



## A Show of Character—a further response to Wiley et al.\*

ANTHONY C. GILL<sup>1</sup> & RANDALL D. MOOI<sup>2</sup>

<sup>1</sup>*Macleay Museum and School of Biological Sciences, University of Sydney, NSW 2006, Australia*

*E-mail: anthony.c.gill@sydney.edu.au*

<sup>2</sup>*The Manitoba Museum, 190 Rupert Ave., Winnipeg, Manitoba R3B 0N2, Canada*

*E-mail: rmooi@manitobamuseum.ca (corresponding author)*

\**In: Carvalho, M.R. de & Craig, M.T. (Eds) (2011) Morphological and Molecular Approaches to the Phylogeny of Fishes: Integration or Conflict?. Zootaxa, 2946, 1–142.*

Wiley et al. (2011) begin their critique of our paper (Mooi & Gill, 2010) with an assertion: “we need to make it clear that the foundation of their arguments rests not on scientific rigor, but rather on opinions about the re-classification of fishes using molecular data. This bias is the reason that they only targeted researchers who proposed changes in the higher-level taxonomy of fishes using phylogenetic hypotheses based on DNA sequence data (Miya *et al.* 2007, Smith & Craig 2007, Thacker 2009). In criticizing these studies, they do not suggest any alternative relationships or provide any counter evidence to the proposed relationships.” And on page 8, they apparently read our thoughts (aside from the title, none of the words in quotations was written by us in that context) and concluded: “Mooi & Gill entitled their paper “A crisis in fish systematics” because they long for the days when “real” ichthyologists found “meaningful” characters and “true” relationships.” Finally (p. 9), they contend that “Mooi & Gill’s various studies are usually focused on Johnson & Patterson’s (1993: 555) “disparate twigs of the [percomorph] tree,” whereas the explicit studies they criticize are large-scale and taxon rich datasets that have not otherwise been analyzed in Percomorpha.”

The implications are that we are old-fashioned stick-in-the-muds that do not want to see change in traditional classifications. Moreover, they imply that our research experience is limited in scope to minor taxa, and, as a result, we do not understand complex issues (such as homoplasy) associated with the bigger picture. In short, the uninformed reader would be given the impression that our paper was self-serving and that we were perhaps not really qualified to address these issues.

However, Wiley *et al.*’s (2011) accusation of an ulterior motive stands in contrast to the following statement from our paper (Mooi & Gill 2010: 27): “We briefly examine how fish systematics has reached this point and explore the consequences of recent practices by presenting examples from work on percomorphs and gobioids, groups with which we are most familiar. This might give the appearance that we are aiming our comments at only a few practitioners when they are, in fact, only illustrative of systematics approaches that are widespread and of general concern.” We had hoped that this statement would have been satisfactory to move on to the issues that we were really concerned about. However, it seems that we need to set the record straight, so we therefore offer this contribution in partial response to Wiley *et al.* (2011); direct discussion on the real issues beyond Mooi & Gill (2010) is offered in Mooi *et al.* (2011) and in Mooi & Gill (2011).

### “Twigs”

The observation that our work is primarily associated with the twigs at the top of the percomorph tree is true enough, but we have at times dipped into the bush beneath. Our published character surveys span approximately one half of fishes; indeed one paper (Mooi & Gill 1995), in covering over 300 species in over 150 acanthomorph families, stands as one of the broadest surveys of a given character system in fish systematics. Our routine unpublished character surveys are even more widespread, and our literature-based surveys span all fishes (Gill & Mooi

2002). Moreover, and ironically, our papers have served to guide two of the authors of Wiley *et al.*'s (2011) comment (Smith & Craig) in designing their sampling strategies for two papers we critiqued in Mooi & Gill (2010):

“Additional outgroup taxa were included because they have a “type 1” epaxial morphology and/or a parietal lateral-line canal, which Johnson (1993), Mooi & Gill (1995), and Mooi & Johnson (1997) suggested might help resolve the limits of the Scorpaeniformes.” (Smith & Wheeler 2004: 630).

“Our analysis includes representatives from all major lineages within these two families as well as all groups previously conjectured to be related to these families in the extensive morphological studies of Katayama (1959), Gosline (1966), Johnson (1983), the character surveys of Freihofner (1963), Mooi & Gill (1995) [...]” (Smith & Craig 2007: 38)

Apparently our work is of broad enough scope to direct “large-scale” researchers to **subsets** of taxa for study. One might also observe that the “twigs” we work on (such as our detailed studies of gobioids – with perhaps 10% of fish species) are somewhat log-like.

### **No changing room?**

Wiley *et al.*'s (2011) conclusion that we are somehow wedded to traditional classification stands in contrast to our publications, for example:

“As a final comment, I do not believe that the tenuous support for paracanthopterygian monophyly is a unique or even unusual situation. For example, despite recent major advances in acanthomorph phylogeny (e.g., Johnson & Patterson, 1993), the monophyletic status of many of the larger taxa, such as Paracanthopterygii and Perciformes, has been assumed rather than tested. This has resulted from the inherent limitations that must be placed on such studies; most important, it is not reasonable (or possible) to examine and analyze all families within large taxa. Nevertheless, it is important that taxa such as the Perciformes and Paracanthopterygii do not become dogma and that the problems imposed by their possible nonmonophyly on hypotheses of acanthomorph intrarelationship are taken into consideration when evaluating the “fine detail” of those relationships.” (Gill 1996: 1027)

“However, one important concept that the investigation of epaxial muscle variation elucidates is the need to shrug off the straitjacket of present classifications when investigating phylogeny of higher taxa. This is particularly true when the taxa are already recognized as non-monophyletic, undefined, or poorly defined (e.g., Percoidei, Perciformes, Paracanthopterygii), but have in essence been reified over time. It is necessary to look beyond the traditional taxonomic boundaries, not only when dealing with undefined groups such as the percoids, but also when investigating apparently well-defined or well-established taxa such as the scorpaenoids and trachinoids.” (Mooi & Gill 1995: 134)

And Wiley *et al.*'s (2011) notion that we are opposed to recent relationships proposed by molecular systematists is also easily refuted by our actual publications (which are cited in the molecular studies we critiqued). For example:

“[...]Mooi & Gill (1995) recently reclassified this assemblage as the perciform suborder Scorpaenoidei because they found no evidence to corroborate its traditional pre-perciform placement, and they believed that this new classification would stimulate additional work on its interrelationships.” (Smith & Wheeler 2004: 628)

Similarly:

“It is interesting to note that most of the groups in our clade S (e.g., scorpaeniforms, zoarcoids, notothenioids, blennioids, and trichodontids) have a sensory canal associated with the parietal as described by Eakin (1981), Johnson (1993), Mooi & Gill (1995), Mooi & Johnson (1997), and Imamura and Yabe (2002).” (Smith & Wheeler 2004: 638)

Nine years earlier, we had stated:

“The occurrence of Type 1 epaxial morphology in few non-percoid perciform taxa (blennioids, some cirrhitoids and some trachinoids) suggests that these should also be included in a search for a scorpaeniform sister group, or considered for inclusion among scorpaeniforms (Mooi & Johnson, in prep). For example, blennioids also resemble scorpaeniforms in having the supratemporal sensory canal enclosed by the parietal (except in most tripterygiids where the cephalic sensory canals are incompletely enclosed by bone; Springer, 1993:487 and pers. obs.). This condition is found in several other perciform taxa, including at least some zoarcoids (sensu Anderson, 1984; Travers, 1984b; all "zoarceoids" according to Gosline, 1968:46), some pseudochromids (Gill, in prep.), and mastacembeloid synbranchiforms (Travers, 1984a), but these taxa do not have a Type 1 epaxial morphology. Champsodontids more closely resemble scorpaeniforms in having a serrate ridge overlying the canal (Johnson, 1993:14; Mooi & Johnson, in prep.), as well as Type 1 epaxials. Although blennioid parietals lack the serrate ridge or spine over the canal, the possibility of a blennioid/scorpaeniform relationship deserves further study. Certain cottoids closely resemble blennioids in dorsal gill arch morphology, notably in lacking an interarcual cartilage, and in having only a single infrapharyngobranchial (infrapharyngobranchial 3), which articulates posteriorly with epibranchials 3 and 4 (e.g., compare cottoids in Rosen & Patterson, 1990: figs. 34A, C and Yabe, 1985: figs. 23, 24E with blennioids in Rosen & Patterson, 1990: figs. 33A--B, 37, 38C--D and Springer, 1993: fig. 1). Members of the cottoid family Liparididae further resemble blennioids in lacking an uncinat process on epibranchial 1 (Kido, 1988: figs. 12A-D).” (Mooi & Gill 1995: 128).

Likewise:

“Finally, given the placement of the former serranid fishes among various mail-cheeked fish clades, we follow Imamura & Yabe (2002) in classifying the revised Serranidae and Epinephelidae in the Scorpaenoidei.” (Smith & Craig 2007: 51).

And, 12 years previous to Smith & Craig (2007):

“Several additional characters suggest that a relationship between scorpaeniforms and at least some of the "percoids" with a Type 1 epaxial morphology is worthy of consideration. For example, some larval serranids (particularly anthiines) bear at least a superficial resemblance to larval scorpaeniforms, with suspensorial and cranial bones highly ornamented by spines and ridges (cf. figs. and descriptions in: Baldwin, 1990; Johnson, 1984; Kendall, 1984; Washington et al., 1984). Moreover, the general physiognomies of many adult serranids bear striking resemblances to certain scorpaeniforms. Although general similarities do not provide the necessary evidence for relationship, they hint that there might be more evidence than shared epaxial morphology; we feel it is premature to dismiss these similarities as being due to convergence before relationships are better understood.” (Mooi & Gill 1995: 128).

If a common theme could be identified in our various studies, it is one of questioning traditional classifications; indeed, we consider that traditional classifications have often restricted character surveys thereby impeding progress in our understanding of character distribution and, subsequently, of relationships.

### **And now for something completely different**

Wiley *et al.* (2011) complain that Mooi & Gill (2010) did not provide alternative relationships, as if progress only can be made by the suggestion of alternative hypotheses. This reaction is surprising, given that previously Wiley in Wiley *et al.* (2000: 348) noted:

“Morphological evidence for the monophyly of Paracanthopterygii (summarized by Patterson & Rosen, 1989) is tenuous at best (Gill, 1997).”

However, Gill (1996, not 1997 as incorrectly cited) did not provide alternative relationships, he simply questioned the validity of the purported synapomorphies of the Paracanthopterygii. But this apparently was seen as a valid contribution by Wiley *et al.* (2000), because it (a non-monophyletic Paracanthopterygii) agreed with their conclusions. It seems then that one can pick and choose when considering what counts as progress. However, regardless of Wiley's inconsistent attitude, there was no need to provide alternative relationships in Mooi & Gill (2010), because alternative relationships were provided in the studies we contrasted: Smith & Wheeler (2004) versus Smith & Craig (2007), Thacker (2000) versus Thacker (2003) versus Thacker (2009) versus Thacker & Roje (2009), and Olney *et al.* (1993) versus Miya *et al.* (2007).

## Summary

In short, the charges leveled against us by Wiley *et al.* (2011) do not stand up to scrutiny. We had no ulterior motive in bringing what we consider to be legitimate concerns to the attention of our research community. We hope that the real issues—as detailed in our paper (Mooi & Gill 2010)—can now be discussed without further distraction.

## Acknowledgements

We thank M.R. de Carvalho and M.T. Craig for arranging the publication of this special issue of *Zootaxa*. M. Ebach and D. Williams commented on earlier versions of this paper. Research was supported by a Natural Science Foundation (USA) award DEB-0541914 (ACG) and a Natural Sciences and Engineering Research Council of Canada Discovery Grant 327844-06 (RDM).

## References

- Gill, A.C. (1996) Comments on an intercalary path for the glossopharyngeal (cranial IX) nerve as a synapomorphy of the Paracanthopterygii and on the phylogenetic position of the Gobioidae (Teleostei: Acanthomorpha). *Copeia*, 1996, 1022–1029.
- Gill, A.C. & Mooi, R.D. (2002) Phylogeny and systematics of fishes. In: P.J.B. Hart & J.D. Reynolds (eds). *Handbook of Fish Biology and Fisheries. Volume 1. Fish Biology*. Blackwell Science, Oxford, pp. 15–42.
- Miya, M., Holcroft, N.I., Satoh, T.P., Yamaguchi, M., Nishida, M. & Wiley, E.O. (2007) Mitochondrial genome and a nuclear gene indicate a novel phylogenetic position of deep-sea tube-eye fish (Stylephoridae). *Ichthyological Research*, 54, 323–332.
- Mooi, R.D. & Gill, A.C. (1995) Association of epaxial musculature with dorsal-fin pterygiophores in acanthomorph fishes, and its phylogenetic significance. *Bulletin of the Natural History Museum, London, Zoology Series*, 61, 121–137.
- Mooi, R.D. & Gill, A.C. (2010) Phylogenies without synapomorphies, a crisis in fish systematics: time to show some character. *Zootaxa*, 2450, 26–40.
- Mooi, R.D. & Gill, A.C. (2011) Why we shouldn't let sleeping dogmas lie – a partial reply to Craig. *Zootaxa*, 2946, 41–44.
- Mooi, R.D., Williams, D.M. & Gill, A.C. (2011) Numerical cladistics, an unintentional refuge for phenetics – a reply to Wiley *et al.* *Zootaxa*, 2946, 17–28.
- Olney, J.E., Johnson, G.D. & Baldwin, C.E. (1993) Phylogeny of lampridiform fishes. *Bulletin of Marine Science*, 52, 137–169.
- Smith, W.L. & Craig, M.T. (2007) Casting the percomorph net widely: the importance of broad taxonomic sampling in the search for the placement of serranid and percoid fishes. *Copeia*, 2007, 35–55.
- Smith, W.L. & Wheeler, W.C. (2004) Polyphyly of the mail-cheeked fishes (Teleostei: Scorpaeniformes): evidence from mitochondrial and nuclear sequence data. *Molecular Phylogenetics and Evolution*, 32, 627–646.
- Thacker, C. (2000) Phylogeny of the wormfishes (Teleostei: Gobioidae: Microdesmidae). *Copeia*, 2000, 940–957.
- Thacker, C.E. (2003) Molecular phylogeny of the gobioid fishes (Teleostei: Perciformes: Gobioidae). *Molecular Phylogenetics and Evolution*, 26, 354–368.
- Thacker, C.E. (2009) Phylogeny of Gobioidae and placement within Acanthomorpha with a new classification and investigation of diversification and character evolution. *Copeia*, 2009, 93–104.
- Thacker, C.E. & Roje, D.M. (2009) Phylogeny of cardinalfishes (Teleostei: Gobiiformes: Apogonidae) and the evolution of visceral bioluminescence. *Molecular Phylogenetics and Evolution*, 52, 735–745.
- Wiley, E.O., Chakrabarty, P., Craig, M.T., Davis, M.P., Holcroft, N.I., Mayden, R.L. & Smith, W.L. (2010) Will the real phylogeneticists please stand up? *Zootaxa*, 2946, 7–16.
- Wiley, E.O., Dimminck, W. & Johnson, G.D. (2000) The interrelationships of acanthomorph fishes: a total evidence approach using molecular and morphological data. *Biochemical Systematics and Ecology*, 28, 319–350.