A Conventional View

Matt Haber
Department of Philosophy and School of Biological Sciences
matt.haber@utah.edu
Katherine Berensen
Department of Philosophy and School of Biological Sciences
Peyton Cooper
Department of Philosophy and School of Biological Sciences

Abstract
A widely held view in biology is that higher Linnaean categories or ranks (genera, families, etc.) ought to be regarded as 'mere conventions'. That is typically understood to mean that their utility and application stem from (relatively) recent conventions that have been adopted for their usage in biological classification. There are no underlying theories or robust ontological concepts of these higher ranks, in contrast to taxonomic groupings like species or populations (disputed as those underlying concepts or theories may be). Absent these conventions, the application of higher ranks are in important ways arbitrary, reflecting their lack of theoretical and conceptual grounding. Indeed, some taxonomists have argued that this lack of grounding justifies adopting rank-free taxonomies.

Michael Devitt (2023a) challenges this entrenched view, calling it an exaggerated skepticism. Instead, he takes the continued use of higher Linnaean ranks as evidence that they reflect "minimal category concepts," which do genuine theoretical and explanatory work by marking out a relative level of taxonomic hierarchy. We disagree. Here, we unpack what it means to treat Linnaean categories as mere conventions, arguing they are better understood as informational proxies for phylogenetic hierarchies. Yet, Devitt is right to focus on this case, as it provides an exemplar of how scientists retain the utility of scientific practices that outlive their underlying initial theoretical and conceptual justification. We conclude by offering an alternative explanation for this in the context of a pragmatic account of science that treats Linnaean classification as a biological formalization.

1 Introduction

> Classifications have only one purpose: utility. If they lack stability and familiarity, they entirely lack utility. (Benton 2000, p. 636)

A widely held view in biology is that higher Linnaean categories or ranks (genera, families, etc.) ought to be regarded as mere conventions. Their utility is in the role they play in navigating information retrieval, nomenclatural stability, taxonomic identification, universality of nomenclatural usage, etc.; this stems from the function of these categories in the conventions we use in classification. There are no theories or robust ontological concepts of higher ranks to which we might attribute this value (Winston 1999), in contrast to species or populations of organisms (disputed as those underlying concepts or theories may be (Millstein 2010; Wilkins et al. 2022)).

Michael Devitt (2023a) challenges this view, calling it an "exaggerated skepticism." Instead, he defends the view that these categories do genuine theoretical work and ought to be regarded as such. More precisely, Devitt argues that these categories meet a standard of being 'minimal category concepts' which do theoretical work by marking out a relative level in "the very real hierarchy of explanatory taxa" (8).

Challenging entrenched, received views is an important role that philosophers play, especially in philosophy of biology (Haber 2014; Watkins & DiMarco 2024; Okasha 2025). This is relevant even when we ultimately affirm those views. It guards against complacency and can productively update their justification in light of new advances, among other virtues. Here we assess Devitt's challenge, evaluating core parts of his argument and taking the opportunity to both refresh and update how we might interpret the continued utility of higher biological categories.

This assessment accomplishes several things that fruitfully advance the discussion. It provides a useful reminder of the importance of phylogenetic thinking in biology, even as what is regarded as phylogenetic thinking has grown more complicated (Haber 2019, 2025a). We also offer a novel interpretation of Devitt's positive claims as promoting a *pragmatic conception of formal scientific theories* (a la Griesemer 2013). On this view, we can treat Linnaean modes of nomenclature and classification as formal systems, and the utility that Devitt observes as an exemplar of treating formalization as a practice. On that account, the utility of higher categories need not be tied to any theory or concept—or the truth of any such theory or concept—but to the work those categories do, even if that work is licensed by convention.

Our argument is structured as follows. We start by offering a short description of what it means to regard higher biological categories as conventions, along with a very brief explanation of *why* biologists came to this view. We then turn to Devitt's response, describing his defense of these categories as entailing some minimal concept of categorization. Next, we assess Devitt's challenge, arguing that the work he attributes to higher categories is, at best, parasitizing phylogenetic explanations (where it is not obscuring or confounding phylogenetic reasoning). This better locates what it is that is doing the explanatory work Devitt describes and avoids conflating informational proxies for explanatory power.

We conclude by offering a competing account of the continued utility of conventional ranks, viewed through the lens of Griesemer's (2013) treatment of formalization in biology. On that account, the Linnaean system of classification may be regarded as a formal system whose utility has outlived its theoretical and conceptual justification. This accounts for Devitt's observation of the continued utility of higher biological categories, but in the context of a pragmatic view of science (in contrast to a semantic or syntactic view).

Defending the established view of higher ranks as conventions does not turn on adopting a pragmatic conception of science. It may be accounted for as well on a Devitt-style truth-functional account of theories. Regardless, what Devitt identifies as an 'exaggerated skepticism' is better described as adopting phylogenetic thinking; he is describing the reconciliation of higher Linnaean categories with that stance. That interpretation better accounts for the explanatory work Devitt describes and avoids conflating *serving as an informational proxy* for *explanatory power*. Yet, seeing in Devitt's argument a path for how a pragmatic conception of science might regard Linnaean systems of classification as exemplars of what Griesemer calls 'theory-ing' formal systems, means that our defense of convention is hardly conventional.

2 Categories as Conventions

2.1 A Shift to Convention

The *Bulletin of Phylogenetic Nomenclature* launched in 2023 (de Queiroz 2023). Published by the *International Society for Phylogenetic Nomenclature* (ISPB), this marks both the culmination of a sustained effort to develop a rank-free nomenclature based on phylogenetic principles (de Queiroz and Gauthier 1990, 1992, 1994), as well as—proponents hope—the beginning of the *PhyloCode* (Cantino and Queiroz 2020).

Rank-free nomenclatures[1] are rules for naming taxa in biology that reject ranked categories, especially Linnaean higher categories (genera, families, classes, etc.). Constructing something like this has been a goal of systematists since at least the development of contemporary phylogenetics (e.g., Hennig 1966; Hull 1966; Griffiths 1976; Wiley 1979). Some of these proposals were intended to amend, append or revise Linnaean classification; in other cases the intent was to propose a distinct approach that could exist alongside Linnaean methods, or as a stand-alone replacement. The *PhyloCode* is the most recent and developed of these rank-free nomenclatures, recognizing only species and clades (monophyletic groups) as taxa that may be named.

The motivation for developing rank-free taxonomies is complex and varied (de Queiroz 1988, Ereshefsky 2000), e.g., aligning our taxonomic nomenclatures with the principles of evolution or with the logical consequence of recognizing the distinctive tasks of *systematizing* rather than *classifying* taxa (Griffiths 1974, Hennig 1975). More broadly, it can be viewed as one response to the observed tensions—and even conflicts—between evolutionary systems and Linnaean classifications.

Not everyone agrees that the correct response to this tension is the adoption of a rank-free nomenclature. The *International Society for Phylogenetic Nomenclature* (ISPB)—the governing body of the *PhyloCode*—helpfully identifies a substantial list of critics of this strategy that instead defend the Linnaean method of classification, ranks and all (e.g., Benton 2000, among many others). Broadly speaking, they argue that the Linnaean approach can be—and has been—reconciled with evolutionary and phylogenetic thinking, primarily by committing to the joint conventions of naming only higher taxa that correspond to monophyletic groups and revising existing taxonomies to reflect phylogenetic hypotheses of those groups. Indeed, as these conventions became widely adopted, a major project of 20th and 21st century systematics has been revising taxonomies to align with phylogenetic analysis (e.g., the re-classification of Canidae by Zrzavý and Řičánková 2004 and others).

Yet, even these defenders of the ranked approach of Linnaean classification largely agree that we ought not over-read their defense of the retention of these supra-specific ranks. Those defenses are typically framed in terms of a cost/benefit analysis of jettisoning such a long-standing system of nomenclature, or

---

[1] Sometimes designated as 'taxonomies' or 'classifications', among other similar terms. These are three distinct but related aspects of systematics and the study of biodiversity (Winston 2018). Rank-free nomenclatures typically recognize biological hierarchies and levels (e.g., in a phylogenetic context) but reject 'ranked' treatment of these. The *PhyloCode*, for example, only recognizes species and nested monophyletic groups as taxa that may be named.

in the way that traditional nomenclature promotes stability independent of phylogenetics. But these sorts of defenses typically take careful pains to divorce that utility from any claims of theoretical value or conceptual underpinning of those higher ranks *as* higher ranks:

> … it is entirely unnecessary to demonstrate that higher category names have any fundamental meaning. Classifications are utilitarian, and inclusive hierarchical categorization is an immensely useful tool in providing a structure that the human brain can comprehend. Nobody has ever pretended that the hierarchical ranks era, period, epoch, stage, and zone have any ontological meaning in the divisions of geological time. But everyone accepts their immense utility, and there are no fatuous calls from semanticists to abandon them. The same is true of the Linnaean categories, and for precisely the same reasons. (Benton 2000, 643)

The upshot is the wide consensus in biology that higher biological categories are best regarded as mere conventions; their boundaries are not tied to any theoretically-motivated concepts of higher taxa, they are stipulated by the conventions of classification (Winston 1999). Moreover, biologists warn that mistaking these categories for theoretically-informed or ontologically grounded groups can be highly misleading and obscure more insightful and fruitful research questions (Raikow 1986; Stevens 1997). That stance has become entrenched, from proponents of rank-free nomenclatures to even the most ardent advocates of the Linnaean system.

Michael Devitt (2023a) challenges this stance. He argues that the continued utility of higher biological categories has been under-appreciated for what it tells us about the theoretical and explanatory value of those categories. Even though the value he attributes here is minimal, it reflects an important difference in kind from how these categories are regarded on the entrenched stance. Let's turn now to his view.

2.2 Challenging Convention: Devitt Digs In

Devitt's gambit is straight-forward. He observes biologists' continued use of and appeal to higher categories and takes it as evidence of genuine theoretical or explanatory value: “despite this skepticism, *biologists keep talking about the higher categories*" (2023a, 3). Rather than dismissing or explaining away that practice, his goal is to extract that value.

Devitt demonstrates this value by considering the sorts of explanatory inferences we are licensed to draw solely from the taxonomic placement of Rufus (a red fox), *Vulpes vulpes* (the red fox) and *Vulpes lagopus* (the arctic fox):

> Rufus is also a member of the taxon, *Vulpes*, the taxon of “true foxes.” … 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 [(*Vulpes lagopus*)], 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 (Devitt 2023a, 5).

Similar inferences are drawn as we move from lower to higher Linnaean ranks and less to more inclusive taxa.[2] The phenotypic properties become less specific to red foxes, but still provide guidelines for inquiry and resources for explanations. On Devitt's view, this is about explanation in a hierarchy of taxa. Devitt continues, ultimately placing these explanations from hierarchy in the context of categories:

> The level of a taxon in the hierarchy of taxa is an indication of its explanatory power. … 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 (Devitt 2023a, 5).

On Devitt's account, this is the theoretical work being performed. Moreover, minimal though it may be, that theoretical underpinning supports taxonomic explanations. So it is that Devitt defends this *minimal concept* of category as genuinely theoretical.

By designating this as a 'minimal' concept, Devitt acknowledges it is thin. That means at least two things. First, that these concepts are 'anchored' by a more robust concept, in this case 'species.' Second, he acknowledges that they are relational concepts; their theoretical and explanatory value is tied to their relative hierarchical rank.

Properties of true foxes, like the arctic and red fox, are not fully explained by the higher classification of their family Canidae. Devitt uses this example to signify the two dimensionality of the Linnaean classification system. First, being a *Vulpes vulpes* explains more of the phenotypic properties than the genus *Vulpes* alone can provide. The second dimension is that being a Canidae (a family) explains phenotypic properties of a wider range of organisms than *Vulpes* (a genus). In this way, higher Linnaean categories allow for a shallow but broader view whereas lower categories like species and genus provide a specific but narrow understanding of organisms. Devitt argues that ignoring this theoretical work contributes to the so-called exaggerated skepticism surrounding the higher Linnaean ranks.[3]

Let's assess Devitt's argument. We first argue that there is some slippage in the target of skepticism, from Linnaean higher categories to hierarchical thinking, that mischaracterizes what it is that biologists are skeptical about. One danger of this kind of slippage is a downstream mis-identification of just what it is that is doing the useful theoretical and explanatory work that Devitt observes. We argue that insofar as Linnaean categories are doing work, they are doing so by parasitizing phylogenetic thinking. To highlight the value of the latter and what can go wrong if we misattribute that work to higher categories,

---

[2] Here we distinguish between taxa and ranks. Briefly, we regard taxa as "named groups of organisms" and ranks as categories to which those taxa may be assigned (de Queiroz and Gauthier 1990, 1992, 1994, among many others). Linnaean ranks are the most familiar of these categorical treatments. Though de Queiroz and Gauthier and Benton (2000) offer two very different ways we might draw conclusions from this distinction, both agree that care should be taken not to conflate 'taxon' with 'rank' or 'category'. See Okasha (2025) for a related discussion with respect to Devitt's views.

[3] In personal correspondence, Devitt disputes our interpretation of his view, attributing these shallower and broader views to *taxa* at different levels of the hierarchy instead. We maintain that categories are still doing explanatory work for Devitt that is licensed by the underlying phylogenetics. Part of this disagreement is reflecting a dispute over what is underwriting explanations from hierarchy (see below), and how we ought to understand the relation of concepts like, 'taxa', 'rank', and 'category' in the context of the systematization/classification distinction in contemporary biology (O'Hara 1993).

let's recall the recent history of how Linnaean categories were reconciled with phylogenetic thinking by the introduction of taxonomic conventions.

3 Hierarchies, Phylogenetic Thinking, and Skepticism of Higher Ranks

Devitt's argument for a minimal concept of higher categories turns on the utility implied by the continued use of Linnaean categories by biologists. This is tied to the role they play in placing taxonomic groups in a more inclusive taxonomic—and, by extension, explanatory—hierarchy. We assess Devitt's example of the "theoretical work" conducted by the higher Linnaean categories. Without the contribution of taxonomists reconciling classification and phylogenies, Devitt's example would no longer stand. Below we argue that the utility he identifies is parasitizing phylogenetics, conflating the role of informational proxy for explanatory power; that attributing the observed utility to even a minimal concept of higher categories risks undermining that very utility by leading us astray or obscuring the value of hierarchical thinking; and that the minimal concept is too thin, a symptom of over-reading the inferences licensed by pragmatic benefits in science. In section 4, we build off this last point to offer a competing way to account for this utility.

3.1 Parasitizing phylogenetic thinking

The force of Devitt's argument turns on identifying the explanatory and theoretical work being done by higher Linnaean categories as embodied by taxa like Canidae and *Vulpes*. We agree that if we could identify that work and attribute it *to* those categories that this would provide evidence in support of treating those higher categories in just the way Devitt is advocating. Here we argue that it is not the *categories* doing that work, but phylogenetic thinking. Mis-attributing that utility to higher categories is to conflate the role they may play as informational proxies for explanatory work and, worse, may even lead us astray.[4]

Before returning to Devitt's example, let's unpack a bit more the why and the how biologists underwrite Linnaean classifications. On their own, Linnaean categories are a bit of a blank slate; it is left entirely to biologists what grouping criteria they might use to designate taxa. The Linnaean system just provides a ranked ordering of categories. The breadth and scope of taxonomic boundaries of Linnaean categories have no constraints, and need not even be explained. They are not required to be phylogenetic or even specify any particular grouping criteria, nor to even be more inclusive as you move up ranks.[5] Contemporary presenters of Linnaean nomenclature regard higher taxonomic categories as arbitrary:[6]

> Although no one species concept applies to all groups of organisms, biologists do at least seem agreed that species represent biological realities.[7] The same cannot be said for the higher-level

[4] Watkins and DiMarco (2024) offer a similar observation in a different context.
[5] Monotypic taxa—higher taxa with only a single species—are the limiting case of this feature. In those cases, the hierarchy can be a bit obscure and seem to collapse on itself.
[6] We should be careful not to read this back into the work of Linnaeus, the person. For more on how we might regard his work on its own merits rather than anachronistically applying contemporary criteria, see Müller-Wille (2007) and Winsor (1999).
[7] A referee observed that Winston's use of 'realities' here is ambiguous, and, in context, may be better understood as a claim that species are natural. The point, well taken, is that we (philosophers) should be careful not to read a realist/anti-realist philosophical debate into the language biologists might be using. More importantly, the referee

> categories of the Linnaean hierarchy; **their definitions are arbitrary**. (Winston 1999, 337-8, emphasis added)

Absent any stipulated criteria, anything goes when it comes to drawing up higher taxonomic boundaries.

As taxonomy developed in the 20th century, biologists began demanding more careful and clearly specified grouping criteria of their classifications (e.g., Sokal and Sneath 1963; see Hull 1970 for a contemporaneous review). An important and influential approach to this was based on phylogenetics (Hennig 1966). Adopting phylogenetic grouping criteria introduces a convention in the form of a constraint: named taxa should be species[8] or monophyletic groups (clades). The degree of inclusion—how wide or deep a clade might be for some taxonomic rank—is typically not specified because Linnaean categories are not definitionally tied to any age or level of inclusion of monophyly. So even on a phylogenetic constraint, there are still elements of arbitrariness, e.g., one rule of thumb is that genera should contain around 30-40 species because that is a workable unit of memory.[9]

The botanist Peter F. Stevens captures the motivation for adopting a phylogenetic convention[10] in taxonomy by highlighting the risk of drawing evolutionary inferences from heritage classifications,[11] or even treating the groupings and trends found in these classifications as data. His argument captures a snapshot of the explicit efforts and work done to push biologists to construct classifications that conform to and reflect phylogeny:

> It is easy to make curves that depict the size of taxa in particular ranks using monographs, floras, and faunas written over the last two centuries, but interpreting these curves is difficult; thus, finding a biological explanation is dubious. We must remember that there are still no generally accepted criteria for grouping or ranking in biological classifications, and no agreement as to what these classifications should represent. (Stevens 1997, 249).

In other words, be very cautious about reading theoretical, ontological, or even data claims off of heritage classifications. In many cases, the grouping criteria used by those authors is obscure or in conflict with contemporary evolutionary accounts. Before we can rely on the empirical trends and patterns found in those classifications, the grouping criteria need to be carefully identified and specified, so they may be reconciled with contemporary approaches. Until that work is done, those classifications should not be regarded as supporting generalized or systematic explanations, much less the sorts of inferences that Devitt is attributing to them.

Stevens continues:

> The situation is changing, however, and monophyly (the inclusion of all and only descendants of a common ancestor in a taxon) is now often a preferred grouping criterion. … Yet if there is still much to do in establishing monophyly as a generally accepted criterion for group recognition,

---

rightly notes that the takeaway to highlight here is that, "*the real issue with higher taxa is that the limits of a taxon in a rank are arbitrary*."

[8] There are ongoing debates on whether sub-species should be included on phylogenetic approaches.

[9] Thanks to Sarah Bush for sharing this mnemonic device (see also Stevens 2002; in personal communication, Stevens drew attention to Miller's (1956) influential work on our capacity for processing information).

[10] The motivation holds for adopting any systematic grouping criteria. Though we follow Stevens here in describing a phylogenetically-informed approach, other empirically and theoretically informed approaches are possible.

[11] We use the term 'heritage classifications' loosely, designating classifications and taxonomies that predate the adoption of taxonomic conventions described by Stevens and others.

> previous classifications, especially those of the last century, are largely opaque as we seek to understand the diversity of life. (Stevens 1997, 249).

This captures a snapshot of where botanical nomenclature stood in 1997. Phylogenetic thinking was becoming widely adopted, with monophyly emerging as the preferred criterion for grouping organisms into higher taxa. Yet he cautions us not to read the hierarchical thinking of phylogenetics off of these heritage classifications for free; we may not take heritage classifications at face value and treat them as reflecting biological (phylogenetic) hierarchies:

> Only by careful study can we hope to uncover the reasoning that led to [these heritage classifications'] establishment and to understand the relationship between such classifications and the shape of nature as their authors saw it. It is already clear that these classifications cannot be easily understood in terms of patterns of diversification, and because of the way classifications have developed, comparable studies using twentieth century evolutionary classifications is compromised. (Stevens 1997, 249-50; see also Stevens 1986, 2001, among many others).

That is, heritage classifications must be evaluated and assessed before we may use them in biological explanations or to draw inferences about taxonomy, hierarchy, or evolutionary relationships. Often, this will require taxonomic revisions aligning classifications with phylogenetic analyses.

Stevens, here, is promoting the adoption of the convention in botany that classifications should reflect and be built upon phylogenetics; that the grouping criteria ought to be monophyly, as opposed to the largely opaque criteria used by previous (and often pre-evolutionary) taxonomists. Different areas of taxonomy—fields that focus on different parts of biodiversity—gradually adopted this convention over the course of the 20th and 21st century. That convention—hard fought for and well-earned (e.g., Clayton 1983 and Stevens 1985)—is now broadly reflected in botany. To attribute the utility this convention brought to classification as reflecting some theoretical or conceptual value of higher categories independent of that convention is to misattribute that utility and obscure the work that went into the adoption of these and other classificatory and taxonomic conventions.

David Hull noted something similar about the asymmetry between Linnaean classifications and phylogenies (here reading 'classification' and its cognates as 'Linnaean classification'):

> Given a phylogeny, it can be classified, but given a classification, almost nothing can be inferred concerning the phylogenetic development of the taxa in question. (The truth of this claim can be tested quite easily. Look up a classification of an unfamiliar group and see what you can infer about the phylogeny of the group from the classification.) (Hull 1966, 14).

Hull attributes this to the distinct modes of representation of phylogenies and (Linnaean) classifications. In the former, nested genealogical groups are represented as clades or terminal taxa; classifications may be constructed from these, following the judgment of taxonomists. Linnaean classifications, on the other hand, have far fewer constraints. Though the ranks are hierarchical, those hierarchies need not represent evolutionary—much less phylogenetic—relationships. Indeed, as Stevens notes, many heritage classifications *don't* represent those hierarchies. Thus, given a classification we may not infer a phylogeny unless that is explicitly built into the construction of that classification.

Classifications—on their own—do not support phylogenetic inference (Haber and Velasco 2021). Reconciling classification with phylogeny has been one of the great and ongoing taxonomic projects of the 20th and 21st century. So though many more classifications reflect phylogeny today than when Hull was writing 50 years ago, that did not come for free. It is the result of an enormous amount of taxonomic research explicitly reconciling and building classifications from well-supported phylogenies (e.g., Zrzavý and Řičánková 2004). It is those phylogenies that underwrite the observed utility of classifications and what has allowed classifications to serve as informational proxies, where they do. Without that work the utility noted by Devitt would be absent or even misleading, i.e., it is not the *higher categories* doing the valuable work that Devitt notes. Mis-attributing that work to some theoretical value of higher biological categories obscures the taxonomic research that provided that value.

Revisiting Devitt's *Vulpes* example illuminates how this mis-attribution can obscure the taxonomic research supporting valuable inferences about biology. Recall, he describes how the hierarchical structure of Canidae, *Vulpes*, *V. vulpes*, and *V. lagopus* license inferences that demonstrate the theoretical utility of higher biological categories, i.e., the Linnaean ranks of family (Canidae), genus (*Vulpes*) and species. It is a good example, yet a bit of unpacking reveals that the lesson to draw is slightly yet importantly different from Devitt's. Let's fill it in.

The theoretical work Devitt attributes to higher Linnaean ranks is intentionally minimal. He describes it as a "minimal concept of 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" (Devitt 2023a, 8). Devitt aims to demonstrate this by offering up examples of inferences we can draw about the species (and specific organisms belonging to) *V. vulpes* and *V. lagopus*, the genus *Vulpes*, and the Candidae family merely by their nested relative level in the Linnaean classification.

The sorts and even precise inferences that Devitt identifies may certainly be drawn of *V. vulpes* and *V. lagopus* by virtue of their belonging to *Vulpes* and Canidae. But that is *only* because of recent taxonomic revisions of canids. As recently as 1992, the arctic fox (*V. lagopus*) was designated as belonging to the distinct monotypic genus *Alopex* and was taxonomically distinct from the true foxes (*Vulpes*). Yet, this is just the sort of heritage classification that Stevens cautions against. Geffen et al. (1992) suggested this heritage classification conflicted with the then unresolved underlying phylogeny of canids (see figure 1a), i.e., the very hierarchy inferred by Devitt. Geffen et al. conclude by calling for further work to resolve this phylogeny and reconcile canid classification. Their work prompted further investigation, including Zrzavý and Řičánková's (2004) phylogenetic analysis of canids and subsequent proposed taxonomic revision placing the arctic fox in *Vulpes* (see figure 1b). That phylogenetically-informed revision was subsequently supported by other research, culminating in Nyakatura and Bininda-Emonds' (2012) inclusion of this revision in their update of Carnivora taxonomy.

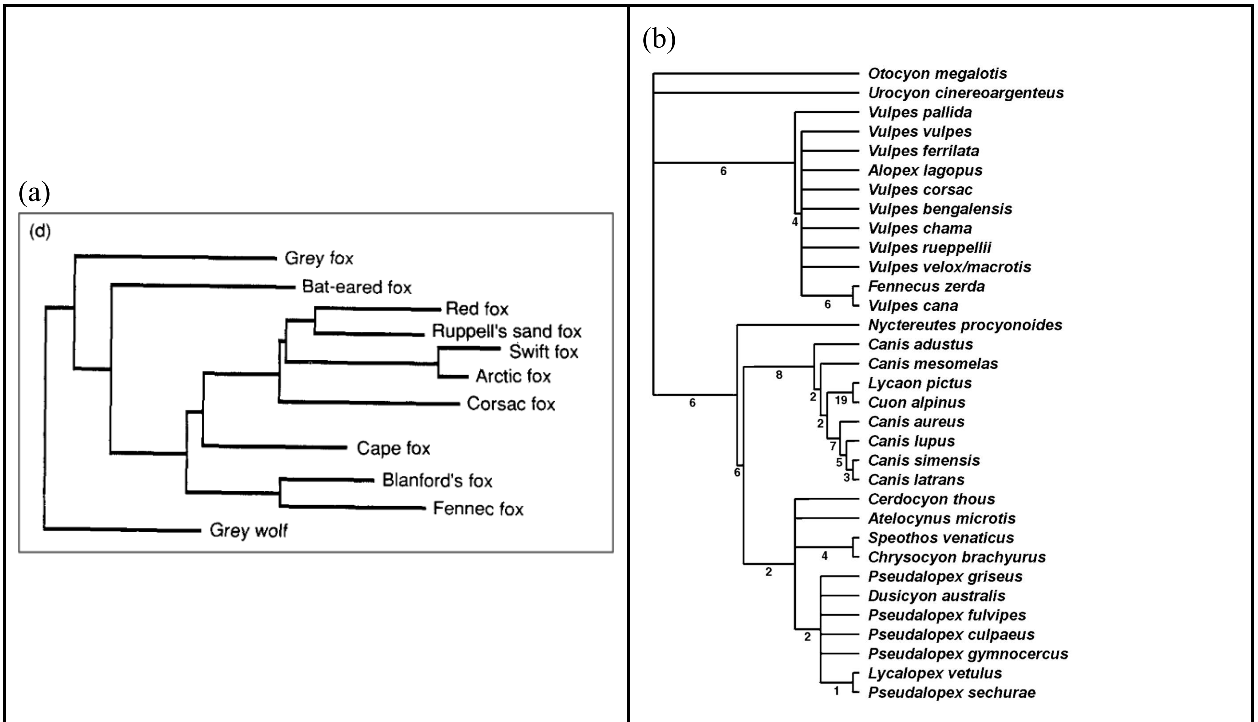

Figure 1. (a) Geffen et al. (1992) considered phylogenies of fox-like canids reconstructed using multiple methods, e.g., their figure 3d presents a ML phylogeny. Though the phylogeny remained unresolved at the time, the competing phylogenies suggested the need for taxonomic revision, largely due to the phylogenetic placement of the Arctic fox (*Alopex lagopus*) relative to other fox species (*Vulpes*). (b) Zrzavý and Řičánková (2004) concur, noting that excluding the Arctic fox leaves *Vulpes s.s.* paraphyletic. They recommend revising *Vulpes* to include the Arctic fox (*Vulpes lagopus*) so it is a monophyletic group. “The monophyly of true foxes (*Vulpes s.l.* including *Fennecus* and *Alopex*) is supported by all analyses” (Zrzavý and Řičánková 2004, 320, figure 9). Note that on their consensus tree they still use the name *Alopex lagopus* for ‘Arctic fox’. Later, they adopt the name *Vulpes lagopus* as they make the case for taxonomic revision.

The recognition and subsequent formal taxonomic representation of the close phylogenetic relationship of the red and arctic foxes is typical of how heritage taxonomies are reconciled with phylogenetic hypotheses. It also exemplifies the operationalization of formal conventions in taxonomy advocated by Stevens and others to employ phylogenetic thinking in taxonomic revision. To be retained and useful for scientific inference, the higher categories must be based on phylogenetic work and serve as informational proxies to the explanatory hierarchies displayed and discovered in phylogenetic analysis.

Prior to 1992 (or, more likely, 2004), the inferences proposed by Devitt would be unsupported and incorrect; it is only after the re-classifications explicitly tied by convention to phylogenetically supported hierarchies that these inferences may be supported and informative. The Linnaean categories are serving as informational proxies thanks to the hard work of taxonomists adopting Stevens-like conventions in taxonomy. Devitt is parasitizing that work and attributing it to higher categories, instead of the underlying phylogenetic analyses that biologists have explicitly tied to those categories by convention.

The mistake Devitt makes is similar to what Hull is describing in observing the asymmetry between Linnaean classifications and phylogenies, only here it is about what licenses the explanatory work being done by hierarchies. Devitt rightly recognizes the powerful role hierarchy plays in biological explanations, attributing that to the hierarchical feature of evolutionary taxa. Yet, Devitt mis-attributes that hierarchy as stemming from a minimal concept of category, rather than being underwritten by phylogenetic structure. In the context of contemporary biology, it is the latter that provides any meaningful empirical and theoretical content to the former.[12]

So the lesson to draw from this case is a bit different than what Devitt reports. The canid taxonomic revision is hardly unusual; it is a familiar path in latter 20th and 21st century taxonomy, as biologists seek to reconcile classification and phylogenetics. In some cases, biologists have even abandoned higher Linnaean categories, but where they are retained their utility is thanks to conventions that permit them to serve as a shorthand for the explanatory hierarchical frameworks based on phylogenies (or whatever other empirically and theoretically informed work might stand in for phylogenies).[13]

4 Pragmatic Conception of Science

Devitt's observation that there is something of value in understanding the continued utility of Linnaean categories—and, by extension, in other cases like this—is important. His account of this value is filtered through a broader commitment to naturalistic realism (Devitt 2018, 2021, 2023b), taking successful inferences and pragmatic benefits as indicative of the truth of underlying theories:

> 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. (Devitt 2023a, p. 4)

Of course, naturalistic realism is not without its critics (e.g., Laudan 1981)., Where for the realist, success, utility, and explanatory power are expressed in terms of truth, for anti-realists these features are expressed in terms of instrumental value, isomorphism, or empirical adequacy (Chakravartty 2017).

The response we offered above is not tethered to either of those stances. Here, we will simply note that even if all true theories generate pragmatic benefits, that, in and of itself, does not conversely entail that the presence of pragmatic benefits indicates true theories. The conditional does not imply the converse.

---

[12] Advocates of rank-free nomenclatures extend this argument, defending a classificatory approach that dispenses with supraspecific ranks and instead transparently displays the biological hierarchy in phylogenetic terms (de Queiroz 1988; de Queiroz and Gauthier 1990, 1992, 1994; Ereshefsky 2000).

[13] A referee offered a brief argument in response, culminating in an observation about the sorts of inferences we might successfully draw from non-monophyletic legacy categories like the Class 'Reptile'. Space constraints restrict us from reconstructing and fully engaging that argument here, but we will offer two observations in response and in anticipation of readers drawing similar questions. First, where these legacy classificatory categories are retained, they are done so in a phylogenetic context. The Class Reptile, for example, is generally regarded as including birds (Aves) (Myers 2001) or, in the legacy sense referee 2 uses, is typically designated by herpetologists as 'non-avian reptiles' (reflecting the phylogenetic understanding of 'taxon' that the heritage usage conflicts with). It is also worth noting that 'Reptile' is an especially interesting example, as herpetologists are among the most enthusiastic and strongest advocates of rank-free taxonomy (e.g., de Queiroz and Gauthier 1990, 1992, 1994). It is not unusual in herpetology and avian studies to find rank-free taxonomies and classifications that simply do away with Linnaean categories altogether (e.g., McTavish et al 2025) or retain Linnaean *categories* by jettisoning Linnaean *hiearchy*, e.g., including the Class Aves in the Class Reptilia (e.g., ADW 2001; see also Ereshefsky 2001).

Of course, realists and anti-realists will evaluate this differently, as will those working in different accounts of scientific theories, be it semantic, syntactic, or pragmatic (Winther 2021).

Devitt's focus on *utility* invites discussion of how a pragmatic view of science might account for the continued utility of higher taxonomic categories. This fills a gap; Devitt's analysis is largely in the shadow of semantic and syntactic accounts of science but does not include a pragmatic framework. Moreover, this framework is especially relevant in phylogenetic systematics given its strong pragmatic tradition (Griffiths 1974, 1976; de Queiroz 1988; O'Hara 1993; Haber 2008). This is particularly notable in resolving conflicts between classificatory projects and the task of reconstructing evolutionary histories (i.e., systematization). O'Hara (1993, 235-6), for example, argues that we ought to adopt the "complexity and pragmatic character of the generalization process" of cartography for biological systematics, including evaluating phylogenies and taxonomies not in terms of whether they are true or false, but if they are useful:

> If I am riding on a subway train, and I look at my city map to find out what the next stop will be, only to discover that the map shows streets only and no subway line, I may be frustrated and declare the map useless, but in such a circumstance I would be unlikely to call it false. There are ways in which a map can be false, of course, by showing a street that simply does not exist, for example; but an ordinary map, one without such outright errors is not so much a true or false hypothesis about the world as a representation of the world by means of which we can answer certain questions and, by implication, not answer other questions. This type of relation to the world—the relation of maps more or less able to answer certain questions, rather than the relation of hypotheses that are true or false—will be important as well at the systematic equivalent of cartographic generalization.

Catherine Kendig's (2016, 2020) treatment of *kinding as a practice* provides on exemplar of applying a pragmatic account to classification. Following that approach here would involve unpacking the values taxonomists pivot into with classificatory conventions and how that changes and clarifies the work taxonomic categories are doing. That offers a powerful way to account for different classificatory practices in a pragmatic framework. Peter Stevens' contributions to the adoption of taxonomic conventions (§3.1) is especially clear on this.

Instead, we bring a pragmatic framework to this discussion by leaning into a complementary approach that treats Linnaean classification as a *formal system*. Though formal methods are often taken to refer to mathematical tools, pragmatic accounts offer a broader view of what counts as formal tools by focusing on the work they do. Formal tools, then, are those devices that accomplish that work. Griesemer provides just such a pragmatic account of formalism in the inexact sciences (2001, 2013), even mentioning Linnaean classification as a candidate formal tool. We aim to flesh that out here, demonstrating that an important role of formal tools in pragmatic accounts can be the work they do preserving utility across changes in conceptual and theoretical commitments (see also Haber 2025b).

Treating the Linnaean system of classification as a *formal* system in a pragmatic account orthogonally re-frames the question about what explains the continued utility of higher taxonomic categories. Rather than looking to truth, empirical adequacy, isomorphism, etc., to explain this utility, the pragmatic stance shifts the explanatory focus back to *utility* having value in itself: [14]

> The practice of theoretical science cannot be stopped by semantic or even syntactic "flaws" by the lights of a particular philosophical framework of formalization, but a theory, true or false, that is no use to scientists is a dead theory, full stop. These points suggest that a pragmatic view of theories is needed to explain theory-ing in practice. (Griesemer 2013, 301)

Theory-ing is an important feature of Griesemer's pragmatic account of formalism, where things like *exactness* or *formalization* are not so much features or properties of science or a theory, but ways theories and science may be useful.

This needs a bit of unpacking. Griesemer identifies three key dimensions that reflect ways that theories may be made useful through the practice of formalization. Those are *conceptual notation*, *model alignment*, and *subject-matter domain control*. Evaluating how and the degree to which these dimensions are expressed tells us how formalization generates theoretically-informed empirical and investigative practices.

Briefly, *conceptual notation* provides a means of abstractly representing the empirical contents of investigations by distinguishing form from content. This is the practice of abstraction and typically involves "(possibly tacit) convention[s] (sets of rules) governing or relating forms in the representations" (Griesemer 2013, 308). *Aligning* those forms means reconciling them with investigative experience and theory, with *domain control* coordinating what gets counted as phenomenon and empirical content.

That is all a bit abstract,[15] but in practice it means that to evaluate ways in which formal theories are made useful or retain their utility, we should identify how and to what degree these dimensions are expressed. In other words, look for these formalizing 'moves' that shift a science towards greater exactness.

On this account, the conventions proposed by Stevens (and others) may be regarded as just these sorts of formalizing moves. These conventions introduce new rules governing the empirical content that formal taxonomic categories represent, i.e., monophyletic groups. This *conceptual notation* detaches those higher taxonomic categories from prior (pre-evolutionary) theoretical and conceptual underpinnings, thus retaining their utility. This *aligns* those taxonomic categories with phylogenetic models and empirical investigation, which coordinate those formal categories with grouping criteria that restrict the *subject-matter domain* to species and monophyletic groups, i.e., phylogenetic taxa. Those alignment moves are asymmetrical. We cannot map phylogenies from (heritage) Linnaean classifications; without the former, the latter lack any theoretical, explanatory, or empirical utility. Thus Linnaean classification is formalized, recast from theory to a useful practice that promotes exactness, stability, and other taxonomic virtues.[16]

---

[14] Arguably, a pragmatic account could supplement or complement either a syntactic or semantic account of science. Griesemer certainly treats it as such.

[15] Intentionally so. After all, what Griesemer is offering is an abstract account of formal abstraction.

[16] Much more may be said about the details here. For example, both phylogenies and Linnaean classifications may be represented as formal sets. That sort of conceptual notation move is a more familiar example of formalization. It also highlights the research problem and tradeoffs needed to align those sets and coordinate them with grouping criteria and domain control. (Though we note that sets might not be a good mode for formalizing evolutionary entities (Haber 2015; see also Sober 1984)).

As the tension between evolutionary theory and the theoretical and conceptual underpinnings of Linnaean classification became more clear, this presented a trade for taxonomists.[17] What Devitt picked up on was that the utility of Linnaean classification outran its theoretical relevance (Haber 2025b). Devitt explains this by tying that utility to "minimal category concepts;" on a pragmatic account, retaining that utility required decoupling it from its theoretical and conceptual underpinnings through the practice of formalization.[18]

Conclusion

Devitt (2023a) challenges what he describes as the skeptical stance that biologists have adopted towards higher biological categories, arguing that the utility of these categories justifies regarding them as carrying a minimal theoretical and explanatory value. We disagree, here arguing that where those categories express utility, it is by parasitizing phylogenetic thinking (or other theoretically grounded biological perspectives). Their value comes from serving as informational proxies rather than providing genuine theoretical or explanatory work; they succeed as informational proxies thanks to conventions that have developed in systematics since Darwin's *Origin of Species*, hence, their treatment as 'mere conventions'. When divorced from those empirically and theoretically motivated conventions, the higher categories can obscure or even conflict with the very value Devitt identifies.

Despite our fundamental disagreement with Devitt, it is worth pausing to recognize the genuine value in philosophers staking out positions that challenge what have become orthodox or entrenched views. These sorts of exchanges advance discussion and provide valuable entry points into a broader literature.

Challenging entrenched views also guards against complacency. Here, that offers the opportunity to remind ourselves what it means to regard higher categories as conventions, and to reevaluate these views in light of new developments in philosophy and biology. The former includes Devitt's own realist account of biological essentialism (Devitt 2018, 2021, 2023b), alongside recent developments of pragmatic accounts of formalism in science. The latter includes a refreshed appreciation for how phylogenetic thinking supports classificatory and taxonomic inferences, even as phylogenetic thinking has grown more complex.

Devitt (2023a) also presses the point that biologists continue to reference and use higher Linnaean categories, despite pronounced skepticism. Devitt rightly notes that if we take biological practice seriously, then we need to account for this practice.

We would add that this practice is just one example of a more general feature of scientific practice. Other fields of science similarly employ practices that lack a strong theoretical foundation yet continue to generate pragmatic benefits that help advance understanding and knowledge (e.g., MacCord 2025). That

[17] Tradeoffs are a hallmark of pragmatic accounts. Typically, it is not so much that there is a single correct strategy for tradeoffs, but that they reflect how values may be operationalized (James 1899).
[18] Interestingly, the Linnaean system was especially well set up for this, as the practice of formalizing its utility independent of theory was baked in from the outset (Müller-Wille 2008).

is, this is a case that generalizes, offering lessons that help us gain a greater understanding of how good science is done.[19]

We provided one way to account for this practice, following Griesemer (2013) by treating it as an example of 'formalization as practice' framed in a pragmatic account of science. Though that is not a necessary component of our argument, we presented this both to display an alternative approach to Devitt's realist framing, and to advocate for a way to account for the retention of formal methods that have outlived their theoretical and conceptual underpinning. To borrow a phrase from computer programming, scientists 'kludge' together the tools available to them if it advances their understanding and research goals; not every tool will correspond to theoretical commitments, despite their utility.

Acknowledgments: Kate Mower, Kalista Leggitt, TJ Perkins, The Bush/Clayton Lab, Michael Devitt, Marc Ereshefsky, Peter Stevens, Aja Watkins, Joeri Witteveen.

Bibliography

Animal Diversity Web [ADW] (2001). "Aves" (On-line), Animal Diversity Web. Accessed April 6, 2026 at https://animaldiversity.org/accounts/Aves/

Benton MJ (2000) Stems, nodes, crown clades, and rank-free lists: Is Linnaeus dead? Biological Reviews 75:633–648. https://doi.org/10.1111/j.1469-185x.2000.tb00055.x

Bokulich, A. (2009). Explanatory fictions. *In* Suárez, M., editor, *Fictions in Science: Philosophical Essays on Modeling and Idealization*, 91–109. Routledge.

Cantino PD, Queiroz K de (2020) International Code of Phylogenetic Nomenclature (PhyloCode). CRC Press, Version 6. | Boca Raton: CRC Press, 2020. | Ratified on January 20, 2019, by the Committee on Phylogenetic Nomenclature, of the International Society for Phylogenetic Nomenclature

Cartwright, N. (1983). *How the Laws of Physics Lie*. Oxford University Press, Oxford.

Chakravartty A (2017) Scientific Realism. In: The Stanford Encyclopedia of Philosophy. https://plato.stanford.edu/archives/sum2017/entries/scientific-realism/

Clayton, W. D. (1983). The genus concept in practice. Kew Bulletin, 38(2):149–153. http://www.jstor.org/stable/4108098

[19] Our focus has been on how scientists use formalized conventions to isolate and retain the utility of scientific practices by stripping them of prior theoretical and conceptual foundations. A slightly different yet adjacent discussion in philosophy of science has focused on the utility of idealizations and falsehoods in science. See Cartwright (1983), Wimsatt (1987), Dupré (1993), Elgin (2004), Bokulich (2009), Potochnik (2017), Sullivan and Khalifa (2019), and Rice and Khalifa (2025), among many others, for related discussions. Where Devitt would, presumably, argue that the retained utility is due to an updated connection to scientific truths, many of these authors provide competing accounts that include useful roles for falsehoods in scientific practice. (Thanks to Aja Watkins for this observation.)

de Queiroz K (2023) Introducing the Bulletin of Phylogenetic Nomenclature. Bulletin of Phylogenetic Nomenclature 1:29–30. https://doi.org/10.11646/bpn.1.1.3

de Queiroz K (1988) Systematics and the Darwinian Revolution. Philosophy of Science 55:238–259. https://doi.org/10.1086/289430

de Queiroz K, Gauthier J (1990) Phylogeny as a Central Principle in Taxonomy: Phylogenetic Definitions of Taxon Names. Systematic Zoology 39:307. https://doi.org/10.2307/2992353

de Queiroz K, Gauthier J (1992) Phylogenetic Taxonomy. Annual Review of Ecology and Systematics 23:449–480. https://doi.org/10.1146/annurev.es.23.110192.002313

de Queiroz K, Gauthier J (1994) Toward a phylogenetic system of biological nomenclature. Trends in Ecology & Evolution 9:27–31. https://doi.org/10.1016/0169-5347(94)90231-3

Devitt, M. (2018). Individual essentialism in biology. Biology & Philosophy, 33(5):39.

Devitt, M. (2021). Defending intrinsic biological essentialism. Philosophy of Science, 88(1):67–82.

Devitt M (2023a) The minimal role of the higher categories in biology. Biology & Philosophy 38:2. https://doi.org/10.1007/s10539-022-09888-5

Devitt, M. (2023b). *Biological Essentialism*. Oxford University Press, Oxford.

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

Elgin, C. Z. (2004). True enough. Philosophical Issues, 14(1):113–131.

Ereshefsky M (2001) The Poverty of the Linnaean Hierarchy: A Philosophical Study of Biological Taxonomy. Cambridge University Press.

Geffen E, Mercure A, Girman DJ, et al (1992) Phylogenetic relationships of the fox-like canids: mitochondrial DNA restriction fragment, site and cytochrome b sequence analyses. Journal of Zoology 228:27–39. https://doi.org/10.1111/j.1469-7998.1992.tb04430.x

Griesemer J (2013) Formalization and the meaning of "theory" in the inexact biological sciences. Biological Theory 7:298–310. https://doi.org/10.1007/s13752-012-0065-z

Griffiths GCD (1976) The Future of Linnaean Nomenclature. Systematic Zoology 25:168. https://doi.org/10.2307/2412743

Griffiths GCD (1974) On the foundations of biological systematics. Acta Biotheoretica 23:85–131. https://doi.org/10.1007/bf01556343

Haber MH (2014) In defense of the organism. Biology & Philosophy 29:885–895. https://doi.org/10.1007/s10539-014-9457-8

Haber, M. H. (2015). The biological and the mereological. In T. Pradeu & A. Guay (Eds.), *Individuals Across the Sciences* (pp. 295–316). Oxford University Press. https://doi.org/10.1093/acprof:oso/9780199382514.003.0016

Haber MH (2019) Species in the age of discordance. Philosophy, Theory, and Practice in Biology 11:. https://doi.org/10.3998/ptpbio.16039257.0011.021

Haber MH (2025a) Biology's Einstein moment: Specifying lineal frames of reference and rejecting absolute biological history. Biological Theory. https://doi.org/10.1007/s13752-024-00475-3

Haber MH (2025b) Introduction to "Formalization and the meaning of 'theory' in the inexact biological sciences". In: Scaffolding: Selected Contributions of James R. Griesemer to History, Philosophy, and Biology. Springer, Switzerland

Haber MH, Velasco J (2021) Phylogenetic Inference. In: The Stanford Encyclopeida of Philosophy. https://plato.stanford.edu/archives/win2021/entries/phylogenetic-inference/

Hennig W (1966) Phylogenetic Systematics. University of Illinois Press

Hennig W (1975) "Cladistic Analysis or Cladistic Classification?": A Reply to Ernst Mayr. Systematic Zoology 24:244. https://doi.org/10.2307/2412765

Hull DL (1966) Phylogenetic Numericlature. Systematic Zoology 15:14–17. https://doi.org/10.2307/2411498

Hull DL (1970) Contemporary Systematic Philosophies. Annual Review of Ecology and Systematics 1:19–54. https://doi.org/10.1146/annurev.es.01.110170.000315

James, W. (1899). *The Will to Believe*. Longmans Green and Co, New York.

Laudan L (1981) A Confutation of Convergent Realism. Philosophy of Science 48:19–49. https://doi.org/10.1086/288975

MacCord, K. (2025). Let's talk about sex . . . cell lineages. Biological Theory. https://doi.org/10.1007/s13752-024-00488-y

McTavish, EJ, Gerbracht, JA, Holder, MT, Iliff, MJ, Lepage, D, Rasmussen, PC, Redelings, BD, Reyes, LLS, Miller, ET (2025). A complete and dynamic tree of birds. Proceedings of the National Academy of Sciences, 122(18):e2409658122. https://doi.org/10.1073/pnas.2409658122

Miller, GA (1956). The magical number seven, plus or minus two: Some limits on our capacity for processing information. Psychological Review 63(2):81-97.

Millstein, RL (2010). The concepts of population and metapopulation in evolutionary biology and ecology. In Bell, M. A., Futuyma, D. J., Eanes, W. F., and Levinton, J. S., editors, *Evolution Since Darwin: The First 150 Years*. Sinauer Associates, Inc.

Müller-Wille S (2007) Collection and collation: theory and practice of Linnaean botany. Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences 38:541–562. https://doi.org/10.1016/j.shpsc.2007.06.010

Myers, P. 2001. "Reptilia" (On-line), Animal Diversity Web. Accessed April 5, 2026 at https://animaldiversity.org/accounts/Reptilia/

Nyakatura K, Bininda-Emonds OR (2012) Updating the evolutionary history of Carnivora (Mammalia): a new species-level supertree complete with divergence time estimates. BMC Biology 10:. https://doi.org/10.1186/1741-7007-10-12

O'Hara RJ (1997) Population thinking and tree thinking in systematics. Zoologica Scripta 26:323–329. https://doi.org/10.1111/j.1463-6409.1997.tb00422.x

Okasha, S. (2025). Does the anti-essentialist consensus about species rest on a mistake? *Australasian Journal of Philosophy*, 1–19. https://doi.org/10.1080/00048402.2025.2499919

Potochnik, A. (2017). *Idealization and the Aims of Science*. University of Chicago Press.

Raikow RJ (1986) Why are There so Many Kinds of Passerine Birds? Systematic Zoology 35:255. https://doi.org/10.2307/2413436

Rice, C. and Khalifa, K. (2025). Thank you for misunderstanding! Philosophical Studies. https://doi.org/10.1007/s11098-025-02311-1.

Rieppel O (2016) Willi Hennig as philosopher. In: The Future of Phylogenetic Systematics. Cambridge University Press, pp 356–376

Rieppel O (2009) Hennig's enkaptic system. Cladistics 25:311–317. https://doi.org/10.1111/j.1096-0031.2009.00260.x

Sober, E. (1984). Sets, species, and evolution: Comments on philip kitcher's "species." *Philosophy of Science*, *51*(2), 334–341. https://doi.org/10.1086/289183

Sokal RR, Sneath PHA (1963) *Principles of Numerical Taxonomy*.

Stevens, P. F. (1985). The genus concept in practice: But for what practice? Kew Bulletin, 40(3):457–465. http://www.jstor.org/stable/4109605

Stevens PF (1986) Evolutionary Classification in Botany, 1960-1985. Journal of the Arnold Arboretum 67:313–339. https://doi.org/10.5962/p.324748

Stevens PF (1997) How to Interpret Botanical Classifications: Suggestions from History. BioScience 47:243–250. https://doi.org/10.2307/1313078

Stevens PF (2001) Angiosperm Phylogeny Website. In: Angiosprem phylogeny website. http://www.mobot.org/MOBOT/research/APweb/. Accessed 30 Mar 2025

Stevens, P. F. (2002). Why do we name organisms? Some reminders from the past. *TAXON*, *51*(1), 11–26. https://doi.org/10.2307/1554959

Sullivan, E. and Khalifa, K. (2019). Idealizations and understanding: Much ado about nothing? Australasian Journal of Philosophy, 97(4):673–689.

Varma CS (2016) Hennig and hierarchies. In: The Future of Phylogenetic Systematics. Cambridge University Press, pp 377–409

Watkins A, DiMarco M (2024) Sex eliminativism. Biology & Philosophy 40:2 https://doi.org/10.1007/s10539-024-09972-y

Wiley EO (1979) An Annotated Linnaean Hierarchy, with Comments on Natural Taxa and Competing Systems. Systematic Zoology 28:308. https://doi.org/10.2307/2412585

Wilkins JS, Zachos FE, Pavlinov IYa (2022) Species Problems and Beyond: Contemporary Issues in Philosophy and Practice. CRC Press

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

Winston JE (1999) Describing Species: Practical Taxonomic Procedure for Biologists. Columbia University Press

Winston JE (2018) Twenty-First Century Biological Nomenclature—The Enduring Power of Names. Integrative and Comparative Biology 58:1122–1131. https://doi.org/10.1093/icb/icy060

Winther RG (2021) The Structure of Scientific Theories. In: The Stanford Encyclopedia of Philosophy. https://plato.stanford.edu/archives/spr2021/entries/structure-scientific-theories/

Zrzavý J, Řičánková V (2004) Phylogeny of Recent Canidae (Mammalia, Carnivora): relative reliability and utility of morphological and molecular datasets. Zoologica Scripta 33:311–333. https://doi.org/10.1111/j.0300-3256.2004.00152.x