 87–88)). This is with good reason. Using a paraphyletic category would be suboptimal for the purposes of tracking patterns of descent and modification, because paraphyletic groups exclude certain descendants of a common ancestor, so they would give one only a partial image of how certain traits have spread down the generations, and how they have been modified. For example, suppose we wanted to track the descent and modification of the mammalian forelimb. Using a paraphyletic group that excluded bats (say) would deprive us of a complete picture of how this forelimb has descended and been modified. The monophyletic group *Mammalia* would be much more useful for this interest, and should thus be preferred.

Ereshefsky (1991) considers other reasons to vindicate higher taxa as legitimate categories. I will deal with them quickly. First, he considers the suggestion (made by others) that a stable evolutionary unit should have 'cohesiveness', or 'uniformity'. As he points out, this notion is very unclear (1991, p. 89). Until such a notion is spelled out, we can't adjudicate whether *Fish* has cohesiveness or uniformity. Second, Ereshefsky notes that higher taxa can share a common ancestor, and thus will share some common genetic material. Ereshefsky suggests that this may act as a homeostatic constraint on the characteristics of members of that higher taxon (1991, p. 92). This homeostatic cohesion may legitimate the higher taxon in question. This is perfectly reasonable but it doesn't save *Fish*. Tetrapods share a common ancestor with fish, so we should expect them also to have inherited the same genetic homeostatic constraints from the common ancestor they share with fish. So the group that has homeostatic cohesion as a result of common ancestry will include the Tetrapods, and will not align with the *Fish* category. Again, the monophyletic category is better for the purposes of biology. Third, he notes that higher taxa may have similar selection regimes, in virtue of which they should be grouped together (1991, p. 92) but this doesn't apply to *Fish,* because they have very different selection regimes (including different predator/prey relationships). This point was already touched upon when I pointed out that different fish have different ecological niches.[13]

[13] Evolutionary taxonomy is a rival view to cladistics, which recognises paraphyletic taxa (Ereshefsky 2001, pp. 51–53). But as Ereshefsky and Reydon say: Cladism has 'led to the positing of countless successful classifications, as well as contributed to the extinction of the Phenetics school, and the near extinction of

5.1.5 Fish is not a biological category

I have surveyed a range of possible ways for higher taxa to be biologically useful, and found that in all cases, either *Fish* is not useful, or a rival grouping (given by cladistics) is more useful. Thus, given that the category does not fulfil the two criteria for a legitimate category, we can conclude that it should be rejected, at least when we're considering biological interests. Obviously, I have not shown that it is *impossible* for *Fish* to fulfil some biological interest. But given that we have examined a range of options, and none has worked out, it is reasonable to infer that it is unlikely that we will find such a use.

Now, if we accept that *Fish* is not a biological category, then we can begin to see the whales/fish question in a new light. Contemporary biology does not rule that whales are non-fish, because *Fish* is just not a category that biology deals with at all. This point can be made clearer by distinguishing between two claims, which differ in scope:

(i) A scientifically respectable taxonomy does not classify whales as fish.
(ii) A scientifically respectable taxonomy classifies whales as not fish.

(i) is true. *Fish* is not a biologically legitimate taxon, so biologists do not classify whales as fish. Incidentally, of course, they do not classify tuna, herring, salmon etc. as fish, for the same reasons. I am not denying of course that biologists themselves will still use the common word 'fish' to refer to herring, salmon etc. But this is just an artefact of the fact that biologists' informal nomenclature usually accords with folk nomenclature. But from the point of view of legitimate biological taxonomy, nothing is categorised either as fish or non-fish. But (ii) is false. For (ii) to be true, 'fish' would have to be a biological category that biology can include and exclude organisms from.

It is not plausible to claim that (ii) follows from (i). That would be to infer from the fact that science does not recognise a particular category to the claim that science classifies something as not falling into that category. This is clearly too extreme. The category 'beautiful painting' is not a scientific category, but it would be absurd to infer from this that science tells us that *The Last Supper* is not a beautiful painting.

Of course, 'mammal' is a good scientific category: it is a perfectly respectable monophyletic taxon, and whales *do* fall into that category. Ereshefsky and Reydon take this to indicate that scientific taxonomy clashes with the claim that whales are fish (2015, p. 984). But this is inaccurate. It doesn't follow from the fact that they are mammals to the claim that they are not fish. They could be both mammals and fish ('mammalian fish' as Dupré calls them (1999)). There is nothing in biology that tells us that mammals cannot be fish, because (to repeat a point already made) fish is not a category that biology deals with.

At this point, another taxonomy can easily be dealt with as well.: the non-modern *scientific* use (lasting until at least the eighteenth century) on which whales are fish (Burnett 2007, ch. 3). Should we accept this one? No. It should be uncontroversial

Footnote 13 continued
Evolutionary Taxonomy' (2015, p. 982). It would be unwise to rest our case that *Fish* is a biological category on an almost extinct research programme such as evolutionary taxonomy. In any case, whether or not *Fish* is recognised by evolutionary taxonomy, the real question is whether it is *useful*. We have already examined this idea, and found that it is not useful.

that modern biology has replaced this eighteenth century biological taxonomy with one that is more useful for the purposes of biology. So, the interest-relative view tells us that we should disregard the old scientific taxonomy in favour of the modern one.

The crucial point is this: once we realise that *Fish* is not a biological category, we should drop the idea that there is a clash between contemporary biology and the claim that whales are fish. For all of these reasons, we need to stop seeing the debate as a clash between folk and scientific taxonomy, as suggested by Eresehefsky and Reydon and Dupré in the quotations I gave above. There is no clash between the folk and science, because science is silent on the question of whether whales are fish.

5.2 Modern folk use

Dupré says:

> 'I have already conceded the obvious fact that whales are not fish. Why not? We might ask both why this fact is obvious, and why it came to be a fact at all. The first question is easy enough. Educated speakers will, I suspect, almost unanimously refuse to apply the word 'fish' to, Blue whales, Killer whales… Ultimately, I suppose that this is the only relevant evidence and that it is decisive.' (1999, p. 467).

Now, it could be that Dupré is here simply reporting a linguistic fact. On this interpretation, when he says that whales are not fish, all he is saying is that this is not how the word 'fish' is used in modern English. If that is all he means, then clearly he is correct. I won't debate this point. However, he could mean something more substantial. He may mean that the fact that people use the word 'fish' in this way is sufficient reason for us to accept this taxonomy. This seems to be indicated by his claim that folk usage is 'decisive' in determining that whales are not fish. But on the view proposed in this paper, it is not enough to rely on linguistic usage. Rather, the categorisation has to be *useful* for an interest, such that no alternative taxonomy is better for that interest.

The modern folk category *Fish* fulfils a broad range of interests. I will restrict myself to examining the interest which drove people to exclude whales from *Fish*. This is the most directly relevant to our purposes. Plausibly, this is because they mistakenly believed that it was *wrong* to class whales as fish. They believed that biology had *proved* that whales aren't fish. As a result, the interest that drove people to exclude whales from *Fish* is that they wanted their usage to accord with the scientific discovery that whales are not fish. But the modern usage fails to do this, because there is no scientific discovery that whales are not fish. As elaborated at length above, this is because *Fish* is not a category that biology deals with, and so it is not one that it can exclude organisms from. So the modern folk category of *Fish* fails to fulfil its intended purpose: it fails to accord with a scientific discovery.[14] For this reason, it can be dismissed as failing on its own terms.

[14] Another suggestion could be made: that the modern folk use arose in order to more closely model a different scientific discovery, the discovery that *Fish* is a bad scientific category. However, if the modern folk usage were to accurately reflect this fact, then it should abandon the *Fish* category entirely, rather than claim that whales are not fish. So either this is not the intended purpose of the modern folk usage, or it is its intended purpose and modern folk usage fails to fulfil it adequately.

Of course, it may be the case that there is some *other* modern folk use that is relevant to legitimating *Fish*, but as I said above, I do not have the space to examine all such proposals. At the very least, we can reach the following conclusion: that for the purpose that plausibly drove people to exclude whales from the *Fish* category, the category fails to fulfil that interest.

5.3 Non-modern folk use

Things are looking good for the non-modern folk use, on which whales are fish. However, this view does not win by default. It must be justified by serving a particular interest better than the rivals. Who would have such an interest? The most clear example is *whalers.* As we know (Burnett 2007) there was a time when whalers claimed that whales are fish. This is an excellent case to examine because there is a clear folk practice, that has a clear purpose, and which has a particular taxonomy. The practice is whaling, the purpose is catching whales (skinning them, preparing their blubber, etc.), and the taxonomic claim is that whales are fish. In order to vindicate this taxonomy, then, we must ask whether this taxonomy is *useful* for this purpose.

There are several factors that may lead us to the conclusion that it is useful for whalers to group whales as fish. Many of the traits that are most important to a whaler are its behavioural traits, which whales share with fish (at least at a coarse-grain). For example, how they move through the water, their threat avoidance behaviour, and (most obviously) the fact that they live in the ocean, rather than on land. These are all properties that whalers are interested in, and they are all properties that whales share to a much greater extent with creatures like sharks, salmon, herring etc. than with any other group of organisms. So (it may be thought) this gives us good reason to think that the grouping of whales as fish is useful for whalers, and thus the claim that whales are fish is vindicated.

At this point, it could of course be pointed out that at a fine-grain of detail, the properties shared by whales and fish are not very similar, and that there are many dissimilarities between whales and things like salmon, etc. (they have lungs, they surface for air, their skeletal structure is different, etc.) But even setting these issues aside, there is a much deeper problem with the reasoning above that attempts to vindicate the non-modern folk usage. Of course, it is true that whalers are primarily interested in properties such as overall body shape, and marine environments, and it is also true that these properties are shared more with herring, sharks etc. than with other groups of organisms. However, to justify the preservation of the whalers' taxonomy, it must be shown that the category *Fish* is useful for the whaler. The issue is that this category is very broad, in a way that is not useful to the whaler. Whaling is not concerned with creatures such as herring, salmon, mackerel etc. The whaler's focus is on whales, and only on whales. Given that they do not have interests that extend to fish as a whole, the taxonomic practice that extends to the whole of fish does not have a clear use for the practice of whaling. All the whaler needs to know is the properties that whales have that impact on how to hunt them and prepare them. Exactly how these relate to the properties of salmon, herring, sharks etc. is not important for the whaler, because the whaler's interests don't extend that far.

Ultimately, the category of *Fish* (whether or not it includes whales) is not useful for the interests of the whaler, because their interests do not extend so far that considering the entire category of fish themselves is really useful. So, since the taxonomy is not useful in this way, it again fails the test.[15] Again, it could be the case that there is some *other* non-modern folk usage for *Fish*. As before, I do not have space to examine all such possible uses, but we can reach the conclusion that at least for the purposes of the folk practice which is most relevant to the inclusion or exclusion of whales into *Fish*, the category fails to fulfil the two criteria in question.

The situation we are left with is this: we had four potential ways of settling the question of whether whales are fish: the modern scientific one, the non-modern scientific one, the modern folk one, and non-modern folk one. But using only the austere resources of the interest-relative view, we have found reasons to be sceptical of the utility of *Fish* for all of these practices. The result is that *Fish* should be rejected as a category, for folk and scientific purposes. In a sense, then, the question of whether whales are fish should be set to one side. The point is just that *Fish* is not itself a useful (and therefore legitimate) category.

6 Conclusion

We can see the ABC island bear case as an example of the way that the interest-relative view can act positively, to vindicate alternative taxonomies of kinds: it justifies preservation of several alternative concepts of species because they are useful for biology. Conversely, the question of whether whales are fish is one way that the interest relative view acts negatively: to dismiss taxonomies as illegitimate. Recall that the main objection to the interest-relative view is that it is too permissive. But in fact, examination of the question of *Fish* reveals that it is extremely restrictive: it has within it the power to reject a great many taxonomies as illegitimate.

To be clear: the problem is *not* that *Fish* is too folksy, or that it is ill-defined, or that it is not a sharp category. A legitimate biological category can be difficult (even impossible) to define, and many (even most) of them do not have sharp boundaries (Hull 1965; Sober 1980; Boyd 1999). Further, being 'folksy' is no problem if we are looking for reasons to think that a *folk* category is legitimate. Rather, the problem with *Fish* is just that it's not really useful.

The purpose of this paper was to see how far the interest-relative view could go, even if we resist the temptation to privilege some interests over others. It turns out it can go surprisingly far: it gives us a good picture of why there are alternative species concepts, and it's powerful enough to give us a principled reason to reject the *Fish*

[15] Here there is a disanalogy with the taxonomy considered in Sect. 3, which groups polar bears, ABC bears and grizzly bears together. Here it was explanatorily important that the taxonomy extended beyond just the ABC bears themselves because interbreeding behaviours between *all three groups* is a core part of the explanation in question.

category. Ultimately, this relatively liberal view may be just the view of taxonomy that we need.[16]

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