# Biological essentialism and the tidal change of natural kinds

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## Abstract

The vision of natural kinds that is most common in the modern philosophy of biology, particularly with respect to the question whether species and other taxa are natural kinds, is based on a revision of the notion by Mill in *A System of Logic*. However, there was another conception that Whewell had previously captured well, which taxonomists have always employed, of kinds as being types that need not have necessary and sufficient characters and properties, or essences. These competing views employ different approaches to scientific methodologies: Mill’s class-kinds are not formed by induction but by deduction, while Whewell’s type-kinds are inductive. More recently, phylogenetic kinds (clades, or monophyletic-kinds) are inductively projectible, and escape Mill’s strictures. Mill’s version represents a shift in the notions of kinds from the biological to the physical sciences.

## Introduction

*Essentialism* is a term that has many meanings. One that most commentators agree upon is that to be a *natural kind*, there must be an essence of some kind that all of the instances or members of that kind have in common.[[2]](#endnote-2) Another is that there must be generalizations true of all members of the kind, preferably lawful or lawlike ones. Moreover, a specific meaning in the philosophy of biology is that to be a natural kind, essences must comprise necessary and sufficient properties that can be defined or discovered. In other sciences, essences are held to be (physically) *necessary* properties as well, but in the case of biological kinds, of *taxa* which are historically contingent, it is the presence or ownership of the properties that can be listed as *characters* or *traits* that makes natural kinds. In what follows, I am concerned with taxonomic essentialism and natural kinds.

Nearly everyone agrees that biological kinds lack essences, or that at best they are convenient fictions. It is widely asserted that Darwin himself held that species were conventional fictive kinds, and the Individuality thesis of Ghiselin and Hull further asserts that species are in fact not kinds at all, but individuals. As we know that individuals lack essences, this marks the death knell of essentialism.[[3]](#endnote-3)

However, the history upon which this truism rests, in my view and that of others, is itself a fiction, written in good faith but based on a misreading of the sources. Historian Mary P. Winsor has dubbed this the “essentialism story” (Levit and Meister 2006, Winsor 2003, 2006). In this paper, I propose to consider what sorts of natural kinds naturalists and pre-Darwinian philosophers actually *did* think species and other biological class concepts were, and what philosophical implications this might have, especially in the light of a couple of recent attempts to “reintroduce” essentialism into biology (Devitt 2008, Walsh 2006).[[4]](#endnote-4) My approach can be called “historical-philosophical”, in that a revision of the history leads to reframing some of the philosophical issues and suggests some resolutions of long-opposed views in the philosophy of biology.[[5]](#endnote-5)

## The method of exemplars

The essentialism story holds that before Darwin, naturalists were required by Aristotelian logic to treat natural kinds as essences (Hull 1982)[[6]](#endnote-6), and there is a kernel of truth to this. For instance, in the middle ages, philosophers often spoke as if sortal terms, as Locke later called them, had necessary properties *de re*. And in the logical literature deriving from the commentaries and writings of the later middle ages, which directly influenced later logic right up until the turn of the twentieth century, the terms for class concepts were *genus*, which was any general predicate that had a definitional essence, and *species*, which was a more specific predicate in which members of the genus were able to be differentiated. The lowest species was the *infima species*, which had only individual objects under it, which had no “essence” other than the *infima species*’ – in other words, individuals lack essences. It looks as if the essentialism story had it right: genera and species were definitionally essentialistic.

The mistake here arises in conflating the uses of the terms *genus* and *species* in *logic* with their uses as terms of art in other areas, in particular in natural history or, later, biology. The reason why these terms were reused is that they were in fact vernacular terms of Latin, which, as we all know, was the lingua franca of the medieval and subsequent early modern period, particularly amongst the educated classes from who scientists were usually drawn. This modern confusion over these two uses was not common in the contemporary philosophical community. Locke, Mill, Whewell, Whately, Jevons, and various others all made note of the different meaning of species in the philosophical logical sense to the naturalist’s sense.

Locke, who writes more than a century earlier than Darwin, before there even *is* a technical meaning in natural history for these terms[[7]](#endnote-7), nevertheless manages to deflate their technical meaning outside logic and metaphysics:

The learning and disputes of the schools having been much busied about genus and species, the word essence has almost lost its primary signification: and, instead of the real constitution of things, has been almost wholly applied to the artificial constitution of genus and species. It is true, there is ordinarily supposed a real constitution of the sorts of things; and it is past doubt there must be some real constitution, on which any collection of simple ideas co-existing must depend. But, it being evident that things are ranked under names into sorts or species, only as they agree to certain abstract ideas, to which we have annexed those names, the essence of each genus, or sort, comes to be nothing but that abstract idea which the general, or sortal (if I may have leave so to call it from sort, as I do general from genus), name stands for. And this we shall find to be that which the word essence imports in its most familiar use. [Bk III, chap. III, §15]

Locke also notes that the terms as used in science, or natural philosophy, cannot be treated as the same as in ordinary pre-scientific discourse, and makes the point about the Latin terms being merely idiomatic:

*This shows Species to be made for Communication*.—The reason why I take so particular notice of this is, that we may not be mistaken about *genera* and *species*, and their *essences*, as if they were things regularly and constantly made by nature, and had a real existence in things; when they appear, upon a more wary survey, to be nothing else but an artifice of the understanding, for the easier signifying such collections of *ideas* as it should often have occasion to communicate by one general term; under which divers particulars, as far forth as they agreed to that abstract *idea*, might be comprehended. And if the doubtful signification of the word *species* may make it sound harsh to some, that I say the species of mixed modes are “made by the understanding”; yet, I think, it can by nobody be denied that it is the mind makes those abstract complex *ideas* to which specific names are given. And if it be true, as it is, that the mind makes the patterns for sorting and naming of things, I leave it to be considered who makes the boundaries of the sort or *species*; since with me *species* and *sort* have no other difference than that of a Latin and English *idiom*. [*Essay,* Bk III, chap. V, §9]

Locke’s comments presage a debate in natural history, and he is very influential later on both the natural historians and taxonomists, and philosophers who considered taxonomy, but it is not the case that his *species* are in any way those of biology. They refer instead to his discussion of General Names or Ideas. That later philosophers understood the difference between logical and biological kind terms is clear from the following comments:

Mill, *System of Logic*, 1.vii.4 (Mill 2006a): “… the word species is used in a different signification in logic and in natural history. By the naturalist, organized beings are not usually said to be of different species, if it is supposed that they have descended from the same stock. That, however, is a sense artificially given to the word, for the technical purposes of a particular science.”

Whewell, *Philosophy of the Inductive Sciences* vol 2: 459 (Whewell 1840): “The "Five Words," *Genus*, *Species*, *Difference*, *Property*, *Accident*, were used by the Aristotelians, in order to express the subordination of kinds, and to describe the nature of definitions and propositions. In modern times, these technical expressions have been more referred to by Natural Historians than by Metaphysicians …

“It has already been shown that even geometry is not founded upon definitions alone: and we shall not here again analyse the fallacy of this belief in the supreme value of definitions. But we may remark that the study of Natural History appears to be the proper remedy for this erroneous habit of thought. For in every department of Natural History the object of our study is kinds of things not one of which kinds can be rigorously defined, yet all of them are sufficiently definite. In these cases we may indeed give a specific description of one of the kinds, and may call it a definition; but it is clear that such a definition does not contain the essence of the thing.”

W. Stanley Jevons (1878), in perhaps the last significant philosophical discussion of classification in English for some time, held that classifications were based on “focusing” on more and more specific groups that resembled each other. And in the work that revitalized English-speaking interest in the field of logic, Archbishop Richard Whately wrote, in 1826, even before Darwin had graduated from Cambridge, that *species* in logic were *explicitly* not the same as the naturalists’ term or concept:

... if anyone utters such a proposition as ... “Argus was a mastiff,” to what head of Predicables would such a Predicate be referred? Surely our logical principles would lead us to answer, that it is the *Species*; since it could hardly be called an Accident, and is manifestly no other Predicable. And yet every Naturalist would at once pronounce that Mastiff, is no distinct Species, but is only a *variety* of the Species Dog. ...

... the solution of the difficulty is to be found in the peculiar technical sense ... of the word “Species” when applied to *organized Beings*: in which case it is always applied (when we are speaking strictly, as naturalists) to individuals as are supposed to be *descended from a common stock*, or which *might* have so descended; *viz*. which resemble one another (to use M. Cuvier’s expression) as much as those of the same stock do. (p183)[[8]](#endnote-8)

Whately is clearly relying on the “standard” received notion of natural *species*, which at the time for most educated readers meant Cuvier’s definition in his *Révolutions de la surface du globe*, the “third” edition of which had been published the year before (Cuvier 1825).[[9]](#endnote-9)

## Philosophy and Biological Kinds

Still, we should be more interested in how these philosophers treat *biological* kinds. There is a history of philosophers using biological *examples* when discussing kind terms, usually in the context of definitions in logic (cf. Eco 1999), but until the nineteenth century, there were few cases in which English speaking philosophers closely attended to the practices and notions of the naturalists’ taxonomy.[[10]](#endnote-10) This makes Whewell particularly interesting, as he effectively gives an account in which what is going on in natural classification is rather more like Wittgenstein’s family resemblance predicate than the modern essentialistic account of natural kinds. He writes, in volume 2 of the *Philosophy of the Inductive Sciences* in 1840, which deals with the life sciences, and in opposition to the traditional definitional notion of *species* in logic:

But it may be asked, if we cannot define a word, or a class of things which a word denotes, how can we distinguish what it does mean from what it does not mean? How can we say that it signifies one thing rather than another, except we declare what is its signification? The answer to this question involves the general principle of a natural method of classification which has already been stated and need not here be again dwelt on. It has been shown that names of *kinds* of things (*genera*) associate them according to total resemblances, not partial characters. The principle which connects a group of objects in natural history is not a *definition* but a *type*. Thus we take as the type of the Rose family, it may be the common *wild* rose; all species which resemble this flower more than they resemble any other group of species are also *roses*, and form one *genus*. All genera which resemble Roses more than they resemble any other group of genera are of the same family. And thus the Rose family is collected about some one species which is the type or central point of the group.

In such an arrangement, it may readily be conceived that though the nucleus of each group may cohere firmly together, the outskirts of contiguous groups may approach, and may even be intermingled, so that some species may doubtfully adhere to one group or another. Yet this uncertainty does not at all affect the truths which we find ourselves enabled to assert with regard to the general mass of each group. And thus we are taught that there may be very important differences between two groups of objects, although we are unable to tell where the one group ends and where the other begins; and that there may be propositions of indisputable truth, in which it is impossible to give unexceptionable definitions of the terms employed.

15. These lessons are of the highest value with regard to all employments [*sic*] of the human mind; for the mode in which words in common use acquire their meaning, approaches far more nearly to the *Method of Type* than to the method of definition. (Whewell 1840, vol 2: 517–519)

The fact that this is presented without further argument, and in contrast to the way the terms are used in philosophy, should be instructive. Whewell initially argues that *unscientific* terms lack definitions, but he extends this a page later to natural history:

We may further observe, that in order that Natural History may produce such an effect [to correct “the belief that definitions are essential to substantial truth”], it must be studied by inspection of the *objects* themselves, and not by the reading of books only. Its lesson is, that we must in all cases of doubt or obscurity refer, not to words or definitions, but to things. The Book of Nature is its dictionary: it is there that the natural historian looks, to find the meaning of the words which he uses … . So long as a plant, in its most essential parts, is more like a rose than anything else, it is a rose. He knows no other definition. (pp519-520)

In point of fact, Whewell is merely repeating the views widely held by naturalists since Linnaeus (ably described by Stevens 1994). As someone once noted[[11]](#endnote-11), if Whewell says it, then it is the received view of the day. Systematists had held to something like a plenum view, as Stevens has shown, in which there was an increasing number of empty “spaces” in the taxonomic continuum for as time went on and more species were described, leaving unfilled intermediates. By the early 1830s, the notion that there was a type *species* in every genus, and a type *genus* in every family, was common. Mill himself says as much in the *System of Logic* (IV.vii.3, where he extensively quotes from Whewell’s *History of the Inductive Sciences*, vol II, 120-122).[[12]](#endnote-12) So by the time Mill writes the *System of Logic*, the method of type is well established. Mill is the one making a novel account, not Whewell.

Where does the error of confusing the logical and natural technical meanings of these terms originate? Oddly, it appears to have been John Dewey’s essay of 1909 (Dewey 1997), “The influence of Darwin on philosophy”, in which he attacks finalism and fixism supposedly in Aristotle, but mentions essences only once: “the logic that explained that the extinction of fire by water through the formal essence of aqueousness” (p14).[[13]](#endnote-13) I say oddly because Dewey himself was aware of the distinction. It certainly was also observed in the last great logical text in the pre-symbolic tradition, published a few years earlier in 1906 (second edition: Joseph 1916). While Dewey himself does not apply the Aristotelian essentialism of *species* to the biological, he noted that “Few words in our language foreshorten intellectual history as much as does the word species”[[14]](#endnote-14), thus suggesting that the logical meaning is to be considered the same as the naturalists’ meaning. Later, Cain, Simpson and Mayr took Dewey’s statement literally in the construction of the essentialism story (see McOuat 2001, Wilkins 2009, Winsor 2006), and Hull (1965) reinforced it.

## Kinds – types or not?

Whewell’s *type-kinds* – the term *typological* has other, unfortunate, connotations and uses, especially in biology – is a classical example (one might even say the exemplar) of the philosophy of biology that Winsor has called the “method of exemplars” that practically every naturalist and systematist held until fairly recently (Winsor 2003). Ernst Mayr uses *typological* and *essentialist* as synonyms, but since Paul Farber’s work in the 1970s, it is clear this is not the case (Farber 1976).[[15]](#endnote-15) However, to call a classification “typological” in modern systematics is to dismiss it; hence one cannot use that venerable term without undue connotations. We could follow Hacking’s example (Hacking 1991) and call these “Whewell-kinds (in contrast to Mill-kinds) but I want something descriptive. So “type-kinds” or “class-kinds” it is. I will later also distinguish between two kinds of class-kinds, the property-based and the definitionally-based.

According to the notion of type-kinds, one finds an exemplary specimen for any taxon and organizes the rest of the group around it. The term *taxon* itself is a very late introduction, although the term *taxonomy* precedes it by at least a century (Mayr 1978, Meyer-Abich 1926, Winsor 2000), but I use it here to avoid using terms like “class” and “group” which immediately present bias to one or another view in this debate.

On this account, there are exemplary species in a genus, genera in a family, and so on (Whewell held that the “unit” taxon was the species, however, and he did not think that a *species* was defined by a type, so far as I can tell, although it was an obvious conclusion from the problem of intermediate forms). How the exemplar is to be recognized is a matter of the abilities of the specialist, a view that later became institutionalized in the Strickland Rules (McOuat 2001).

Mill disagreed:

We form our groups round certain selected Kinds, each of which serves as a sort of exemplar of its group. But though the groups are suggested by types, I cannot think that a group when formed is *determined* by the type; that in deciding whether a species belongs to the group, a reference is made to the type, and not to the characters; that the characters “cannot be expressed in words.” This assertion is inconsistent with Dr. Whewell’s own statement of the fundamental principle of classification, namely, that “general assertions shall be possible.” If the class did not possess any characters in common, what general assertions would be possible respecting it? Except that they all resemble each other more than they resemble anything else, nothing whatever could be predicated of the class. [*System of Logic* IV.vii.4 (Mill 2006b, 720-721)]

Mill’s disagreement with Whewell marks a tidal change from taking taxonomy in natural history seriously to philosophy attempting to define *a priori* what a scientific class must be. His criticism of the “fundamental principle of classification” is most telling. Mill thinks that one cannot make general claims without knowing the defining properties. Mill thought that induction was the only way in which truths are ascertained, but that classification is, as the section title has it, an operation of the mind "subsidiary to induction". Hence the definitional aspect of classification comes to the fore.[[16]](#endnote-16) For Whewell, on the other hand, classes are formed inductively, and for him types have no necessary and sufficient properties.

There is a “taxonomic fork” here. The modern literature on natural kinds takes the Millian tine of the fork, while the Whewellian, and I think cladistic, approach to classification is that it is the *outcome* of inductive generalization (Whewell’s *consilience*, a method later called *abduction* by Peirce, is a method of forming a general prediction). Mill thinks that to make generalizations, a set of properties have to be shared by all (and only) the members of the class. Whewell thinks that to make generalizations, types serve as anchor points for our taxonomic descriptions. Modern cladistics, especially in the way phylogenetics was set up by Rosen, Patterson, Nelson and Platnick, holds that a phylogeny permits inductive generalizations without the clades being definitionally tight, and so their position fits in between Whewell and Mill, as will be discussed later.

## Essential inference

It has become something of a cliché to say that something is not a natural kind as a way to deny that it is a correct way of “cutting nature at its joints”, as Plato has Socrates say in the *Phaedrus* (265d–266a)[[17]](#endnote-17). Targets have included color terms (Maund 2008), emotions (Barrett 2006, Barrett et al. 2007, Charland 2002, Griffiths 2004), and of course, taxa, especially species (Dupré 1981). This concern equally exercised the early nineteenth century taxonomists and philosophers. Whewell spent a considerable amount of his discussion on what made a classification “natural”, and the naturalist William Swainson, in two influential books (1834, 1835), also had long discussions. All agreed then, and it seems agree now, that if we are to have *natural* kinds, we must ensure that our classes are able to divide the world appropriately so that we can make general claims. If emotions, say, are not natural kinds, but instead represent a social consensus, then investigating particular emotions will systematically (or, rather, unsystematically) mislead us, since the underlying etiologies are not being considered as wholes.

Mill’s tine of the fork supposes that we merely establish *terms* around types, and that when we have a proper understanding of the kind, we are able to give a full account of all and only those properties that cause the types denoted by the terms to come into focus (and which may force a revision of the terms). Whewell’s tine supposes that taxonomic types are largely irreducible facts about the world, and that definitions are arid. This is an epistemic dispute. Taxonomists then, as now, realized that there was considerable variation in their taxa, with intermediate forms causing all kinds of trouble in classification (an anecdote about a student of Agassiz’s, Stimpson, has him grinding intermediate forms of shells to dust with his heel, as a solution, Simpson 1925, 178f); setting up types as exemplars enabled them to organize data that was variable, and sometimes vague. Mill supposes that if there is a *natural* kind, although that phrase arose later, perhaps with Venn’s *Logic of Chance*, (Hacking 1991, Venn 1866, 181),[[18]](#endnote-18) then that natural kind must have necessary *causal* properties that makes it what it is. Hence, Mill replaces the arid traditional, definitional, predicate-based notion of (logical) kinds by a causal notion that can be defined in some ideal scientific account. In many ways, as Hacking notes, this goes back to the gloss Leibniz made in the *New Essay* on Locke’s *Essay*. Locke supposed that nominal kinds are made for convenience of communication and that “real essences” are hidden from us (*Essay* III.v.9, III.vi.25). Leibniz agrees with the first point, but disagrees with the second, holding that, given enough time, scientists will indeed be able to achieve knowledge of the real essences of things (Leibniz 1996, 319).[[19]](#endnote-19)

The notion that species are or could be natural kinds, and by extension groups that species comprise could be too, depends upon several not entirely consistent preconceptions. Before “natural kind” developed its particular modern meaning as a set that was, in effect, defined intensionally and by necessity, all taxonomists held that species were kinds, and kinds of nature. Linnaeus himself asserted that *genus* and *species* were the work of God, and all else the work of men. But this does not mean that anyone, so far as I can tell, held that species necessarily have essential properties, as I argue in my 2009. Moreover, importantly, taxonomists consciously and explicitly distinguished between the “marks” or “characters” by which we *recognize* or *diagnose* species and larger taxa (which they often refer to as the “essential characters”, *vide* Macleay (1819, 10n)[[20]](#endnote-20), and the *ontological* “what-it-is-to-be” that made them thus. Diagnostic essences were *themselves* conventional, in the sense of being convenient for communication and instrumental action like field identification.[[21]](#endnote-21) So in terms of capturing the actual behavior of taxonomy in natural history, Whewell had the right of it, and paid closer attention to naturalists’ intents and goals, than Mill did. Mill’s is something of a philosophical invention, based on taking the logic he had learned from his father and the Benthams more seriously than the biology. [Ironically, as McOuat has shown (McOuat 2003), Jeremy Bentham’s nephew, George Bentham, before he became a leading botanist, had published a logical text (Bentham 1827) critiquing Whately and attempting to reform logic entirely to support the utilitarian and radical political educational program[[22]](#endnote-22), but when he later did his botany, those principles of the “five words” in logic were not present, and he followed Linnaean principles, and taxonomic convention (e.g., Bentham 1832-1836, Bentham and Hooker 1920).]

To an English-speaking taxonomist at the time, some classification is “natural” if it groups taxa in a way that is objective and independent of the needs and predilections of the taxonomist.[[23]](#endnote-23) There was extensive debate over this in the early nineteenth century, and no real agreement as to what *made* this or that system “natural”, [[24]](#endnote-24) but nobody I know before Mill suggested that it was because we had not found the “essences” of the taxa. By contrast, in chemistry, geology and later unification through the periodic table in physics, kinds of minerals, elements and compounds were regarded as having some naturalness if there were essential constitutions such as atomic number. What we now call the “microstructural” account of natural kinds (LaPorte 1996) was developed in the context of the general sciences of chemistry and physics. And this is significant, I think, as it points out two rather distinct activities that get called “classification” in science, and by extension distinct things that get called “classes” in the philosophies of science.

One type of classification, if we may be metalevel about it, is to do what Mill and practically every commentator on natural kinds since, including the critics, have done and specify that a natural kind is characterized by the objects included in the kind possessing *necessary and sufficient properties*, which (in principle) can be listed in a definition of the kind (here called *class-kinds*). Hence, the essence of gold is the atomic number 79, the essence of water is H­2O, and the essence of intelligence is some rational capacities, and so on. It is in this sense that the Individuality Theorists Michael Ghiselin and David Hull (Ghiselin 1974, Hull 1976) argued that species are not classes, or natural kinds. Hull characterizes species essentialism, for example, as “each species is distinguished by one set of essential characteristics. The possession of each essential character is necessary for membership in the species, and the possession of all the essential characters sufficient” (Hull 1994, 313). This is correct, if we take Mill-kinds, as Hacking called them, as the model of what a natural kind is, as *property class-kinds*. Incidentally, this is distinct from the logical tradition’s view of essentialism in play before the nineteenth century, where kind terms were *definitional class-kinds*. We can tabulate this:

|  |  |
| --- | --- |
| *Type-Kinds*or exemplary kinds | *Class-Kinds* |
| *Definitional*, or logical classes | *Property-Based*, or necessary causal classes |

Conflating these two notions has caused a lot of confusion in the interpretation of the historical literature. Mill himself notes the distinction:

There can be no doubt that when the schoolmen talked of the essences of things as opposed to their accidents, they had confusedly in view the distinction between differences of kind, and the differences which are not of kind; they meant to intimate that genera and species must be Kinds. Their notion of the essence of a thing was a vague notion of a something which makes it what it is, i.e. which makes it the Kind of thing that it is—which causes it to have all that variety of properties which distinguish its Kind. But when the matter came to be looked at more closely, nobody could discover what caused the thing to have all those properties, nor even that there was anything which caused it to have them. Logicians, however, not liking to admit this, and being unable to detect what made the thing to be what it was, satisfied themselves with what made it to be what it was called. Of the innumerable properties, known and unknown, that are common to the class man, a portion only, and of course a very small portion, are connoted by its name; these few, however, will naturally have been thus distinguished from the rest either for their greater obviousness, or for greater supposed importance. These properties, then, which were connoted by the name, logicians seized upon, and called them the essence of the species; and not stopping there, they affirmed them, in the case of the *infima* *species*, to be the essence of the individual too; for it was their maxim, that the species contained the “whole essence” of the thing. Metaphysics, that fertile field of delusion propagated by language, does not afford a more signal instance of such delusion. [I.vii.5, p126f]

By contrast, Mill held that individuals had names that denoted them, but no essences. And he does notice that biological species have a nomenclature.

On the property-class kinds approach, which Mill expounds in IV.vii.4 (p721), one begins with the *infimae species*, or lowest kinds, and builds larger more inclusive kinds by aggregation. Mill’s criterion for inclusion, based on deduction from observations, is more than resemblance – it must be based on necessary and sufficient properties, and the hope is that when we know what we need to know about these classes, we will know what the causes of the class are. This relies upon his view of science as being an attempt to at least in part deductively derive consequences from known laws (III.6.1), which are themselves acquired through inductive processes. Whewell has the inverse view, in which induction *leads to* classifications based on resemblance, from which general claims can be made. Neither author, at that point in the debate, can engage with Owen’s definition of *homology* made a little later (Owen 1843, 374), which replaces resemblance with identity under all forms (“**Homologue.** The same organ in different animals under every variety of form and function"), but more of that anon[[25]](#endnote-25). Whewell took Mill to be agreeing with him in a response that considered only the physical sciences (Whewell 1849, 85), where, having noted that Mill replies on deduction from previously known laws, while he relies on induction and observation to new classes of facts, he says that Mill rightly takes “classes, in which we have not a finite but an *inexhaustible* body of resemblances among individuals, … as groups made by nature…”. I suspect this is faint praise, given that Mill does not think such classes are very useful in science.

## The role of classification in science

Some time around the late 1870s, classification as a way of organizing natural divisions in the world ceased to be discussed very much by philosophers.[[26]](#endnote-26) One of the last discussions of natural classification can be found in W. Stanley Jevon’s *Principles of Science* (Jevons 1878). Until the debates over phenetics and cladistics in the late 1960s (cf. Hull 1979, 1984, 1988), there was little interest from philosophers about the specifics of this activity in science for nearly a century[[27]](#endnote-27). Popper, although he has been cited by many taxonomists in support of their methodologies, has only two rather brief dismissive mentions of classification (as noted by Hull 1988, 252, Popper 1957, §27, 1959, 65), all in the style of Rutherford’s famous remark “in science there is only physics; all else is stamp collecting”.[[28]](#endnote-28) Apart from work on library classification, which is a matter of convention rather than a natural grouping of things, the subject appeared to have dropped off the radar of philosophy. Why this is, I think, is indicated by Rutherford’s quip: the exemplar of what science was held to be, was modeled entirely upon physics, and physics, it was held, had no place for taxonomy.[[29]](#endnote-29) All natural kinds in physics were derived from successful theories – it appears that the development of the periodic table was popularly revised to ignore its inductive origins (e.g., Hettema and Kuipers 1988, a much better account is Scerri 2007).

But at best this applies solely to so-called “*general*” sciences, and it requires that the discipline that covers the domain be advanced sufficiently to have working theories from which to derive these kinds. “*Special”* (or as Whewell called them, the *paletiological*, or “past cause”) sciences, which for my purpose are those in which the general kinds are formed by contingent historical events at particular times and places, do not seem to have these theory-derived class-kinds. Instead, the Mill–Whewell debate is over how such kinds as occur in historical sciences are to be formed. Mill thinks the scientific process is to inductively generate hypotheses, form classes based on these, and then deduce consequences. This is very like Quine’s theory-relative ontology (Quine 1948, reprinted in Quine 1953) and in effect under the Mill-Quinean view, classifications are just classes of objects that the theory requires. The type-kind approach exemplified by Whewell, on the other hand, presumes that induction forms not the theory, but the classification, by noting types and organizing taxa around them, and *then* deriving theories that account for these type-kinds. What doesn’t seem to have been proposed is that *both* operations are useful in science, and neither takes either temporal or epistemic priority over the other.

In his essay “Natural Kinds” (Quine 1969a), Quine argued that what calls for explanation, in the light of Goodman’s “new riddle of induction” (Goodman 1954), is why our classifications match the things they classify, and famously gave an account based on the evolution, first of our quality spaces (“Creatures inveterately wrong in their inductions have a pathetic but praiseworthy tendency to die before reproducing their kind.” [page 13]), and then of our scientific models that deliver to us our natural kinds, in an early version of what has come to be known as the teleosemantic account of meaning (Millikan 1984). This is rather like Mill’s view of how classification proceeds, except that where Mill held that the classes are formed by enumerative induction, giving us theories from which we deductively draw inferences and explanations, Quine holds that this occurs by selective processes at the biological and cultural (scientific) levels.

But while the latter process may explain how we come to have more or less *natural* – in the sense of being structurally similar – classifications, it doesn’t in fact explain why we have *classifications*, for Quine holds that ontologies are derived from theories, rather than from classification schemes (in the title essay of Quine 1969b) – to be is to be the value of a bound variable. In this view, classification falls out of formal models and theories, which is very different from the traditional view of the nineteenth century. How is a classification related to a theory inductively, under Quine’s approach? It isn’t. It is related deductively. Induction gives the theory, and deduction gives the classes. These classes are as essentialistic as anything can be – if there are electrons in the Standard Model, they are particles with a certain mass, charge and other properties called for by the model, and all electrons have these properties necessarily, and nothing else does (or it *is* an electron).

With the arrival into biological systematics of what is called *phylogenetics*, and more popularly *cladistics*, an interesting and different approach came about, one that may resolve the conflict between the two kinds of kinds. Under the phylogenetic approach, one arranges taxa[[30]](#endnote-30) into *monophyletic groups* (or *clades*) which are interpreted as branches of the evolutionary tree with all descendants of a single species being included (the property is *monophyly*). The resulting clades in effect summarize a quorum of the data. Importantly, that data must be *homologous*, not *analogous* (or, in systematics terminology, *homoplasious*)[[31]](#endnote-31). Using homologies alone as the basis for grouping species together permits some interesting inferences to be made. If one has a group based on a definition of some functional property, say *flight* or *predation*, what can be said in advance about some as yet unknown member of the class-kind? You know nothing more, nor can you even inductively infer more, than the definition of the class itself. I tell you unknown species *X* is a predator, and you know that it eats other species, and presumably has adaptations for that purpose. Nothing else can be said. But, as Gareth Nelson noted many years ago (Nelson 1978), if I tell you that *X* is a feline, then you know that it has a lot of shared traits with other known felines, including molecular traits like particular enzymes. Some of these inferences may not be quite right – *X* may have a derived *form* of the enzyme, or may have secondarily lost it, but overall one is warranted in making inferences about unknown taxa on the basis of clades. Likewise, if I tell you that an organism is a Raptor, you have a wealth of information already available to you about the beak structure, claw structure, feathers, anatomy, physiology and genetics of that bird just on the basis of knowledge of its nearest relatives, even before it is studied.[[32]](#endnote-32)

Phylogeny is thus a kind of “straight rule” for inductive projectibility (Goodman 1954) – it makes inductive generalization possible over kinds of organisms that share evolutionary history.[[33]](#endnote-33) However, it is important to note that this does not depend upon “similarity” as such, but on the *identity* between homologues “under every variety of form and function”.[[34]](#endnote-34) Clades are formed, methodologically, by grouping as many homologies as possible, with as few homoplasies/analogies. This permits inferences on the assumption that plesiomorphies, or ancestral states, are mostly conserved, and that only a few traits are modified. This is sometimes called “generative entrenchment” (Wimsatt 1986) or “phylogenetic inertia” (Blomberg and Garland 2002). Where too many traits are modified, then even though there are homologies, they aren’t obvious enough for taxonomists to form their clades. It is possible there are clades we cannot identify (as yet), and hence inductive generalizations we cannot make.[[35]](#endnote-35) But where we can, these generalizations are possible, and so Mill’s “fundamental rule of classification” does not require property or definitional class-kinds.

## Deviant essences and individuals

Several “essentialist” accounts have been proposed in recent times for biological taxa. Two specific proposals of note are Richard Boyd’s *homeostatic property cluster* account (Boyd 1999) and Paul Griffith’s *historical essence* account (Griffiths 1999), and recently a more general account by Michael Devitt, *intrinsic biological essentialism* (IBE, 2008). How does the revision of the history and concerns regarding biological natural kinds affect the arguments made here, and what impact does it have on the Individuality Thesis? Let us begin with the Individuality Thesis.

The claim made by Ghiselin and Hull was that species are not class-kinds. To be sure, they thought that the pre-evolutionary view was that species *were* class-kinds, which I reject, but that doesn’t affect their argument that species are in fact particulars, spatiotemporally restricted contiguous objects that are unique in evolutionary history. Whether or not *anyone* thought species were class-kinds at all, the argument that they are does not depend upon eliminating the view that species have definitional or property criteria. It depends upon the positive argument that species that evolve have beginnings and endings. Unfortunately, later versions of the Individuality Thesis committed it to the view that species were functionally coherent individuals, relying on a version of the “biological” species concept (cf. Ghiselin 1997). A species can be a metaphysical particular without being functionally integrated. Another formulation is to say that to some degree, individual organisms within a species are, as Templeton put it, demographically replaceable (Templeton 1989). Since functional integration usually requires differentiation of parts rather than homogeneity, a species need not be an integrated individual. In other words, species can be individuals and yet form kinds, because to a first approximation individual organisms are indiscernibly different, at least ecologically. The indiscernibility of members is a key characteristic of a kind. At any rate, species-as-metaphysical-particulars, and as historical objects, remains untouched by the distinction between type-kinds, class-kinds, and clade-kinds.

Essentialisms of a non-taxonomic but explanatory kind have been offered, a recent example being Dennis Walsh’s “adaptive essentialism” approach (Walsh 2006). Here, and in Michael Devitt’s *intrinsic biological essentialism* (Devitt 2008), the emphasis is on the developmental “natures” of the organisms. This is a rather benign form of essentialism – that there are underlying causal processes – including but not restricted to genes, parental investment, ecological niches, constructed niches, social inheritance and the like, that make a typical member of the species, well, typical – is not at issue. What it cannot be is a Millian class kindship, which is what Devitt’s version requires (although I cannot convince him he does not need it). This sense of essence effectively *is* a type-kind: the essence of the species is just the developmentally typical lifecycle (which is in fact a tautology, as the species would not *be* a species if it lacked at least one typical developmental lifecycle, however that might play out as reaction norms in different environments). Consequently, Boyd’s *homeostatic property cluster* (HPC) account doesn’t provide a malignant essence either. However, one might say that HPCs, being causally maintained, can only apply to populations that are in constant (enough) causal contact, which supports the “metapopulational” account of Kevin de Queiroz (2007); higher taxa above the metapopulation level cannot be maintained by HPC kinds. If a particular species does not comprise a metapopulation, then it cannot be a HPC kind, and clades that are not in causal contact (say, because they are isolated biogeographically) cannot be HPC kinds either.

Finally, there is Paul Griffiths’ *historical essence* account (1999). On this, species have essences in virtue of a shared genealogy, which is consistent with, for example, the genealogical concordance view of species of Avise and Ball (1990). This “essence” is not really an intrinsic essence, but one of genealogical relations between individuals and populations distributed over time. As such it is quite consistent with Whewellian type-kinds in a cladistic manner, because it is caused by conserved developmental homologies.

## Conclusion

If the revision of kind concepts by Mill is as I have presented here, it means that a problem with natural historical kinds – species and other taxa – arose because an older sense was gradually abandoned in favor of the senses that apply in the general sciences only. Of course there is a problem, since the general sciences are not the special sciences, or vice versa. If one is a reductionist, then one might strive to find the microstructure of biological kinds in terms of physics and/or chemistry; but if there are no common physical properties in all biological kinds (and I suspect there are not), then a physicalist might infer that there are no such kinds in biology, and they would subsequently be held to evaporate. However, that begs the very question at issue. Clearly, to any biologist, there *are* some kinds in biology (they may or may not be taxa). If the argument of the physicalist is that these kinds are somehow unreal because they do not map onto a physical kind based on theory, all historical kinds must likewise bet held to be unreal. To ask that of natural history is, I think, a rather onerous claim to make, and is based on a wrong notion of what makes a science.

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1. Philosophy, University of Sydney; email: john@wilkins.id.au . [↑](#endnote-ref-1)
2. Of course, non-natural kinds, like *pencil* can also have essences, as Michael Devitt pointed out to me (see his Devitt 2005). [↑](#endnote-ref-2)
3. Which of course is not unchallenged in modern philosophy of language and logic. Kripke and Plantinga, among others (e.g., Parsons 1969), permit individual essences, although Hull (Hull 1977) noted that with respect to taxa, this would mean that any object that had the same individual essence would need to be the same object (which is, of course, Leibniz’s principle of the identity of indiscernibles). [↑](#endnote-ref-3)
4. Since I hold there never was any kind of *taxonomic* essentialism in natural history, of course this is an *introduction*, not a *re*introduction. And it is not clear to me that Devitt’s version is taxonomic. [↑](#endnote-ref-4)
5. Gordon McOuat covers much the same territory I do here, although he tends to take a more political view than I do (McOuat 2009). I believe he is correct that underlying much of this debate is concern over the political implications of these ideas but I think that is ancillary to the point I am making here. McOuat also cautions against being an essentialist about essentialism – we should not presume that historical periods have essential characters either, with which I fully agree; but he sees essentialists where I see merely taxonomists using diagnostic definitions (for example, in his McOuat 1996). [↑](#endnote-ref-5)
6. Something Aristotle did not do, in the modern sense of *essentialism* (Charles 2002). He was not engaged upon any kind of taxonomic project, so far as I can tell, and was neither a Lockean conventionalist, nor a Leibnizian modal essentialist. However, he did in his natural histories sometimes seem to think that science could be done, at least in part, by definition. Where Locke was the clearer of the undergrowth for science, Aristotle is its gardener, tidying up the mess of ordinary knowledge. Charles, in his chapter 12, has a full discussion, in which he notes that Aristotle’s distinct classes are formed on the basis of his “soul function” theory from *De Anima*. Aristotle’s kinds are functional kinds, not taxonomic kinds. This is a recurring theme in biological essentialism, right up to Darwin’s distinction between adaptive similarity and affinities. [↑](#endnote-ref-6)
7. Locke’s friend John Ray introduced the technical natural history meaning of *species* in 1686, in his *Historia Plantarum Generalis* (Ray 1686, Tome I, Libr. I, XX: 40):

After long and considerable investigation, no surer criterion for determining species has occurred to me than the distinguishing features that perpetuate themselves in propagation from seed. Thus, no matter what variations occur in the individuals or the species, if they spring from the seed of one and the same plant, they are accidental variations and not such as to distinguish a species … Animals likewise that differ specifically preserve their distinct species permanently … one species never springs from the seed of another nor vice versa”.

Within a century, this was the consensus meaning of naturalists. It is, for example, Linnaeus’ usage, for Linnaeus never defines what he means by the term *species* (although Müller-Wille and Orel 2007, make out a good case that he had something in mind based on reproduction). [↑](#endnote-ref-7)
8. My edition is the ninth (1875). The quoted text is unchanged from the first to the ninth editions. [↑](#endnote-ref-8)
9. It was first published in 1812 under the title “Discours préliminaire” to his *Recherches sur les ossemens fossiles de quadrupeds* (Cuvier 1812). It was reprinted separately as the *Discours sur les révolutions de la surface du globe: et sur les changements qu'elles ont produits dans le règne animal* in 1825 and listed as the third edition, and is often referred to as the *Règne Animal*. It may be this that Whately has read. The English edition (Cuvier 1831, 73) gives this translation:

This inquiry calls for the definition of a *species,* which may serve as the foundation for the use which is made of the term. A species, then, includes *the individuals which descend from one another, or from common parents, and those which resemble them as strongly as they resemble one another.* Thus we only call the *varieties* of a species, those races more or less different, which may have proceeded from them by generation. Our observations on the distinctions between ancestors and descendants are consequently our only rational rule; for every other would enter into hypothesis without proofs. [italics original, p125 in the 1825 French edition] [↑](#endnote-ref-9)
10. Among the *philosophes*, however, things were different. Diderot, Maupertuis and others discussed the nascent taxonomic conventions extensively. [↑](#endnote-ref-10)
11. I think it was Paul Griffiths. [↑](#endnote-ref-11)
12. Mill saw himself in conflict with Whewell for political and moral reasons as well as for logical and methodological reasons (Snyder 2006, chapter 3). [↑](#endnote-ref-12)
13. If the thesis of the historical portion of this paper is correct, that Mill is the founder of essentialistic natural kinds into philosophy, then he may be taken to have uttered the first such conflation. [↑](#endnote-ref-13)
14. Dewey is not to be dismissed lightly, however. His treatment of *species* focused quite correctly on the generative aspects:

This formal activity which operates throughout a series of changes and holds them to a single course; which subordinates their aimless flux to its own perfect manifestation; which, leaping the boundaries of space and time, keeps individuals distant in space and remote in time to a uniform type of structure and function: this principle seemed to give insight into the very nature of reality itself. To it Aristotle gave the name, είδος. This term the scholastics translated as *species*.

In my book I argue that generation and morphological similarity were the constituent elements of what I term the *Generative Conception of Species*, which underlies pretty well all research on biological kinds from the classical era until the turn of the twentieth century and the discovery of genetics. Dewey’s rhetoric is a little over-extended, but is well within that tradition. [↑](#endnote-ref-14)
15. The history and reasons for this confusion are given in my (Wilkins 2009), and by Chung (2003). The originator of the notion that before Darwin, everyone was a “typologist” is, I believe, George G. Simpson (1961, 36–47). [↑](#endnote-ref-15)
16. In his discussion of Hamilton’s philosophy, he writes: “We have now attained a theory of Classification, of Class Notions, and of Class Names…” showing that for him, classification was a matter of conceptual kinds (Mill 1889, 402). As Snyder observes (Snyder 2006, 164), Mill had less direct experience of science than Whewell, whose classificatory ideas were formed when he studied mineralogy with Mohr. Oddly, Whewell doesn’t employ the type-kind approach to minerals. [↑](#endnote-ref-16)
17. But the “nature” that Socrates is discussing here is social: the cut he is considering is that of delineating what is Justice (*dike*). [↑](#endnote-ref-17)
18. Despite this claim of priority being repeated often, it is unlikely that Venn meant much by this; see his discussion in chapter III, sect 3 of the 1876 and later editions. He did not mean by “natural kind” what Mill did. In fact he most probably meant what Whewell or even Cuvier did (see the later editions, III.6, 17) – kinds formed by descent. In particular I don’t think that he was keying into the debates about artificial and natural classification earlier that century. [↑](#endnote-ref-18)
19. I discuss this in my 2009. [↑](#endnote-ref-19)
20. In the text he says, “… how erroneous is the idea of assuming any particular organ as the sole source of the *Essential Character*” and in the note, “It is not to the Essential Character itself, as defined by Fabricius, (*Phil. Ent.* P. 96,) that I object; but to the impossibility of finding such. “Character essentialis optimus facillimus at vix possibilis.” Why then trouble ourselves with hunting after such a chimera?” Clearly this is for diagnosis – Macleay believes diagnostic characters can be found. The term “Essential character” was widely used in botanical works of the time as the diagnostic properties of a genus or order, etc. (vide Lindley 1830). [↑](#endnote-ref-20)
21. Linnaeus may have, according to speculation by his biographer Lisbet Koerner (Koerner 1999, 48f), developed his scheme of short names and simple diagnostic descriptions based on generic and specific characters, in order for his students to name the food plants that cattle were eating, for agricultural research, in his 1749 tract, *Pan Svericus*. [↑](#endnote-ref-21)
22. As McOuat points out (McOuat 2003), Bentham was continuing his uncle’s program of reforming education and politics (Bentham 1983), and in the process of critiquing Whately he introduced a number of innovations, including quantification. [↑](#endnote-ref-22)
23. Some French commentators held that a classification was “natural” if it was agreeable to human cognition (McOuat pers. comm.). However, the debate over natural classification that began in English via the impact of Locke on systematists, continued with Milne-Edwards in French (Milne-Edwards 1844), famously influencing Darwin. Most of those taxonomists who used the phrase “natural system” and cognates, meant by it something like the Adansonian view that many characters were to be used. [↑](#endnote-ref-23)
24. Other than apparent resemblance; a particularly nice instance of which is John Lindley’s influential botanical work (Lindley 1830), in the preface of which he says that the modern botanists are able to “make their classification coincide with natural affinities” (vi), and that “the natural system of Botany” is founded on the principles that “those plants which are most alike should be arranged next to each other”, and that “those which have the smallest mutual resemblance should be placed at the greatest distance”. This is, he writes, “obviously the method of classification pointed out by nature and reason” (v). Any higher taxon (that is, of an arrangement of species, for “Nature herself, … creates species only”, vii), is “called natural, not because it exists in Nature, but because it comprehends species naturally resembling each other more than they resemble anything else” (viii). Lindley is clearly influenced by Adansonian principles in respect of the naturalness of classification (Stafleu 1963, Winsor 2004). [↑](#endnote-ref-24)
25. In the *History*, Whewell ascribes the term *homology* to the German school (presumably Oken and Goethe), and implies that Owen has merely taken it over into English (Whewell 1884, 639). The definition Owen gives is in a glossary to the work, so it may be he was using a term that had some currency already, but which most readers would find unfamiliar. I have been unable to find prior uses in English. Geoffroy does use the term “analogy” in French to mean something more like what Owen meant by *homology*, and the term “homologue” is used to apply to chemical kinds, but nothing biological that I can locate. However, both Whewell and Owen (1848) are right, in that the Germans used the term, particularly Oken in the *Physiophilosophie* (Oken 1847). I am indebted to Gareth Nelson for assistance with tracking down these uses. [↑](#endnote-ref-25)
26. After this time, the topic was generally the domain of librarians, such as Richardson (1901), although Dewey treated it as an instrumental good (Dewey 1920). [↑](#endnote-ref-26)
27. Woodger’s (1937) and Gregg’s (1954) set theoretic accounts are exceptions, but influential mostly later on. One person Woodger did affect was Hennig. [↑](#endnote-ref-27)
28. There is considerable variation for the wording of this quote. A common variant is “All science is either physics or stamp collecting.” Rutherford *should* have used the quote given in the text, however. [↑](#endnote-ref-28)
29. Another interpretation here is that Rutherford recognized that physics needs to have observed clusters of properties in order to apply to the world, and with this I must agree, but I doubt that was his intent. I discuss this in my forthcoming book *The Nature of Classification* coauthored with Malte Ebach. [↑](#endnote-ref-29)
30. Or rather, specimens – the raw “data” of a phylogenetic inference are individual specimens. One may *take* them to be stand-ins for their species, but even in the molecular period, the data sets being analyzed are usually those of single individuals who have been sequenced [↑](#endnote-ref-30)
31. Darwin used the terms “affinity” for sets of homologous characters, and “analogy” for homoplasious characters, directly influenced by the discussions of, among others, Swainson (1835) and Milne-Edwards (1844). See, for instance, Whewell’s discussion in Bk VIII, chapt. II, sect 4, in which he notes that mere resemblance is not enough for a natural system; one must also use “natural affinity” (Whewell 1847, , vol 1, p488). In 1843 Owen defined “homology” (1843), and in 1870 Lankester the term “homoplasy” (1870). A slew of similar terms in molecular systematics have also since been invented, such as *paralogy*, and *orthology*, *xenology*, *gametology*, and *synology* (Fitch 2000, Mindell and Meyer 2001). For a review of the history of homology, see Laubichler (2000). [↑](#endnote-ref-31)
32. Alas for philosophy, “Raptors” has now been divided into two disparate clades, the order Falconiformes, which include around 290 species of diurnal birds of prey, and the order Accipitriformes, which include around 225 species, including eagles, hawks and New World vultures (see Hackett, et al. 2008). Though this makes the example less pleasing, it in fact strengthens the point, as inductive inferences will now no longer be as projectible between them (just as the group “vultures” ceased to be so projectible when it was discovered that Old World vultures were more closely related to the Accipitridae, eagles and kites, etc., than to New World vultures, which are more closely related to Cathartidae, such as storks). [↑](#endnote-ref-32)
33. Hans Reichenbach proposed a “straight rule” for induction in his *The theory of probability* (Reichenbach 1949), in which induction was justified when increasing observations converged upon an asymptote. See also Salmon 1991. [↑](#endnote-ref-33)
34. Hence Elliot Sober’s recent proposal for a form of logical inference he calls *modus Darwin* (Sober 1999, 2008), in which common ancestry can be inferred from similarity, misrepresents what Darwin and subsequent phylogeneticists actually did when inferring phylogenies. With the notions of “analogy”, and later with “homoplasy”, being clearly understood and used, it has always been known that mere similarity is insufficient to infer common ancestry, or else bats, birds and pteranodons can be assigned to a single group, a point made by, among others, T. H. Huxley. In fine case of historical irony, Huxley’s grandson Julian tried to hoist a homoplasious group for thinking beings – *Psychozoa* (Huxley 1957) [↑](#endnote-ref-34)
35. However, I really doubt this will be true – even when traits like legs are lost, as in the case of snakes, the genes that were involved in making legs are not excised, but are silenced or downregulated. They remain there and are available to sequencing (and what is more, I predict this in the absence of empirical knowledge based on phylogenetic considerations). [↑](#endnote-ref-35)