The Minimal Role of the Higher Categories in Biology

*Biology and Philosophy* 38:2 (2023)

<https://doi.org/10.1007/s10539-022-09888-5>

Michael Devitt

**1. Skepticism about Categories**

Biologists talk of taxa like *Vulpes vulpes* (red foxes), *Vulpes* (“true foxes”), and *Canidae* (a group including foxes, dogs, jackals, and other “dog-like” animals). And they talk of the Linnaean categories (ranks) that such taxa are said to exemplify; in these cases, **Species**, **Genus**, and **Family**, respectively. For the most part, biologists are not skeptical of the *taxa*. But there is widespread skepticism about the *categories*, particularly about the “higher” ones, those above **Species**. I shall argue that the skepticism is exaggerated.

Let us start with doubts about the most secure category, **Species**. It is a familiar fact that there are many different species “concepts”, many different theories (definitions) of what it is for a taxon to be a species. Controversy rages over which is right; this is “one of the thorniest issues in theoretical biology” (Kitcher 2003, p. xii). Some doubt that any one is objectively right: “There is no available fully objective ranking criterion for species” (Baum 2009: 76). The controversy has led some (Mishler and Donoghue 1982; Kitcher 1984; Sterelny and Griffiths 1999: 194-201) to the view that “there is no unique factor common to all species” (Ereshefsky 1992, p. xv). So, we should reject the view that there is *just one* good species concept. We should reject “species monism” and adopt “species pluralism”, the view that there are *many* (Kitcher 1984; Ereshefsky 1998; Dupré 1999). This puts the species category in trouble. Indeed, Marc Ereshefsky claims, “The species category does not exist” (1998, p. 113). Rather than saying this, or that the category is not “real”, it would be better to express these doubts about the category, I argue, by simply “denying the *explanatory significance* of kinds being species” (2011, p. 161). But denying this is not denying that there are explanatory significant categories at the level of species in the hierarchy of taxa. Consider Ereshefsky, for example. Given his view that there are several true species concepts, he argues that biology should advert to “biospecies”, “phylospecies”, and “ecospecies” (1998, p. 117). These are equally good categories at the species level. Although it is explanatorily significant for a taxon to be in one of these categories, it is not for it to be a “species”, which is a mere disjunction of the significant categories with no “distinctive commonality” (p. 115). So, in Ereshefsky’s terminology, the species category does not exist but these categories do.

In sum, pluralists who reject the category **Species**, nonetheless leave us with several substitute categories. And, of course, many biologists do not reject the category. So, one way or another, talk of species categories is generally regarded as acceptable. Not so, talk of the higher categories.

Ereshefsky rightly points out that if a certain category is to be acceptable, the taxa that fall under that category should be “comparable”, and he draws attention to reasons for thinking that this condition is not met (1999, p. 299). Brent Mishler claims that “practicing systematists know that groups given the same rank across biology are not comparable in any way” (1999, pp. 310-11). In a lengthy critique of the Linnaean hierarchy, Ereshefsky mentions disagreements about the rank of certain taxa (2001: 226); and the drive to introduce more ranks, leading to a hierarchy in flux (p. 215). He cites evidence “that Darwin doubted the existence of the Linnaean categories” (2001, p. 231). In their helpful textbook, *Sex and Death* (1999), Kim Sterelny and Paul Griffiths are also negative about the higher categories. “[W]hat is a genus? A family?” On such issues, “[s]ystematics has gone through a long period of controversy, some of it extraordinarily bitter” (p. 194). They describe the various taxonomies, phenetic, evolutionary, and cladistic, pointing out that the evolutionary and the phenetic taxonomies may group lineages into genera differently (p. 196). They think that cladism “presents the best view of systematics” (p. 200), but from that perspective, the higher ranks “make little sense”:

[cladists] do not think there will be any robust answer to the questions when should we call a monophyletic group of species a genus? a family? an order? Only monophyletic groups should be called anything, for only they are well-defined chunks of the tree. But only silence greets the question are the chimps plus humans a genus? It has long been received wisdom in taxonomy that there is something arbitrary about taxonomic classification above the species. These decisions are judgment calls. So cladists show only a somewhat more extreme version of a skepticism that has long existed. (p. 201)

Ereshefsky (1999, 2001) and Mishler (1999) argue persuasively that the Linnaean hierarchy of categories should be abandoned.

In light of the controversy over the species category, it is helpful to describe this skepticism about the higher categories in terms of “concepts”, in terms of theories (definitions) of what it is for a taxon to be in a certain category. Thus, whereas we have “robust” concepts for species, the cladists doubt that there will be any such concepts for the higher categories. Biologists generally have not been convinced by any concepts that have been proposed. So, there is widespread skepticism in biology about the higher categories.[[1]](#footnote-1)

**2. Talk about Categories**

But, despite this skepticism, *biologists keep talking about the higher categories.* Indeed, it is hard to exaggerate just how much biologists talk about them. Consider, for example, the International Code of Zoological Nomenclature (1999), surely something very important to biological theorizing. It is full of talk of genera, families, etc., starting with this:

The Code regulates the names of taxa in the family group, genus group, and species group. Articles [1](https://code.iczn.org/zoological-nomenclature/article-1-definition-and-scope/)-[4](https://code.iczn.org/chapter-2-the-number-of-words-in-the-scientific-names-of-animals/article-4-names-of-taxa-at-ranks-above-the-species-group/), [7](https://code.iczn.org/criteria-of-publication/article-7-application/)-[10](https://code.iczn.org/chapter-4-criteria-of-availability/article-10-provisions-conferring-availability/), [11.1-11.3](https://code.iczn.org/chapter-4-criteria-of-availability/article-11-requirements/#art-11-1), [14](https://code.iczn.org/chapter-4-criteria-of-availability/article-14-anonymous-authorship-of-names-and-nomenclatural-acts/), [27](https://code.iczn.org/formation-and-treatment-of-names/article-27-diacritic-and-other-marks/), [28](https://code.iczn.org/formation-and-treatment-of-names/article-28-initial-letters/) and [32.5.2.5](https://code.iczn.org/formation-and-treatment-of-names/article-32-original-spellings/#art-32-5) also regulate names of taxa at ranks above the family group. (1.2.2)

And here is some typical Code talk about the hierarchy:

The genus group, which is next below the family group and next above the species group in the hierarchy of classification, encompasses all nominal taxa at the ranks of genus and subgenus (see also Articles [10.3](https://code.iczn.org/chapter-4-criteria-of-availability/article-10-provisions-conferring-availability/#art-10-3) and [10.4](https://code.iczn.org/chapter-4-criteria-of-availability/article-10-provisions-conferring-availability/#art-10-4)). (42.1)

*Wikipedia* articles about biological taxa are replete with talk of the categories. So too is Sterelny and Griffiths’ textbook, including this:

especially in paleontology, evolutionary patterns are often studied at the level of the genus or family, rather than by identifying individual species….So when evolutionary theorists writing on the history of life…the information they extract will mostly be patterns of family extinction, survival, or spread. (1999: 194)

Finally, note the talk of a very high category that begins a paper by James Valentine: “It is consistent with fossil evidence that all living metazoan phyla originated during the late Precambrian and Cambrian”. Valentine goes on to address the issue of “why there was not a continuing evolutionary innovation at the level of phyla” (1995, p. 190).

The ubiquitous talk of the higher categories in biology is surely not just idle rhetoric. Ereshefsky thinks that “the widespread acceptance of the Linnaean system” stems from “virtues” that are “just pragmatic and not theoretic”, and cites evidence that this is a common view (Ereshefsky 2001, p. 233). But the view strains credulity. Talk of the categories does, of course, *have* pragmatic benefits, but *so too does the talk of any true theory*: a *test* of truth is success in practice. The apparent commitments of a theory must be taken seriously, as Quine (1961) emphasized; one cannot have one’s theoretical cake and eat it too. So, if the virtues of biology’s talk of the categories were really “just pragmatic”, then we ought to be able to paraphrase the talk away *without theoretical loss*. We have no reason to believe that this is possible; that, for example, Valentine could raise his issue without talking of phyla. Rather, we should accept that this talk of the higher categories is doing some theoretical work and address the question, “What work?”. Sterelny and Griffiths mention “the importance of the information expressed” by this talk (1999, p. 201). We need to consider: What information?

**3. “Minimal Concepts”**

The first step in answering is to emphasize that doubts about the Linnaean hierarchy are not doubts that there is a hierarchy of taxa; “The challenge here is not to the assumption that life is hierarchically arranged” (Ereshefsky 1999, p. 299). *Canidae* really does include *Vulpes*, which includes *Vulpes vulpes*. The challenge is to the view that this hierarchy of taxa can be captured by the Linnaean categories.[[2]](#footnote-2) Next, with this very real hierarchy in mind, we note the Code’s description of the “genus group” being “below the family group and next above the species group”. This typical description points the way to my “minimal” proposal. *If nothing else*, a category is a kind of taxon at a certain rough level in the hierarchy of taxa. As I shall point out, the level of a taxon in the hierarchy has consequence for its explanatory power. So, the categories do have an informational and explanatory role, even if not one as “robust” as biologists who worry about the categories may have hoped for. We *may* have to settle for “minimal” category concepts, according to which Linnaean categories capture, in a somewhat rough and ready way, the explanatorily significant levels in the hierarchy of taxa.

Let us start by considering the explanatory work done by taxa. Mohan Matthen points out that “many biologists seem committed to the idea that something is striped *because* it is a tiger” (1998: 115). Ereshefsky and Matthen ask, “Why does this bird have black feathers?”, and rightly answer, “Because it is a crow” (2005, pp. 2-3). I have developed the case for this way of thinking (2008, 2018, 2021, 2023). I summed up the way as follows: “the fact that an individual organism is a tiger, an Indian rhino, an ivy plant, or whatever, explains a whole lot about its morphology, physiology, and behavior” (2008, p. 352). Now, apply this thinking to a particular red fox, Rufus, a member of the taxon, *Vulpes vulpes*. The fact that Rufus is a member of that taxon explains very many of his phenotypic properties; for example, that he has a bushy tail with a white tip. But how could this be so? The very nature or “essence” of red foxes *causes* them to have such tails (in their normal environment); it is *because* of that essence that Rufus has that tail.[[3]](#footnote-3) This reflects an idea, going back at least to Aristotle,[[4]](#footnote-4) that, as Elliott Sober puts it: “A species essence will be a causal mechanism that acts on each member of the species, making it the kind of thing that it is”; thus, an essence “must be explanatory” (1980, p. 354). The explanations in question might be Ernst Mayr’s (1961) “proximate” ones of “functional biology”, describing the developmental causes of the phenotypic properties of members of a taxon; or the explanations might be his “ultimate” ones of “evolutionary biology”, describing the historical causes of those properties in the taxon.[[5]](#footnote-5)

Moving up a level in the hierarchy, Rufus is also a member of the taxon, *Vulpes*, the taxon of “true foxes”. Many of Rufus’ phenotypic properties that are explained by his being a *Vulpes vulpes*, including having that tail, are *not* explained by his being a member of the higher taxon, *Vulpes*; many true foxes, including Arctic foxes (*Vulpes lagopus*) do not have such tails. However, the fact that Rufus is a *Vulpes* does explain *some* of his properties and, importantly, explains those same properties in animals from many other taxa at the lower level. Rufus’ property of having partially retractable claws is an example, for Rufus shares this property with Arctic foxes, Cape foxes, (*Vulpes chama*), and other true foxes. Their having this property is explained by their being members of the taxon *Vulpes*; for, the very nature of *Vulpes* *causes* them to have such claws (in their normal environment); it is *because* of that nature that these various true foxes have those claws.

There is a similar story when we move up another level in the hierarchy of taxa. Rufus is also a member of the taxon, *Canidae*, that includes many dog-like animals. Many of the properties of true foxes, including their claws, are not explained by their being members of the higher taxon, *Canidae*; dogs and jackals do not have retractable claws. However, the fact that Rufus is a *Canidae* does explain some properties he shares not only with other true foxes but also with members of the taxon *Canis*; for example; the property of walking on toes (“digitigrade”). This property of true foxes, dogs, jackals, and many other taxa is explained by the nature of *Canidae*.

We can see then that the explanatory significance of being a member of a taxon comes in degrees along two dimensions. On one dimension, being a *Vulpes vulpes* explains more of the phenotypic properties of Rufus than does being a *Vulpes*, which in turn explains more of those properties than being a *Canidae*, and so on up the hierarchy. On the other dimension, being a *Canidae* explains *some phenotypic properties of more organisms* than does being a *Vulpes*, which in turn explains more of those properties than being a *Vulpes vulpes*, and so on down the hierarchy. So, a taxon’s place in the very real hierarchy of taxa is explanatorily significant.

There has been no talk of categories so far, but now we introduce them: we claim that *Vulpes vulpes* is in the category **Species**, *Vulpes*, in the category **Genus**, *Canidae*, in the category **Family**. This claim conveys, *at least*, information about the relative explanatory power of these taxa. Thus, on the one hand, the claim informs us that, since *Vulpes vulpes* is a species, being in that taxon, explains more of the phenotypic properties of its members, than simply being in their genus, *Vulpes*. And, on the other hand, simply being a *Vulpes* explains some phenotypic properties not only of members of the species *Vulpes vulpes*, but also of all the members of other species in the genus. Moving up the hierarchy, we get a similar comparison between the explanatory powers of genus *Vulpes* and family *Canidae*. And so on up.

The level of a taxon in the hierarchy of taxa is an indication of its explanatory power along the two dimensions. According to the minimal concept of a Linnaean category, an ascription of the category to a taxon, if nothing more, is an attempt to identify that explanatorily significant level of the taxon. Whereas ascription of a taxon to an organism explains its phenotypic properties, ascription of a category to a taxon explains (at least), the scope of explanations that the taxon can yield. The criticisms of the Linnaean categories show that ascriptions of categories are *imperfect* guides, providing *imprecise* information about levels. Nonetheless, these ascriptions do provide important information about those levels, as we have just seen.

Other sciences have hierarchies of kinds, of course; for example, chemistry, geology, and astronomy. The same sort of explanatory significance attaches to levels in those hierarchies: a kind *L* that is below kind *H* in a hierarchy explains more of the observable properties of its members than does *H*; but *H* not only explains some observable properties of members of *L* but also those same properties of members of other kinds below *H* in the hierarchy. And with the significance of levels goes an interest in categorizing levels. Consider chemistry, for example. Its situation seems to be interestingly different from that in biology (Djoumbou Feunang et al. 2016). Whereas biology has, sort of, settled on the Linnaean hierarchy of categories, controversial though it is, chemistry has several such hierarchies. But, whereas biology struggles to find definitions for its categories, chemistry seems to have promising definitions available for its; see those accompanying a recently proposed “automated chemical classification” with a hierarchy of 11 levels (Djoumbou Feunang et al. 2016).

**Objection**. “According to your minimal concepts, the identification of a category is purely relational. The concept for category *K*, identifies *K* as the category of taxa with the appropriate explanatory power relative to taxa in category *J* above and taxa in category *L* below. But how are *J* and *L* identified? Take *J*. It is the category of taxa with the appropriate explanatory power relative to taxa in category *I* above and *K* below. And so on. Categories identified in this purely relational way can provide no information about the explanatory power of any of these taxa.” This is a good point. Absent an independent *non*-relational identification of at least one category, this account is fairly empty. We need the explanatory power of taxa in that independently identified category, a category with a robust not minimal concept, to serve as the anchor of this hierarchy of categories; comparison with that category can then yield genuine information about the explanatory power of taxa in other categories. We know immediately where to go for the anchor, the **Species** category; “one taxonomic category, the species, has been thought by many to be real and nonarbitrary” (Futuyma 1986, p. 111). For, there are many robust competing species concepts each of them identifying the **Species** category without adverting to any other category. We already have, of course, a good idea of the explanatory power of the taxa in that category, as those concepts indicate. In any case, the actual explanatory power of species serves as the anchor. Thus, knowing that a taxon is in the category **Genus**, provides information about the taxon’s explanatory power given what we know about the explanatory power of taxa in the independently identified category **Species**. And so on up.

So, despite the imperfections of the Linnaean categories, and the lack of robust category concepts above **Species**, the categories are at least explanatory in this minimal way of marking out, *in a somewhat rough and ready way*, a level in the hierarchy of explanatory taxa. The categories are not as explanatorily important as the taxa they categorize but they are indeed informative, as Sterelny and Griffiths noted. The ubiquitous talk of the higher categories in biology is not “just pragmatic”; it serves a theoretical purpose.

If we think of the Linnaean hierarchy as simply an attempt to distinguish explanatory levels, its imperfection is not surprising. It can be a tricky matter deciding whether to propose a taxon at a level between those of two taxa, between a taxon that would include the proposed taxon and a taxon that the proposed taxon would include. It may be hard to say whether our explanatory needs justify such a proposal. Indeed, there may be a deal of indeterminacy about the appropriate number of levels. And it is not surprising that decisions about levels in one group of organisms and decisions in another do not match up levels neatly.

Still, the Linnaean categories seem *not* to be simply distinguishing levels but attempting to tell us something about each level. This is brought out vividly by monotypic taxa, taxa that contain only one immediately subordinate taxon. Consider, for example, the beluga (white) whale: one and the same group of animals form the monotypic genus, *Delphinapterus*, and its only species, *Delphinapterus leucas*. We should see our calling that group a genus as an attempt to convey something theoretically important beyond calling it a species. The minimal concept of **Genus** does not remove the need for a robust concept to justify this talk.

**4. Conclusion**

Talk of higher categories (ranks) like **Genus** and **Family** is ubiquitous in biology. Yet there is widespread skepticism about these categories. We can locate the source of this skepticism in the lack of “robust concepts” for these categories, substantial theories of what it is to be in a certain category. A common defense of category talk is that its virtues are “just pragmatic and not theoretic”. But this strains credulity. We should suppose rather that talk of the higher categories does theoretical work. What work? I have proposed a “minimal concept” for a category, according to which the category is *at least* explanatory in marking out, in a rough and ready way, a level in the very real hierarchy of explanatory taxa. The skepticism is exaggerated.

REFERENCES

Baum, DA (2009) Species as ranked taxa. Syst. Biol. 58(1): 74–86 DOI:10.1093/sysbio/syp011

Boulter SJ (2012) Can evolutionary biology do without Aristotelian essentialism? Royal Institute of Philosophy Supplement 70: 83–103

Devitt M (2008) Resurrecting biological essentialism. Philosophy of Science 75:344-182.

Devitt M (2011) Natural kinds and biological realisms. In: Campbell JK, O’Rourke M, Slater M (eds) Carving nature at its joints: Natural kinds in metaphysics and science. Cambridge, MA: MIT Press, pp. 155-173

Devitt M (2018) Historical biological essentialism. Studies in History and Philosophy of Biological and Biomedical Science 71:1-7. DOI: 10.1016/j.shpsc.2018.05.004.

Devitt M (2021). Defending intrinsic biological essentialism. Philosophy of Science 88: 1–16. 0031-8248/2021/8801-00XX$10.00

Devitt M (2023). Biological essentialism. Oxford: Oxford University Press

Djoumbou Feunang Y, Eisner R, Knox C, Chepelev L, Hastings J, Owen G, Fahy E, Steinbeck C, Subramanian S, Bolton B, Greiner R, Wishart DS (2016) ClassyFire: automated chemical classification with a comprehensive, computable taxonomy.Journal of Cheminformatics 8:61 DOI 10.1186/s13321-016-0174-y

Dupré J (1981) Natural kinds and biological taxa. Philosophical Review 90:66-90

Ereshefsky M (1992) Introduction. In: Ereshefsky M (ed) The units of evolution: Essays on the nature of species. Cambridge, MA: MIT Press, pp. xiii-xvii.

Ereshefsky M (1998) Species pluralism and anti-realism. Philosophy of Science 65:103-120.

Ereshefsky M (1999). Species and the Linnaean hierarchy. In: Wilson RA (ed) Species: New interdisciplinary essays. Cambridge, MA: MIT Press, pp. 285-305.

Ereshefsky M (2001) The poverty of the Linnaean heirarchy: A philosophical study of biological taxonomy. Cambridge: Cambridge University Press.

Ereshefsky M, Matthen M (2005) Taxonomy, polymorphism, and history: An introduction to population structure theory. Philosophy of Science 72:1–21. 0031-8248/2005/7201-0001$10.00

International Commission on Zoological Nomenclature (1999) International code of zoological nomenclature, 4th edn. The International Trust for Zoological Nomenclature <http://www>.nhm.ac.uk/hosted-sites/iczn/code/.

Kitcher P (1984). “Species”. Philosophy of Science 51: 308-333. Reprinted in Kitcher 2003, 113-134

Kitcher P (2003) In Mendel’s mirror: Philosophical reflections on biology. New York: Oxford University Press

Mayr E (1961) Cause and effect in biology. Science 134:1501-1506

Mishler BD (1999) Getting rid of species? In: Wilson RA (ed) Species: New interdisciplinary essays. Cambridge, MA: MIT Press, pp. 307-315

Mishler BD, Donoghue MJ (1982) Species concepts: A case for pluralism”. *Systematic Zoology* 31:491-503

Okasha S (2002) Darwinian metaphysics: Species and the question of essentialism”, Synthese 131:191-213

Quine WV (1961) From a logical point of view. Cambridge MA, Harvard University Press

Sober E (1980) Evolution, population thinking and essentialism. Philosophy of Science 47:350-383.

Sterelny K, Griffiths P (1999) *Sex and death*. Chicago: University of Chicago Press.

Valentine JW (1995) Why no new phyla after the Cambrian? Genome and ecospace hypotheses revisited”. Palaios V(10):190-4

Walsh D (2006) Evolutionary essentialism. British Journal for the Philosophy of Science 57:425–448

1. Which I shared (2011: 167). [↑](#footnote-ref-1)
2. Ereshefsky (1999, p. 299-302) and Mishler (1999, p. 311-312) discuss other ways to capture the hierarchy. [↑](#footnote-ref-2)
3. What is this essence? The consensus answer is nicely captured by Samir Okasha: species are identified “in terms of evolutionary history…as particular chunks of the genealogical nexus” (2002: 200). I have argued that there is an intrinsic underlying component to the essence of biological taxa, along with the historical component (2008, 2021, 2023; see also, Walsh 2006). [↑](#footnote-ref-3)
4. According to Stephen Boulter, it is “relatively uncontroversial among Aristotle scholars” that Aristotle held that “the essence of a kind has an explanatory role in that it is adverted to when explaining why an instance of the kind has the properties and behavior patterns that it does.” (2012: 86) [↑](#footnote-ref-4)
5. The taxon’s place in the phylogenetic tree will, of course, be central to the ultimate explanations. [↑](#footnote-ref-5)