



How would Darwin classify?

A counterfactual history and philosophy of systematics
With added comments about the use of precursors

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Counterfactual history

Counterfactual history

For some years, historians have been debating the role of counterfactual history as an explanatory style:

Much of the alleged counterfactual history ... is in the end little more than a rather obvious form of wishful thinking. “What if” is really little more than “if only”; and in this form **it contributes nothing to our understanding of what actually did happen, because its concern is not really with examining how and why people ... took the decisions they took, but rather with pointing out supposedly preferable alternatives bemoaning the fact that they never came to pass.**

(Evans 2004)

Others have argued that it is essential to just doing history:

... Questions about counterfactual history raise philosophical puzzles about the nature of causation: What does it mean to say that one thing, or one event, “caused” another? (Sunstein 2016)

Simply ascribing actual causes counterfactually implies what would have happened (or not) had the cause not occurred

Counterfactual history of science

Recently, several historians and philosophers of science have argued for or implemented counterfactuality as a means of understanding what *did* occur in science

- Gould's contingency thesis – the 'tape of life' (Gould 1991)
- Peter Bowler – evolution without Darwin (Bowler 2013)
- Joachim Dagg – motives and merits (Dagg 2019)

I wish to extend counterfactuality today to include what might have happened if the past were *in* the present:

What style of taxonomy/systematics would Darwin do if he walked into a modern biology faculty, after suitable education?

Three styles of systematics

David Hull (Hull 1976) defined three “systematic philosophies” in two categories:

1. Phyletic
 - Evolutionary Systematics
 - Phylogenetic Systematics
2. Phenetic
 - Numerical Systematics

However, Hull’s metataxonomy is not, in itself “natural”. Nor are these “philosophies”. They are methodological stances that may or may not have philosophical undertones.

Similarity and Identity

All three involve the identification of “natural” or “real” groups, and all three generate diagrams to represent them:

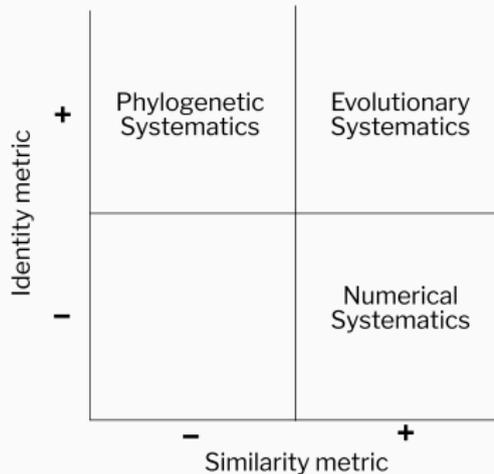


Figure 1: Phylogenetic Systematics uses character identity (*homology*) only, while Numerical Systematics uses character *similarity* only; Evolutionary Systematics relies on both.

Logic of Phylogenetic systematics

1. Homology of character → Apomorphy
2. Apomorphies → Monophyly
3. Monophyly → Natural ('real') groups

Generates **cladograms**; B-tree representations of homologies, explained by shared ancestry (single cut method):

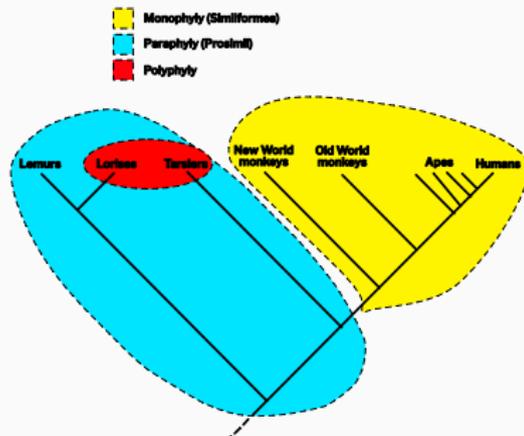


Figure 2: Monophyletic groups (yellow) are natural in cladograms. Polyphyletic (red; 'grade') and paraphyletic (blue; 'primitive') groups are not. Redrawn from: Wikimedia Commons

Logic of Numerical Taxonomy

1. Measure all characters
2. Set up character matrix
3. Perform cluster analysis using distance metrics (Hamming, Manhattan, etc.)

Generates **phenograms**: a 2-dimensional representation of an n -dimensional data matrix in terms of percentage similarity

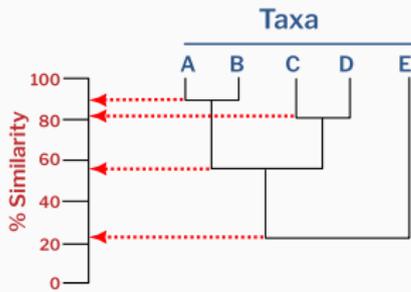


Figure 3: Phenograms do not provide phylogenies and group by similarities. Resemblance to cladograms is coincidental. Redrawn from R. L. Mayden

Logic of Evolutionary Systematics

1. Identify characters (homologies)
2. Identify similarities caused by adaptation
3. Construct tree diagram showing both common ancestry of homologies and grades of adaptation

Generates **phylograms**: where topology shows ancestry and the x-axis shows degree of difference. Also uses phenogram type diagrams, specifying a root to show ancestry.

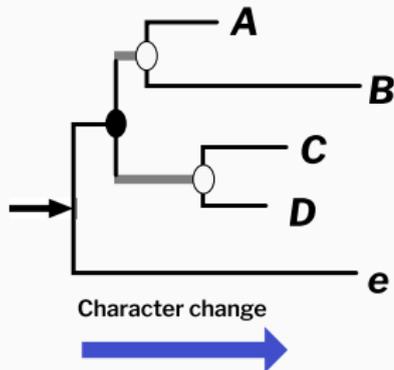
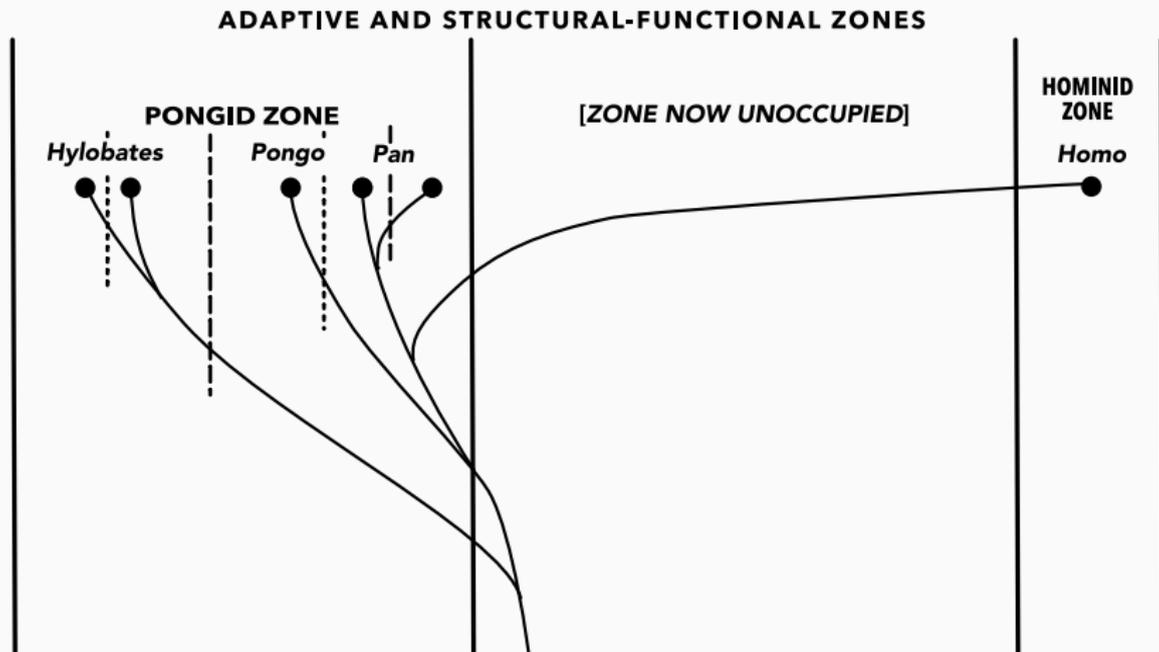


Figure 4: Phylograms show the reconstructed ancestry and the “degree of difference” of taxa. Monophyly is not the sole criterion of natural groups. Source unknown.

Simpson's "Adaptive Zones"



Based on adaptive zones or niches (redrawn from Simpson 1964, 26)

Not concerned with natural groups

Nevertheless, employed in the service of the other three approaches

Uses ranks, which are not considered “natural”

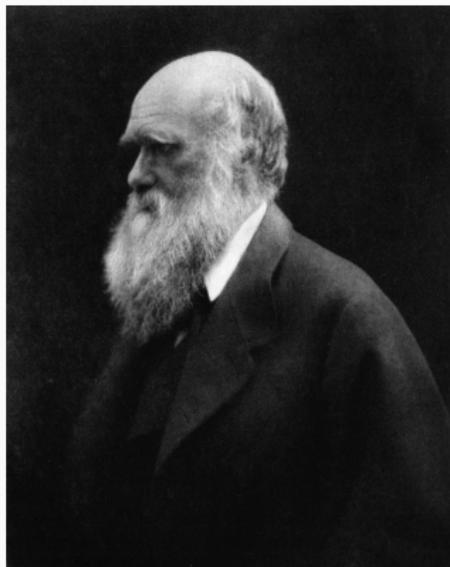
- Except for species, genus, family, phylum, and whatever else the taxonomist considers important in that group
- Mostly, just rules of nomenclature and priority
- Modern practice is to make taxa monophyletic

Linnaean ranks

Kingdom
Phylum
Subphylum
Superclass
Class
Subclass
Infraclass
Cohort
Superorder
Order
Suborder
Infraorder
Superfamily
Family
Subfamily
Tribe
Subtribe
Genus
Subgenus
Species
Subspecies

As at 1945 (Simpson 1945, 15); Over 36 ranks now listed and many more not official (ITIS, accessed 7 May 2019)

Darwin on Classification



Cited by evolutionary systematists:

On this idea of the natural system being, in so far as it has been perfected, **genealogical in its arrangement, with the grades of difference between the descendants from a common parent, expressed by the terms genera, families, orders, &c.,** we can understand the rules which we are compelled to follow in our classification.

(Darwin 1859, 433, emphasis added)

Cited by phylogenetic systematists:

All the foregoing rules and aids and difficulties in classification may be explained, if I do not greatly deceive myself, on the view that the Natural System is founded on descent with modification;—that the characters which naturalists consider as showing true affinity between any two or more species, are those which have been inherited from a common parent, **all true classification being genealogical;—that community of descent is the hidden bond which naturalists have been unconsciously seeking**, and not some unknown plan of creation, or the enunciation of general propositions, and the mere putting together and separating objects more or less alike.

(Darwin 1859, 369, emphasis added)

Numerical taxonomy does not claim Darwin as a precursor, but Adanson (1727–1806):

In 1957 Peter Sneath, a founder of numerical taxonomy, identified Michel Adanson as his precursor, proposing the term *Adansonian* for principles Sneath advocated.

(Winsor 2004, 1)

In his historical review, Vernon ... said that the major factor in [numerical systematics] was a healthy scepticism of taxonomic dogma. He likened it to the Renaissance questioning of medieval dogma. Numerical taxonomy in the broad sense (i.e., including both phenetic and phylogenetic approaches) has been the greatest advance in systematics since Darwin or (because Darwin had relatively little effect on taxonomic practice ...) since Linnaeus. Much was swept away. The method of division from above, the hierarchy of characters, and the primacy of differential characters or of functional characters are little heard of today.

(Sneath 1995, 282)

Still, if they had only used Mill properly:

We did not, I think, emphasize sufficiently Mill's theory of general causes in the early days, which would have linked numerical taxonomy more closely to Darwin's great work (Op. cit., 285)

Mill's "theory" is described as

theory of general causes (i.e., phenetic groups are due to as yet undiscovered causes ...) (Loc. cit.)

However, this is not more closely referenced, and it does not appear in the *System of logic* under that terminology

Even non-Darwinians seek Darwin's approval one way or another, it seems

What did Darwin really think?

Genealogy and degrees of modification

Darwin certainly thought that natural groups are based upon common ancestry. But he also said, immediately following the passage quoted by cladists:

But I must explain my meaning more fully. I believe that the *arrangement* of the groups within each class, in due subordination and relation to each other, **must be strictly genealogical in order to be natural**; but that the *amount* of difference in the several branches or groups, though allied in the same degree in blood to their common progenitor, may differ greatly, being due to the different degrees of modification which they have undergone; and **this is expressed by the forms being ranked under different genera, families, sections, or orders.**

He distinguishes relationship and resemblance, or as he would say, “propinquity of descent” and “affinity”.

Affinity is a loaded term in the debates of the day (Winsor 2015a, 2015b, 2015c), particularly when talking about “natural” classes (*vide* Macleay and the Quinarians) (Gregorio 1996)

Darwin distinguished between affinity and “analogy” (Strickland 1840); redefining the terms to mean homologies and homoplasies, as they came to be known

He wrote to George Robert Waterhouse on 26 July 1843

... classification consists in grouping beings according to their actual relationship, ie their consanguinity, or descent from common stocks— In this view all relations of analogy &c &c &, consist of those resemblances between two forms, which they do not owe to having inherited it, from a common stock.— To me, of course, the difficulty of ascertaining true relationship ie a natural classification remains just the same, though I know what I am looking for.— This being the case viz ignorance of a distinct object I think, we ought to look at classification as a simple logical process, i.e. a means of conveying much information through single words ...

—Finally then I protest against number or grades of organization being used as elements in classification, though I believe they have *silently* been used.—

From <https://www.darwinproject.ac.uk/letter/DCP-LETT-684.xml>

Darwin's "philosophy" of classification

Hence, Darwin held the following ideas, principles and practices

- Natural classes were *monophyletic* (in the cladistic sense); that is, genealogical
- Grades, or degrees of modification, are *not* the basis for classification of natural groups
- Grades *can* be used to group higher taxa for communication purposes (using names)

So while ideally he would have been a cladist, in practice he used similarities (analogies) as convenient groupings.

I think we can conclude that Darwin was a systematist transitional form. To the extent that he had an *a priori* (or theoretical) philosophy of classification, he was cladistic (Padian 1999); but it looks to me that he does not clearly separate affinity from analogy, and hence homology from similarity. But he was a Linnaean in practice.

Scientists and the use of precursors

Scientists' use of history as a weapon

Why do scientists care about being “disciples”?

My hypothesis: it permits polemics in which one can dismiss an opponent's approach as “unscientific” since they do not follow the “precursor”

This then means that history has to be revised to suit the narrative

A good example is the revisionary history by Ernst Mayr

- He is Whiggish and often gives an incomplete history
- He approaches the historical data through prior lenses
- He evaluates people, hypotheses and techniques as “good” or “bad”, or cognates

I will outline a debate between him and his collaborators and those who argue that Darwin is a cladist

Mayr versus the cladists

- Mayr turned to history of biology after he effectively completed his research career in 1975, resulting in his *Growth of biological thought* (Mayr 1982)
- He unashamedly took a Whiggish approach, on the grounds that science made progress
- He also attacked cladists, beginning with Hennig, for not being “traditional”



In *Growth*, he wrote

... the [cladistic] method ignores the fact that phylogeny has two components, the splitting of evolutionary lines and the subsequent evolutionary changes of the split lines. The reason why this latter component is so important for classification is that the evolutionary history of sister groups is often strikingly different. Among two related groups derived from the nearest common ancestor, one may hardly differ from the ancestral group, while the other one may have entered a new adaptive zone and have evolved there into a drastically novel type ... (*Op. cit.*, 230)

His criticisms were announced in his 1974, where he asserted that

The method in which cladistic and phenetic components are combined was originated by DARWIN... (p. 95)

Like Sneath, Mayr cites Mill (Mill 1974) although he gets the page numbers wrong, making it hard to track down.

However, Hennig did *not* ignore difference; he simply rejected the idea that it was useful in classification, as Darwin had:

Mayr's opposing viewpoint is his "evolutionary systematics".... This, too, begins with the construction of a cladogram, but does not produce a hierarchic system directly from the cladogram. Instead, it considers both phylogenetic ("cladistic") and, in particular, adaptiogenetic (ecofunctional) information. Thereby it preserves, according to Mayr, the information of adaptiogenetic divergence in the evolution of sister-groups. As a result, "evolutionary systematics," according to Mayr, satisfies much better than phylogenetic systematics the requirements of a scientific system: to permit the most useful generalizations and explanations (Mayr's "predictions"...). Mayr considers this format of "evolutionary systematics" its "theory"...). But it seems to me that he thereby confuses the concept of a scientific theory with that of a schedule, of taxonomic activity. I doubt, therefore, if Mayr's "evolutionary systematics" is really based on scientific theory ...

(Hennig 1975, 244f)

Hennig is **much** fairer to Mayr's views than *vice versa*

Others joined the fray. Nelson (Nelson 1974) held also that Mayr was misrepresenting Darwin, and that he neither matched modern cladistic criteria nor evolutionary systematics criteria. He adds:

Most taxonomists may be unconcerned if Darwin's philosophy of classification is better exemplified today by Mayr or, alternatively, by Hennig. After all, taxonomic theory and practice should meet modern scientific standards, rather than conform to tradition for its own sake. But what of tradition, even in science? **Traditions, no doubt, exist and exert influence; sometimes, as exemplified by Mayr, they are advocated as orthodoxy.** To me, Darwin's remarks about classification do not lend themselves to the orthodoxy advocated by Mayr. (p. 452)

Nelson's rejection of tradition for its own sake was not well received by the evolutionary systematists.

Responses by cladists ii

Michael Ghiselin (Ghiselin 1985) also defended Darwin against evolutionary systematics:

Yes, as Mayr said, “Darwin’s was a well thought out philosophy of classification.” Mayr’s philosophy could use a few improvements. The root of his difficulties may be that Mayr’s contributions to systematics have been mostly at the species level. Hence, his views about phylogenetics and classification at higher levels have been largely derivative of Simpson. **Darwin and I, on the other hand, had the advantage of devoting some years of study to the evolutionary history of invertebrate subclasses. Such experience provides the sort of familiarity with gradism that breeds contempt.** (p.462)

And there it was left until Mayr and Bock (Mayr and Bock 2002) returned to the topic, and the debate broke out anew (Ghiselin 2004). Although, evolutionary systematics survives (mostly in Germany), cladistics is almost universally adopted.

Conclusions

What would Darwin do?

I think Darwin would be a cladist today.

I do not think he would use phenetic or evolutionary systematic criteria **to classify**, although, like many today, he would find the algorithms and techniques of those fields useful in other respects.

He would not, I think, treat molecular systematics as anything different in principle from comparative (morphological) systematics

But maybe I am projecting my own biases onto the past

What should we do?

With respect to science, no historian nor philosopher has any business dictating how science should be done (*qua* historian or philosopher)

With respect to history, though, historians of science should continue to point out poor history, historicism, and other issues in the ways scientists treat history

And in the end, we are all doing philosophy, so philosophers of science should restrict themselves to pointing out the usual errors, thinking reflectively about science and what scientists do, and generally “clearing the undergrowth” as Locke said

Amen

Thank you.
Questions?
Objections?
Bueller?

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PS: Mill on scientific classification

Cited by Sneath and Mayr¹, Mill actually wrote:

The ends of scientific classification are best answered, when the objects are formed into groups respecting which a greater number of general propositions can be made, and those propositions more important, than could be made respecting any other groups into which the same things could be distributed. The properties, therefore, according to which objects are classified, should, if possible, be those which are causes of many other properties; or, at any rate, which are sure marks of them. Causes are preferable, both as being the surest and most direct of marks, and as being themselves the properties on which it is of most use that our attention should be strongly fixed. But the property which is the cause of the chief peculiarities of a class, is unfortunately seldom fitted to serve also as the diagnostic of the class. Instead of the cause, we must generally select some of its more prominent effects, which may serve as marks of the other effects and of the cause.

(*Logic*, 8th edition, Bk IV, ch. 7, §2, p. 499; the orange text undermines Mayr's reading)

¹It always pays to check Mayr's sources. I have found him trimming quotes before.

First Notebook (Barrett, Paul H. 1960):

Every species is due to adaptation and hereditary structure (*latter far* chief element, therefore little service habits in classification, or rather the fact that they are not [by] far the most serviceable) [279]

The Notebooks (February to July 1838; (de Beer, Gavin 1960))

We now know what is the natural arrangement. It is the classification of relationship, *latter word* meaning descent. [Notebook II, 155]

The one end of classification [is] to express relationship and by so doing discover the laws of change in organization [Notebook II, 158]