

Cladism, Monophyly and Natural Kinds

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Cladism, today the dominant school of systematics in biology, includes a classification component—the view that classification ought to reflect phylogeny only, such that all and only taxa are monophyletic (i.e. consist of an ancestor and all its descendants)—and a metaphysical component—the view that all and only real groups or kinds of organisms are monophyletic. For the most part these are seen as amounting to much the same thing, but I argue they can and should be distinguished, in particular that cladists about classification need not accept the typically cladist view about real groups or kinds. Cladists about classification can and should adopt an explanatory criterion for the reality of groups or kinds, on which being monophyletic is neither necessary nor sufficient for being real or natural. Thus the line of reasoning that has rightly led to cladism becoming dominant within systematics, and the attractive line of reasoning in the philosophical literature that advocates a more liberal approach to natural kinds, are seen to be, contrary to appearances, compatible.

Keywords: Classification; cladism; pheneticism; natural kinds; systematics; species.

1. Introduction

Cladism is today the dominant school of classification in biology. It incorporates a classification component, a metaphysical component, and a methodological component (Sterelny and Griffiths 1999). The classification component involves the idea that the goal of classification is, or ought to be, to represent phylogeny and only phylogeny, i.e. evolutionary relatedness, or common ancestry. It follows that taxa must

be monophyletic (a taxon is monophyletic iff it consists of an ancestor¹ and all and only its descendants; in a monophyletic group each member of the group shares a more recent common ancestor with every other member of the group than they do with any organisms outside the group²). The metaphysical component is the claim that all and only the really existing groups or kinds of organisms in nature are monophyletic: if a taxon is monophyletic it is an objectively real group or kind, and if a taxon is not monophyletic it is unreal or artificial, in that it does not correspond to a group with a unified evolutionary history.³ The methodological component is a set of techniques for inferring phylogeny, the most dominant of which is the Parsimony approach (although other methods, such as the Maximum Likelihood approach, have been preferred by some cladists (Quinn 2017)).⁴

Much of the literature on cladism has focused on its methodological aspect (e.g. the classic discussions of cladism in Hull (1979) and Sober (1988) are almost entirely concerned with this). While recognising that of course the methodological and theoretical components of cladism are not unrelated (the insistence that classification respect only phylogeny would be idle if cladism's methods for inferring phylogeny were unworkable), I propose to focus primarily on the classification and metaphysical components. More precisely, I propose more or less to take for granted the truth of the classification and methodological components, and explore whether, once these are accepted, we must also accept the metaphysical component.

The plan of the paper is as follows. In section 2 I argue that while they have typically been treated as the same question, the classification question and metaphysical question are logically distinct—the answer we give to the former is logically independent of the answer we give to the latter. In section 3, I argue that the characteristic cladist metaphysical position ought to be rejected: monophyly is neither necessary nor sufficient for defining real groups/kinds of organisms. And in section 4, I offer an alternative explanatory criterion for the reality of groups/kinds.

¹ This is 'ancestor', not, as it is commonly stated, 'species', for reasons that will become apparent.

² See Podani (2010) for a discussion of the different ways in which monophyly has been understood. He calls the definition I am using the 'consensus' view. Monophyletic taxa contrast with paraphyletic taxa (consisting of an ancestor and some but not all of its descendants) and polyphyletic taxa (sets of species not including a common ancestor of the group). See Ashlock (1971) for an early, useful discussion of these matters.

³ See e.g. Cracraft, who says that groups lacking a unified evolutionary history are 'nonexistent' (1981, 462).

⁴ See Quinn (2017) for a discussion of the many different (sometimes conflicting) meanings 'cladism' and 'cladist' have taken on over the years. Despite these different uses of the terms, the characterisation I offer here (taken from Sterelny and Griffiths 1999) is fairly standard and should be reasonably uncontroversial.

2. *Distinguishing the metaphysics and classification questions*

In the literature on cladism, the metaphysical question and the classification question are typically treated as the same question. That is, the question: which groups should be recognised in classifications, i.e. should be regarded as taxa, is thought to be equivalent to the question, which groups should be recognised as real, natural, objective, groups or kinds in nature?⁵ In particular, cladists have held that the view that all and only monophyletic groups are taxa is equivalent to the view that all and only monophyletic groups are real.

But the questions are logically distinct. For instance, many theorists hold that species are real, objective units in nature, whilst higher taxa—families, classes and the like—are ‘constructs of the systematist’s mind, not existing in nature in any real sense’ (Eldredge and Cracraft 1980: 250).⁶ Those who hold this view do not, typically, hold that species are the only taxa. They may recognise that higher taxa do have a role in classification. It is just that as such, they do not correspond to really existing units in nature. In particular, it would seem to be perfectly consistent for one to be a cladist about classification while accepting the popular view that only species, not higher taxa (even if they are monophyletic), are objectively real (indeed this combination of views is explicitly defended by some cladists e.g. Eldredge and Cracraft (1980)). Of course cladists about classification have tended to accept the traditionally cladist view about the latter question, according to which all and only monophyletic groups are real, whether species or not. But this is not, I suggest, compulsory once one has accepted the cladist view on classification.

Conversely, one may hold that certain groups or kinds are real, without holding that they are taxa (as I shall discuss below). So it would seem that, conceptually, being a real group or kind is neither necessary nor sufficient for being a taxon. Of course one *may* hold that all and only taxa are real groups; but this would be a substantive position, it does not follow analytically from the concepts of ‘taxon’ and ‘real group/kind’. One who recognises taxa they do not believe are real are not conceptually confused, I maintain.

In the context of cladism, the classification question is: given a phylogeny, is it the case that the taxa that are recognised by the correct classification are all monophyletic? While the metaphysical question is: is it the case that the only groups of organisms that are objectively real are the monophyletic taxa?

⁵ See e.g. Cracraft (1981: 459).

⁶ See Mishler and Donoghue (1982). Often this is expressed in ontological terms: species are individuals, higher taxa are collections of species, and thus ‘classes’ (Eldredge and Cracraft 1980).

It will be helpful to distinguish three views on classification from three views on ontology:

Classification:

1. All and only monophyletic groups are taxa
2. Taxa may be monophyletic or paraphyletic (but not polyphyletic)
3. Taxa may be monophyletic, paraphyletic or polyphyletic

Ontology:

4. All and only monophyletic groups are real⁷
5. Real groups may be monophyletic or paraphyletic (but not polyphyletic)
6. Real groups may be monophyletic, paraphyletic or polyphyletic

Characteristically, (1) and (4) have been held by cladists;⁸ (2) and (5) by evolutionary taxonomists (see e.g. Mayr (1942), Simpson (1961));⁹ and (3) and (6) by pheneticists.¹⁰ The claim I defended above about the logical independence of the classification question and metaphysical question can be understood as the claim that this traditional combination of views is not logically compulsory. If they are logically distinct *one may combine any of the views on classification with any of the views on metaphysics*. Some of these combinations would be odd—e.g. combining the phenetic view on classification with the cladist view on metaphysics; odd but not perhaps logically contradictory. One may combine the cladist view on classification with the phenetic view on metaphysics less oddly perhaps. But defending the consistency of *all* of the positions on taxonomy with *all* of the positions on metaphysics is not required for my argument. All that is required is that the cladist view on classification be consistent with all three positions on ontology.

As I have noted, this consistency has not been generally recognised. It has been assumed that the classification question *just is* the metaphysical question.¹¹ Once we distinguish the questions, it still remains

⁷ Of course, here and throughout the paper this should be understood as referring to the question of which groups of *organisms* count as real groups or kinds.

⁸ In the philosophical literature (4) has been defended by Rieppel (2005).

⁹ Evolutionary taxonomists allow paraphyletic but not polyphyletic groups because they believe classification (and metaphysics) ought to represent and take account of divergent, but not convergent evolution (Ridley 1986).

The group comprising lizards and crocs but excluding birds is paraphyletic. Birds and crocs are more closely related to each other than either is to lizards, so grouping crocs and lizards together apart from birds can only be justified on phenetic grounds: by the fact that crocs and lizards are more similar to each other than either is to birds. This is the case because birds have diverged morphologically from other members of their clade. But convergence is not respected by the second view. So in some cases where evolutionary relatedness clashes with overall similarity (ones deriving from divergence) the view opts for the similarity criterion; in other cases where they clash (ones deriving from convergence) it opts for evolutionary relatedness.

¹⁰ The metaphysical positions are not as explicit in evolutionary taxonomy and pheneticism as in cladism.

¹¹ For instance, Sober, in his characterisation of cladism (1988), only mentions the classification and methodological components, presumably because he takes it that the classification component encompasses the metaphysical component.

the case of course that cladists *have in fact* defended (1) and (4). But my claim is that they needn't have done so; that accepting (1) does not logically compel them to accept (4). This ought to be an agreeable fact for cladists given that, as I will argue below, (4) is very implausible.

It is not surprising that the metaphysical and classification questions have not generally been distinguished. It is often said indeed that the aim of biological classification is to identify 'natural' groups (Ridley 1986). The goal is the construction of a 'natural' classification that identifies and names all and only the real, objective groups and kinds in the area under study, one that 'cuts nature at its joints', i.e. the distinctions it draws correspond to real, objective, mind-independent divisions between things in the world. On this view, the classification question and the metaphysical question go together: in a natural classification, a group is a taxon iff it is a natural group or kind. And certainly there is a sense of 'classification' on which this is reasonable: on which there is no meaningful distinction between classifying and identifying kinds. Nonetheless, I think the question of classification can be and often is understood in a different sense, a sense in which it is an open question whether the groups picked out by a (the?) correct and objective classification system are all and only the natural or real groups or kinds in nature. We can accept that the aim of a classification is to carve at joints, and mark objective distinctions in nature. For instance, Ridley says that an 'objective classification' is one in which 'the choice of characters is dictated by a theoretical principle. The principle must specify some discoverable hierarchical property of nature, which it is desirable and technically possible for classification to represent' (1986, 3). Cladism arguably satisfies this condition in its aim of representing the objective branching order of the tree of life. If humans and chimps are more closely related to one another than either is to gorillas, this is an objective fact about the world in a way that relations of similarity can never be. Hence cladism's (in my view) justified claim to being a more objective, and thus more adequate, system of taxonomy than either pheneticism or evolutionary taxonomy. But it is quite another thing to expect of a classification that it identify all and only the really existing groups or kinds in nature. It is far from obvious that the reasonable requirement that a classification be 'objective', or 'natural', should be interpreted as the requirement that such a classification should achieve this much stronger and more ambitious aim.

More specifically, I will understand biological classification in a relatively minimal sense, as involving an objective, non-arbitrary, unambiguous system of organising, grouping, ranking and naming. In biology we expect a classification to be hierarchical, i.e. involve classifying into ever more inclusive, non-overlapping categories. Two points are important here. First, such a system must respect natural divisions *sensu* Bird (2018), in the sense that it maps *only* natural divisions among organisms; it need not map *all* the natural divisions: this would be asking too much. Secondly, biological classification need not pick out

all and only natural *kinds*. I follow Bird (*ibid*) in claiming that natural divisions are necessary but not sufficient for natural kinds. A classification may identify and name taxa that are not natural kinds, and there may be natural kinds (involving natural divisions) that it does not identify or name.¹² (These points will become clearer in due course.)

One uncontroversial way of distinguishing the classification and metaphysics questions would be to argue that classification is or ought to be pragmatic, i.e. relative to human interests and purposes (scientific and/or non-scientific), such that a classification system need not identify all and only the real groups or kinds in nature (Dupre 1981, 1993). It is important to see that this is not the view I am defending. I am suggesting that even if we accept (as I think we should) that a classification system ought to be objective—ought to capture objective divisions in nature—it still may not identify all and only the real groups or kinds.

The logical independence of the classification and metaphysics questions is implicit in Sterelny and Griffith's (1999) discussion of cladism. On the classification question, they side with cladism (196–197). They reject pheneticism, as well as the compromise, or 'mixed' approach to classification favoured by evolutionary taxonomists, on the standard grounds that of the three systems, only the phylogenetic approach has a chance of being systematically objective, in that what it aims to capture—the order of evolutionary branching and thus what Darwin called propinquity of descent—is genuinely objective, whereas both pheneticism and evolutionary taxonomy must appeal to judgments of similarity and extent of evolutionary divergence, which can never be rendered fully objective.

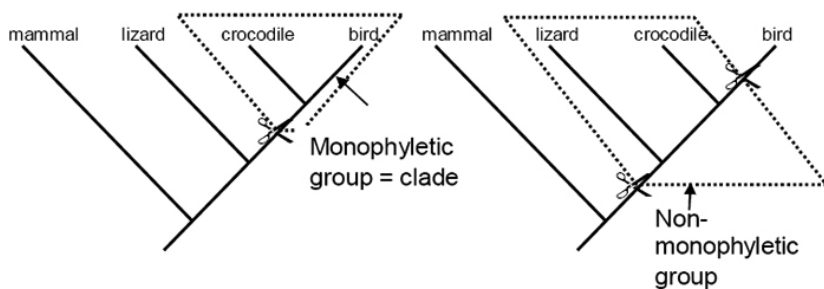
But on the metaphysical question, they adopt the compromise (characteristically evolutionary taxonomy) view. 'To the extent that cladists really do want to reject truncated monophyletic [i.e. paraphyletic] groups—groups that contain nothing but a single species' descendants, but not all of them—their views are too extreme' (198). This is because, they think, there are real groups that are paraphyletic: 'We think it quite likely that there can be good evolutionary hypotheses about such *paraphyletic* groups. For example, there may well be sensible evolutionary hypotheses about all the nonmarine mammals... it's easy to imagine events that affect all of, and only, that truncated group.' (198) Note the implicit criterion for recognising groups—are there good/sensible evolutionary hypotheses about them? Are there events that affect all and only their members? I will return to this. Although they don't explicitly present it this way, I take it that Sterelny and Griffiths are accepting the cladist position on the classification question, while accepting the evolutionary taxonomy view on the metaphysical question, on the grounds that we use different criteria to determine a taxon and

¹² Thank you to an anonymous referee for encouraging me to be more explicit about what I take classification to be.

to determine a real group: the criterion for the former is evolutionary relatedness (phylogeny); the criteria for the latter at least includes whether there are good evolutionary hypotheses about the putative group. Even in Sterelny and Griffiths, the distinction between the metaphysical and the classification question, and the possibility of accepting the cladist view of classification while rejecting the characteristically cladist metaphysical view, is only implicit. But they must accept the distinction, if they think the compromise view on the classification question is untenable (196), but think also that we should recognise paraphyletic groups. This only makes sense if these are addressing different questions, that is, if being a taxon is not the same thing as being a real group. In particular, it follows that even if the compromise view of classification must be rejected, the compromise view on the metaphysical question may still be accepted.

Below I will argue that once we have accepted paraphyletic real groups, there is no justification for stopping there: we can, and perhaps should, also accept polyphyletic real groups. That is, the compromise view on the metaphysical question is unmotivated, and we should adopt the characteristically phenetic view on the metaphysical question (which, recall, is the view that real groups may be either monophyletic, paraphyletic, or polyphyletic), though not on traditionally phenetic grounds.

To conclude this section, consider the well-known phylogeny of birds, crocs and lizards:



The cladist about classification holds that birds and crocs should be grouped together apart from lizards, while evolutionary taxonomists would group lizards and crocs together apart from birds. But this, I suggest, is entirely a question concerning classification. It is a further, distinct question whether the group including crocs and birds but excluding lizards is objectively real in a way that the group including crocs and lizards but excluding birds is not. To put it another way, according to cladists, classification is all about the sister-group relation. Crocs and birds are sister groups relative to lizards. Birds/crocs and lizards are sister groups relative to mammals. But it is hard to see why the sister-group relation should tell us anything very much about the metaphysics of real groups or kinds.

3. *Against monophyly as a metaphysical criterion*

So cladists (about classification) *may*, logically speaking, reject the traditional cladist view on the metaphysical question. In this section I will argue further that they *should* reject it. Monophyly is, I will argue, neither necessary nor sufficient for a group of organisms to count as a real group or kind.

The cladist metaphysical criterion is notoriously strict; too strict, according to many. There are arguably real groups that, because non-monophyletic, it does not count as real. I concur with this judgment. But I will argue that it is also too liberal: it counts too many groups as real. In short, some real groups are not monophyletic, and some monophyletic groups are not real. Since being monophyletic is neither necessary nor sufficient for being real, the criterion should be rejected.

3.1 *Questioning the necessity*

The most obvious sense in which the monophyly criterion of reality is too strict is that it rules out all ancestral, that is, non-monophyletic species. I discuss this in the following section (3.1.1). Setting species aside for the moment and focusing on higher taxa, it has seemed to many that in ruling out the reality of certain higher taxa counted as real by commonsense and received taxonomic theory—reptiles, fish, dinosaurs (minus the birds), great apes (minus humans) etc.—because paraphyletic, cladism is committed to the ‘absurd’ conclusion that ‘there is no such thing as a fish/reptile/dinosaur/ape...’ Whether or not this is indeed absurd, or just a somewhat surprising consequence of an otherwise sound taxonomic philosophy that we can and must learn to live with, the point I wish to make here is that it has been assumed that in adopting the cladist view of classification, and thus refusing to admit paraphyletic taxa, the cladist is thereby committed to rejecting the reality of non-monophyletic groups, as the classification question and metaphysics question have not been distinguished. If I am right that these questions are distinct, and accepting the cladist answer to the former does not entail accepting the monophyly criterion for the reality of groups, it follows that in refusing to accept reptiles, fish etc. as taxa, the cladist need not deny that they form real groups, and thus *need not* embrace the ‘absurd’ conclusions. For the conclusion follows from the rejection of non-monophyletic real groups, not the rejection of non-monophyletic taxa. As Sterelny and Griffiths note, the view that ‘there is no such thing as a reptile’ follows directly from the cladist *metaphysical* thesis—it follows from the claim that reptiles do not form a real group. One could it seems hold that there is no reptile taxon, yet hold that reptiles are a real group, and thus that there are reptiles, just as Sterelny and Griffiths appear to hold that there is no terrestrial mammal taxon (as they accept the cladist view on classification) but there is a terrestrial mammal real group (see Devitt 2011).

I do not here propose to offer a verdict on the reality of particular paraphyletic groups. In the final section I will suggest a criterion for reality that may be used to decide on such questions. My point here is simply that if we reject the strict cladist metaphysical view then we are not committed to denying the reality of taxa such as reptiles, fish and so on merely on the grounds of their non-monophyletic character. Whether these traditional taxa, or other paraphyletic groups, are real groups or kinds will depend on whether they satisfy the criteria I will outline in the final section; the point here is just that we are not *compelled* to rule them all out automatically just on the grounds that they are not monophyletic.

In this context it is worth considering Griffiths' suggestion in an earlier paper that 'reptiles' is example of reference failure, because the reptile taxon is paraphyletic, and thus there is no real division in nature corresponding to it (1994: 210).¹³ On the view I am defending it *might* be correct to say that there is reference failure here, but not because the group is paraphyletic. On my view paraphyletic groups *can be* real but often are not. Whether 'reptile' names a real group (and thus whether or not it refers) depends not on whether it is monophyletic or paraphyletic, but on whether it is explanatory (I will say more about this criterion in the final section). And it is worth noting that Griffith's position here—that all paraphyletic groups are unreal—conflicts with his and Sterelny's position (*ibid*) that some paraphyletic groups, such as terrestrial mammals, are real. Thus 'terrestrial mammal' presumably refers, despite referring to a paraphyletic group.

3.1.1 *Species and monophyly*

Species have always presented a problem for cladism, on both the classification and metaphysics fronts, given that to the extent that species may be ancestral to other species, they may fail to be monophyletic (Sober 2000: 166). Different species concepts will have different implications about when and why species may fail to be monophyletic. For instance, on Mayr's Biological Species Concept (BSC), which defines species in terms of interbreeding and reproductive isolation, one interbreeding population may give rise to another from which it is reproductively isolated. These would each count as separate species despite the parent species being paraphyletic (Ereshefsky 1998: 105–106). Cladists tend to adopt one or other of the various historical species concepts, either a version of Simpson's evolutionary species concept, according to which 'a species is a lineage evolving separately from others and with its own unitary evolutionary role and tendencies' (Wiley 1992), or a version of the phylogenetic species concept, according to which a species is a branch of the phylogenetic tree, beginning at a speciation (branching) point, and terminating either at another speciation point, or at the

¹³ See also Rieppel, who argues that 'Reptilia' doesn't designate a natural kind because it is not monophyletic. It is rather an 'artificial' kind (2005: 467).

extinction of the lineage.¹⁴ But even on these species concepts, species will, on the face of it, still be paraphyletic, if they have any descendants.¹⁵ Even if, as the phylogenetic species concept states, new species may not arise through phyletic evolution in a lineage without splitting, but may only arise through branching (subdivision of an existing lineage), it will still be the case that some species will be ancestral to others, and thus will be paraphyletic. Of course cladists are notoriously wary of the ancestor-descendant relation. But phylogenetic cladists do have to accept, as an ontological claim, that there are such things as ancestral species that give rise to daughter species. Their point is the purely epistemological (and reasonable) one that we can never *know* on the basis of the evidence which species have been ancestral to others.

The uncontroversial case in which admitting the existence of ancestral species conflicts with the cladist principles is where a species continues to exist after budding off a daughter species (as some cladists e.g. Wiley (1992), and others sympathetic to cladism e.g. Hull (1979), think can happen). The parent species will then be paraphyletic, and thus illegitimate: after the split, there will be organisms/populations in the parent species that are more closely related to (share a more recent common ancestor with) organisms/populations in the daughter species than they are to organisms/populations in the earlier phase of the parent species before the split, yet are being classified with the latter and not with the former (just as, in the case in which the stem species does go extinct when the lineage divides, so that species *a* gives rise to species *b* and *c*, *b* and *c* are grouped together in the cladogram apart from *a*: the group *a* and *b*, apart from *c*, would be paraphyletic). Yet even if we follow Hennig and other cladists in their view that a species always goes extinct when it splits, it will still seemingly be the case that the parent species will be paraphyletic, as we are excluding from it some of its descendants.¹⁶

Hennig originally intended his criterion of monophyly only to apply to supra-specific taxa (Ereshefsky 1998). Later cladists went to the opposite extreme and merely *assumed* species were monophyletic, which assumption underlies the popular definition of monophyly: a species along with all (and only) its descendants if it has any. On this view '(s)pecies are taken to be monophyletic *a priori*' (Brandon and Mishler 1987: 118). Subsequent cladists, such as Brandon and Mishler, urged that species need to be, as it were, *internally* monophyletic. After all, if a species comprises, say, three disjoint populations, and does not in-

¹⁴ There are several phylogenetic species concepts (Baum and Donoghue 1995; Wilkins 2009), but the differences between them are not important for our purposes.

¹⁵ This hasn't always been recognised, for instance Ereshefsky in his (1998) seems to suggest that there are no paraphyletic ancestral species on the phylogenetic species concept, as do other cladists: see below.

¹⁶ Ridley disagrees (1989). He suggests that in such a case, the species that goes out of existence at the point of branching counts as monophyletic. I criticise this view below.

clude the common ancestor of those populations, the species will be non-monophyletic even if terminal. Thus Brandon and Mishler suggested replacing the above definition of monophyly with the following definition: 'A monophyletic taxon is a group that contains all and only descendants of a common ancestor, originating in a single event' (118). The common ancestor here is thought to be an individual organism or local population (118-119). One consequence of this conception is that populations below the species level may be monophyletic, though the species is the least inclusive monophyletic *taxon*.¹⁷

This shift in perspective to a more fine-grained understanding of monophyly is well motivated. But the problem of ancestral species remains, as we shall see. In this section I will survey some attempts to reconcile species with the principles of monophyly.

Ridley (1989) accepts the cladist classification principle (that all and only monophyletic groups are taxa), but argues that all species satisfy it on the cladistic (phylogenetic) species concept. Other species concepts, such as the BSC, fail to satisfy it. The BSC allows paraphyletic taxa, because in the case when a species splits, with one branch diverging and the remaining branch remaining much the same, while the cladistic concept (as he understands it) says the unchanged species has become a new species at the branch point, the BSC says it remains the same species, as former and later segments could potentially interbreed (13). Ability to interbreed is not sufficient for conspecificity on the cladistic concept. On the cladistic concept, species are monophyletic in the sense that they are monophyletic *up to the next speciation event* (if there is one). All the descendants of the species are included in the taxon *so long as no speciation takes place*. This is a bit like saying my grandfather is alive because he was alive up to the point when he died. It's true that paraphyletic taxa are monophyletic if you ignore the branches that make them non-monophyletic.

Ridley's view is that only if the parent species continues to exist after budding off a daughter species does it count as paraphyletic; if it goes extinct at the point of branching it counts as monophyletic. 'The species before and after the split are different branches of the phylogenetic tree, and both branches are monophyletic.' (13) Again, this seems to involve an unmotivated revision of the standard understanding of monophyly: an ancestor along with *all* and only its descendants. Even if a species ceases to exist at the point at which it gives rise to descendant species, insofar as it has descendants, the taxon consisting of that species minus its descendants is paraphyletic.

Brandon and Mishler, in their influential (1987);¹⁸ similarly argue that species are monophyletic on their version of the phylogenetic spe-

¹⁷ If this is accepted, we would need to revise the cladist classification principle, since it is no longer the case that all monophyletic groups are taxa.

¹⁸ They follow Mishler and Donoghue (1982); see also Donoghue (1985) for similar position.

cies concept, according to which a species is ‘the least inclusive taxon into which organisms are grouped due to monophyly’. (Monophyly is their grouping criterion, while they adopt a pluralistic ranking criterion, to accommodate the plurality of evolutionary forces responsible for making species into coherent and separate lineages. Monophyly is only the grouping criterion because taxa other than species can be monophyletic; thus being monophyletic is necessary but not sufficient for being a species.) But ‘the least inclusive monophyletic group’ can only apply to species as terminal taxa. Ancestral species are, as we have seen, not monophyletic.

Brandon and Mishler attempt to get around this problem by denying that any species are ever ancestral. Only smaller units (e.g. organisms or populations) are ancestral to species. Their point seems to be that the full implications of the rejection of anagenetic speciation have not been understood, inasmuch as the idea of species being ancestral to other species has been retained in a cladogenetic setting. But with the acceptance of the idea of speciation by splitting, the idea of ancestral species can be rejected. This doesn’t appear to solve the problem of ancestral species however. Take the individual or population X that is considered the ‘ancestor’ of all members of monophyletic species S in Brandon and Mishler’s analysis. X did not spring into being from nowhere; it itself descended from ancestors. Those ancestors belonged to a different species, *ex hypothesi*. Call it S*. The members of S*, let’s suppose, all descended from a common ancestor, X*. So S* contains only descendants of X*. But S* does not contain *all* the descendants of X*, since it does not include the members of S. Thus S* is not monophyletic.

The theorists I’ve been discussing can only continue to uphold the cladist metaphysical and classification principles if they revise the definition of monophyly to include all phylogenetic species (species as understood on the phylogenetic species concept) by definition. Instead of defining a monophyletic group as ‘an ancestor and all and only its descendants’ we would have to define it as follows:

A taxon is monophyletic so long as it satisfies one of the following conditions:

1. It is a phylogenetic species
2. It consists of an ancestor plus all and only its descendants

So in the case of a stem species *a* budding off two terminal daughter species *b* and *c*, rather than there being three monophyletic groups as per usual—*b*, *c*, and *a+b+c*—there would be four: *a*, *b*, *c*, and *a+b+c*. Such a revision would appear *ad hoc*, if motivated in no other way than by a desire to maintain the cladist principles. The alternative is to accept that ancestral species are non-monophyletic, and revise the cladist principles accordingly.

Eldredge and Cracraft, in their classic text (1980), accept the point I have been urging against cladists such as Ridley, that ancestral spe-

cies cannot be monophyletic (90). They note that a strict application of cladistic principles would require all taxa to be terminal (as all taxa must include every descendant species in order to be monophyletic). Thus if we are to accept some ancestral taxa, cladist principles would need to be modified.

Eldredge and Cracraft are robust realists about species. Throughout their book they defend the view I have adverted to above, that species are ontologically real, discrete, objective, mind-independent units in nature (particular, concrete things, or individuals), while higher taxa are subjective and more or less arbitrary projections of our minds. Thus if, as they accept, ancestral species are non-monophyletic, we have here a clear counterexample to the cladist metaphysical principle: it's not the case that only monophyletic groups are real. (They are also implicitly rejecting the view that all monophyletic groups are real, in their view that higher taxa are conventional projections of our minds.)

At times however they appear to wish to continue to defend the traditional cladist metaphysical principle. So they claim elsewhere in the book (266) that non-monophyletic groups are 'non-existent', which would imply that ancestral species are non-existent, which directly contradicts their above-mentioned realism about all species. This illustrates the tension that exists in cladist thought with respect to this question. Cladists cannot say both that all species are objectively real, and that only monophyletic groups are real. One of these has to give way.

The best solution, I would suggest, is to reject monophyly as a necessary condition of reality. All phylogenetic species are real, including those that are paraphyletic, and thus it's not true that only monophyletic groups are real.

Christofferson (1995) accepts that ancestral species are not monophyletic and that this creates a *prima facie* problem for traditional cladism (446–447). His response is that there are fundamentally two (equally real and important) types of taxa, species and monophyletic higher taxa, and these belong to quite different ontological categories. Species are understood dynamically as evolving lineages (we take a *transformational* view of them), while monophyletic higher taxa are understood statically as hierarchically organised sets of taxa (we take the *taxic* view of them). 'Phylogenetic systematics involves integration of these two world views [the transformational and taxic] by recognition of two ontological kinds of taxa: species, which are continuous strings of ancestor-descendant populations ranked serially (the transformational approach), and monophyletic taxa, which are discontinuous taxa ranked hierarchically (the taxic approach)' (444). Thus species are exceptions to the strict cladist metaphysical principle.

It would seem to be an implication of Christofferson's view that no species, even terminal species, are ever monophyletic. Treating any species as monophyletic is akin to a category error. This is a return to Hennig's original view. I would argue that terminal species can be monophyletic if they satisfy Brandon and Mishler's conditions on mono-

phyly. But Christofferson is right (as against Brandon and Mishler and Ridley) that (a) there are ancestral species, and (b) they are non-monophyletic, and thus we need to revise the cladist metaphysical principle.

I have been focusing on the need to revise the cladist metaphysical principle to accommodate realism about species. But of course if ancestral species are non-monophyletic, they are also a counterexample to the cladist classification principle (all and only taxa are monophyletic). If ancestral species are taxa, then the cladist classification principle would need to be modified. The only other option would be to deny that ancestral species are taxa. This may seem like a radical proposal, but it is a straightforward implication of, for instance, the definition of species taxa advanced by Mishler and Donoghue: 'a species is the least inclusive taxon recognised in a classification, into which organisms are grouped because of evidence of monophyly' (1982), or that advanced by Mishler and Theriot: 'taxa are ranked as species because they are the smallest monophyletic groups deemed worthy of formal recognition' (2000, quoted in Wilkins 2009: 213). If these definitions are accepted, ancestral species are not species taxa. The only species taxa are terminal species (species that are either extant, or went extinct without speciating). This appears to have been Hennig's view (1966; see Richards 2016: 163). Hennig suggested there were no stem species taxa apart from the entire clades they gave rise to, that is, a stem species is identical to the entire clade it is the stem species for: 'in the phylogenetic system [the stem species] ... is equivalent to the totality of species in the group' (1966, quoted in Richards 2016: 163). Similarly, Mishler and Donoghue (1982) raise the possibility of peripheral isolate-type allopatric speciation, where the parent species would be paraphyletic (499). Their solution is that in such a case we should say that the parent species is not in fact a species at all. In other words, since species cannot be paraphyletic, there are no ancestral species. All species are either still living, or went extinct without branching.

I have suggested above however that ancestral species are real groups (and thus that the cladist metaphysical principle should be modified). The notion that ancestral species are real groups but are not taxa may seem strange, but one of the main themes of this paper is that the issue of metaphysics and the issue of classification should be kept distinct. I am suggesting that there are likely to be a wide range of real groups that are not monophyletic, so are not taxa, if we accept, as I think we should, the cladist classification principle. Sterelny and Griffiths' terrestrial mammals are an example. They are a real group on their criterion for reality, but do not count as a taxon on their cladist criterion of classification. Ancestral species would just be just another example, and do not seem to raise any special, further difficulties.

3.2 Questioning the sufficiency¹⁹

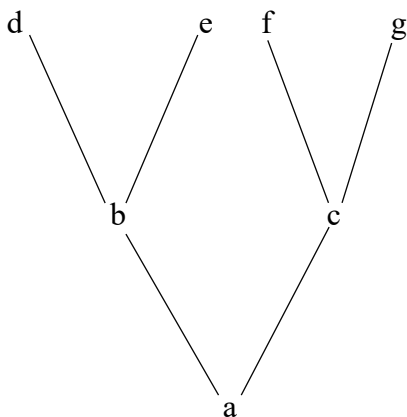
Whether or not it is too strict in ruling out some real groups, the monophyly criterion of reality is arguably too liberal. It holds that every monophyletic taxon is a real group. The first potential worry is that this commits one to the reality of a vast number of groups or kinds. If monophyly is a sufficient condition for the reality of groups we end up with a proliferation of real groups, organised hierarchically, that may seem metaphysically profligate. There is after all a separate monophyletic group for every species that has ever lived (assuming all species are monophyletic in Brandon and Mishler's sense): the group consisting of that species along with its descendants if it has any, or that species alone, if it doesn't.

Ridley (1993: 369-70) notes the huge number of evolutionary branching points in the history of life. Each represents a distinct monophyletic clade, so are all equally taxa for the cladist, but there are obviously far too many to all be given a Linnaean rank (see also Eldredge and Craft 1980: 221, Ereshefsky 1997: sect. 3). He argues that this doesn't matter because Linnaean ranks are subjective and conventional anyway, so in assigning them we can ignore lots of 'real' taxonomic levels. Each monophyletic clade is a taxon, but very few of them can or should be assigned a Linnaean rank.²⁰ Equally, one may argue, many of them should not be considered real groups or kinds.

To bring the question of ontological profligacy into sharper focus, consider three ontologies: according to the first, we ought to accept as real all phylogenetic species, and all monophyletic groups: call this the S+M ontology. According to the second, we ought to admit only species, not higher taxa, into our ontology: call this the SO ontology. According to the third, we ought to admit only monophyletic groups into our ontology (where ancestral species are ruled out as paraphyletic): call this the MO ontology. How do these ontologies score for ontological parsimony? Suppose a species *a* splits and gives rise to two species *b* and *c*, each of which splits and give rise to two species *d* and *e*, and *f* and *g*.

¹⁹ I focus in this paper mainly on the real groups/natural kinds interpretation of the sufficiency thesis, but it should be noted that treating all monophyletic taxa as (objectively existing) concrete individuals (not kinds) may be an alternative way of elaborating the thesis. Some defenders of the species-as-individuals thesis have argued that monophyletic higher taxa are individuals in much the same sense, i.e. chunks of the genealogical nexus. See Boyd (1999) for a critique.

²⁰ I agree with the widespread (though not universal) view that cladism requires the abandonment of the Linnaean ranking system (Ereshefsky 1997; Griffiths 1994; Richards 2016: 153) and its replacement by an alternative. The Linnaean system, even in its greatly expanded modern form, doesn't contain anywhere near enough ranks for all the monophyletic taxa in the tree of life to be given a Linnaean rank.



According to S+M, there are ten real groups or kinds here: a , b , c , d , e , f , g , $b+d+e$, $c+f+g$, and $a+b+c+d+e+f+g$. According to SO there are seven real groups: the seven species. According to MO, there are seven real groups— d , e , f , g , $b+d+e$, $c+f+g$, and $a+b+c+d+e+f+g$. So SO and MO are equally parsimonious, but S+M is less parsimonious than both. Perhaps then, other things being equal, SO and MO should be preferred to S+O.

This may not be considered a very serious worry. The appeal to parsimony here may be questioned, and in any case parsimony considerations will only count against those who accept S+M, not those who accept MO (assuming that SO is the only serious alternative), and I have suggested that MO, not S+M, is the appropriate ontology for adherents of the cladist metaphysical principle.

A more telling concern may be that the sufficiency of monophyly position conflicts with the widespread view (including among cladists) mentioned above, that species are real in a way that higher taxa are not. According to the cladist metaphysical principle, all monophyletic taxa are equally real. So a monophyletic higher taxon is just as real as a monophyletic species. This runs contrary to the views expressed by at least some cladists (e.g. Eldredge and Cracraft 1980: 249) concerning the reality of species vis-à-vis higher taxa. Although the conflict between these views has not always been recognised, *if*²¹ we think that species are real but higher taxa are not, we obviously have to reject monophyly as a sufficient condition of reality.²²

²¹ I am not endorsing this view, merely noting that if it is correct, it undermines the sufficiency view.

²² It may be tempting to assimilate this view to the pragmatic view about classification I mentioned above: that the erection of higher taxa is purely a matter of convention or convenience, not answering to facts about nature. In the case of the cladists who hold the view (such as Eldredge and Cracraft), this would be a mistake. These theorists are *cladists* after all, meaning minimally that they accept the claim that classifying by phylogeny, and thus erecting monophyletic higher taxa, is a more objective and thus a superior approach to classification than rival

So there may be reasons for thinking that many monophyletic groups are not real groups or kinds, and thus that monophyly fails as a sufficient condition for reality. But further, once we have distinguished the classification and metaphysical questions, and noted that the (persuasive) arguments for the cladist classification principle do not obviously carry over to the cladist metaphysical principle, arguably we are left with few positive arguments *for* the view that all monophyletic groups are real. It is worth comparing the debate over the reality of species. A number of arguments have been offered for the view that species are objectively real, including the fact that anthropological evidence seems to show that many different kinds of human societies and cultures identify the same species taxa in nature (Atran 1999); the fact that species realism follows from certain well-supported macroevolutionary theses, such as Punctuated Equilibrium (Gould and Eldredge 1972, Gould 2002); and the fact (sometimes connected to the previous point) that species have a certain ontological status—they are concrete, cohesive, spatiotemporally bounded individuals, and thus are real, objective, discrete objects (Eldredge and Cracraft 1980).²³ Whatever we think about the cogency of such arguments, they do not appear to carry over to monophyletic groups in general, which should not be surprising, since the thrust of such arguments tends to be that species are ontologically special: they are real units or agents, in a way that higher taxa, whether monophyletic or not, are not (Mishler and Donoghue 1982: 491).

We can however interpret Griffiths' defence of the value of cladistic classifications (1994: 216–217) as an argument for the sufficiency view. Cladistic classifications (of both organisms and traits), he argues, are more informative than functional-adaptive classifications, because they are more predictively and explanatorily useful. If we know that a species belongs to a certain clade, we can predict more about its traits than we can on the basis on knowing that it occupies a certain ecological

approaches that make use of criteria other than ancestry. It is more plausible to interpret the view as a version of the one I am defending in this paper: classification by strict monophyly is objective, in that it respects real, objective divisions ('joints') in nature (the branching order of the tree of life), hence cladism with respect to classification is justified; however the higher taxa erected by such classifications (unlike species) may fall short of qualifying as real groups or kinds. Of course an alternative interpretation is that these thinkers are simply confused, not realising it is not coherent to endorse cladism while rejecting the reality of monophyletic higher taxa. I reject this interpretation since I do not regard this position as incoherent.

²³ We tend to be unreflective realists about particular, concrete, individual objects—tables, trees, horses, etc. So if species are, as the species-as-individuals (SAI) view claims, concrete, particular individuals, it may be hard to resist species-realism. Of course SAI may not be *necessary* for species-realism: one could hold that species are natural kinds (not individuals), for instance, and still be a species-realist. But SAI may still be *sufficient* for species-realism (or at least, strongly support it). Thank you to an anonymous referee for urging me to clarify the connection between SAI and species-realism.

niche. Species may share *some* superficial similarities with unrelated species that occupy the same niche, as a result of evolutionary convergence; but they share a great deal more similarities with other species in their clade, as a result of common ancestry. 'Kiwis owe more of their characteristics to their descent from the common ancestor of birds (and, more recently, of the New Zealand rattites) than to adaptation to their current role as nocturnal, forest floor omnivores' (216). Griffiths' argument might be seen as supporting the view that all monophyletic clades are real kinds in the following way. Fundamentally, natural kinds support inductive inference, explanation, prediction, and generalisation. Knowing that an organism belongs to a particular monophyletic clade allows us to predict and explain a large number of its characters. Hence all clades are natural kinds (albeit of a historical nature).

One problem with the line of argument I'm attributing to Griffiths is that in its focus on relations of similarity, it may give too much ground to pheneticism. So Griffiths, following Fink, notes that crocs and birds share important, deep similarities (especially behavioural), as a result of their being closely related (216). The standard way of thinking about the relationship between similarity and phylogeny in this case is that on phenetic criteria, crocs would be grouped with lizards apart from birds (due to the divergence of the birds), while on phylogenetic criteria, crocs would be grouped with birds apart from lizards. But Griffiths is suggesting (I take it) that this is superficial: it may be that even on phenetic grounds of similarity of form and function (including behaviour), a good case could be made for grouping crocs with birds apart from the other 'reptiles'. (Ridley (1986: 4–5) makes the parallel point about convergence: on a superficial interpretation, barnacles would be grouped with limpits apart from crabs (to whom they are more closely related as crustaceans) by pheneticists due to morphological convergence; but a closer study of the morphology of barnacles may well find that they more closely resemble crabs than limpits, such that the phenetic and cladistic classifications would agree with one another in this case.)

But the cladist holds that we should not go down the path of similarity at all: *even if* crocs share more similarities with lizards than they do with birds, they should still be grouped with birds because they share a more recent common ancestor. The similarity justification of cladistic classifications seems inherently risky, in its assumption that phylogenetic and phenetic classifications will tend to line up. What happens if they don't? The whole motivation for cladism was precisely that arguments about similarity are irresolvable: no doubt crocs do share many interesting similarities with birds due to common descent (synapomorphies for the bird-croc clade); they also share many similarities with other 'reptiles' due to common descent (synapomorphies for the reptile-bird clade). Which set of similarities is more important for classification? Of course, in the context of crocs, birds and lizards, the shared characters of crocs and birds are synapomorphies, while the

shared characters of crocs and lizards are symplesiomorphies. But this presupposes the cladist framework where the reconstruction of phylogeny is the goal. For pheneticists focusing on shared characters, with no interest in phylogeny, all shared characters are equivalent, and the question whether crocs are overall ‘more similar to’ lizards or birds may have no objective answer.

The second point I would make here is that there are challenges facing any attempt to vindicate the idea of clades as natural kinds in terms of the traditional notion of kinds as sets of similar entities that support induction and explanation, and are defined by an essence that explains why the members of the kind possess the features they do. As Griffiths notes, clades are fundamentally historical entities. If they are kinds defined by an essence, it would be a historical essence, presumably the clade’s evolutionary origin in a common ancestor (Rieppel (2005) explicitly endorses this view). To count as the essence of a clade on the standard understanding of essences, this ancestry would need to be causally responsible for, and help to explain, the traits of the organisms in the clade. This would be the clade-level analogue of the historical essence view about species defended by Griffiths (1999), LaPorte (2004) and others. Even when applied to species, however, the historical essence view faces serious objections. Okasha, for example, has argued that an organism’s ancestry (and indeed any other relational properties, such as ability to interbreed with other members of the species, that might be candidates for the species essence) does not cause, or help to explain, the organism’s morphological traits (2002: 203–204).²⁴ ‘...the causal explanation of why an organism has the particular morphological traits it does will cite its genotype and its developmental environment ... its belonging to (a particular chunk of the genealogical nexus) is not the explanation—or at least not the proximal explanation—of why it has the morphological traits that it does’ (204). Okasha is not in fact rejecting the historical essence view: he thinks relational properties such as ancestry can count as species essences even if they don’t cause or help to explain the traits of organisms. But most defenders of the historical essence view of species or clades do accept the traditional requirement that essences play this causal and explanatory role. If we accept the requirement, and if Okasha is right that a species’ ancestry does not satisfy it, it follows that that ancestry cannot be the essence of a species. If that is true for species, it is just as true (if not more true) for clades.

A further problem for the historical essence account of clades is presented by Pedroso (2012; see also 2014). He notes that the main argument for the historical essence view is that it is required by cladism. That is, it follows from cladism that the essence of a biological taxon is its ancestry in the sense that if taxon X is the common ancestor of the

²⁴ See Nanay (2011) for a more metaphysical argument against the view that species essences cause and/or explain the features of organisms.

members of clade C in the actual world, then X is the common ancestor of the members of C in every possible world in which C exists. It is not possible to be a member of C and not to have descended from X. But, Pedroso argues, this does not follow from cladism. It is consistent with cladism that there are possible worlds in which C exists but its members do not have X as their most recent common ancestor. All that is required by cladism is that C be a monophyletic clade in every possible world. The common ancestor of the members of C can vary across worlds. Cladism entails only that the *cladogram* true of the clade in the actual world is true of the clade in all possible worlds. But of course the same cladogram is consistent with multiple incompatible phylogenetic *trees*, specifying different ancestors for the members of the clade. Thus it is not a necessary truth that some Y is a member of C just in case Y descends from X. Historical essentialism fails.

Pedroso, like Okasha, has presented serious problems for the historical essence approach to justifying the sufficiency thesis. In particular, in line with the argument of this paper, Pedroso has shown that one may be a cladist about classification without accepting (at least the historical essence version of) the sufficiency thesis. Of course, this does not show that there might not be ways of defending the sufficiency thesis other than that associated with the natural-kinds-defined-by-historical-essences view. But I am not aware of any plausible candidates.²⁵

We have seen that one motivation for conflating the metaphysics and classification questions is the idea that both taxonomy and the metaphysics of kinds aim to ‘carve nature at the joints’. There is

²⁵ It has been suggested by some theorists (e.g. Rieppel 2005) that the homeostatic property cluster (HPC) account of natural kinds associated with Richard Boyd (1991, 1999) (possibly in connection with the historical essence account) can be applied to taxa, including higher taxa, in a way that would justify the sufficiency (and possibly the necessity) thesis. Rieppel suggests that monophyletic taxa are HPC natural kinds (the sufficiency thesis), and that nonmonophyletic taxa are ‘artificial’ (the necessity thesis). I do not have the space to consider in detail HPC theory and its relation to monophyly; suffice it to say that it is questionable whether HPC theory is compatible with cladism. Ereshefsky (and his co-thinkers) have been arguing for a number of years that while cladism classifies by ancestry and genealogy irrespective of similarity, HPC kinds are ultimately similarity-based kinds, with the result that cladistic kinds will not always map onto HPC kinds (see Ereshefsky 2010, Ereshefsky and Matthen 2005, Ereshefsky and Reydon 2015). If that is correct, HPC theory will not be compatible with the sufficiency or necessity theses. Indeed this is how Boyd sees matters. He argues that some HPC kinds are paraphyletic and some are polyphyletic (2010: 693); and he suggests that to be a real kind it’s not enough that a higher taxon be monophyletic—it has to satisfy other conditions as well (to do with his ‘accommodation thesis’). Thus he rejects the necessity and sufficiency theses, though on different grounds from those presented here. Boyd’s views about kinds, monophyly, and higher taxa are complex and subtle, and I can’t hope to do justice to them here. But the following upshot of his argument seems in any case highly congenial to the line of reasoning I have been pursuing: ‘We need not think of monophyletic groups as occupying some especially privileged ... position relative to other natural kinds in evolutionary biology in order to insist that higher taxa must be monophyletic’ (2010: 694).

no question that monophyly, and the objective order of evolutionary branching, represent real ‘joints’ in the natural world and its history. That there is an objective fact about the order of branching in the tree of life, and hence about evolutionary relationships, is the main argument supporting phylogenetic systematics (an argument I accept). But it does not follow, I have urged, that taxa formed on this basis are necessarily real groups or kinds. My rejection of monophyly as a sufficient condition for real groups or kinds can be understood as the claim that the kind of joint-carving that the construction of monophyletic taxa exemplifies is not of itself sufficient for carving up organisms into real groups or kinds. I do not claim that the cladist metaphysical principle does not carve at joints. I claim that carving at joints in the minimal sense is not sufficient for identifying real kinds. Other conditions must be satisfied.²⁶

As an analogy, consider Kitcher’s discussion of real kinds in astronomy (1992: 105). Kitcher, in his discussion of shifting ‘reference potentials’ of theoretical terms in science, highlights the ways in which the term ‘planet’ has shifted its reference throughout history; at one time referring to the known planets of our solar system excluding the earth; later referring to all the planets of our solar system including the earth; and finally referring to all the planets orbiting all the stars in the universe. He suggests that in the first case (reference to planets of our solar system excluding the earth) the term did not pick out a natural kind, but in the two subsequent cases it did. Thus, there is a natural kind comprising all and only the planets of our solar system. But this is, I’d suggest, implausible. There is certainly an objective ‘joint’ of a sort here—an objective division in nature—and ‘planet’ as referring to all and only the planets of our solar system carves at this joint. But despite this, many, I am assuming, would hesitate to regard the set of objects thus designated as a genuine natural kind (as opposed to the set of all planets of all stars, which has a stronger claim to constituting a natural kind). The predictive and explanatory value of the kind term ‘planet’ used in this restrictive sense is very limited indeed, and there are presumably no interesting laws true of all and only the objects picked out by it. Thus, a term can carve at a natural joint without picking out a natural kind.

²⁶ Here I follow Bird (2018), who distinguishes natural *divisions* in nature from natural *kinds*. He notes that green things are naturally similar to one another, such that there is a natural division of the world into green and non-green things, but green things do not form a natural kind. And he suggests we can imagine a world in which there are natural divisions but no natural kinds. One could thus be a weak realist about natural divisions without committing to the reality of natural kinds. I suggest that if our concepts correspond to natural divisions, they ‘carve at the joints’. But only some natural divisions correspond to natural kinds. Thus, I claim that there is a natural division of the tree of life into monophyletic clades, but that this is not sufficient for those clades to count as natural kinds. Thank you to an anonymous referee for encouraging me to be clearer on this issue.

One may be tempted then to retreat to the claim that carving at the joints is necessary, but not sufficient, for picking out real kinds. That may be true, but I don't find any support here for the necessity of monophyly thesis, since there is no reason to suppose that monophyly is the only relevant 'joint' at which to carve up organisms into real kinds. If, say, 'predator', picks out a real kind then it carves at a joint: just not the ancestry joint (see below).

I conclude that since (a) there are no very convincing arguments for monophyly as a sufficient condition of reality, and (b) there are some good arguments against it, we should reject monophyly as a sufficient condition of reality. In the previous section I argued it is not necessary either. To paraphrase Dupre,²⁷ monophyly makes good sense for classification; it is something of a disaster for metaphysics.

4. *Beyond monophyly*

So monophyly does not appear to be the right criterion for determining the reality of groups and kinds. If not monophyly, what should be our criterion? A clue to this can be found by considering again Sterelny and Griffiths' suggestions about paraphyletic groups.

Sterelny and Griffiths' discussion makes clear the differing motivations for, and differing status of, the compromise classification view, and the compromise metaphysics view. With respect to the former, evolutionary taxonomists have wanted to allow paraphyletic taxa in large part because of morphological considerations. It is the great morphological dissimilarity of birds and reptiles, due to divergence,²⁸ that motivates the desire to keep Reptilia as a respectable higher taxon, and to elevate the birds to the same rank as the reptiles. And it is the inability of evolutionary taxonomy to consistently and non-arbitrarily apply this morphological criterion that ultimately undermines it, according to Sterelny and Griffiths and many others.

²⁷ 'Strict monophyly is an obvious desideratum from the point of view of mapping evolution. But from the point of view of classification it is something of a disaster' (2002: 431). Dupre, unlike me, is of course rejecting the cladist classification principle.

²⁸ In fairness, evolutionary taxonomists have not appealed merely to morphological criteria to justify their taxonomic decisions. They have also elaborated the concept of an adaptive 'grade': reptiles, mammals and birds are legitimate taxa of the same rank (traditional classes of chordates) because they possess different integrated adaptive complexes—they are each characterised by certain sets of adaptive innovations. Reptiles possess a certain suite of characters adapting them to a certain broad niche, as do mammals, and birds (Ridley 1986: 32-33; Brysse 2008: 305). So we have adaptive, not purely phenotypic, divergence and differentiation. The concept of adaptive grades has been criticised by cladists as being vulnerable to the same problems of subjectivity and arbitrariness as the purely morphological criteria (Ridley 1986, 33). Whether a putative taxon has evolved a sufficiently novel suite of adaptive innovations to count as a new 'grade' is not something that may be determined using objective criteria. The emphasis on adaptation in the notion of a grade has also been criticised by anti-adaptationists.

With respect to the metaphysical question on the other hand, the considerations are quite different. The reason Sterelny and Griffiths give for keeping paraphyletic groups is not primarily morphological. If the non-marine mammals constitute a real group or kind it is not primarily because the marine mammals have diverged morphologically from their non-marine ancestors and cousins, with the latter retaining a suite of features uniting them into a coherent higher taxon. Rather, it has to do with whether there are respectable evolutionary hypotheses about the non-marine mammals. It has to do with their role in evolutionary explanations. Thus we can frame an alternative *explanatory* criterion for the reality of groups or kinds: groups or kinds are real to the extent that positing them does important explanatory work for scientists.²⁹

Devitt (2011) has defended a similar explanatory criterion for biological natural kinds, focusing on whether an entity's being a member of a putative kind is explanatory of the features of the entity. But he suggests that the question of realism—whether certain kinds exist objectively—has been conflated with the question of which kinds are *natural* kinds. '...the non-natural is being confusingly described as the non-real.' (165) On the realism question, reptiles obviously exist, he argues: the reptile kind is clearly a real kind that exists objectively. The interesting question is whether it is a *natural* kind: this depends on whether it is an *explanatorily significant* kind. Being a reptile may, he says, be like being a cousin: cousins exist, but being a cousin is not explanatorily significant. Against Devitt, I agree with Griffiths and others that *if* 'reptile' does not name a natural (explanatory) kind, then it does not name a kind at all: there is no reptile kind and reptiles do not exist. 'Reptile' would be non-referring. The appropriate analogy is not with 'cousin', but with 'witch', or 'phlogiston'. The latter are posits of false theories: when we reject the theories, we reject the existence of the kinds posited by the theories, and declare the putative kind terms non-referring. 'Witch' does not refer to a non-explanatory but real kind—it doesn't refer to a kind at all (which is not to say, of course, that the women who this term was applied to did not exist); 'phlogiston' does not refer to a non-explanatory substance—it doesn't refer to a substance at all. 'Reptile' is theory-laden in just the way that 'witch' and 'phlogiston' are; if the theories that treat the reptile kind

²⁹ This is a version of the Quinean explanatory criterion for ontology, which says that we should be ontologically committed to the entities the positing of which is required for our best scientific (and perhaps philosophical) explanations, or those that enhance the explanatory power of our theories. As in the literature on the broader Quinean criterion, the notions of 'explanation', 'explanatory power', and 'best' in 'best explanations' will here be assumed to be sufficiently intuitively clear. But for a summary of different accounts of the nature of scientific explanation see Woodward and Ross (2021); and for a useful discussion of what makes for a good inference-to-the-best-explanation, in particular in biology, see Lewens (2007: ch. 4). Thank you to an anonymous referee for suggesting I clarify this point.

as an explanatorily significant natural kind are false, there are no reptiles.³⁰ Thus, while I agree with Devitt about the explanatory criterion for natural kinds, unlike Devitt I take this to be a criterion for reality, not just naturalness.

On this way of looking at it, Sterelny and Griffiths' sympathy for the compromise metaphysical view, but lack of sympathy for the compromise classification view, is intelligible. While the latter involves a 'mixed' criterion that attempts to do justice to both similarity and relatedness in classification, and as such cannot avoid subjectivity and arbitrariness with respect to the aims of classification, the former is an application of a quite straightforward explanatory criterion of naturalness and reality. The criteria have different statuses, so it is not surprising that the compromise paraphyletic-friendly positions they give rise to inherit these different statuses.³¹

This is relevant when considering the following natural response to my view. If we think there are paraphyletic real groups or kinds, then why not allow paraphyletic higher taxa corresponding to those kinds? Conversely, if we are rejecting paraphyletic taxa, how can we allow paraphyletic real groups? The response to this is that there are persuasive arguments against allowing paraphyletic taxa, but that these don't carry over to paraphyletic real groups/kinds. As we have seen, paraphyletic higher taxa (at least, in the evolutionary taxonomy tradition) can only be justified on phenetic grounds of similarity and dissimilarity.³² And such grounds do not provide for objective classifications. Only strict monophyly, corresponding to the objective order of branching of the tree of life, can provide objective classifications.³³ But

³⁰ I do however agree with Devitt that it is not obvious that such theories are false, i.e. not obvious that paraphyletic kinds such as Reptilia are not explanatorily significant kinds.

³¹ Another way of putting this is that the evolutionary taxonomy position on classification involves a compromise with phenetics, whereas the explanatory argument for allowing paraphyletic groups does not. (So really it's wrong to call the compromise metaphysical view a *compromise* view.)

³² Paraphyletic ancestral species are a somewhat different case.

³³ That paraphyletic taxa should be rejected is common ground among cladists, but there has not always been sufficient clarity about *why* they should be rejected. For instance, Eldredge and Cracraft (1980) argue that the problem with paraphyletic taxa is that they are 'not-A' groups, i.e. groups defined by the *lack* of some property or set of properties. They suggest (a) that not-A groups are less natural than A groups (defined by possession of positive properties); (b) that eliminating them has been important in making progress in systematics; and (c) that cladism is the natural culmination of this tendency. Only A groups, i.e. monophyletic groups, should be allowed in a classification. The problem with this is that A groups are defined phenetically: by possession of certain defining (essential) properties. If cladistic groups are not *defined* by (but rather are identified using) synapomorphies (Ridley 1986) then being monophyletic is not sufficient for being an A group (one whose members all possess the defining property); and if A groups can be phenetic (not phylogenetic) groups, then being monophyletic is also not necessary for being an A group. Eldredge and Cracraft suggest (164) that reptiles, fish etc. are illegitimate

this argument does not apply to the paraphyletic real groups/kinds my analysis allows, because these are not identified using phenetic criteria. They are identified using explanatory criteria.

The explanatory criterion is more general than the criteria that define the different schools of classification, in that it says nothing about either similarity or evolutionary relatedness. It says that we should be ontologically committed to all and only those groups that feature in in well-confirmed scientific explanations and hypotheses. It does not say ‘and these must be groups of organisms that form a coherent evolutionary unit (i.e. are monophyletic)’ or ‘and these must be groups of organisms that are united by similarity’. No doubt often the groups that satisfy the former, general, condition will also satisfy one or both of the more specific conditions. But they need not. Groups that satisfy the general condition may not be defined by relations of similarity, and may not be monophyletic.

Arguably they need not even be paraphyletic. There is no reason in principle why the explanatory criterion could not certify the reality of some polyphyletic groups. It is generally agreed that polyphyletic groups defined phenetically (merely in terms of shared characters)—creatures with wings; creatures with eyes etc.—are not legitimate taxa, and do not form real kinds. That they don’t form real kinds is supported by the explanatory criterion: there are no interesting biological hypotheses concerning these ‘kinds’. They play no role in biological explanations. Other polyphyletic groups may have a greater claim to being real kinds however (even if they are not legitimate taxa in the context of systematics), for example ecological kinds such as ‘predator’ (Wilson et al. 2007: 194–5) or ‘parasite’. In ecology ‘predator’ has real biological significance, appears to play an essential role in ecological explanations, and so on.³⁴ It plausibly count as a real kind on the ex-

because they are not-A groups. But this just shouldn’t be the issue from a cladist point of view: even if they *were* defined by a particular (positive) property or set of properties, so counted as A-groups, they would still be illegitimate because paraphyletic. The cladist ought to insist that the whole question of possession of (intrinsic) properties, and thus the issue of positive vs. negative properties, is a red herring. The sole issue for classification is common ancestry and monophyly. It is this that makes birds and mammals, but not fish and reptiles, legitimate taxa, not any issue to do with A vs. not-A groups.

³⁴ ‘...biologists see [categories such as ‘predator’] as corresponding to kinds because of their explanatory and predictive value. Individual predators are predators not in virtue of being integrated parts in a larger individual, but in virtue of certain intrinsic and relational properties that they tend to share and which underwrite certain explanations, predictions, and generalisations...’ (Wilson et al 2007: 195. See also Devitt 2011. However see Griffiths (1994) for reasons to be sceptical about the value of ‘purely’ functional/ecological categories such as ‘predator’. All useful functional categories, he suggests, are historically constrained, and historically constrained functional kinds can be paraphyletic, but not polyphyletic; 218). Wilson et al are here arguing that some real kinds in biology are not individuals, but their point also supports my claim that some real kinds of organisms in biology are not

planatory approach.³⁵ Griffiths notes that generalisations about such ecological kinds occur at the functional-adaptive level of biological explanation, in which organisms and traits are classified in terms of their adaptive or ecological role (Griffiths 1994: 215–217). Such (abstract) functional roles are multiply realised by underlying cladistic kinds (the kind ‘predator’ is realised by many different lineages within different clades).³⁶ On this picture there are polyphyletic real kinds, identified at the functional-adaptive level, but the taxa that realise those kinds are monophyletic clades, identified at the historical-phylogenetic level.³⁷

Thus, as with the above paraphyletic examples, on the explanatory criterion such putative ecological (polyphyletic) kinds are not ruled out simply by virtue of being non-monophyletic. It is a major virtue of that approach that it is flexible enough to potentially accommodate a wide range of biological kinds quantified over by workers in different areas within biological science.

Here the difference between the classification question and the metaphysical question is especially clear. The explanatory argument for admitting polyphyletic kinds such as ‘predator’ is not at all impugned by the widely accepted (even by evolutionary taxonomists) and persuasive arguments for rejecting polyphyletic taxa. There is clearly no predator taxon, but it is plausible that there is a predator real kind.

The explanatory criterion provides, I suggest, a sounder criterion for identifying real groups or kinds of organisms than does the cladist metaphysical principle of the necessity and sufficiency of monophyly. Of course, these may not *necessarily* have been in conflict: it might have turned out that in applying the explanatory criterion, the necessity and sufficiency principles were vindicated. Indeed this is likely to be the response from supporters of the cladist metaphysical principle to my opposing to it the explanatory criterion—that these are not in competition, that rather the cladist principle is *justified by* the prior and

monophyletic (given the close relationship between cladism and the species-as-individuals thesis, this should not be surprising).

³⁵ In this respect Sterelny and Griffiths’ defence of the reality of paraphyletic groups proves too much (for their liking). They appear to want to allow paraphyletic but not polyphyletic kinds. But the criterion they appeal to—whether there are interesting biological hypotheses about the group in question—would appear, as we have seen, to certify the reality of at least some polyphyletic groups. That is, the compromise metaphysical view ((5) above) is unstable. Once you recognise paraphyletic groups on those grounds, you also have to recognise polyphyletic groups. There is no argument *of this sort* to show paraphyletic groups can be admitted that does not also show polyphyletic groups can be admitted.

³⁶ Griffiths presents this two-level picture and acknowledges its attractiveness but goes on to criticise it somewhat later in the paper.

³⁷ Consider, as another example, Hull’s suggestion (1988: 215) that ‘cosmopolitan species’ is a candidate for a natural kind that may feature in laws of nature, presumably by virtue of its explanatory credentials. If this is a real kind it is a polyphyletic one that is realised by cladistic taxa (i.e. species) but is not itself a taxon.

more general explanatory principle (or something like it). But it should be clear why I hold that in fact they do conflict. It is very plausible that positing paraphyletic ancestral species, and polyphyletic ecological kinds, is explanatorily valuable. And I have challenged the claim, implicit in Griffiths and others' work, that all monophyletic groups are explanatorily significant kinds. The explanatory principle undermines, rather than supports, the necessity and the sufficiency theses.

It is important to see, firstly, that the explanatory criterion I have proposed is a criterion for determining only which groups of organisms are real, it is not intended to be an account of the nature of natural kinds in biology generally, much less a theory of natural kinds in general. Secondly, it is intended to be a *criterion* for determining whether certain groups of organisms are real groups or kinds, not a complete account of the metaphysics or epistemology of these kinds, or natural kinds in general. There has been much philosophical work recently devoted to the question of whether or not natural kinds are mind-independent, whether they should be defined in metaphysical or epistemic terms, and if the former, what their metaphysical status is—whether they are reducible to sets, universals, or something else, or are *sui generis* entities (for important recent contributions see Bird 2018 and Franklin-Hall 2015).

These are interesting and important questions but I do not need to take a stand on them. In particular, the fact that their role in biological explanations is our criterion, that is, best (perhaps only) evidence, for the reality or naturalness of groups of organisms does not entail that their explanatory role or value is *constitutive* of their naturalness, in a way that would suggest an anti-realist or epistemic account of natural kinds, such as those defended by (on some interpretations) Boyd (1991, 1999), Magnus (2012, 2014) and Ereshefsky and Reydon (2015). Groups of organisms may *be* natural kinds in virtue of entirely mind-independent facts, yet it might still be the case that we only *know* they are natural kinds in virtue of their role in scientific explanations. As far as I can see the explanatory criterion I have defended is consistent with all (or at least most) of these more abstract theories of the fundamental nature of natural kinds.

Conclusion

It has been assumed that if one accepts cladism with respect to classification, one must accept what I have called the cladist metaphysical thesis, the claim that all and only real groups or kinds of organisms are monophyletic. In section 2 I argued that this is not the case, that the classification and metaphysics questions are logically distinct, such that cladists (with respect to classification) *can* reject the cladist metaphysical thesis. In section 3 I argued that the cladist metaphysical thesis is implausible: there are real groups or kinds that are not monophyletic, and plausibly monophyletic groups that are not real or natural.

Thus cladists with respect to classification (and others) *should* reject the cladist metaphysical thesis. In section 4 I explicitly endorsed an alternative and superior explanatory criterion for the reality of groups or kinds (implicit in my earlier criticisms of the cladist metaphysical thesis). This need not amount however to a rejection of cladism in general, so long the metaphysical question is sharply distinguished from the question of classification. Cladistic classification may survive the rejection of cladistic metaphysics.

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