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Non-essentialist methods in pre-Darwinian taxonomy

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Abstract. The current widespread belief that taxonomic methods used before Darwin were essentialist is ill-founded. The essentialist method developed by followers of Plato and Aristotle required definitions to state properties that are always present. Polythetic groups do not obey that requirement, whatever may have been the ontological beliefs of the taxonomist recognizing such groups. Two distinct methods of forming higher taxa, by chaining and by examplar, were widely used in the period between Linnaeus and Darwin, and both generated polythetic groups. Philosopher William Whewell congratulated pre-Darwinian taxonomists for not adhering to the rigid ideal of definition used in the mathematical sciences. What he called the ''method of types'' is here called the ''method of exemplars'' because typology has been equated with essentialism, whereas the use of a type species as the reference point or prototype for a higher category was a practice inconsistent with essentialism. The story that the essentialism of philosophers dominated the development of systematics may prove to be a myth.

The received view goes something like this: ''Prior to the acceptance of evolutionary theory, essentialism was the standard mode of classification in biological taxonomy.'' (Ereshefsky 2001: 95). Here we have the gist of a well-known story about the development of systematics, prominent in many standard sources (Mayr 1982). Leaving aside its first and last chapters, we may summarize the central chapters of the story thus: 2.) Linnaeus followed the method of logical division developed by neoPlatonic and Aristotelian philosophers, 3.) taxonomists after Linnaeus continued to deal with species as Platonic types, 4.) Darwin achieved, or at least initiated, a revolutionary change by thinking in terms of populations. The received view, which promotes the importance of philosophy for the history of science, is widely repeated, especially in introductory courses, but its foundation in historical evidence is remarkably poor. It is inconsistent with another narrative that is better substantiated, a version of history in which experience plays a dominant role. In the ''empiricism story,'' $2^{'a}$.) taxonomists from the Renaissance onwards adhered to a wide variety of world views, but usually chose practical considerations over theoretical purity, $2^{'b}$.) Linnaeus gave them a flexible framework that proved useful for managing a vast and expanding body of data, $3^{'}$.) Experience forced naturalists in the early 19^{th} century gradually to relinquish belief in continuity, replacing it with belief in the naturalness of higher taxa, $4^{'}$.) Darwins's theory provided an explanation for the hierarchical shape of nature that taxonomists had discovered.

Future historians will have their hands full when they try to explicate the importance and many meanings of the word ''essentialism'' in the 20th century. What we may call the classical sense of the word refers to the misguided notion that biological species ''must possess definitional essences that define them in terms of necessary and sufficient, intrinsic, unchanging, ahistorical properties,'' but it is now proposed that the word be applied to the sophisticated notion that a natural kind, including a biological species, exhibits a cluster of properties ''homeostatically'' caused (Boyd 1999: 146). How very different the past will look when we recognize that the pre-Darwinians were essentialists in the homeostatic property cluster sense rather than the classical one. Instead of a picture of long stasis in taxonomy, caused by the stranglehold of an ancient idealism, the history of systematics will become a scene of progressive change, fueled by collection, preservation, exchange, and observation of organisms.

History has the same responsibility as does science to be based on good evidence and to be open to testing, but for reasons stemming from interdisciplinary dynamics, the received view continues to elude critical evaluation. Fresh historical research is needed, but just reviewing material already published is enough to raise serious doubts about the essentialism story. Mayr (1968, 1982) has described the years between Linnaeus and Darwin as a time of ''empiricism'' and Panchen (1992) points out that ''the acceptance of polythetic taxa'' in the last half of the 18th and first half of the 19th centuries ''negates essentialism.'' In that hundred-year period, between the publication of the 10th edition of Linnaeus's *Systema Naturae* in 1758 and Darwin's (1859) publication of the *Origin of Species*, natural history was progressing dramatically, with museum and herbarium collections growing, workers better trained, and the number of named taxa at all levels increasing at an explosive rate. The notion that botanists and zoologists, during this busy period of achievement, were frozen in the grip of ideas derived from Plato, Aristotle, and medieval scholasticism makes Darwin's breakthrough nearly miraculous. The story of the dominance of essentialism is as dramatic, in its way, as the myth that has Columbus's crew fearing they would fall off the edge of the earth, and I believe it is equally fictitious.

The voyages of Columbus took place in the late 15th century, but the tale about sailors who believed in a flat earth was concocted long afterwards (Russell 1991). Likewise the essentialism of pre-Darwinian naturalists was not apparent to 18th or 19th century eye-witnesses but came to light only in the mid-20th century. The essentialism story was the creation of two biologists and a philosopher. In 1958 Arthur Cain, zoologist at Oxford University, hit upon the idea that Linnaeus had been following the rules of Aristotelian logical division. The distinguished Harvard zool ogist Ernst Mayr proposed in 1959 that what he called Darwin's ''population thinking'' had replaced ''typological thinking,'' a world view derived from Plato, and Mayr repeated this idea emphatically in 1964. In 1963 David Hull, then a graduate student in the history and philosophy of science at Indiana University, made use of Karl Popper's critique of ''methodological essentialism'' to argue that biologists should stop expecting a simple definition of the species category. Although Hull's analysis was almost entirely philosophical, the title of the ensuing

article (Hull 1965), ''The effect of essentialism on taxonomy – two thousand years of stasis,'' constituted an historical claim. In 1968 Mayr took the critical step of accepting the word ''essentialism,'' which Popper (1944: 94) had coined, as a synonym for typological thinking (1968, 1969, 1976, 1982).[1] Although a number of authors over the years have expressed various reservations, corrections, and doubts about parts of the Cain-Mayr-Hull story (Sokal 1962; Farber 1976; Winsor 1976, 1979, 2001, 2003; Sober 1980; Pratt 1981, 1985; Van der Hammen 1981; Stevens 1984, 1994; Atran 1990; Greene 1992; McOuat 1996; Müller-Wille 1999, 2001; Camardi 2001), it is still the established view.

In truth, Columbus's crew, after many weeks at sea, did fear for their lives and begged him to turn back, because they thought the distance around the globe was so great that food and water would run out before they hit land. Likewise, beliefs allied to neoPlatonism, including morphologists' ideal archetypes, did make it hard for some biologists to accept evolution, though not so hard as is often said (Amundson 1998). Yet just as we can better understand the westward expansion of Europeans when we know what was shared by Columbus and his men, which included both academic geography and practical sailing skills, likewise we will better understand the dynamic connection between taxonomy and evolution when we consider what Darwin shared with his fellow naturalists, which was the perception that living things ''resemble each other in descending degrees, so that they can be classed in groups under groups,'' (Darwin 1859: 411), a perception allied with a set of practices for describing and naming these groups. That perception and those practices were firmly rooted in the work of Linnaeus.

Much of the literature relating essentialism to systematics is seriously flawed by the failure to separate ontology and epistemology. For the history of systematics, special effort is needed to give due weight to practice as well as theory, because many of the workers whose contributions to taxonomy have been influential have not been articulate about whatever principles were guiding them, as Peter Stevens (1994: xxi) reminds us. Above all, we must treat it as an empirical question whether various naturalists in the past were essentialists ontologically (in their world view), and as a separate question, requiring separate evidence, whether they were essentialists epistemologically (in their method). Although we may think that people's beliefs about the nature of reality should be tightly correlated with their research procedure, we ought not to prejudge the connection. If we assume, for example, that a person who believed in the existence of essences must have used the essentialist method, we run the risk of distorting the past through the lens of our expectation, thereby missing the opportunity to learn anything from history.

We are not free to concoct out of our own reason or imagination what we think should count as the essentialist method. The warp cords around which Mayr, Cain

[1] Ernst Mayr emphasized to me, after reading a draft of this paper, that his writings on typological thinking and essentialism were all aimed squarely at the level of species, which is certainly true. He holds that it is improper to apply these ideas to the higher categories, but other writers, I believe, have not respected, nor even understood, this stricture. I describe the origin of the essentialism story in some detail in an article now in preparation.

and Hull wove the threads of their story were the historical figures of Plato, Aristotle, and their medieval followers, whose influence, they claimed, reached into the 19th century. That claim degenerates into vague hand-waving unless the method actually belonging to that philosophical tradition is kept in sharp focus. The scholastic logic from which Linnaeus took the Latin words *genus*, *species*, *character essentialis*, and *differentia* taught that things recognizable as belonging to a kind given by nature must have essential properties, that is, features always present in each instance. The Aristotelian method was supposed to consist in giving a correct definition by listing only those properties. The standard example was that being two-legged does not belong in the definition of humans, for birds also have two legs, but being rational is an essential feature for our species. (An Aristotelian would escape the objection that babies lack reason by saying they have it potentially, while adults who lack it would be held to have ceased to be really human.)

It was this approach to definition that was the villain in the essentialism story. Hull in 1965 quoted Popper's statement,

> Aristotle considers the term to be defined as a name of the essence of the thing, and the defining formula as the description of the essence. And he insists that the defining formula must give an exhaustive description of the essence or the essential properties of the thing in question. (Hull 1965: 318, quoting Popper 1950: 208)

Whether Popper was historically accurate about Aristotle himself is not relevant, as long as Hull, and his readers, believed it. David Balme argues that Aristotle's biology was after all not essentialist, but Thompson (2001) correctly insists that no matter how accurate Balme's view, it changes nothing for later history, for naturalists of the 18th and 19th century had not read Balme. Hull's argument was based on Popper's Aristotle, not on Balme's Aristotle. Likewise Cain built his case around the argument (erroneous according to Müller-Wille (1999), Winsor (2001)) that Linnaeus applied scholastic logic to the problem of characterizing plants and animals. And although Mayr did not describe the issue in terms of definition, he did say that the reason typological thinking stood in the way of Darwinism was that for natural selection to work, every character must be allowed to vary, while for typologists some characters are essential, so that gradual change by replacement of characters is impossible: essential characters must always be present. Hull identified the essentialist method thus: ''Disregarding all the talk about essences, what Aristotle was advocating in modern terms is definition by properties connected conjunctively which are severally necessary and jointly sufficient'' (1965: 318).

An alternative to the essentialist method is to let a list, or cluster, of properties count as a definition without insisting that any particular property be always present. Members of the group share many properties, and this gives a general resemblance that makes a strong impression on us. In other words, the group is polythetic. Boyd's and any other claim that a cluster-based method of definition is compatible with ontological essentialism is historically irrelevant, because there is no doubt that allowing a group to be polythetic is a complete, radical, and significant aban-

donment of both the spirit and the letter of what the followers of Plato and Aristotle understood to be the proper, essentialist method of definition. The story sketched by Cain, Mayr, and Hull took for granted, indeed its force depended upon, the assumption that using clusters of properties in definitions, that is, allowing polythetic groups, was a recent innovation within taxonomy. Hull said (1965: 323), ''In defining taxa names as cluster concepts, taxonomists have (whether they realise it or not) adopted a new and rather controversial philosophical position.'' Philosophers associate the breakthrough with Ludwig Wittgenstein's mid-20th century insight that classes have no essences, consisting rather of members linked by ''family resemblance.'' The fact that the word ''polythetic'' was of recent coinage (Sneath 1962) may have reinforced the impression that the break from Aristotelianism was a recent event.[2]

Information did exist to suggest that polythetic definition was used by taxonomists even before Darwin, but allusions to it in the 1960s always implied that such cases were not typical. For example, the founders of numerical taxonomy reported,

> It is to the credit of John Ray and Caspar Bauhin that they were less bound by the iron fetters of Aristotelian logic than other early workers. They had a strong intuitive sense of what natural taxa were, although they did not express themselves clearly. This is what de Candolle (1813, p. 66) aptly called ''groping'' (*tâtonnement*), though he only attributes this to later authors such as Magnol. According to de Candolle, Magnol claimed to have a clear idea of a natural family of plants even though he could not point to any one character which was diagnostic of the family. (Sokal and Sneath 1963: 13)

(These ''iron fetters of Aristotelian logic'' show the effect of Cain's 1958 article on Sokal and Sneath, for de Candolle had not portrayed his predecessors in that light.) The most frequently mentioned exception was Michel Adanson, who had insisted in 1753 that classification must take all characters into account, because no single character correlated with natural families, but the fact that Adanson's contemporaries had ignored him reinforced the point. The normal practice between Linnaeus and Darwin, the background of essentialist method against which such exceptional naturalists supposedly stood out, was not directly entered in evidence; rather, it was inferred from the premise of the dominance of ancient ideas.

The idea that ''it might not be possible to find *any* single diagnostic character for a natural taxonomic group . . . is a point of the very greatest importance, which can scarcely be overemphasized,'' Sokal and Sneath rightly said (1963: 13), but the notion that this point had to wait for the dogmas of ancient philosophy to be challenged by Darwinian population thinking underestimates the success of the 17th century scientific revolution, when scholastic wordplay was ridiculed and experience celebrated. There is already evidence suggesting that in the 18th and 19th

[2] Sneath coined the word ''polythetic'' without claiming to originate the idea, which he credited to Morton Beckner. Beckner (1959, p. 61n) used the word ''polytypic,'' which he credited to systematists including A.C. Kinsey and G. G. Simpson. Beckner also cited Wittgenstein.

centuries normal taxonomic practice permitted polythetic groups. Such groups were sanctioned by two distinct procedures, the method of chaining and the method of exemplars.

Chaining was a practice explicitly discussed and named by the taxonomists of the time. Peter Stevens has shown, in closely argued detail, that the early Lamarck and the eminent Antoine-Laurent de Jussieu believed that plants resembled one another in continuous series, rather than as lumpy groups separated by gaps. A natural world with such continuity was one in which every taxonomic group must be polythetic, as Stevens takes pains to illustrate (1994: 21; 1997). Stevens (1994: 78) quotes from the 1815 botanical textbook by Charles-François Brisseau de Mirbel, who categorized both genera and families into three kinds: the artificial ones, based only on one feature; *polytypique* ones, which had no distinctive feature, and the *monotypique*, obvious at a glance because its members resemble one another closely. In the polytypic taxa, which Mirbel called genera or families *par enchaînement*, each species or genus was linked to the next by clear similarities, but by the end of the chain all the characters of the first link had been lost (Mirbel 1815, vol. 1, pp. 482–5). Stevens points out (1994: 427n77) that these sorts of groups had already been noted by Lamarck in 1783.

While the *familles par enchaînement* were loudly polythetic, I suggest that the *familles en groupes* were liable to being polythetic as well, because rather than being defined by a list of obligatory features, they were formed by comparisons around their typical member. The practice of describing one form in careful detail and then comparing other forms to it constitutes what I will call the method of exemplars. The significance of this method is that a group could be created by association or agglomeration, each new member being judged similar to the exemplar in most of its characters, without any particular character of the exemplar being privileged by making it a requirement of membership.

Linnaeus certainly proceded by the method of exemplars. The great botanical scholar William Stearn quoted Linnaeus saying that to compose the *character naturalis*, the *fructificatio* of one chief (or first, *primus*) species is described very accurately, and then other species are compared with it, and any characters at variance are excluded. ‘‘In other words,’’ Stearn explained,

> the flowers and fruits . . . of a single species are to be described in detail; then from this description are to be removed all features found on comparison to vary from species to species; the resulting statement of features common to all species is the natural character of the genus. That this was indeed Linnaeus’s original procedure, and doubtless always his aim though not always his achievement, becomes evident from critical comparison of the characters of the genus as stated in the *Genera Plantarum* with those of the individual species listed in the *Species Plantarum*. (Stearn 1957: 37)

Pennell 1939, quoted in Stearn 1957: 38, reported that it is ‘‘certain that [Linnaeus’s] customary procedure was to select a certain illustrative species and from it to describe his genus.’’ In other words, he did not behave as a logician would have

him do, spreading on a table specimens of the ten species to which he is about to assign one generic name, then writing down whatever characters he notices they all have. Instead, he began by writing a generic description based upon only one species. We may assume that Linnaeus expected that from the several dozen characters noted in the first-described, exemplary species, a few would be disqualified from being essential with each subsequent comparison to another species, leaving at last a hard core of constant characters that would be the final, corrected definition of the genus. In practice, however, what he really did was keep the exemplary species in mind, and compare to it any new candidates for the genus as they presented themselves. If their resemblance was strong, they were admitted. Linnaeus often never got around to deleting from the next edition of the *Genera Plantarum* the features by which they differed. As a result, as all the botanists who used his works knew, the characters given in the *Genera Plantarum* ''may apply [in full] only to some or even to none of the species'' in the *Species Plantarum* (Stearn 1957: 37).

Why did Linnaeus's failure to keep updating the *Genera Plantarum* not cause complaint, confusion, and chaos? Surely because naturalists were all equipped with ordinary human powers of recognition. A competent botanist was expected to hold in his memory all the Linnaean genera, which did not mean he had to remember all the species that had ever been described, but that he had to be so familiar with one exemplary species for each genus, that upon seeing an unfamiliar plant, he would recognize which genus it probably belonged to. He could make this judgment because his memory was a storehouse of what cognitive scientists call prototypes (Lakoff 1987; Taylor 1995). As we all do when we increase our vocabularies, the botanist compared the unfamiliar to the familiar without prejudging particular characters as absolutely required. ''Every systematist knows of instances where a character previously considered to be diagnostic of a taxon is lacking in a newly discovered organism which clearly belongs to the taxon,'' (Sokal and Sneath 1963: 13) and there was no world-view powerful enough to protect pre-Darwinian taxonomists from the same experience.

If Linnaeus had really been imprinted in his youth with a respect for scholastic logical division, the erosion of defining characters ought to have distressed him, but there is much to suggest he was comfortable with it. Indeed he made sure his students understood that they should not worry about polythetic groups if they appeared to be natural. His student Paul Giseke published in 1792 a report of Linnaeus's teaching in 1771. As Stevens relates it,

> In a socratic exchange, Linnaeus showed that the characters Giseke thought defined one of the most natural of families, the Umbelliferae, either occurred elsewhere – for example, its two-seeded fruits—or did not occur in all its members – for example, the umbellate inflorescence itself . . . . Thus any simple definition of the Umbelliferae was problematical. (Stevens 1994: 407n24).

Arthur Cain, in his retirement, examined Linnaeus's writings more thoroughly than

he had had time to do in the 1950s, and he concluded that Linnaeus had ''recognized orders consisting of a chain of genera linked successively by overall affinity and without any single diagnostic character.'' (Cain 1995: 73)

In 1828 Georges Cuvier put aside any pretense of theoretical analysis and frankly admitted his procedure for dealing with the families and genera for over 5,000 species of fishes.

> We choose one of them – the best known, the most interesting, or the easiest to procure – and we describe it in the greatest detail .... After this first description it is simple for us to reduce that of the the other species of the same group to comparative terms. (Cuvier 1828, quoted in Eigen 1997: 203)Unable to assign to each family an unequivocal and exclusive character, we indicate them for the moment by names derived from the most widely known genus of each, the genus that may be regarded as the type from which it is easiest to get an idea of the family. At the beginning of each family will be found a more extensive list of its characters, as well as the combinations according to which we are subdividing the family and that lead us to the different genera that compose the family. (Cuvier, 1828: 282; quoted in Eigen 1997: 204)

For Cuvier to practice the method of exemplars is particularly significant, because he is now best remembered for his ''top-down'' (deductive) rather than ''bottom-up'' (inductive) philosophy of systematics, based on his famous functional principles (Cain 1959). Cuvier did, early in his career, talk as though one could build up natural classes by reasoning about physiological integration. Presumably he believed his own talk; certainly his ability to theorize impressed his contemporaries. Yet they were even more impressed by the large store of taxonomic characters created by his program of cutting animals open.

In subsequent decades, while Darwin was quietly nursing his theory of natural selection and working on the barnacle monographs, one thoughtful observor noticed that naturalists had been making great progress by ignoring the essentialist method. In 1840 William Whewell, ''the greatest of nineteenth-century philosophers of science'' (Hacking 1975: 166), published a fat book on scientific method based upon his survey of the history of the sciences; an enlarged edition of his *Philosophy of the Inductive Sciences* appeared in 1847. Whewell noted that while logicians spoke in terms of defining characters, the mineralogists, botanists and zoologists actually responsible for the impressive recent progress in the ''classificatory sciences'' had been using ''peculiar and technical processes'' (1847, vol. 1: 479). Chief among these was what Whewell called the ''*Method of Type*'' (1847, vol. 2: 423). After giving instances in which botanical families are characterized by features frequently but not always found within them, Whewell declared,

> These views, — of classes determined by characters which cannot be expressed in words, — of propositions which state, not what happens in all cases, but only usually, — of particulars which are included in a class though they transgress the definition of it, may very probably surprize the reader.

> They are so contrary to many of the received opinions respecting the use of definitions and the nature of scientific proposition, that they will probably appear to many persons highly illogical and unphilosophical. (1847, vol. 1: 493)

But in Whewell's judgment, it is those persons' expectations, and not naturalists' behaviors, that were wrong. He congratulated naturalists for recognizing groups that were not artificial, and described the procedure they had devised.

> 10. *Natural Groups given by Type not by Definition*. . . . though in a Natural Group of objects a definition can no longer be of any use as a regulative principle, classes are not, therefore, left quite loose. . . . The class is . . . determined . . . not by what it strictly excludes, but by what it eminently includes; by an example, not by a precept; in short, instead of Definition we have a *Type* for our director.
>
> A Type is an example of any class, for instance, a species of a genus, which is considered as eminently possessing the characters of the class. All the species which have a greater affinity with this Type-species than with any others, form the genus and are ranged about it, deviating from it in various directions and different degrees. Thus a genus may consist of several species which approach very near the type, and of which the claim to a place with it is obvious; while there may be other species which straggle further from this central knot, and which yet are clearly more connected with it than with any other. (1847, vol. 1, pp. 493–495)

Whewell, who had made a considerable study of mineralogical nomenclature and classification himself, was very firm that what deserved respect was the practice of skilled, working naturalists rather than time-honored but armchair opinion.

> We may detect among speculative men many prejudices respecting the nature and rules of reasoning, which arise from pure mathematics having been so long and so universally the instrument of intellectual cultivation. Pure Mathematics reasons from definitions . . . .Hence it has come to pass that in other subjects also, men seek for and demand definitions as the most secure foundation of reasoning . . . . (1847, vol. 2: 369)But . . . 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 . . . .the definition does not even apply to all the tribe . . . . (1847, vol. 2: 370)
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> 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? . . . The answer to this question involves the general principle of a natural method of classification . . . . 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 groups of objects in natural history is not a *definition*, but a *type*. (1847, vol. 2, pp. 370–371)

Whewell's appreciation of how words are given meaning is remarkably like the finding of current experimental linguistics, his ''*type*'' very like Rosch's ''prototype''.

> These lessons are of the highest value with regard to all employments 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. The terms which belong to our practical concerns, or to our spontaneous and unscientific speculations, are rarely capable of exact definition. They have been devised in order to express assertions, often very important, yet very vaguely conceived: and the signification of the word is extended . . . by apparent connexion or by analogy . . . . (1847, vol. 2, pp. 371–372)

The implications of all this for education, proclaimed by Whewell, should be put into effect by the introduction of laboratory-based teaching in schools.

> Thus the study of Natural History, as a corrective of the belief that definitions are essential to substantial truth, might be of great use; and the advantage which might thus be obtained is such as well entitles this study to a place in a liberal education . . . .in order that Natural History may produce such an effect, 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 (1847, vol. 2: 372).

Historians of philosophy may be interested in explaining how the pragmatism of Whewell's extensive discussion of classification relates to the idealistic aspects of his thought, but his relevance here is simply as a witness to the normal taxonomic practice of the early 19th century.

I have chosen the term ''method of exemplars'' rather than copying Whewell's label ''type method'' because Mayr's campaign against ''typological thinking'' has been so successful that many people think they know what a terrible thing ''types'' were. Yet we have no hope of understanding the history of systematics if we cannot domesticate that word. Paul Farber tried many years ago to inject some historical data into the discussion, but his sensible article did not have the impact it deserved. He said that in the first half of the 19th century there were three different type concepts, which he labelled the classification type-concept, the collection type-concept, and the morphological type-concept. Naturalists at that time, Farber warned, used the word ''type'' ''in a very loose manner'' (1976: 94n1), and indeed

several meanings that fall outside Farber's categories can be traced (Sara Scharf, personal communication). Farber's collection-type, a method rather than a concept, is the careful preservation of individual specimens used by the first describer of a species. This very concrete sort of ''type'' remains in use today, and Mayr was careful to exclude it from his criticism. The type-concept of morphologists, or archetype, was an abstraction rather than an individual organism or particular taxon. Obviously it was this kind of type that attracted criticism from those who thought science should avoid idealism. Farber's ''classification type-concept'' largely corresponds to what I call the method of exemplars, and his description shows that it belonged to the realm of practice rather than theory.

> reduce tiresome repetition of characters of closely related groups in a classification system, naturalists chose one form (usually the indigenous, most ''perfect,'' or best known) and used it as a model. It could be, and was, applied on several taxonomic levels; one could use a single species to characterize (be the type for) a genus, a genus for a family, and so on. Before the nineteenth century the classification type-concept most often was used implicitly . . . . During the first half of the nineteenth century, the use of the classification type-concept increased dramatically and became explicit . . . . (Farber 1976, pp. 93-94)

Farber quotes Hugh Strickland in 1844 recommending that taxonomists should be ''invariably selecting a type, to be permanently referred to as a standard of comparison. Every family, for instance, should have its *type-subfamily*, every subfamily its *type-genus*, and every genus its *type-species*. But it must not be supposed, with some theorists, that these types really exist as such in nature; they are merely examples . . . .'' What Strickland meant, in modern terms, is that although the exemplar really exists, it is we and not nature who make it the point of reference for a higher category. ''By adhering to this notion of types,'' Strickland continued, ''we may often indicate these groups with greater precision than it is possible to do by means of definition alone'' (Farber 1976: 95n5).

This paper has been limited to information already available in the scholarly literature but overlooked because of the dominance of the essentialism story. Clearly more study of the pre-Darwinian taxonomic literature is now called for. It has also been limited to the essentialist method, setting aside the difficult question of the degree to which taxonomists used to believe in occult essences. While that question may also be of interest, it is important to guard against the assumption that scientific practice has always been correlated with belief in ways we imagine to be reasonable.

People who like history to unfold in seemingly logical order may find the loss of the essentialism story disturbing. The Platonic types played satisfying roles in a drama in which Darwin was the hero. Surely those early naturalists must have given some idealistic meaning to their chains and exemplars, we may reason, since they lacked our solution that it is descent from a common ancestor that confers reality upon taxa. Some people, like Louis Agassiz, did explain the naturalness of taxonomic categories as direct expressions of the thoughts of God, but many appeared willing to leave this phenomenon as a fact of nature to be recorded without

explanation. Pointing to obviously natural genera like roses and carnations, Mirbel said, ''These groups are independent of our systems; they have a metaphysical reality as evident to us as the material existence of individuals'' (1815, vol. 1: 483). Mirbel was writing more than two hundred years after Columbus opened the world to European exploration, and those years had been packed with the activity of naturalists struggling to catalogue and make sense of what was collected. The result of their work was the conviction, solidly in place before 1859, that arranging living things in nested sets represents their actual degrees of similarity. Their work constituted the foundation of Darwinism.

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