

## OPINION

## Some repentance would not hurt taxonomy either: a junior taxonomist's response to Quentin Wheeler

JIRI HULCR

Department of Entomology, Michigan State University, East Lansing, MI, U.S.A.

I was fortunate enough to attend a talk by Dr Quentin Wheeler at the Entomological Society meeting in Reno, 17 November 2008 entitled 'Unrepentant Taxonomy for the 21st Century'. The talk was a captivating presentation of the ideas also outlined in Dr Wheeler's contribution to the January 2008 issue of this journal (Wheeler, 2008a). The agenda is clear – contemporary taxonomy is an irreplaceable service to humanity as it documents and publicizes the quickly disappearing wonder of biological diversity. Despite its inherent value, it is appallingly underfunded and suffers from low recognition compared with other sciences.

Without doubt, the taxonomic community shares these sentiments, and can unite behind the efforts suggested by Dr Wheeler in his many publications. However, as a taxonomist just entering the field, and in trying to see the situation as pragmatically as possible, I feel that several points in Dr Wheeler's thesis deserve to be revisited and examined for unexplained assumptions. Some may turn out to be the main causes of disagreements not only between the taxonomic community and the outside world, but among taxonomists themselves. First, Dr Wheeler seems to imply that the Hennigian revolution was achieved by us, the taxonomic community, but somehow was lost within funding sources and society at large. Although it is certainly true that the funding sources' appreciation of systematic biology is desperately low, I feel that it may be to some extent our (the taxonomists') own fault, and that it is us who may need to discipline ourselves before we ask grant agencies to do the same. Second, the very definition of 'value' may differ for each of us.

Have we all been doing enough to produce taxonomy that will earn more recognition than in the past? My experience comes from systematics of ambrosia beetles (Curculionidae: Scolytinae), but I suspect you have heard similar scenarios before: despite more than a 100 years of eager collecting and describing species of ambrosia beetles by many authors, the taxonomy of this group is in such chaos, that ecologists or

foresters often have no choice but to designate *de novo* morphospecies for their work. I am glad to say that such scenarios are becoming a thing of the past, as the efficiency of the taxonomic community has improved incredibly. Please do not mistake my critical observations with placing blame on our predecessors. I am fully aware that in the past decades, efficient analytical methods and information technology simply were not available, and that in every scientific field, hypothesis-driven approaches require a few generations to become a standard. But it should not make us excuse the inefficiency, either, because the technological and methodological revolution in taxonomy is by no means finished. A common symptom of this is that, instead of choosing a method that is best for an *a priori* identified goal, for many taxonomists a fondness for a certain method comes first, and is used to define their work. It can be a perfectly valid approach, especially when one masters a skill set as complicated as dissection of insect genitalia. But it starts to be a problem, when the fondness is acknowledged as a more important criterion than utility. Then methods can easily become labels of factions within a divided community.

Dr Wheeler is an accomplished proponent of the necessary transformation of taxonomy into a data- and technology-based science (Wheeler, 2008b). So why the resentment against incorporating modern analyses of molecular data into taxonomy? Why the peculiar disdain for the maximum-likelihood approach, dismissed as a new phenetics (which, of course, it is not, it is a character-based inference)? In principle, a morphological systematist deeply thinking about putative homologies or character state boundaries during the assembly of a dataset for cladistic analysis inevitably is formulating these hypotheses on the background of his or her experience with the variation of such characters. Assumptions about developmental or evolutionary plasticity inherently are present, and represent nothing else than subjective models of processes that gave rise to the variation. Hypotheses about processes, not just patterns, are present in both approaches. The difference I see as more important between a standard maximum-likelihood analysis of molecular characters and a standard way of scoring complex morphological characters is that, in the former, both the data and the model are explicit,

Correspondence: Jiri Hulcr, Department of Entomology, Michigan State University, East Lansing, MI 48824 U.S.A. E-mail: hulcr@msu.edu

whereas morphological variation often has to be coded based on 'experience'. That is not a complaint about morphological taxonomists: morphological characters are often difficult to use despite the best efforts, and we should be honest about their limitations.

The same is true, of course, about the molecular characters. However, the frequent negative references to molecular methods in Dr Wheeler's writing imply a slight bias against them, rather than readiness to test the utility of each, and a readiness to incorporate the useful ones into one's own toolbox. I do not believe that attempts to restore the prestige of morphological systematics need to resemble the fighting of infidels that plagued the competition between pheneticists and cladists. The reason why phenetics succumbed to cladistics was less because of its philosophical inferiority, and more because it simply failed even in obvious cases. It was the empirical correctness and reliability of the cladistic approach that ultimately was chosen by the community. The reason why today molecular characters are often preferred in systematics is not because of 'undisciplined thinking' of molecular systematists, or because of 'political correctness' (Wheeler, 2008a). It is because the amount of molecular data in many cases allows us to gain greater insight into the relationships between taxa than the available morphological characters. Or sometimes they do not. But the decision needs to be a pragmatic one, not philosophical.

My second question is, what values are we working towards? Today, generally, it is accepted that good taxonomy requires a rigorous hypothesis-driven approach, as does any science that wants to thrive. However, the distance between words and deeds may often be a long one. Many fellow taxonomists seem motivated only by the joy of interacting with their organisms. Nothing wrong with it per se, but it is not enough, and some of us do not seem to have a serious agenda beyond that (Evenhuis, 2008). All serious taxonomists I have ever met have been extremely driven people. Often, however, it is not clear what exactly it is that we are driven to. Many of us would probably agree with Dr Wheeler's statement that 'Taxonomy is typically performed best when done for its own sake' (Wheeler, 2008a), but I suspect that few of us agree on the definition of the 'best'. This lack of definition of optimality criterion is bound to cause problems. The history of taxonomic activities in many groups of organisms calls for some notion of direction. The case of ambrosia beetles mentioned above is a story about decades of essentially a lack of usable results, and I do not believe that this would be sustained in any other scientific field without being seen as unworthy of funding. And it is not the grant agencies' fault: for most donors, values lie in results, not merely in activity.

The question of how taxonomists define and conduct good taxonomy is only one part of the problem. Another more complicated one is that the criterion of value may be very different between the taxonomic community and the outside world. Will the quality of systematic papers be enough, or are we expected to deliver more than just good taxonomic research? Will an unrepentant demand for

recognition be enough to persuade our funders and supporters? I believe that the claim that taxonomy should be 'unapologetic' and not judged by its utility to other sciences or other human endeavors is a sentiment that many taxonomists share, but that will only continue to isolate the field. A number of other young taxonomy students around me have confessed similar feelings. For many colleagues from my cohort, such proclamations seem unnecessarily self-centered, not reflecting what we want to achieve with our research, not reflecting the feeling that in the current world, creative exchange of information is often more productive, and more needed, than its accumulation for its own sake. My colleagues and I do not feel threatened by being judged on the grounds of how useful our work is for others. On the other hand, many of us feel estranged by the 'l'art pour l'art' taxonomy denying objective criteria of utility. Many of us feel that somewhere there may lie the source of isolation and lack of funding – in the world where every dollar is competed for, we cannot just claim support based on intrinsic values. I am not proposing that taxonomy should be a service to experimental sciences, and I am the first to defend the inherent intellectual value in describing the Earth's biodiversity. After all, that is why I chose this field for myself. But at the same time, to make the support for taxonomy stable in the long run, we cannot forget to search for ways to return values that will be appreciated by those whose support we need, or explain why our work is valuable to them.

Although Dr Wheeler suggests that taxonomy does not need external criteria of utility, I am inclined to say that he seems to be doing just what he said is not important. His International Institute for Species Exploration is offering taxonomic data to be used in nature conservation, ecology, and other initiatives. Most importantly, the amount of advertisement-type work he has done for taxonomy is unparalleled. Thanks to the efforts of people like Dr Wheeler, the appreciation of taxonomy by funding sources is now greater than in the past, as exemplified by the NSF PEET program. I applaud these efforts, and I support teaching this to the next generation of taxonomists.

There are probably very few differences in opinion between Dr Wheeler, myself, and other taxonomists. We all want taxonomy to thrive, and we are all seeking ways to make it happen. Perhaps it is not the particulars on which we differ, but rather the vocabulary or the tone each of us is using. However, that may actually be the core of the problem. Often it seems, that the tone of taxonomists promoting themselves has been more in the fashion of tribalism, than in the fashion of openness to novel tools and approaches, and often more self-righteous than warranted by our results. That is where a revolution should happen.

### Acknowledgements

This contribution was inspired by discussions with Dr James B. Woolley, Andrea Lucky and Dr Anthony I. Cognato.

**References**

- Evenhuis, N.L. (2008) The “Mihi itch” – a brief history. *Zootaxa*, **1890**, 59–68.
- Wheeler, Q.D. (2008a) Undisciplined thinking: morphology and Hennig’s unfinished revolution. *Systematic Entomology*, **33**, 2–7.
- Wheeler, Q.D. (2008b) Introductory: toward the new taxonomy. *The New Taxonomy* (ed. by Q. D. Wheeler), Systematics Association Special Volume Series, pp.1–18. CRC Press, Boca Raton, FL.

Accepted 31 December 2008