



## **Nomenclatural errors in moa taxonomy: A reply to Worthy**

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- Pages 18–29 in *Festschrift for Ned K. Johnson: Geographic Variation and Evolution in Birds* (C. Cicero and J. V. Remsen, Jr., Eds.). Ornithological Monographs, no. 63.
- BAKER, A. J., L. HUYNEN, O. HADDRATH, C. D. MILLAR, AND D. M. LAMBERT. 2005. Reconstructing the tempo and mode of evolution in an extinct clade of birds with ancient DNA: The giant moas of New Zealand. *Proceedings of the National Academy of Sciences USA* 102:8257–8262.
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**Nomenclatural errors in moa taxonomy: A reply to Worthy.**—In reproducing the control-region gene tree from Baker et al. (2005), I was careful to state in the text of my review (Baker 2007:22) that “Bayesian analysis of these sequences recovered 14 monophyletic lineages, 9 of which are currently recognized, plus 5 new lineages that may warrant species status.” It certainly was not my intention to revise the taxonomy of the moas, but rather to draw attention to some lineages that almost certainly deserve species status on the basis of their phylogenetic depth in the tree. I erred by changing the caption of the figure to include several “n.sp.” labels, and provided Worthy (2007) with an opportunity to accuse me of doing

a disservice to moa taxonomy. However, although he had no difficulty in suggesting what the correct names should be in the event of a taxonomic revision of the moas, I am not as confident as he in making these assertions. Unless the types have been identified correctly (as they obviously had not been in the past; e.g., Worthy 2005) and they have also been genotyped, there is still doubt as to what nomenclature is correct.

The recent update on moa systematics that Worthy chides me for overlooking was published in the journal *Tuhinga* (Worthy 2005). I was unfamiliar with this journal, as most readers probably are. This does not excuse me for not locating it, because in the paper he purports to have rediscovered the types of *Dinornis curtus* Owen and *Palapteryx geranoides* Owen. Examining the features of a left tibiotarsus confusingly marked with four different catalogue numbers, Worthy determined that this is the missing lectotype of *D. curtus* and that it is referable to *Euryapteryx curtus*. Genomic DNA had been extracted from this bone and a sequence would be published later, but to my knowledge this has not been done. So we lack concrete proof that the above synonymy is correct, though it could well be. DNA sequences of what was then called *E. curtus* and *E. geranoides* were shown to be very similar (Baker 2007), thus invalidating claims by others, including Worthy, that there were two species of *Euryapteryx* in New Zealand. However, as Worthy (2007) pointed out, this does mean that I should have referred to this lineage as *E. curtus* if the above synonymy is correct. Equally, it means that Worthy (2005:40) was wrong to propose that *E. curtus* should be applied to the “small exclusively North Island form” and *E. gravis* to “a larger form found in both the North and South Islands.” Instead, this is probably an example of geographic variation in one species. Worthy really ought to practice what he preaches about nomenclatural confusion being a disservice to moa taxonomy.

The other type, an almost complete cranium labeled *Palapteryx geranoides*, was judged on morphological characters to be conspecific with *Pachyornis mappini*, even though it is very similar to the cranium of *E. curtus*. This judgment may well be correct as well, but again it needs to be confirmed with DNA typing. If it turned out to have a DNA sequence identical to that of *E. curtus*, the synonymy proposed by Worthy (2005) would be a “taxonomic disservice” and much of his criticism of my use of taxonomic names would crumble. I note that Worthy has made nomenclatural errors in labels he has attached to specimens in the Canterbury Museum in New Zealand involving *Pachyornis* and *Euryapteryx*, thereby further confusing the taxonomic identity of these lineages and leaving me wondering how one can be certain about the above synonymy. Femurs of AV8264 from Kapua and THW214 from Cheviot were labelled *E. geranoides* by

Worthy, but they both have DNA sequences of *P. elephantopus*. Worthy rightfully corrected me about misassigning *Megalapteryx* species, but nevertheless his synonymy (Worthy 1988:107) of the lineages from either end of the South Island on the basis of “a north–south cline combined with temporal variation” is clearly rejected by DNA typing (irrespective of whether the type specimen of *M. benhami* has been sequenced). Worthy (2007:1448) argues that we “did not sample any specimens that could be referred to *M. benhami* based on size,” and therefore that our sequences have no bearing on its taxonomic status. This is exactly counter to the reasoning he used in synonymizing the different size forms (Worthy 1988) and is, therefore, contradictory logic. Further work is desirable before names are applied to the new lineages of *Dinornis* identified in Baker et al. (2005), but—as was pointed out in that paper—the distribution of the specimens we typed roughly coincides with previously recognized taxa that have been lumped together in more recent taxonomic revisions. The bottom line is that the assignment of taxa based on isolated bones recovered from sites with mixtures of species or composite specimens has led to numerous nomenclatural errors, including those made by Worthy and me, but this debate will no doubt make all of us more careful in the future. Ancient DNA will provide a critical source of characters in any future taxonomic revisions and, hopefully, resolve the tangled web of splitting and lumping that has characterized moa taxonomy in the past. I am sure that if Ned Johnson were still alive, he would agree that this represents a molecular advance in the study of geographic variation and speciation.—ALLAN J. BAKER, Department of Natural History, Royal Ontario Museum, Toronto, Ontario M5S 2C6, Canada. E-mail: allanb@rom.on.ca

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**The foot of *Archaeopteryx*: Response to Feduccia et al. (2007).**—Without examining the original specimen first-hand, Feduccia et al. (2007) questioned some observations in our recent description of a new skeleton of *Archaeopteryx* (Mayr et al. 2005, 2007). We disagree with several of their comments and believe that they gave an inaccurate account of the results of our studies in several instances.

Our statement that most specimens of the Archaeopterygidae are known from fragmentary or poorly preserved specimens was based on the fact that four of the nine specimens are known from very incomplete remains only (Maxberg, Haarlem, eighth and ninth specimens) and that only three skeletons are largely complete and have well-preserved bones (Berlin, Eichstätt, and Solnhofen specimens). Because only three of the nine specimens are well preserved, we cannot see how our statement “is easily discredited by the numerous published photographs of these specimens” (Feduccia et al. 2007:373).

Feduccia et al. (2007:373) further wrote that we reinterpreted the anatomy of the new specimen “to conform to the now largely discredited terrestrial theory for the origin of flight.” This is not correct. In fact, we did not comment at all on the lifestyle of *Archaeopteryx* in our 2005 study and, in the detailed description, only concluded that *Archaeopteryx* probably “spent most of its time on the ground” (Mayr et al. 2007:114). In the latter study, we further noted that *Archaeopteryx* did not have a perching foot (i.e., a foot with a large retroverted hindtoe), but nowhere did we comment on whether or not it was able to perch.

Likewise, although we believe that the hallux of archaeopterygids was spread medially, we did not claim that it was spread “at a right angle to the other claws [sic]” (Feduccia et al. 2007:374). Instead, we wrote (Mayr et al. 2005:1485) that

on both feet, the first metatarsal attaches to the medial surface of the second metatarsal..., not to its plantar surface as in extant birds with a retroverted first toe.... The shaft of the first metatarsal does not exhibit the torsion that is characteristic of birds with a fully retroverted first toe.... The proximal phalanx of the first toe further exposes its mediadorsal surface.... Because the metatarsals are visible