Why do we name organisms? Some reminders from the past

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The naming systems of Linnaeus and Bentham in particular are examined to clarify the relationships between naming and ideas of relationships. Linnaean binomials were adopted largely for practical reasons. Furthermore, Linnaeus proposed his names in the context of system, putting organisms in groups of 10. This allowed botanists of moderate capabilities to know at least the genera. Although binomials are names of taxa of the two lowest levels of a rank hierarchy, much of Linnaeus' work does not fit easily in the currently widely accepted view of Linnaeus as a hard-bitten essentialist. Neither Lamarck nor the later Bentham believed in a rank hierarchy, although to name organisms both used what is here called a flagged hierarchy: name terminations indicating only a set of inclusion relationships, not ranks of nature implied by a rank hierarchy. Bentham was clear that the adoption of a flagged hierarchy with groups of a particular size in the *Genera plantarum* was to facilitate botanists' understanding of the system as a whole. Systematists like Bentham and Linnaeus managed information and presented classification systems simultaneously. I conclude that the lower level of Linnaeus' hierarchy is a special case of the noun + adjective combination that pervade folk classifications in particular and human language in general. Linking essentialism and "Linnaean" nomenclature is at best a red herring, thus few nineteenth-century botanists believed in a fully-developed rank hierarchy. Naming hierarchies are mostly such that at each level members belong to only one group, and this is at a higher level; most such hierarchies are fairly shallow. Historically, uninomials have seemed more attractive when generic limits were in flux, but suboptimal when relationships were more stable. Naming systems in general incorporate a substantial element of convention, emphasizing particular numbers of groups and groups of particular size; this facilitates comprehension and communication. Similar conventions will be needed whatever naming system is used.

KEYWORDS: Bentham, convention, flagged hierarchy, informal hierarchy, Linnaeus, nomenclature, phylogeny, rank hierarchy.

> *There are also Idols arising from the dealings or associations of men with one another, which I call idols of the Marketplace. For speech is the means of association among men, and in consequence, a wrong and inappropriate application of words obstructs the mind to a remarkable extent (Bacon).*
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> *But unfortunately a "unique & stable numbering system" probably won't ever happen as the community will not support such. ...it isn't numbers that is the requirement for "unique & stable". It is the community acceptance of a system to make anything "unique & stable". Zoological Nomenclature is an International Standard which should give you "unique & stable" identifier (keys, etc.), but it fails because people will not follow it nor allow its modification to better provide the "unique & stable", etc. Set up a registration system like the Bacteria people did, and you get "unique" names. "Stable" fails because of taxonomic progress and classification paradigms (Christian Thompson, taxacom@usobi.org).*

INTRODUCTION

Much is made of the fact that systematists have been using a Linnaean hierarchy for the last two and half centically over that period, it is suggested that how we go about naming organisms should change as well (e.g., Pennisi, 2001). Linnaeus believed that the species he described and named had fixed essences, did he not? His ideas, and his names, surely are in conflict with evolution-based naming systems of the twenty-first century, and so his names should go. To help clarify the issues involved, I examine some aspects of the relationship between names, classifications, and nature. I ask two questions. What were those who named groups trying to do? What in consequence might we learn about the role naming plays in systematics? These questions seem naïve, and their answers self-evident, yet I will show that neither is as simple as it seems. I focus on vascular plants, but any principles we can derive will be generally applicable. However, it is no purpose of this article to criticise or defend in detail either proposals for a *PhyloCode* or the so-called Linnaean hierarchy.

I look very briefly at some 18th and 19th century classifications, focusing mainly on Linnaeus and George Bentham. I shall suggest that the details of the hierarchy that Linnaeus used owe much to his belief that almost all plants had been discovered and to his desire that the system as a whole should be easy to grasp. The oft-repeated claims that Linnaeus was an essentialist, and that essentialistic thought pervades his work, turn out to be questionable. When we look at other 18th and 19th century systematists, and at George Bentham in particular, we find hierarchies being used by systematists whose understanding of nature was very different from that of Linnaeus. Again, concerns with essences are not at all obvious, and the desire that the system as a whole be easy to grasp shapes its structure.

My conclusions are two-fold. Firstly, even if one thinks that Linnaeus was a essentialist, this is surely tangential to the general issue of the use of hierarchical naming systems in biology. Hierarchical naming systems pervade our whole language and thought, and from this point of view, the Linnaean hierarchy is simply one such system. Secondly, there is a recurring tension between the need to communicate and the apparent implications of a "natural" classification of whatever stripe for the naming system used. The naming systems I discuss—undeniably successful—were conceived as conventions in which emphasis was placed on the structure of the hierarchy, on particular levels in it, and on taxa of a particular size. Making systems easy to memorise or, more generally, to comprehend by the average botanist was an important goal. Linnaeus and Bentham were as much managing information as naming or classifying nature, and it is wise not to forget this.

Before going further, terminological clarification is in order. I distinguish between three kinds of hierarchies. In **informal hierarchies** names suggest nothing about the position of the taxon or group named relative to others, even if the relationships of those taxa or groups can be represented by an hierarchy or a diagram with the form of an hierarchy such as a phylogeny. In **rank hierarchies** the form of the name suggests a particular position of the taxon bearing the name relative to others, and also that there is a rank or class of taxa at that level of the hierarchy in nature. In a **flagged hierarchy** the form of the name suggests a position of the taxon bearing it relative to others, at least in the immediate group, but without carrying an implication that there are ranks in nature.

CLASSIFICATIONS AND NAMING IN HISTORY

LINNAEUS

Linnaeus on how to "package" classifications. — The *Philosophia botanica* of 1751, an elaboration of the 365 aphorisms in *Fundamenta botanica* of 1736, explains many aspects of Linnaeus' theory and practice and serves as the backdrop to Linnaeus' work (Stearn, 1957: 71–72). Issues such as identification, organisation, standardisation, and economy of space guide much of Linnaeus' practice (e.g., Sprague, 1955; Eriksson, 1980). As for this last issue, the hierarchical structure of the information in books like *Species plantarum* meant that higher-level characters are mentioned only once; what was true for the class *Hexandria* would be true for its included orders and genera, and would never need to be repeated (see also Cesalpino, 1583). There are other devices to help the reader readily assimilate at least the outline of the Linnaean system. Binomials are one such device, but the whole classification was structured in accordance with a few overarching principles to achieve precisely this end.

Linnaeus' rules of nomenclature governed the names of genera and species. Linnaean names are polynomials, the generic name—one word—being followed by a short species phrase (Linnaeus, 1736, 1737b; see Nicolson, 1991, for a history of botanical nomenclature). Although we often talk of Linnaean binomials, single-worded specie epithets were no part of the Linnaean name proper. To refer unambiguously to species, mention could be made of the generic name and the number assigned to each species or to the polynomial as a whole. However, as Linnaeus started to use binomials, he noted that they alone could function as unique identifiers, being particularly useful because they would remain unchanged even if species differentiae changed (Linnaeus, 1751a: 202). Single-worded epithets also saved space, and so were particularly convenient when used in indices, as in that to *Öländska och gothländka resa* (Linnaeus, 1745, see also

1749; Stearn, 1957: 69–70; Stafleu, 1971: 106–107)[1]. Stafleu (1971: 78) suggested that the introduction of binomials might have been "psychologically difficult", but the reverse is more likely. Binomials were much easier to memorise than polynomials (Linnaeus, 1763: 315), as Augustin-Pyramus de Candolle early recognised (1813: 223–224; see also Cain, 1959; Stafleu, 1971: 104–106). By the time that Linnaeus died in 1778, most botanists were using binomials as *the* species name (Stearn, 1957: xiv, 67–71, 76–80; Stafleu, 1971: 109–110). They were simply so convenient.

But genera and species are only part of the Linnaean system. Although a discussion about Linnaeus' sexual system would logically seem to be the next step, I want to think about what Linnaeus meant by "system" in general. Linnaeus opposed system to method or synopsis, and it was systematists, arranging plants in groups ("Phalanges") who produced classifications, not methodists. Method was arbitrary dichotomising and was, Linnaeus suggested, prevalent in the 16th and 17th centuries. System, introduced in the 18th century by Tournefort and Rivinius, was grouping by tens (Linnaeus, 1751a: 10–12). [Stafleu (1971: 45, fn.) confused this important distinction between system and method when he claimed that for Linnaeus' system was the application of logical division, the result of this process being what we would call "a" or "the" system, although for Linnaeus this was in fact a *method*.] Since there were five ranks, class, order (equivalent to families in current usage), genus, species, variety[2], in the Linnaean hierarchy, using method would allow there to be sixteen species—two classes, each divided into two, so four orders, eight genera, and sixteen species. On the other hand, system would yield 10 classes, 100 orders, 1000 genera, 10,000 species and 100,000 varieties, although the latter were of little interest (Linnaeus, 1736: 18; 1751a: 98–99, 101). Linnaeus' calculations were correct. His system could cope with all known plants, since in the first edition of *Species plantarum* he estimated that there were fewer than 10,000 species[3] of plants in existence (Linnaeus, 1753: [ix]). He included there 25 classes, 110 orders, 1098 genera and 5900 species (see also Stearn, 1957: 730); hardly surprisingly, the real and the ideal worlds were not identical, so some taxa have over ten members and many have fewer. These figures are well under half those estimated by Ray (1691), but Linnaeus, even early in his career, had suggested that there were not as many species as some might expect, most plants having already been described (Linnaeus, 1738: [4]).

System was thus a convenient way of organising Linnaean nature. But convenient in what sense? It was system, Linnaeus said, that was "Ariadne's clue" through the labyrinth of form that constituted the living world (Linnaeus, 1736: 18; 1751a: 98; Stafleu, 1971: 156–157); without system there would be chaos. Theseus did not need to know how the labyrinth was constructed, he just needed to be able to get out—which he could, thanks to the thread that Ariadne wove. The botanist did not need to know the shape of nature, he just needed to name plants—which he could, using Linnaeus' system. As Linnaeus (1751a: 202) said, the novice needed to know all the classes, the candidate all the genera, and the master most species. This would be impossible if *Species plantarum* were based on method: "a multitude of genera is a burden on the memory, to be lightened by *System*" (Linnaeus, 1764, *Ordines naturales*: 1). Orders were needed so that genera could be readily distinguished, and this would be easier if there were 10 genera in an order rather than if there were 100 (Linnaeus, 1751a: 101, see also 1736: 19). Size mattered in Linnaeus' attempts to avoid chaos.

Linnaeus also thought that there were unlikely to be more than 100 species in a genus. To distinguish these species a synoptical "semidichotomous" differentia of only twelve words would suffice (it is surely no coincidence that an overly long or sesquipedalian generic name had more than twelve letters; Linnaeus, 1751a: 249–250). Using a combinatorial of six words describing different parts of the plant, each word qualified by an

[1] *Heller (1964) argued that Linnaeus hit on the use of trivial names because he had been using a similar system to refer unambiguously to publications (e.g., Sloane flora = Hans Sloane, Catalogus plantarum quae in insula Jamaica sponte proveniunt...). Koerner (1999: 43-55) argues strongly that the convenience of binomials might have seemed particularly attractive to Linnaeus and his students as they attempted to inventory nature; her account is more in agreement with Linnaeus' brief comments.*

[2] *Linnaeus' initial division of natural history objects into plants, animals, and minerals is not part of this hierarchy (cf. Ereshefsky, 2001b).*

[3] *Elsewhere Linnaeus (1751b: 47) estimated that there were 20,000 species of both plants and animals. As Stillingfleet (1762: 125, fn.) noted, these numbers did not match the numbers of species Linnaeus was actually describing. Koerner (1999: 45) suggests that Linnaeus meant by 20,000 simply "a large number", or he may have been exaggerating. In any event, the number of 10,000 need not reflect Linnaeus' belief in creation (cf. Ereshefsky, 2001a: 212); how broadly or narrowly a taxonomist circumscribed species seems unconnected with religious beliefs (Stevens, 1997b). The first European visitors to the tropics tended to remain near the main centers of European settlement where they would find a weed and crop flora that was becoming rapidly homogenised world-wide (e.g., Merrill, 1954). Coupled with broadly-drawn species limits, such a low number is reasonable.*

adjective describing an alternative form of the part, all 100 species could be distinguished. Linnaeus provided the figures 50, 25, 13, 7, 4, 2 showing how this would work. The descending series of numbers again suggests that Linnaeus was thinking of identification. Even here, however, simple dichotomy could be improved upon. Linnaeus noted that one word could be qualified by several adjectives, so differentiae would rarely need to be even twelve words long (Linnaeus, 1751a: 227–228). Such differentiae are methodical in the sense just mentioned.

Linnaeus as an essentialist. — A specter that hovers over these debates over naming is that of essentialism. Popper (1945, 1966, 1: 31–34, 216) emphasised that what he called methodological essentialism impeded progress in a science. Hull (1965: 317) dubbed such methodological essentialism in taxonomy, "typology" (see also Mayr, 1959, for typological, as opposed to po-pulational, thinking in systematics). Typology consisted of three essentialistic tenets: the ontological assertion that forms or essences exist, the methodological assertion that the task of taxonomy as a science was to discern these essences, and the logical assertion that the description of the essence of an organism was called a definition (Hull, 1965: 317). Hull suggested that the likes of Lamarck and Darwin were typologists (or essentialists) "only in the sense that they retained part of the third element of essentialism—the logic of Aristotelian definition" (ibid.). Systematists remained essentialists, therefore, both as regards species names and the species rank (ibid.: 318, cf. 1) whether or not they were consciously aware of the logic of Aristotelian division (ibid.: 317, fn. 1).

The preceding section suggests, however, that Linnaeus had very definite expectations as to the size and shape of nature at all levels of the hierarchy that he used. Or perhaps the hierarchical structure of his published works reflects in part the constraints of the human mind (Stevens, 1994, 1997a). Or perhaps there is some combination of both. Nevertheless, it is now commonplace to think of Linnaeus as a thoroughgoing Aristotelian essentialist who arranged organisms in a rank hierarchy. However, as Polly Winsor (2001) has emphasised, this is an interpretation of Linnaeus' work developed within the last 50 years [e.g., Cain, 1958; Hull, 1965; see also Sachs, 1875, but, as Wilmott (1950) suggested, caveats are in order when reading Sachs]. Prior to this time, essentialism does not figure largely in the discussion of Linnaeus' work. Thus for Svenson (1945), the major influence on Linnaeus was Ray and, through Ray, Morison; neither Aristotle nor even Cesalpino play major roles (see also Sprague, 1950; Hopwood, 1950). Winsor (2001) notes than Cain's much-cited analysis is flawed, being based on a particular interpretation of Aristotle's logic (see also Atran 1990: 84–87 for Aristotle's ideas when he discussed animals). Indeed, even in the seventeenth century the widely-read Port-Royal logic had made it clear that scientific classification was more than a matter of simple logic (Winsor, 2001; she also notes that just what logical works Linnaeus might have read is unknown). In any event, Cain (1994) later modified his early claims.

It is not only the structure of the Linnaean system just described that does not lend itself to a simple essentialist interpretation. Linnaeus (1751a: 129–131, see Stafleu, 1971: 72–74) emphasised as much the importance of the natural character as the essence when describing genera. The former included features separating the genus from others in the natural order as well as those that separated it from others in artificial orders. He was at pains to say that the character did not make the genus, but the genus the character; the character "flowed" from the genus, not vice versa (e.g., Linnaeus, 1751a: 119). Similarly, Linnaeus's son suggested that Linnaeus put species together in a single genus if he thought that they looked like each other, but not necessarily if they had the same generic or even higher-level characters (Cain, 1958; Stafleu, 1971: 71–72). Linnaeus was also quite clear that if a species was added to a genus, the differentiae of the species it contained might have to be changed; no natural character was infallible until all the species in the genus were known (Linnaeus, 1736: 21; 1751a: 131, 202). When it came to families things were still more difficult. No single character taken *a priori* was useful, rather, the "simple symmetry" of all the parts had to be examined (Linnaeus, 1738: 487).

Thus Linnaean descriptive practice did not entail the listing of all organisms with their genus- and species-level essences. But what about his theory? Linnaeus certainly claimed that both genera and species were natural. That is, the individual genera and species he described were to be found in nature and were part of a rank hierarchy, forming distinct ranks in nature. Linnaeus' goal was to reform generic level nomenclature, quoting Cesalpino (1583; see Greene, 1983, vol. 2: 216), he noted that "if the genera are confused, all is confusion, necessarily" (e.g., Linnaeus, 1751a: 100, 225, 1764: ii). Indeed, the first edition of *Genera Plantarum* appeared in 1737, the first edition of *Species Plantarum* in 1753 (the associated edition of *Genera Plantarum*, published in 1754, is the fifth). Genera were delimited by characters of the fructification, species mostly by vegetative features. Linnaeus thought that the former were more important to the organism; certainly the 26 parts of the fructification, varying in number, figure, position and proportion (Linnaeus, 1737a: [xii]) and appropriately permuted, allowed generic characters to be devised for more genera than Linnaeus knew about (Broberg, 1990).

These parts were like letters of the alphabet: "All these features ["nota"] are letters of plants for us, from which characters of the plants can further be learned by the readers; these the Creator writes" (Linnaeus, 1737a:[ix]). Whether or not he thought that genera were ontologically more important than species (Ereshefsky, 1999, 2001a: 210) is a separate issue; there is no evidence that this is a necessary element of his thought. The question of any difference in ontological importance between genera and orders or classes is moot. Linnaeus did not understand natural orders and classes (Linnaeus, 1737a: [viii], 1751a: 99–101), and since they lacked essential characters, they were like a bell without a clapper (Linnaeus, 1771: [iv]). An understanding of higher-level natural relationships always eluded him.

There is a final issue. We might all agree that "[b]eing parts of a unique and uninterrupted causal sequence is essential" for taxa recognised following a historical approach (e.g., Ereshefsky, 2001a: 29, 209), but such a sequence was just as essential for Linnaeus' species. God created species (e.g., Linnaeus, 1736: 19, 1751a: 991), initially as only one (most plants) or two (many animals) individuals. Successive instantiations of these species were linked to God's original creation by an unbroken genealogical connection (Linnaeus, 1751a: 99)[4].

In the 18th century it was commonly, but not universally, believed that plants self-pollinated, so this genealogical connection could be represented by a single line joining each extant individual to its parent, and so on, finally linking with the originally created form—a kind of genealogical tree. This allows us to understand the "work" done by Linnaean descriptions and essences, be the two identical or not (see also Larson, 1971: 93). Descriptions and essences were features common to all individuals that allowed the naturalist to recognise the units of God's creation; they were means to this end, as much as the end in itself. In an Aristotelian nature, the essence of a species was, loosely, what it did, and knowing such essences was the goal of the observer. Linnaeus, too, was interested in the roles of organisms played in nature, but beyond his essences (however understood) lay God, and Linnaeus' self-appointed task was to name all of God's creations.

We can conclude briefly. The Linnaean system was explicitly designed to produce a classification that the human mind could easily encompass. The great virtue of the binomial was its convenience and its ease of memorisation; it was not, however, the name of the species. Hence, although Linnaeus' vocabulary smacks of scholasticism and essentialism, such an easy interpretation is questionable (see also Jahn, 2000). There is a superstructure of essentialistic rhetoric, and it is rather more than this in places, but the structure of his system, and even his descriptions and taxonomic practice, do not clearly reflect Aristotelian thought. There may be connections between Linnaeus' evident interest in combinatorics, logic (in the sense of methodical ordering of knowledge), memory, and the shape of nature, but they would link Linnaean thought to ideas particularly popular from the 15th to the 17th centuries, a calculus of nature, logic trees that were as much memory trees, and the like (e.g., Broberg, 1990; Rossi, 2000; Stafleu, 1971: 57–58, linked memory and essentialism).

GEORGE BENTHAM

Rank and system in the 19th century. — In the 19th century one of the most pressing questions seems to have been, not whether nature was organised all or in part in a rank hierarchy, but whether it could even be divided into discrete groups and how groups could be recognised. As A.-H. G. de Cassini, a fifth generation Academician, observed, even the apparently most essential generic characters in Asteraceae were liable to change as more species were discovered (Cassini, 1827: 448).

Of course, Cassini's comment may be considered hardly representative, since Asteraceae are a notoriously difficult group. But Linnaeus' immediate successors such as Michel Adanson, Lamarck, and Antoine-Laurent de Jussieu are unlikely to have believed in essences, particularly for taxa above the level of species. They all thought that nature was some sort of continuum, and the relationship between characters and the groups they "characterised" was often not at all straightforward. Jussieu divided families ("orders") and genera when they became too large, as well as being disinclined to recognise monogeneric families (Stevens, 1994: 23–62, 1997a). Lamarck thought groupings above the level of the species were arbitrary and made by people. Classifications were products of human artifice, and appearances to the contrary, corresponded to nothing in nature (Lamarck, 1809 [1984: 56]; Stevens, 1994: 14–22). He also suggested that Linnaean genera relieved the memory by grouping species under a single name (Lamarck, 1778, 1: lxxxiii), or, as he later observed, "the essential object in the formation of genera is absolutely to reduce the number of principal [i.e., generic] names to be retained in the memory" (Lamarck, 1791, 1: xv).

Species, too, did not exist in nature, as Lamarck

[4] *Later Linnaeus (e.g., 1764: v) toyed with the idea of complex series of hybridizations generating the diversity of plants on earth (Bremekamp, 1953; Stevens & Cullen, 1990, for references).*

made clear after he provided a conventional definition for them: "Any collection of like individuals which were produced by others similar to themselves is called a species....to this definition is added the allegation that the individuals composing a species never vary in their specific characters, and consequently that species have an absolute constancy in nature. It is just this allegation that I propose to attack, since clear proofs drawn from observation show that it is ill-founded. ... Let me repeat that the richer our collections grow, the more proofs do we find that everything is more or less merged into everything else, that noticeable differences disappear, and that nature usually leaves us nothing but minute, nay puerile, details on which to found our distinctions" (Lamarck 1809 [1984: 34, 36]). It is difficult to recognise an essentialist position here (cf. Hull, 1965: 317; Ereshefsky, 2001a: 95–96).

Bentham on ranks in nature. — The ideas of George Bentham are particularly interesting given his almost mythical position in the history of botanical systematics, his early background in logic and as an amanuensis of his uncle, Jeremy Bentham (e.g., Bentham, 1823, 1827), and his somewhat grudging acceptance of the idea of evolution. He was also a close confidant of J. D. Hooker and corresponded with Darwin. His interest in botany as a youth began as he used Lamarck and Candolle's *Flore Française*, and this contains the unchanged introduction Lamarck had written for his *Flore Françoise* (Lamarck, 1778).

In his *Outline of a New System of Logic* Bentham noted that naturalists' descriptions consisted of two parts. There was the characteristic phrase, a definition *per genus et differentiam*, which included characters that were frequently difficult to perceive, and a detailed description; this whole process he distinguished from individuation, an individual's only characteristic properties being those of time and place (Bentham, 1827: 79-83). In his early systematic work he suggested that there was a rank of species in nature: "But if, in regard to genera, I have laid down as a principle that the question is not whether such a group is a genus or section? but whether it would be *most convenient to rank* such a group as a genus or as a section? On the other hand there seems still every reason, in the case of species, to consider that it has a really distinct existence in nature as a group of individuals, varying from each other only within the limits of individuals descending from one common stock; and the question is therefore here, are two plants of the same species or not" (Bentham, 1835: xlviii, emphasis original).

Species were different from other taxa. They existed, even if their existence could be demonstrated only by analogy: specimens placed in the one species behaved *as if* they had descended from a common stock (Bentham, 1835: xlviii). Bentham seems not to have stated that species were created by God, although it was common at that time to think of species as having been originally created (Bentham, 1875b: 32), but he allowed that species alone were descended from a common stock, or from "a common parent", as he mentioned in an otherwise very similar definition of species (Bentham, 1858b: xl), later qualifying parent as being "one original plant, or pair of plants" (Bentham, 1861a: 133). Species existed both as a rank, since they alone were united by descent, and as individual groups. Since they had existed for far longer than they had been classified by humans, their initial common parentage was not susceptible of direct proof, rather, inductive evidence used to demonstrate that a particular group of specimens was a species (Bentham, 1861a: 134).

Bentham was clear that there was nothing that made the genus a distinctive rank; it was simply an aggregation of species. This allowed the limits of genera (and families) to be broadly drawn (Bentham, 1858a, 1861a). He argued repeatedly against the existence of generic characters as he discussed the limits of individual taxa (e.g., Bentham, 1861b). But did higher taxa represent groups of species that existed in nature? The quotation above, taken from Bentham's monograph on Labiatae, suggests they might not. He also noted "But although our genera be not in nature, the nearer we follow what is in nature in grouping our plants the more useful is our labour" (Bentham, 1835: xlvii). What guidelines did nature have to offer? Bentham proceeded to discuss the classificatory hierarchy, emphasising that nature could not be represented by a logical hierarchy. He did not say there were no groups in nature, just that they might be hard to characterize; certainly, they could *not* be defined or, as he put it, "positively delimited".

One issue Darwin's *Origin* seems to have raised for Bentham was how to delimit species, not so much in practice, but in theory, given that descent now would unite all taxa. How could the rank of species remain distinct when evolution had destroyed his ranking criterion, that of common descent? The last page of the *British Flora* manuscript has a heavily corrected paragraph in which Bentham toyed with the use of age as some sort of ranking criterion for species. However, this idea appears nowhere else in his writings. Bentham did not suggest that species were separated by breeding barriers (indeed, these would be unnecessary since he thought that self-fertilization was frequent: Bentham, 1861a), so some sort of biological species concept was not an option for him as it was for his contemporaries like Asa Gray and Thomas Henry Huxley. All taxa were related genealogically if the idea of evolution were to be allowed, and there was no fundamental difference between taxa of any rank. As Bentham observed later in life: "In the pre-

Darwinian state of the science we were taught, and I had myself strongly urged, that species alone had definite existence, and that genera, orders, &c. were more arbitrary, established for practical use, .…As there is thus no difference but in degree between a variety and a species, between a species and a genus, between a genus and an order, all disputes as to the precise grade to which a group really belongs are vain" (Bentham, 1875b: 33, 34).

The hierarchy was a flagged hierarchy, a set of words hierarchically arranged, not a rank hierarchy. This did entail a change in how species were treated, Bentham thought. Previously, species had been both diagnosed, the diagnosis consisting of the supposedly fixed characters, and then described, the description including all characters. Now the diagnosis should be no more than a brief indication of the most distinctive characters that would help in preliminary determination—it could even take the form of a key—whereas the detailed description would verify one's determination (Bentham, 1875b: 44, 47–48).

How to make classifications comprehensible. — The limitations of human memory were a matter of concern for Bentham. He thought that systematists should be able to comprehend flowering plants in their entirety, and so the number of families needed to be kept low: "[I]t is felt how useful it is, in the study of affinities, to define correctly and give names to all natural groups of every grade, however numerous they may be, and how easy it is, in the immense variety of language, to coin these names indefinitely; but it is not perceived that in attempting to introduce them all into ordinary botanical language, the memory is taxed beyond the capabilities of any mind, and the original and legitimate object of the Linnean nomenclature is wholly lost sight of. ... So also, so long as the number of orders can be kept within, or not much beyond, a couple of hundred, it may reasonably be expected that a botanist of ordinary capacity shall obtain a sufficient general idea of their nature and characters to call them at any time individually to his mind for the purpose of comparison; but double that number, and all is confusion" (Bentham, 1858a: 31–32).

Given Bentham's visualisation of the natural system and the way he evaluated variation, having decided that there should be around 200 families, the number would not need to change. Bentham saw a conflict between having relatively few families of equal size (presumably this would make key-writing easy) and of equal value, presumably distinctness (Stevens, 1997a); this is perhaps part of the conflict between language and science (see below). The size of the gaps between his taxa was not fixed, and Davis (1978) noted of the *Genera plantarum* (Bentham & Hooker, 1862–1883) that members of small taxa were separated by larger gaps than were used for large taxa at the same rank. This would help prevent the proliferation of small groups.

Genera, too, should be broadly delimited: "for language, the great implement, without which science cannot work, it is of the greatest importance that the groups that give their substantive names to every species they represent [i.e., genera] should remain large" (Bentham, 1858a: 32). If the problem at the species level was the microspecies of those who concentrated on the flora of a limited area, then at the generic level was the small genera recognized by workers who concentrated on a single family alone (Bentham, 1864). Bentham disliked monotypic taxa, since they would tend to clutter the memory, and there are relatively few monotypic families in the *Genera* compared with other classifications (Clayton, 1974; Stevens, 1997a); there are also relatively few monotypic genera.

Such a system of relatively few and large natural taxa would be easy to commit to the memory, especially when memory was reinforced by language. The binomial consisted of a noun for the genus linked to adjectives denoting the species it contained, whereas adjectives treated as nouns signified families (Bentham, 1858a). Grammar and hierarchy were linked.

But beyond this, there is fundamental and comprehensive fine-structuring of the classificatory structure throughout the *Genera*. Bentham wanted to ensure "that there should never be more than ten or twelve, and seldom more than five or six groups of any grade under an immediately superordinate one" (*Genera plantarum* ms., Royal Botanic Gardens, Kew, 169 *recto*). To meet this goal Bentham interpolated a complex hierarchy between his formal taxonomic ranks whenever the size of the group necessitated this. Having such divisions meant that genera in all except monotypic families were placed in groups of similar size, up to six, sometimes 12 members. This, too, would make it easier to memorise the system (Stevens, 1997a). The structure of the *Genera* is guided by cognitive constraints and manages information so that it can be easily assimilated.

A final point is that Bentham's manipulations of taxon size emphasise that taxa at any one hierarchical rank in the *Genera* could not be equivalent. Indeed, when discussing plant distributions of, say, tribes, Bentham himself might merge or divide some of the tribes that he had elsewhere formally recognised so that he was talking about comparable units (e.g., Bentham, 1860, 1875a: Stevens, 1997a).

DISCUSSION

Binomials and essences. — How "natural" taxonomists thought different ranks is an interesting issue, although it is difficult to unravel because a distinction

Table 1. Perspectives of different authors regarding nature of variation at various levels of the hierarchy, whether as groups and/or ranks in nature[1].

Author	Belief in evolution	Genera Group[3]	Genera Rank[3]	Species Group	Species Rank	Varieties[2] Group	Varieties[2] Rank
Linnaeus, 1751	No	Yes	Yes	Yes	Yes	Unimportant	
Lamarck, 1778	No	No	No	Yes	Yes	Unimportant	
Jussieu, 1789	No	Yes	No	Yes	Yes	Unimportant	
Willdenow, 1792; Candolle, 1813; Gray, 1842; Jordan, 1846, 1860	No	Yes	No	Yes	Yes	Unimportant	
Lamarck, 1809	Yes	No	No	No	No	No	No
Lindley, 1832	No	Yes	No	Yes	No	Yes	No
Bentham, 1835	No	Yes?	No	Yes	Yes?	No?	No
Herbert, 1837	Some[4]	Yes	Yes	Yes	No	Yes?	No
Gray, 1850	No	Yes	No?	Yes	Yes	Yes?	No?
Naudin, 1852; Darwin, 1859; Hooker, 1859[5]; Watson, 1859; Bentham, 1875b	Yes	Yes	No	Yes	No	Yes	No

[1]*This table expresses in a very crude way attitudes that were usually delicately nuanced. That a group was discrete, i.e., sharply bounded, and "real", even having an individual essence, existing in nature, did not necessarily mean that the rank to which it belonged had the same properties. But discrete and real are not always synonyms; thus Gilmour (1940) might have allowed that taxa could be discrete, but they might not exist in nature. See Stevens (1994: 177) for a similar table focusing on higher hierarchical levels.*

[2]*Even at the end of the eighteenth century, other infraspecific categories were in use. For example, cultivated plants like cauliflowers and savoy cabbages might be placed in subspecies.*

[3]*"Group" refers to the belief by an author that there were entities at this level, "rank" to his belief that these entities occupied a definite rank in nature.*

[4]*Herbert thought that God created genera; species, etc., evolved from the generic originals.*

[5]*Hooker (1856) thought that fewer than half of the flowering plants could readily be assigned to discrete groups.*

between grouping and ranking was rarely drawn until recently (e.g., Anderson, 1940)[5]. I list in Table 1 a number of systematists for which the distinction can be made. Two points are immediately evident. The first is that there is no uniformity of opinion, some systematists even taking different positions at different stages of their careers. The second is that few systematists believed in a rank of genus, even if they believed in the rank of species (see also Hull, 1965; Stevens, 1992; cf. de Queiroz, 1997: 130). As Asa Gray (1879: 323) noted, for genera even more than species "their limits and content is a matter of judgement, and even of conventional agreement", and his position was a common one. Alphonse de Candolle (e.g., 1855, 1862) was an exception in favouring the idea that higher ranks were more "natural" than lower ranks (Hooker, 1856: 182). Bentham's final dismissal of the idea that there was a ranked hierarchy in nature is by no means unique (e.g., Ereshefsky, 2001a: 231).

Thus botanists' use of the Linnaean hierarchy was usually not accompanied by any obvious belief in either a generic level essence or even essences for individual genera. A belief in the rank of species was commoner, but it was a rank only because God created species. Systematists towards the middle of the 19th century looked for characters that distinguished between species, carefully trying to distinguish those that remained unchanged from those that did not, but such characters are rarely discussed as being essences. Many botanists and zoologists treated species limits as being in appreciable part matters of convention, and their desire to keep "Linnaean" circumscriptions for species was not because such species had essences, but because they were broadly delimited (Stevens, 1997b; McOuat, 2001). The circumscription of essentialism seems rather broad if such people are to be called essentialists. Certainly Darwin (1859: 414–418) downplayed the importance of individual characters, preferring to emphasize the correlation of characters that were individually of "trifling" importance (cf. Hull, 1965). Yet in the 19th century there was an interesting shift in the relationship between the species and the systematist, or at least a shift in the rhetoric of the relationship. With a belief in special creation, a systematist could hardly be said to make species, or call

[5]*Coley & al. (1999) discuss the relative importance of different ranks in folk-biological classifications.*

them his species, or hypotheses; God had done that, and the species were His—they were facts. However, as the 19th century wore on, the species became those of the systematist (Stevens, 1992; the shift had nomenclatural ramifications in terms of how author names were cited, see La Vergata, 1989). We see similar rhetoric in action when the preamble to the *PhyloCode* suggests that those who name following the *PhyloCode* discover species, whereas others create them.

In an important sense the only Linnaean system is that of Linnaeus and his immediate followers. The particular structure of his hierarchy is shaped by the application of the principles of system to a plant world that he thought contained fewer than 10,000 species, a number based as far as can be ascertained on empirical evidence. Neither Aristotelian logic nor special creation seem central to it. *Species plantarum* and *Systema naturae* are alike based on divisions ideally with ten members, not binary or dichotomous (Aristotelian) method (cf. Gould, 2000: 66, 68). Even if Linnaeus is considered to be an essentialist (and we all may be innately essentialists, perhaps even Aristotelian essentialists: Atran, 1990, 1999; Ghiselin, 1997; Griffiths 1997: 180–192; Gelman & Hirschfeld, 1999; Hull, 1999), his classification represents box-in-box relationships among taxa.

Binomials and hierarchy. — The general structure of the Linnaean hierarchy is that of folk taxonomies world-wide, although these are usually shallower (there are usually three ranks) because—and this is the right word—they include about 600 or fewer species-type units, whether plants, animals, or even place names (Berlin, 1992: 96–101). The size and structure of such hierarchies, as well as the semantic structure used to describe them, are such that they are convenient to memorise. The noun + adjective combination in Linnaeus' *Species plantarum* is similar to comparable combinations in folk taxonomies (e.g., Greene, 1888, 1983; Stearn, 1959; Berlin, 1992: 54–60), even if there monotypic genera may be uninomials (uninomialism and essentialism are not necessarily linked, cf. Stafleu, 1971: 57–58). As Bartlett (1940: 353) observed, there is a solid psychological basis for binomial nomenclature, and he saw the nomenclatural reforms instituted by Tournefort and Linnaeus as bringing "the Latin names of plants back into conformity with the usages of common speech" (ibid.: 350). Of course, the names of plants were in Latin since that was still the language of scholars.

The grammatical structure of the binomial, noun plus modifying adjective, and the extended hierarchy fit different understandings of nature so long as their underlying topologies are the same. The noun + adjective combination suggests box-in-box groupings. This is how Darwin saw relationships (Darwin, 1859: 411–413; for Darwin and classification, see Padian, 1999), and similar relationships are depicted in Venn diagrams (for early examples, see de Candolle, 1825–27; Milne Edwards, 1844) and are evident in non-reticulating trees. Any nomenclature which employs binomials, but allows that they do not have to reflect such relationships (e.g., de Queiroz & Gauthier, 1992; Mayr, 1995; Brummitt, 1997; Cantino & al., 1997, 1999: 805) would seem to fly in the face of common understanding (Moore, 1998). That paraphyletic groups of organisms (reptiles, dicotyledons) are commonly or "instinctively" recognised by humans, or, as has been claimed, accord better with vernacular language, cannot be used as a defence of paraphyletic taxa (Ghiselin, 1997; Brummitt, 1997). This confuses naming with interpretation. If no other information is provided about the relationships of a group of taxa, e.g., families in an order, it will hardly be assumed that some are likely to be derived from others. [I am not very happy invoking "common understanding", far less "instinct", but thinking about the former seems appropriate given the emphasis here on communication.]

The binomial implies neither belief in a Linnaean ontology, essentialism, or some outdated paradigm (cf. de Queiroz in Withgott, 2000; Ereshefsky, 2001a, b; Mishler in Pennisi, 2001), it is theory independent (Lidén & al. 1997; Moore, 1998), although it does imply group membership. It is hard to see any logical or philosophical incompatibility between it and phylogenetic nomenclature (Griffiths, 1976; de Queiroz 1988: 241–243, 257; de Queiroz & Gauthier, 1992; Cantino & al., 1999: 791, 806). The argument that binomials necessarily reflect an essentialist world view and so should be discarded, should itself be discarded.

This raises two issues. The first is that we have to be careful when we proscribe words because of the context in which they were earlier used, or we think they were earlier used. At the beginning of the 19th century natural history had little implication of time (Stevens, 1994; Farber, 2000). Cuvier, a self-described antiquarian of nature, saw parallels between fossils as antiquities and the findings of political and moral history, but not of natural history (Rudwick, 1997: 34–35, 174, 183–184). Revolution and evolution carried no connotation of fundamental change (e.g., Gould, 1987), and botany little idea of physiology or anatomy (Stevens, 1994). Should we ban the use of an historicised natural history and evolution, and stop talking about botany, along with gene, homology, species, natural (as in natural system), and so on because they all have had very different meanings in the past?

The second issue concerns the principles of nomenclature. Names for organisms should indeed be clear, universal, and stable, although of course these words themselves do not have clear, universal and stable meanings; flexibility is also important (Lidén & al., 1997). But we give things names primarily because we want to talk more conveniently about them, the very reason why pre-Linnaean polynomials were replaced by binomials. Names are for communication, and for this not any kind of name will do, as the reception of Linnaeus' work suggests and as de Candolle (1813: 222–226) concluded. Names do not have to somehow mirror the nature of the thing being named, however, and the demise of numerous ideal languages or naming systems that incorporated such an idea suggests that it has inherent flaws (see Eco, 1995, for a summary). Hence, whether taxa are some kind of individual or not is irrelevant when it comes to naming; indeed, the complexities of relationships between organisms suggests that any naming system that attempts to take into account all these complexities will fail. Rather, names should not get in the way of ideas. In chemistry the adoption of a new nomenclature profoundly affected the discipline at the end of the 18th century. However, there is no substance that we can equate with "caloric", not all acids contain oxygen as Lavoisier, Guyton de Morveau and the others who proposed the new names believed, and the particular dualistic idea of the composition of salts they adopted was incorrect (Poirier, 1996: 182–190). The principles that guided the formation of the new names were not unlike those that guided Linnaeus.

It is in part because binomials in particular, and the taxonomic hierarchy in general, are not linked to a particular view of the world, but are integral to how we communicate, that they have persisted so long. Binomials, noun + adjective combinations, are effectively invented anew in the context of different beliefs or understandings of the shape of nature, and they take their ontological force from the contexts in which they are used. Nevertheless, education is clearly in order; users need to be alerted that taxonomic hierarchies are simply names that reflect inclusion relationships, they are simply flagged hierarchies (hierarchies, of course, have been misinterpreted, e.g., Stevens, 1997a). Indeed, Bentham himself complained about the law of monotony, uniform terminations for all families (Stevens, 1991), in part because of this possibility. But Bentham was not the only systematist who used a flagged hierarchy while realising that taxa at any one level in the hierarchy were not necessarily comparable. Hewett Cottrell Watson (1859), for example, was also perfectly aware of what classifications could or could not do. Indeed, before rushing off to expunge all ideas of rank from our naming system because of the way in which people interpret hierarchies, we should remember that it is only in the last fifty years or so that we have really been able to analyse and articulate systematic theory (e.g., Mayr & al., 1953), rather than pointing to a body of systematic practice and saying, "go, thou, and do likewise". Education—and change—often take time.

When do uninomials seem preferable? — Linnaeus was clear about both the structure of his classification and the kinds of names to use. His reforms were made in the context of a nature which was broadly understood (at least as far as he was concerned). However, the efficiency of the binomial did not long discourage people from suggesting alternatives. Uninomials will appear a more attractive proposition when generic limits are in flux, because then only the specific epithet appears to be constant (see also Cain, 1958); the name of a species is then independent of its place in the system. As Candolle (1813) noted, Buffon, as well as some early 17th century botanists, favored uninomials, and Buffon was vehemently opposed to Linnaeus' practices in both describing organisms and placing them in a system. Candolle, however, rejected uninomials because they provided no aid to memory, moreover, some organisms really did look strikingly like others, and hence "nomenclature" was a guide to the relationships of species.

Candolle also investigated other options such as giving species separate numbers, but he thought that these, too, were unworkable (Candolle, 1813). However, it was clear to him that the utility of binomials could not be gainsaid: with just 2000 generic names and 1000 specific epithets, two million species could be named unambiguously.

Another reaction to uncertain generic limits is the adoption of a standardised vernacular nomenclature, an option followed by some ornithologists in the later 19th century (Barrow, 2000: 96). As a variant on this theme, the *Rochester Code of Nomenclature*, developed in the United States in part as a revolt against the hegemony of European botanists and their associates in matters nomenclatural, used a similar line of argument in its plea for absolute priority. Because generic limits were uncertain, it was easier to find who first described a species than who first used an epithet in a particular genus (Britton & al., 1888; for the position of Bentham and others, see Stevens, 1991).

Some of the issues that shape current naming proposals concern similar change and uncertainty. Not only are there new principles for taxon circumscription (monophyly), but phylogenetic relationships at the level of conventional genera are seen as being uncertain (Cantino & al., 1999: 798, 804). New methods of analysing data, as well as the production of large amounts of molecular data, are indeed causing extensive revisions to our ideas of relationship, and this forces changes in names for those using a flagged hierarchy. However, if we expect knowledge of relationships to stabilise, then naming systems that are designed to deal with uncertainty may be inappropriate.

Why conventions? — Although we may pooh-

pooh talk of conventions, classifications succeeded in the past in part precisely because they were conventions. Bentham elected to talk about only some of the groups that he thought existed, not about all of them. This is Benthamian knowledge management, his 200 families, his interpolated hierarchy. The groups that he and Hooker recognised, whether in the *Genera plantarum* or in the Colonial Floras, functioned in just this way (Stevens, 1997a). Certainly, given his later belief that there was no fundamental difference between a variety and an order, conventional acceptance or the limits of taxa would be the only way to ensure stability of names.

The situation has not changed. Given the detail of our current hypotheses of the branching of the tree of life, and the many and severe problems surrounding the level of species, we will still need conventions as to which taxa we commonly refer to in conversation (cf. Sterelny & Griffiths, 1999). This will be true if either the number of ranks is increased (Farris, 1976; de Queiroz, 1996: 131; Kron, 1997, for an example; Crane & Kenrik, 1997, for discussion of nomenclatural issues involved), or we adopt a totally new naming system. As ideas of relationships become more detailed, a flagged hierarchy is unlikely to be able to cope, as Daubenton long ago suggested (Llana, 2000). There may ultimately be hundreds of thousands of named clades, but if they are all unflagged and we use them indiscriminately in communication, I see little hope of building up general knowledge. This is why I am interested in the Angiosperm Phylogeny Group's attempts to forge consensus in terms of what larger groups of flowering plants are named and used in general discussion (Angiosperm Phylogeny Group, 1998). This does not disenfranchise the student of a local flora or fauna, of a particular clade, or somebody who wishes all branches of the tree of life to have names. Whatever clades are called, such conventions will be needed. To paraphrase Linnaeus, without convention, all is chaos.

SUMMARY

There are seven principles we can extract from this discussion that are germane to naming in general.

(1) The "Linnaean" hierarchy, in particular the Linnaean binomial, is a special case of how we commonly name objects.

(2) Given the changes in our ideas of relationships over the last two and a half centuries, no naming school can lay an historically-based claim to own "the" Linnaean system.

(3) Mention of essences and of taxonomists as essentialists seems at best a red herring in discussions on how best to name organisms; binomials should not be demonised by being equated with ideas of essentialism and rank hierarchy.

(4) In hierarchies generally, members of individual lower-level groups usually belong to only one higher-level group and do not include groups that are placed at the same hierarchical level; they function best if they refer to objects that have such relationships.

(5) As relationships become more detailed, flagged hierarchies cannot readily develop the depth that is needed to reflect those relationships.

(6) Naming systems for entities whose interrelationships are in flux and those for entities whose relationships are more stable may be different.

(7) What we choose to name and discuss in general conversation, and how we interpret those names, is always a matter of convention and education. The names we commonly use, certainly above the species, are a subset of those we can use.

CONCLUSIONS

Turning now more particularly to the *PhyloCode* and contemporary conventional or not-so-conventional flagged hierarchies, one of the problems dogging the discussion seems to be that particular biological phenomena are invoked to support particular approaches to naming. The problem of ancestors (e.g., Brummitt, 1997; Knox, 1999) looms large for some proponents of flagged hierarchies. Similarly, some supporters of the *PhyloCode* worry about ancestors, others don't care. Justifying paraphyly or the *PhyloCode* because of the problems caused by ancestors is an example of how a special case (detection of an ancestor) makes bad laws (general acceptance of paraphyletic groups or a completely new naming scheme). If ancestors are identified, a flagged hierarchy in which only monophyletic groups are named may well need a convention as to the form their names should take—that is all. Some proponents of all naming schools think there is a rank of species, others do not, or that the idea of monophyly is applicable to species, or it is not (this is linked to the problem of ancestors). Such ideas can be pressed into service to support a particular nomenclatural position, or again, they can be dismissed as being irrelevant. There are parallels here with other debates within systematics (Hull, 1988). Note that these naming arguments do not divide simply along the lines cladistic *versus* evolutionary systematics, or along other conceptual divides. A major—I would argue, *the* major—conceptual divide is between advocates of a Mayrian approach to naming and those who are more interested in strictly monophyletic groups (Stevens, 2000), and if this divide persists, then systematists will "officially" become pluralists in classification (Knox, 1998; Ereshefsky, 2001a, b). We are ultimately arguing about names, and as the last thirty years or so testifies, the politicisation of words has profound and sometimes unforeseen effects.

Nevertheless, there is wide agreement that cuts across schools of naming as to the importance of phy-loge-netic trees and monophyletic groups, so there is a great chance to forge an effective consensus on the relationship between trees and names. If we are mistaken in our current approach to phylogeny detection, however, then this whole discussion may be beside the point![6] We are in a time of change, and it is difficult to realise that if we have any hope of detecting the tree of life, changes to the topology of the tree in areas where it is already elaborated and strongly supported will relatively quickly become few.

I see no fundamental scientific or philosophical issues at stake over the continued use of a flagged hierarchy; differences can surely be negotiated. I repeat what is to me a central point of this essay, that flagged hierarchies in general and binomials in particular are ontologically neutral. There are advantages to both the unmarked hierarchy of the *PhyloCode* and a flagged hierarchy that names monophyletic groups. The *PhyloCode* has more to offer at higher levels of the tree, the flagged hierarchy, perhaps more at lower levels and in general communication. Compromise here is surely possible (see also Cantino, 2000), and the purposes of language and science, at odds for Bentham (1858a: 31), can be reconciled. If relating names to the tree is a common goal, a flagged hierarchy can help in attaining it.

It is also important for us to avoid the equivalent of a train wreck, or worse. If both para- and monophyletic groups are named (the particular form names take is not important here) indiscriminately throughout the tree of life, at the very least we will need some way of distinguishing between taxa that are hypothesised to be monophyletic and those that are paraphyletic (e.g., Wiley, 1981). Taxonomic freedom (Moore, 1998) is not the issue, communication is. If different groups of people apply the same name to different groups of organisms, or different names to the same group of organisms, it will be decidedly unsettling for society and perhaps damaging for our discipline. Along the same lines, Candolle (1813) thought that, having started using binomials, changing names would break the rapport of science with the public, and vice versa (the public that Candolle had in mind is rather different from the public we talk about now). It was such problems with Michel Adanson' s *Famille des plantes* (1763–1764) that reduced its impact (Nicolas, 1963; Stafleu, 1963).

In this context, we should certainly not forget our public stereotype. Louis-Jean-Marie Daubenton noted in 1753 that "naturalistes nomenclateurs" retarded progress by naming things before they were fully described (Daubenton, 1753, pp. 115, 151; see Larson, 1971, pp. 136-137; 1980, for the context of Daubenton' s remark). Botany in particular had been taken over by the nomenclatural issues that should really be only a part of it. Complaints about botanists' love of changing names and about botanical terminology have been a staple for over 200 years, along with others that are equally flattering such as systematists as glorified philatelists or botanists as collectors of hay (e.g., Stevens, 1994; Gould, 2000). Will we live up to our reputation? The value of any naming system is how effectively it establishes conventions that allow people to communicate and to develop their ideas, an issue that geneticists are facing (Pearson, 2001). The Mars Climate Orbiter is what happens when we get our conventions confused, although even there, it would have been a simple matter to convert them. In systematics, we are potentially in a far worse situation.

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[6]*We must not forget the frequent origin of vascular plant species through hybridization, the complex web that constitutes relationships in bacteria, and the series of symbiotic events that underpin the evolution of eukaryotes. As to the last, perhaps informational elements of the genome that control transcription, translation, etc., may be parts of a complex whole into which individual products are tightly integrated; study of their sequences may yield a fairly unambiguous tree. Genes such as thioredoxin reductase coding for housekeeping operations may move more readily between bacteria via lateral gene transfer, sometimes, it seems, almost at will, and different trees may result from sequence analyses of different genes (e.g., Doolittle, 1999; Jain & al. 1999; Gould, 2000; Woese, 2000; Kidwell & Lisch, 2001). Even so, naming is not easy.*

LITERATURE CITED

Adanson, M. 1763–64. *Famille des Plantes.* 2 vols. Vincent, Paris.

Anderson, E. 1940. The concept of genus. II. A survey of modern opinion. *Bull. Torrey Bot. Club* 67: 363–368.

Angiosperm Phylogeny Group. 1998. An ordinal classification for the families of flowering plants. *Ann. Missouri Bot. Gard.* 85: 531–553.

Atran, S. 1990. *Cognitive Foundations of Natural History.* Cambridge University Press, Cambridge.

Atran, S. 1999. Itzaj Maya folkbiological taxonomy. Cognitive universals and cultural particulars. Pp. 17–45 in: Medin, D. L. & Atran, S. (eds.), *Folkbiology.* Massachusetts Institute of Technology Press, Cambridge.

Baldauf, S. L., Roger, A. J., Wenk-Siefert, I. & Doolittle, W. F. 2000. A kingdom-level phylogeny of eukaryotes based on combined protein data. *Science* 290: 972–977.

Barrow, M. V., Jr. 2000. *A Passion for Birds: American Ornithology after Audubon.* Princeton University Press, Princeton.

Bartlett, H. H. 1940. The concept of genus. I. History of the generic concept in botany. *Bull. Torrey Bot. Club* 67: 349–362.

Bentham, G. 1823. *Essai sur la Nomenclature et la Classification des Principales Branches d'Art-en-Science, Ouvrage Extrait de "Chrestomathia" de Jérémie Bentham.* Boussanges Frères, Paris.

Bentham, G. 1827. *Outline of a New System of Logic.* Hunt and Clarke, London.

Bentham, G. 1832–1836. *Labiatarum Genera et Species.* James Ridgeway and Sons, London.

Bentham, G. 1858a. Memorandum on the principles of generic nomenclature as referred to in the preceding paper. *J. Proc. Linn. Soc., Bot.* 2: 30–33.

Bentham, G. 1858b. *Handbook of the British Flora.* Lovell Reeve, London.

Bentham, G. 1860. Synopsis of the Dalbergieae, a tribe of Leguminosae. *J. Proc. Linn. Soc., Bot.* 4 (suppl.): 1–134.

Bentham, G. 1861a. On the species and genera of plants, considered with reference to the practical application to systematic botany. *Nat. Hist. Rev. n.s.* 1: 131–151.

Bentham, G. 1861b. Notes on Ternstroemiaceae. *J. Proc. Linn. Soc., Bot.* 5: 53–65.

Bentham, G. 1864. De Candolle's "Prodromus". *Nat. Hist. Rev. n.s.* 4: 506–528.

Bentham, G. 1875a. Revision of the suborder Mimosoideae. *Trans. Linn. Soc. London* 30: 335–364.

Bentham, G. 1875b. On the recent progress and present state of systematic botany. *Report British Assoc. Advancem. Sci.* (1874): 27–54.

Bentham, G. & Hooker, J. D. 1862–1883. *Genera Plantarum...* 3 vols. Lovell Reeve and Williams and Norgate, London.

Berlin, B. 1992. *Ethnobiological Classification. Principles of Categorization of Plants and Animals in Traditional Societies.* Princeton University Press, Princeton.

Bessey, C. E. 1908. The taxonomic aspect of the species problem. *Amer. Nat.* 42: 218–224.

Bremekamp, C. E. B. 1953. Linné's views on the hierarchy of the taxonomic groups. *Acta Bot. Neerlandica* 2: 242–253.

Britton, N. L., Sterns, E. E., Poggenburg, J. F., Brown, A., Porter, T. G. & Hollick, A. 1888. *Preliminary Catalogue of Anthophyta and Pteridophyta Reported as Growing Spontaneously within One Hundred Miles of New York City.* [Torrey Botanical Club], New York.

Broberg, G. G. 1990. The broken circle. Pp. 45–73 *in:* Heilbron, J. & Rider, R. (eds.), *The Quantifying Spirit.* University of California Press, Berkeley.

Brummitt, R. K. 1997. Taxonomy versus cladonomy, a fundamental controversy in biological systematics. *Taxon* 46: 723–734.

Cain, A. J. 1958. Logic and memory in Linnaeus's system of taxonomy. *Proc. Linn. Soc. London* 169: 144–163.

Cain, A. J. 1959. The post-Linnaean development of taxonomy. *Proc. Linn. Soc. London* 170: 234–244.

Cain, A. J. 1994. Numerus, figura, proportio, situs; Linnaeus's definitory attributes. *Arch. Nat. Hist.* 20: 17–36.

Candolle, A.-L.-P.-P. de. 1855. *Géographie Botanique Rraisonnée...* Masson, Paris.

Candolle, A.-L.-P.-P. de. 1862. Étude sur l'espèce à l'occasion d'une révision de la famille des Cupulifères. *Arch. Sci. Phys. Nat. Genéve sér. 2*, 15: 211–237.

Candolle, A.-P. de. 1813. *Théorie Elémentaire de la Botanique.* Déterville, Paris.

Candolle, A.-P. de. 1825–1827. *Memoires sur la Famile des Légumineuses.* A. Belin, Paris.

Cantino, P. D. 2000. Phylogenetic nomenclature: addressing some concerns. *Taxon* 49: 85–93.

Cantino, P. D., Bryant, H. N., de Queiroz, K., Donoghue, M. J., Eriksson, T., Hillis, D. M. & Lee, M. S. Y. 1999. Species names in phylogenetic nomenclature. *Syst. Biol.* 48: 790–807.

Cantino, P. D., Olmstead, R. G. & Wagstaff. S. J. 1997. A comparison of phylogenetic nomenclature with the current system: a botanical case study. *Syst. Biol.* 46: 313–331.

Cassini, A.-H. G. de. 1827. Synanthérologie. Pp. 443–455 in: *Dictionnaire des Sciences Naturelles*, vol. 51. STI-SYST.-L. Levrault, Strasbourg.

Cesalpino, A. 1583. *Des Plantis Libri XVI.* Marescottum, Florence.

Clayton, W. D. 1974. The logarithmic distribution of angiosperm families. *Kew Bull.* 29: 271–287.

Coley, J. D., Medin, D. L., Proffitt, J. B., Lynch, E. & Atran, S. 1999. Inductive reasoning in folkbiological thought. Pp. 205–232 in: Medin, D. L. & Atran, S. (eds.), *Folkbiology.* Massachusetts Institute of Technology Press, Cambridge.

Crane, P. R. & Kenrick, P. 1996 [1997]. Problems in cladistic classification: higher-level relationships in land plants. *Aliso* 15: 87–104.

Darwin, C. 1859. *On the Origin of Species.* John Murray, London.

Daubenton, L.-J.-M. 1751. In: Buffon, G.-L. L. de (ed.), *Histoire naturelle générale et particulière*, vol. 4. Imprimerie Royale, Paris.

Davis, P. H. 1978. The moving staircase: a discussion on taxonomic rank and affinity. *Notes Roy. Bot. Gard. Edinburgh* 36: 325–340.

de Queiroz, K. 1988. Systematics and the Darwinian revolution. *Phil. Sci.* 55: 238–259.

de Queiroz, K. 1996 [1997]. The Linnaean hierarchy and the evolutionization of taxonomy, with emphasis on the problem of nomenclature. *Aliso* 15: 125–144.

de Queiroz, K. & Gauthier, J. 1992. Phylogenetic taxonomy. *Ann. Rev. Ecol. Syst.* 23: 449–480.

de Queiroz, K. & Gauthier, J. 1994. Toward a phylogenetic system of biological nomenclature. *Trends Ecol. Evol.* 9: 27–31.

Doolittle, W. F. 1999. Phylogenetic classification and the universal tree. *Science* 284: 2124–2128.

Eco, U. 1995. *The Search for the Perfect Language* [transl. J. Fentress]. Blackwell, Oxford.

Ereshefsky, M. 1994. Some problems with the Linnaean hierarchy. *Phil. Sci.* 61: 186–205.

Ereshefsky, M. 1997. The evolution of the Linnaean hierarchy. *Phil. & Biol.* 12: 493–519.

Ereshefsky, M. 1999. Species and the Linnaean hierachy. Pp. 286–305 in: Wilson, R. A. (ed.), *Species: New Interdisciplinary Essays.* Massachusetts Institute of Technology Press, Cambridge,.

Ereshefsky, M. 2001a. *The Poverty of the Linnaean Hierarchy.* Cambridge University Press, Cambridge.

Ereshefsky, M. 2001b. Names, numbers and indentations: a guide to post-Linnaean taxonomy. *Stud. Hist. Phil. Biol. Biomed. Sci.* 32C: 361–383.

Eriksson, G. 1980. The botanical success of Linnaeus. The aspect of organization and publicity. Pp. 57–66 in: Broberg, G. (ed.), *Linnaeus: Progress and Prospects in Linnaean Research.* Hunt Botanical Library, Pittsburgh.

Farber, P. 2000. *Finding Örder in Nature.* Johns Hopkins University Press, Baltimore.

Farris, J. S. 1976. Phylogenetic classification of fossils with Recent species. *Syst. Zool.* 25: 271–282.

Gelman, S. A. & Hirschfeld, L. A. 1999. How biological is essentialism? Pp. 403–446 in: Medin, D. L. & Atran, S. (eds.), *Folkbiology.* Massachusetts Institute of Technology Press, Cambridge.

Ghiselin, M. T. 1997. *Metaphysics and the Origin of Species.* State University of New York Press, Albany.

Gilmour, J. S. L. 1940. Taxonomy and philosophy. Pp. 461–474 in: Huxley, J. (ed.), *The New Systematics.* Oxford University Press, Oxford.

Gould, S. J. 1987. *Time's Arrow, Time's Cycle.* Harvard University Press, Cambridge.

Gould, S. J. 2000. Linnaeus's luck? *Nat. Hist.* 102(9): 18, 20, 22–25, 66, 68–70, 74, 76.

Gray, A. 1842. *The Botanical Textbook.* Wiley and Putnam, New York.

Gray, A. 1850. *The Botanical Textbook*, ed. 3. Wiley and Putnam, New York.

Gray, A. 1879. *Gray's Botanical Text Book*, ed. 6, vol. 1. *Structural Botany, of Organography on the Basis of Morphology, to Which is Added the Principles of Taxonomy and Phytogeography.* Ivision, Blakeman, Taylor & Co., New York.

Greene, E. L. 1888. Botanical literature old and new. *Pittonia* 1: 176–194.

Greene, E. L. 1983. *Landmarks of Botanical History.* Stanford University Press, Stanford.

Griffiths, G. C. D. 1976. The future of Linnaean nomenclature. *Syst. Zool.* 25: 168–173.

Griffiths, P. E. 1997. *What Emotions Really Are.* University of Chicago Press, Chicago.

Heller, J. L. 1964. The early history of binomial nomenclature. *Huntia* 1: 33–70.

Herbert, W. 1837. *Amaryllidaceae.* James Ridgway and Sons, London.

Hopwood, A. T. 1950. Animal classification from the Greeks to Linnaeus. Pp. 24–32, 46–59 in: *Lectures on the Development of Taxonomy***.** Systematics Association, London.

Hooker, J. D. 1856. Géographie botanique raisonnée, par M. Alph. De Candolle. *Hooker's J. Bot. Kew Gard. Misc.* 8: 54–64, 82–88, 112–121, 151–157, 181–191, 248–256.

Hooker, J. D. 1859. *The Botany of the Antarctic Voyage… Part III. Flora Tasmaniae. Vol. 1. Dicotyledones.* Lovell Reeve, London.

Hull, D. L. 1965. The effect of essentialism on taxonomy—two thousand years of stasis. *British J. Phil. Sci.* 15: 314–326 [part I]; 16: 1–18 [part II].

Hull, D. L. 1988. *Science as a Process.* University of Chicago Press, Chicago.

Hull, D. L. 1999. Interdisciplinary dissonance. Pp. 477–500 in: Medin, D. L. & Atran, S. (eds.), *Folkbiology.* Massachusetts Institute of Technology Press, Cambridge.

Jahn, I. 2000. *Geschichte der Biologie*, ed. 3. Spektrum Akademischer Verlag, Heidelberg.

Jain, R., Rivera, M. C. & Lake, J. A. 1999. Horizontal gene transfer among genomes: the complexity hypothesis. *Proc. Natl. Acad. Sci. U.S.A.* 96: 3801–3806.

Jordan, A. 1846. Observations sur plusieurs plantes nouvelles, rare ou critiques de la France. *Ann. Soc. Linn. Lyon* 1: 159–443.

Jordan, A. 1860. Diagnoses d'espèces nouvelles ou méconnues pour servir de matériaux a une flore de France réformée. *Ann. Soc. Linn. Lyon* 9: 373–522.

Jussieu, A.-L. de. 1789. *Genera plantarum…* Hérissant and Barrois, Paris.

Kidwell, M. G. & Lisch, D. R. 2001. Perspective: transposable elements, parasitic DNA, and genome evolution. *Evolution* 55: 1–24.

Knox, E. B. 1998. The use of hierarchies as organizational models in systematics. *Biol. J. Linn. Soc.* 63: 1–49.

Koerner, L. 1999. *Linnaeus: Nature and Nation.* Harvard University Press, Cambridge.

Kron, K. A. 1996 [1997]. Exploring alternative systems of classification. *Aliso* 15: 105–112.

La Vergata, A. 1987. Au nom de l'espèce: classification et nomenclature au XIXe siècle. Pp. 193–225 in: Atran, S. & al. (eds.), *Histoire du Concept d'Espèce dans les Sciences de la Vie.* Fondation Singer-Polignac, Paris.

Lamarck, J.-B.-A.-P.-M. 1778. *Flore Françoise...* 3 vols. Imprimerie Royale, Paris.

Lamarck, J.-B.-A.-P.-M. 1791(–1823). *Tableau Encyclopédique et Méthodique...*3 vols. Panckoucke, Paris.

Lamarck, J.-B.-A.-P.-M. 1809 [1984]; [trans. H. Elliott.]. *Zoological Philosophy* University of Chicago Press, Chicago.

Larson, J. L. 1971. *Reason and Experience.* University of California Press, Berkeley.

Larson, J. L. 1980. Linné's French critics. Pp. 67–79 in: Broberg, G. (ed.), *Linnaeus: Progress and Prospects in Linnaean Research.* Hunt Botanical Library, Pittsburgh.

Lidén, M., Oxelman, B., Backlund, A., Andersson, L., Bremer, B., Eriksson, R., Moberg, R., Nordal, I., Persson, K., Thulin, M. & Zimmer, B. 1997. Charlie is our darling. *Taxon* 46: 735–738.

Lindley, J. 1832. *An introduction to Botany…* Longman, Rees, Orme, Brown, Green and Longman, London.

Linnaeus, C. 1736. *Fundamenta Botanica.* Schouten, Amsterdam.

Linnaeus, C. 1737a. *Genera Plantarum.* Conrad Wishoff, Leiden.

Linnaeus, C. 1737b. *Critica Botanica.* Conrad Wishoff, Leiden.

Linnaeus, C. 1738. *Classes Plantarum.* Conrad Wishoff, Leiden.

Linnaeus, C. 1745. *Öländska och Gothländka Resa.* Kiesewetter, Stockholm.

Linnaeus, C. 1749. *Pan Suecicus.* Uppsala. [Thesis defended by N. L. Hesselgren.]

Linnaeus, C. 1751a. *Philosophia Botanica.* Kiesewetter, Stockholm.

Linnaeus, C. 1751b. *Amoenitates Academiae*, vol. 2. Salvius, Stockholm.

Linnaeus, C. 1753. *Species Plantarum.* Salvius, Stockholm.

Linnaeus, C. 1754. *Genera Plantarum,* ed. 5. Salvius, Stockholm.

Linnaeus, C. 1758 [–1759]. *Systema Naturae*, ed. 10. Salvius, Stockholm.

Linnaeus, C. 1763. *Amoenitates Academiae.* Vol. 6. Salvius, Stockholm.

Linnaeus, C. 1764. *Genera Plantarum*, ed. 6. Salvius, Stockholm.

Linnaeus, C. 1766[–1768]. *Systema Naturae*, ed. 12. 3 vols. Salvius, Stockholm.

Linnaeus, C. 1771. *Mantissa Plantarum altera.* Salvius, Stockholm.

Llana, J. 2000. Natural history and the Encyclopédie. *J. Hist. Biol.* 33: 1–25.

Mayr, E. 1959. Darwin and the evolutionary theory in biology. Pp. 1–10 in: Meggers, B. J., (ed.), *Evolution and Anthropology: A Centennial Appraisal.* Anthropological Society of Washington, Washington, D.C.

Mayr, E. 1995. Systems of ordering data. *Biol. & Philos.* 10: 419–434.

Mayr, E., Linsley, E. G. & Usinger, R. L. 1953. *Methods and Principles of Systematic Zoology.* McGraw-Hill, New York.

McOuat, G. 2001. Cataloguing power: delineating 'competent naturalists' and the meaning of species in the British Museum. *Brit. J. Hist. Sci.* 34: 1–28.

Merrill, E. D. 1954. The botany of Cook's voyages. *Chron. Bot.* 14: 164–383, pl. 80–92.

Milne-Edwards, H. H. 1844. Considérations sur quelques principes relatifs à la classification naturelle des animaux et plus particulièrement sur la distribution méthodique des mammifères. *Ann. Sci. Nat. Zool. Sér.* 3, 1: 65–99.

Moore, G. 1998. A comparison of traditional and phylogenetic nomenclature. *Taxon* 47: 561–579.

Naudin, C. 1852. Considérations philosophiques sur l' espèce et le variété. *Rev. Hort. Sér. 4*, 1: 102–109.

Nicolas, J.-P. 1963. Adanson, the man. Pp. 1–121 in: Lawrence, G. H. M. (ed.), *Adanson, the Bicentennial of Michell Adanson's "Famille des Plantes".* Part 1. Hunt Botanical Library, Pittsburgh.

Nicolson, D. H. 1991. A history of botanical nomenclature. *Ann. Missouri Bot. Gard.* 78: 33–56.

Padian, K. 1999. Charles Darwin's view of classification in theory and practice. *Syst. Biol.* 48: 352–364.

Pearson, H. 2001. Biology's name game. *Nature* 411: 631–632.

Pennisi, E. 2001. Linnaeus's last stand? *Science* 291: 2304–2307.

Poirier, J.-P. [trans. R. Balinski]. 1996. *Lavoisier: Chemist, Biologist, Economist.* University of Pennsylvania Press, Philadelphia.

Popper, K. R. 1945. *The Open Society and its Enemies.* Routledge and Keegan Paul, London.

Popper, K. R. 1966. *The Open Society and its Enemies*, ed. 5. Routledge and Keegan Paul, London.

Ray, J. 1691. *The Wisdom of God manifested in the Works of Creation.* Samuel Smith, London.

Rossi, P. 2000. [trans. S. Clucas]. *Logic and the Art of Memory: the Quest for a Universal Language.* University of Chicago Press, Chicago.

Rudwick, M. J. S. 1997. *Georges Cuvier, Fossil Bones and Geological Catastrophes.* University of Chicago Press, Chicago.

Sachs, F. G. 1875. *Geschichte der Botanik.* Oldenburg, München.

Sprague, T. A. 1950. The evolution of botanical taxonomy from Theophrastus to Linnaeus. Pp. 1–23 in: *Lectures on the Development of Taxonomy.* Systematics Association, London.

Sprague, T. A. 1955. The plan of the *Species Plantarum. Proc. Linn. Soc. London* 165: 151–156.

Stafleu, F. A. 1963. Adanson and his "*Famille des Plantes*". Pp. 123–264 in: Lawrence, G. H. M. (ed.), *Adanson, the Bicentennial of Michell Adanson's "Famille des Plantes".* Part 1. Hunt Botanical Library, Pittsburgh.

Stafleu, F. A. 1971. *Linnaeus and the Linnaeans.* Oosthoek, Utrecht.

Stearn, W. T. 1957. An introduction to the *Species Plantarum* and cognate botanical works of Carl Linnaeus. Pp. ix–xiv, 1–176 in: Linnaeus, C., *Species Plantarum* [reprint]. Ray Society, London.

Stearn, W. T. 1959. The background of Linnaeus's contributions to the nomenclature and methods of systematic biology. *Syst. Zool.* 8: 4–22.

Sterelny, K. & Griffiths, P. E. 1999. *Sex and Death.* University of Chicago Press, Chicago.

Stevens, P. F. 1991. George Bentham and the Kew Rule. Pp. 157–168 in: Hawksworth, D. L. (ed.), *Improving the Stability of Names: Needs and Options.* Koeltz, Königstein.

Stevens, P. F. 1992. Species. Pp. 302–311 in: Keller, E. F. & Lloyd, E. A. (eds.), *Keywords in Evolutionary Biology.* Harvard University Press, Cambridge.

Stevens, P. F. 1994. *The Development of Biological Classification: Antoine-Laurent de Jussieu, Nature, and the Natural System.* Columbia University Press, New York.

Stevens, P. F. 1997a. How to interpret botanical classifications–suggestions from history. *BioScience* 47: 243–250.

Stevens, P. F. 1997b. J. D. Hooker, George Bentham, Asa Gray and Ferdinand Mueller on species limits in theory and practice: A mid-nineteenth-century debate and its repercussions. *Hist. Rec. Australian Sci.* 11: 345–370.

Stevens, P. F. 2000. Botanical systematics 1950–2000: change, progress, or both? *Taxon* 49: 635–659.

Stevens, P. F. & Cullen, S. P. 1990. Linnaeus, the cortex-medulla theory, and the key to his understanding of plant form and natural relationships. *J. Arnold Arbor.* 71: 179–220.
Stillingfleet, B. 1762. *Miscellaneous Tracts Relating to Natural History, Husbandry, and Physic*, ed. 2. R. and J. Dodsley, London.
Svenson, H. K. 1945. On the descriptive method of Linnaeus. *Rhodora* 47: 273–302, 363–388, pl. 990–991.
Watson, H. C. 1859. *Cybele Britannica…* , vol. 4. Longman and Co., London.
Wiley, E. O. 1981. *Phylogenetics: the Theory and Practice of Phylogenetic Systematics*. Wiley, New York.
Willdenow, C. L. 1792. *Grundriss der Kräuterkunde zu Vorlesungen*. Haude and Spener, Berlin.
Wilmott, A. J. 1950. Systematic botany from Linnaeus to Darwin. Pp. 33–45 in: *Lectures on the Development of Taxonomy*. Systematics Association, London.
Winsor, M. P. 2001. Cain on Linnaeus: the scientist-historian as unanalysed entity. *Studies Hist. Phil. Biol. Biomed. Sci.* 32C: 239–254.
Withgott, J. 2000. Is it "so long, Linnaeus"? *BioScience* 50: 646–651.
Woese, C. R. 2000. Interpreting the universal phylogenetic tree. *Proc. Natl. Acad. Sci. U.S.A.* 97: 8392–8396.