What is systematics and what is taxonomy?

Over the past few years there have been increasing numbers of calls for governments to properly fund systematics and taxonomy (and a number of largely molecular-focused biologists insisting they can do the requisite tasks with magic molecule detectors, so don't fund old-school, fund new-fangled-tech). But I think that there is considerable confusion about what systematics and taxonomy *are*.

Now the usual way a philosopher resolves such questions, apart from ~~interrogating their intuitions~~ relying upon what they learned in grade school, is to go find a textbook or some other authoritative source and quote that. If it is someone they already know, all the better, like Mayr or Dawkins. This is problematic, so I thought I'd do a slightly better job at reviewing what people think. And then I will of course give my own view.

Randall Schuh, in his *Biological systematics: principles and applications* (2000) says that systematics and taxonomy are the same thing, and consist of three activities: recognition of species, classification into a hierarchical scheme, and placing this in a broader context. Gurcharan Singh, in his *Plant systematics: An integrated approach* (2004) also treats them as identical activities. Richard Mayden in his *Systematics, historical ecology, and North American freshwater fishes* (1992) does not discuss taxonomy, but defines systematics as "the field of science concerned with reconstructing the evolutionary or ancestor-descendant relationships of groups of organisms, whether fossil or recent, on the basis of heritable traits". Kevin de Quieroz defined it as "the branch of science devoted to the study of the different kinds of organisms (biological diversity, in contemporary terms)".

Overall, there is much confusion, as summarised by Peter F. Stevens in his classic work *The development of biological systematics: Antoine-Laurent de Jussieu, nature, and the natural system* (1994):

Words like "method," "system," and "systematics" are perhaps *the* key words used by [his subjects for the book], and I must clear up some of the ambiguities surrounding their use. First, as to the distinction between taxonomy and systematics, Simpson offered a much-quoted definition of systematics: "the scientific study of the kinds and diversity of organisms and any and all relationships among them." Classification was the grouping of organisms into the hierarchy of a classification; taxonomy was the theoretical study of classification [in his 1961: 7-11]. For Frans Stafleu, on the other hand, taxonomy was represented by keys, systematics by interpretive relationships. Recently, a different distinction has been drawn between systematization and classification, the former being an ordering according to element/system or part/whole relationships, the latter of categories based on common properties.

Ornduff (1969) proposed same view as Simpson:

Taxonomy: classification of taxa (units of classification) in a system that expresses their relationships

Systematics: comparative studies of a systematic unit (i.e., a group of organisms or species and higher), the fact-finding field of taxonomy

However, most systematists today would invert this. What in the sam hell is going on?

The terms were defined independently of each other, by A. P. de Candolle for *taxonomy*, 1813, Lindley for *systematics*, 1830. De Candolle defined classification as having three components in his *Théorie élémentaire de la bontanique* (second edition, page 19): Glossologie (we would call it nomenclature now), Taxonomie (the theory of classifications), and Phytographie (the rules of describing plants). Lindley, an adherent of the Jussieu scheme of multiple lines of evidence rather than single keys in the Linnaean system, used the term “systematic botany” for this approach, which he thought was a natural classification system in contrast to the artificiality of Linnaeus. So far as I can find he did not use the term “systematics” directly.

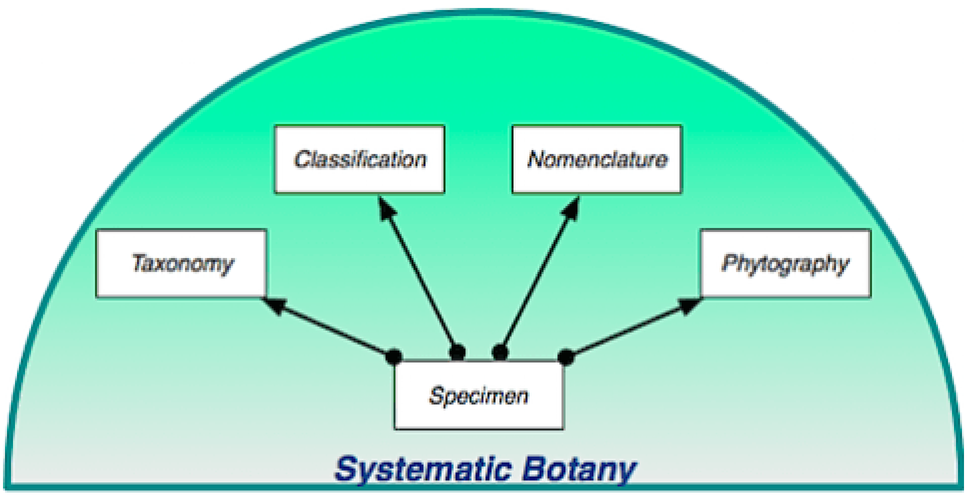
What is significant with de Candolle’s scheme is that he includes arbitrary and natural facets under the single heading of “classification”. Rules of description, or *phytography*, are roughly the same thing as what a modern systematist would mean by “taxonomy”: the description of species from specimens, and what he would mean by *taxonomy* is what we would now mean by *systematics* – the arrangement of the species into schemes of relationships.

A student of both de Candolle’s approach and of Lindley’s in botany, was the very influential Asa Gray. His influence is threefold. One, he was American, and hence influenced the later generations of American botanists and their theoretical ruminations. I found him being quoted as late as 1935 as an authority on just this matter in the *Proceedings of the Linnean Society of London*. Second, he was a botanist, and the botanical discussions ran rather parallel to and in some cases in contradiction to the zoological ones. And third, he was a Darwinian, and so his strictures were regarded as modern enough to accept.

Gray adopts de Candolle’s overall view, despite the fact that classification is now thought to be explained by evolution. Gray, in his *Structural Botany* (1879: 3) distinguishes between

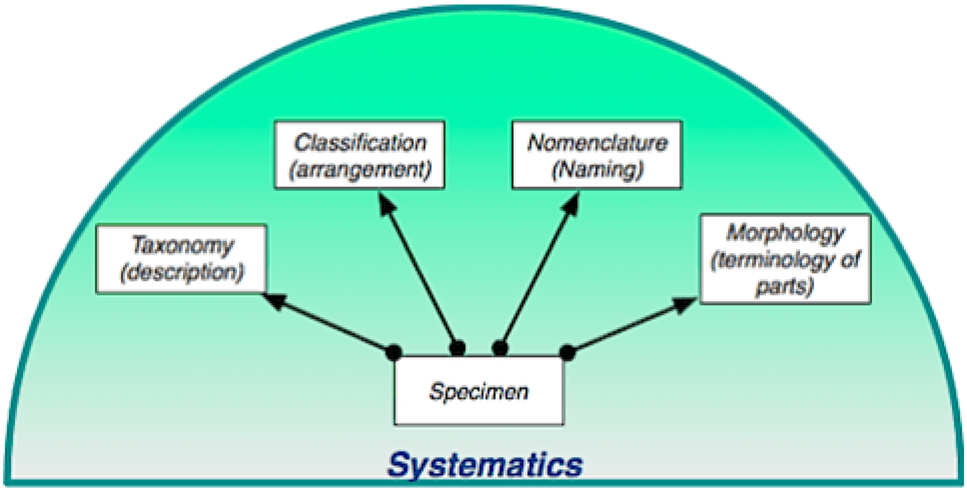
Taxonomy, or the principles of classification, as derived from the facts and ideas upon which species, genera, &c., rest; Classification or the System of Plants, the actual arrangement of known plants in systematic order according to their relationships …

as well as several other aspects of “Systematic Botany” such as Phytography (rules for description), Glossology or Terminology, and Nomenclature (the methods and rules adopted for the formation of botanical names”). Here, Systematics includes both taxonomy and classification. This can be shown as a diagram:



Asa Gray's scheme of classification, 1879: 3

Gray thought, as most systematists did at the time, that the taxonomic and classificatory aspects were in fact dealing with natural truths, the former from a theoretical or methodological point of view, the latter from an empirical or epistemological point of view. The communicative and conventional aspects of nomenclature and phytography, respectively, were not natural. We might generalise this for all scientific classificatory activities thus:



This is eminently sensible, so why is it not currently the default view? A lot happened in the twentieth century, and not all of it was to do with Julian Huxley's *New Systematics* (1940). In a symposium in 1935, held by the Linnean Society of London, Walter B. Turrill introduced the terms "alpha" and "omega" taxonomy, and it caught on. As he defined it (Turrill 1935),

… the time has come when the student of floras whose taxonomy on the old lines is relatively well known should attempt to investigate species by much more complete analyses of a wider range of characters than is now the rule. There is thus distinguished an alpha taxonomy and an omega taxonomy, the latter being an ideal which will probably never be completely realized. … The aim of the alpha taxonomist must be to complete the preliminary and mainly morphological survey of plant-life … Some of the criteria which those who aim at an omega taxonomy are … ecological, genetical, cytological, and biometrical.

So, alpha taxonomy replaced taxonomy in general, conflating the older term for all of systematic biology. But the way Turrill introduced this term indicates that he saw the omega taxonomy as the final form of all classification, along some continuum of completeness and naturalness (cf. his 1940 and 1942).

As the field changed under the influence of Simpson and Mayr's insistence that taxonomy/systematics were the same, and that they were aimed solely at the reconstruction of evolutionary history (the view that Mayden evinces), two further developments arose. One was the *numerical taxonomy* movement started by Sneath and Sokal (1963, Sokal and Sneath 1973). Here several issues were in play. They wished to make classification nonsubjective (that is, objective) and take the decisions out of the hands of biased observers. To do this they introduced mathematical algorithms that could be implemented in computer programs or done by hand. They relied upon *any* data whatsoever, without prior filtering, in order to achieve naturalness. Another issue was that they wished to make the process of classification purely operational, following Percy Bridgman's philosophy that all that counted in science was how things were measured (operations of measurement, hence the name *operationalism*). The problem here that arose was that depending upon the principal components chosen, different taxa fell out. While numerical taxonomy, which came to be known as "phenetics" (from the

Greek *phaneros*, to seem) found structure in the data, it seemed it was not always, or even often, taxonomic structure.

But Sneath and Sokal's books were extremely clear and coherent, and they set up the contrasts in modern taxonomy, which was that taxonomy and systematics were the same thing. Around this time also, Hennig's *Phylogenetic Systematics* (1966) was published in English, along with Lars Brundin's classification of midges using these techniques (1966, 1972a, 1972b, attacked by Darlington 1970), leading to the school[s] of thought now called (following an insult of Mayr's) *cladistics*. On this view, initially, classification was solely aimed at reconstructing evolutionary history (which was something Hennig appealed to Simpson and Mayr in support of). Later, critics argued that classification did not give the history but that the history was an inference or hypothesis based upon and tested by the classification, which was independent (Nelson 1972, Nelson and Platnick 1981, Patterson 1982). The history view was confirmed for philosophers by Elliot Sober's very influential *Reconstructing the Past* (1988).

Finally, a fourth approach arose, one which as yet has no simple name. On this view, classification is dispensed with entirely, and systematics is only about phylogenetic reconstruction, usually employing statistical methods of analysis of very large and mostly molecular data sets. The champion of this view, both theoretically and practically, is Joe Felsenstein, whose recent book *Inferring Phylogenies* (2004) is now the standard bible for methods and algorithms, as well as espousing what he calls the "It Doesn't Matter Very Much" school of classification. Joe, who is one of the most personable folk you'll ever meet in a field that tends towards the fractious, also maintains a site from which you can download nearly every computer program used in systematics these days. I am going to baptise this view *statistical phylogenetics*. Joe holds that classification and the philosophy that underpins it, is a matter of personal preference, and nothing much hangs on what one chooses:

A phylogenetic systematist and an evolutionary systematist may make very different classifications, while inferring much the same phylogeny. If it is the phylogeny that gets used by other biologists, their differences about how to classify may not be important. I have consequently announced that I have founded the fourth great school of classification, the It-Doesn't-Matter-Very-Much school. [2004: 145]

With this, the assimilation of systematics into phylogenetics is complete. All is now subordinated to the finding out of historical pathways that explain our present biodiversity. Evolution über alles!

I reject this for several reasons, some of which I have previously given on this blog; basically it is that classification is a separate activity in science from theory and history. By all means we should try to reconstruct the past, but we do this not by subordinating classification to phylogeny, but by doing phylogeny on the basis of classificatory information. I am not here taking the pattern cladist line: I *do* think we can, to some degree of confidence, reconstruct past sequences. But this is always hypothetical, and requires that we have empirical foundations for our reconstructions independent of our prior assumptions about how biological history unfolds, because biology is a bitch, and she won't be tamed by simplistic schemes, not even of common descent and models of speciation.

So I would urge that we take the generalised version of Gray's "taxonomy" of classification and adopt it in general, and not just for the biological sciences either.

References

Brundin, Lars Zakarias. 1966. *Transantartic relationships and their significance, as evidenced by chironomid midges. With a monograph of the subfamilies Podonominae and Aphroteniinae and the austral Heptagyiae, etc*.: Almqvist & Wiksell: Stockholm.

Brundin, Lars Zakarias. 1972. Evolution, Causal Biology, and Classification. *Zoologica Scripta* 1 (3):107-120.

Brundin, Lars Zakarias. 1972. Phylogenetics and Biogeography. *Systematic Zoology* 21 (1):69-79.

Candolle, Augustine-Pyramus de. 1819. *Théorie élementaire de la botanique, ou exposition des principes de la classification naturelle et de l'art de décrire et d'étudier les végétaux*. 2nd ed. Paris.

Darlington, P. J., Jr. 1970. A Practical Criticism of Hennig-Brundin "Phylogentic Systematics" and Antarctic Biogeography. *Systematic Zoology* 19 (1):1-18.

Felsenstein, Joseph. 2004. *Inferring phylogenies*. Sunderland, Mass.: Sinauer Associates.

Gray, Asa. 1879. *Structural botany, or Organography on the basis of morphology. To which is added the principles of taxonomy and phytography, and a glossary of botanical terms*. New York, Chicago: Ivison, Blakeman, Taylor.

Huxley, Julian, ed. 1940. *The new systematics*. London: Oxford University Press.

Lindley, John. 1830. *An introduction to the natural system of botany: or, A systematic view of the organisation, natural affinities, and geographical distribution, of the whole vegetable kingdom: together with the uses of the most important species in medicine, the arts, and rural or domestic economy*. London: Longman, Rees, Orme, Brown, and Green.

Mayden, Richard L. 1992. *Systematics, historical ecology, and North American freshwater fishes*. Stanford, Calif.: Stanford University Press.

Nelson, Gareth J. 1972. Phylogenetic Relationship and Classification. *Systematic Zoology* 21 (2):227-231.

Nelson, Gareth J., and Norman I. Platnick. 1981. *Systematics and biogeography: cladistics and vicariance*. New York: Columbia University Press.

Ornduff, Robert. 1969. *The systematics of populations in plants*, Systematic Biology Pubn 1692. Washington DC: NAS.

Patterson, Colin. 1982. Classes and cladists or individuals and evolution. *Systematic Zoology* 31:284-286.

de Queiroz, Kevin. 2005. Ernst Mayr and the modern concept of species. *PNAS* 102 (Supp. 1):6600-6607.

Schuh, Randall T. 2000. *Biological systematics: principles and applications*. Ithaca, NY: Cornell University Press.

Simpson, George Gaylord. 1961. *Principles of animal taxonomy*. New York: Columbia University Press.

Singh, Gurcharan. 2004. *Plant systematics: an integrated approach*. Enfield, NH: Science Publishers.

Sneath, P. H. A., and Robert R. Sokal. 1973. *Numerical taxonomy: the principles and practice of numerical classification*, A Series of books in biology. San Francisco: W. H. Freeman.

Sober, Elliott. 1988. *Reconstructing the past: parsimony, evolution, and inference*. Cambridge, Mass.: MIT Press.

Sokal, Robert R., and P. H. A. Sneath. 1963. *Principles of numerical taxonomy*, A Series of books in biology. San Francisco,: W. H. Freeman.

Stevens, Peter F. 1994. *The development of biological systematics: Antoine-Laurent de Jussieu, nature, and the natural system*. New York: Columbia University Press.

Turrill, Walter B. 1935. The investigation of plant species. Proceedings of the Linnean Society of London 147:104–105.

Turrill, Walter B. 1940. Experimental and synthetic plant taxonomy. In *The new systematics*, edited by J. Huxley. London: Oxford University Press:47-72.

Turrill, Walter B. 1942. Taxonomy and Phylogeny. *Botanical Review* 8 (4):247-270.