

Again on the meaning of categorial ranks in modern evolutionary biology?

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Response to:

Giribet, G., G. Hormiga & G. D. Edgecombe (2016). The meaning of categorial ranks in evolutionary biology. *Organisms Diversity & Evolution* (in press; doi: 10.1007/s13127-016-0263-9)

Main issues:

- 1) “For the great majority of systematists, however, taxa have an objective basis because of the requirement of monophyly.” (p. 1, right column, first paragraph)
- 2) “The Linnaean system is in fact robust to accommodating new organisms, unlike the “ideal” system proposed by Lambertz and Perry (2015).” (p. 2, left column, second paragraph)
- 3) “But when the new taxa are to be accommodated into a pre-existing classification system (e.g., a new species that cannot be placed phylogenetically within any known genus or family), there is little one can do than to utilize such ranks.” (p. 2, right column, fourth paragraph)

Main arguments:

- 1) Giribet et al. (2016) make several fundamental mistakes in recognizing taxa, categories, and concepts, and overall

- still fail to provide a well-substantiated reasoning why categorial ranks should be maintained being used.
- 2) The traditional rank-based system in fact feigns a false robustness, which is what we pointed out in a response to a recent debate on the meaning of phyla, superphyla, and infrakingdoms rather than proposing a new and “ideal” system in our severely length-restricted commentary, as incorrectly suggested by Giribet et al. (2016).
- 3) Phylogenetics and the assignment of a rank are independent processes, and even though the binomial nomenclature inevitably requires the maintenance of the genus as a concept for a taxon, it does not justify it as a rank and even less can be used as an argument why ranks—especially the higher ones originally discussed by us—in general are useful.

“We consider that Lambertz and Perry’s (2015) view is a misinterpretation of how biological classification operates in practice” is one of the conclusions by Giribet et al. (2016), and we must admit that we were quite surprised to read such an interpretation of our comment on the usage of ranks. Especially, as one of the anonymous reviewers stated “[t]he manuscript, Chordate phylogeny and the meaning of categorial ranks in modern evolutionary biology by Markus Lambertz and Steven F. Perry needs to be published. It escapes my scientific (though not social) expertise, how the paper by Noriyuki Satoh to which the current manuscript is a direct reply, could have been published by such an excellent journal in the first place.” We usually do not receive such fiercely positive reviews of our admittedly often controversial manuscripts.

The paper we commented on (Satoh et al. 2014) proposed that the Chordata should be regarded as a superphylum instead of a phylum (thereby making the Deuterostomia an infrakingdom within the subkingdom Bilateria, which

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together with another subkingdom (the “non-bilaterians”) make up the kingdom of the Animalia). It was published online on 17 September 2014. One of us (ML) became aware of it on the evening of 18 September 2014, and the comment was submitted after a brief discussion on 19 September 2014: less than 24 h later. It was a severely length-restricted comment intended to point out the obvious: we are now in the 21st century. It was intended to bring to mind what most readers probably already knew: there are many problems with the rank-based system and a debate on superphyla and such lacks any biological significance. None of this represents a new insight (see e.g., Zachos 2006, 2011), but there obviously was need for a reminder.

Giribet et al. (2016) respond to our “major problem with the rank-based classificatory system [being] the lack of any objective criteria to assign them” that “[f]or the great majority of systematists, however, taxa have an objective basis because of the requirement of monophyly.” They make a major conceptual mistake here in that they equate the assignment of a rank with the taxonomic identification of a group of organisms as a monophylum. These are completely independent processes. Taxa have an objective basis if they are monophyletic, as they then can be regarded as proper evolutionary entities with a degree of individuality (cf. Willmann 1985). Phylogenetic reconstructions lead to a hierarchical topology with several encaptic monophyla represented by the different nodes in the tree. In a process completely independent of recognition of these monophyla, the systematist may name particular nodes of the phylogenetic hypothesis presented. With this subsequent nomenclatural act, a name is assigned to a given (hypothesized) taxon. If, however, a rank is assigned, this represents yet an additional step for most higher taxa.

It escapes our attention why our brief comment was treated as a full analysis of the principles of zoological systematics by Giribet et al. (2016), suggesting that we proposed an “ideal” system, which requires the constant re-ranking of taxa. It was far from our intention, since we realize that such a re-ranking system is highly impractical. In fact, we brought up the need for a perpetual re-ranking in a logically consistent (!) phylogenetic system in order to illustrate how pointless it would be, stating that this is “neither desirable nor attainable.” Instead, our ideal scenario rather would be characterized by the elimination of the avoidable ranks, particularly of infrakingdoms and superphyla.

Only once do we touch on the family as the lowest rank mentioned, when we state that knowing that Emydidae is ranked as a family will not tell you that it is a taxon of turtles, which we still consider accurate. We find it inappropriate to criticize us for not considering the “nitty-gritty details of the Linnean system where >95 % of the potential ranks exist, at the genus and family levels,” because it was neither intended, nor possible in such a brief comment to consider them.

We were also puzzled by the following statement: “We have ourselves named many clades, some using formal Linnaean ranks and others using rankless labels, and it may be the case that in some instances, it is not needed to specify ranks when they are supported by a phylogenetic tree. But when the new taxa are to be accommodated into a pre-existing classification system (e.g., a new species that cannot be placed phylogenetically within any known genus or family), there is little one can do than to utilize such ranks.” If it is not phylogenetically supported, you should not name a supraspecific “taxon,” because it at best is a paraphyletic assemblage (see also Zachos 2014). Nobody forces you to formally describe a name of a taxon according to the rules for e.g., family-group names and what does it change? You should be able to accommodate the taxon in question within a certain monophylum, and if this is not possible with full resolution and it must remain a taxon *incertae sedis* for the time being, so be it. We would like to specifically express that we are not advocating that this should be done, but we would like to point out that endings of taxon names could be just as good as ranks be used to suggest a hierarchical level (Naomi 2014).

There is no way to avoid the genus, due to its role in binomial nomenclature. However, we would like to ask how important the genus as a category actually is if considering it a taxon and a concept (cf. Bock 2004 for his distinction of the species concept, category and taxon)? As evidenced from various debates (e.g., Wheeler and Meier 2000), there is no consensus on a species concept, but most biologists would agree that there is at least a justification for one. Developing a concept for the genus probably is even more difficult than for the species, but this becomes already much more difficult at the family level. Now think about a superphylum or an infrakingdom. More than 200 years ago, Jacob Christian Gustav Karsten (1781–1866) already addressed the genus problem (Karsten 1803) and although this was in a world that was not only pre-Hennigian and pre-Darwinian, but even pre-Lamarckian, we find his conceptual approach to the genus very intriguing: “Nennt man überhaupt Gattung, den Inbegriff der Merkmale mehrerer, unter einander verwandter Arten; so vergesse man nicht dabei zu bemerken, dass diese, so wie überhaupt alle systematischen Eintheilungen, keinen andern Zweck haben, als die große Menge der natürlichen Körper leichter übersehen zu können, oder uns eine dauerendere Kenntniss von denselben, nach gewissen Merkmalen, leichter zu verschaffen. Diesem Zwecke gemäß muss man bei Errichtung einer neuen Gattung verfahren: die Anzahl der Gattungen in einem Systeme muss nicht zu groß sein; aber es müssen auch nicht zu viele Arten unter einem gemeinschaftlichen Begriffe zusammengedrängt sein. In beiden Fällen wird der Zweck, einen gegebenen Körper mit andern, deren man sich sogleich wieder erinnert, zu einem Begriffe zu vereinigen, oder ihn zu erkennen, verfehlt.”

(interpretative English translation by the authors: “With regard to the genus, the quintessence of the characters of related species, one should not forget that this like all other systematic divisions has no purpose other than to make the plethora of natural entities easier to survey or to provide us with an enduring knowledge of them according to certain characters. The erection of new genera has to be carried out according to this purpose: the number of genera within a system must not be too large but neither must there be too many species crowded under a common term. In both cases, the purpose – to unite a given entity with others that are readily recognized under one term – will be missed.”). More recent attempts for instance consider the capability for interbreeding when delimiting genera (Dubois 1988). Monotypic genera may have their justification as well, especially a phylogenetic one.

Böhme (2015) recently presented an analysis of selected amphibian and reptilian taxa over time and showed an intriguing correlation between the occurrence of taxa that are attributed a particular rank and the geological period they originated in and concluded “Given that all of these higher categories into which biodiversity is classified are not objectively definable but human constructs, and that at most (if at all) the concept of a species, with many limitations, corresponds to a real unit in nature, it is even more surprising that this correlation works fairly well over time” (translated by the authors with approval). While it may work fairly well in this case study, it has been noted that there is no “temporal consistency” for such a correlation among disparate lineages (Avisé and Liu 2011) and all proposed attempts to adjust the ranking procedure accordingly have significant shortcomings (Zachos 2011).

As comparative anatomists, we have a desire for precision. Taxonomy at all levels, and the concomitant nomenclature in particular, needs to be as precise as possible, or the effective communication about evolutionary entities is significantly impeded. It is the role of taxonomic consensus to decide which nodes in the tree of life are “relevant” and which name they should bear. This is already debated enough in many cases (e.g., Lambertz 2016), so why should we add another debate about which rank each has to carry? We should not forget that it is the actual evolutionary lineages that we are trying to fit into the drawers of our museums, not vice versa. We still do

not see any advantage in making the label on that drawer larger in order to be able to add an additional rank to the name of the taxon deposited in it.

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