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Books, book reviews, anoles, and do we really know what we believe we know?

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McCranie & Köhler (2015, hereafter M&K) recently published a book on the systematics, distribution, and conservation of the anoles (Squamata: Dactyloidae) of the Central American country of Honduras. Those authors included 39 species placed in two genera. Gray (2015; hereafter G) published a largely well-done review of that book. However, G made a few incorrect statements, some of which I feel need to be addressed. In addition, the anole systematics at the species, and especially at the generic level, are extremely contentious issues that affect anyone trying to publish any manuscript regarding anole systematics.

One of the biggest challenges facing M&K was space limitations, which forced M&K to eliminate their environmental section in the pages leading up to the species sections. That environmental section is extremely important to an understanding of the distribution and conservation of anoles in Honduras. Instead, M&K referred the reader to the recently published McCranie (2011, 22–27; also see McCranie 2007) for a discussion of those extremely important forest formations and ecophysiographic areas (hereafter EA). M&K (pp. 257–264) also provided a section after the species accounts covering several distributional categories of the Honduran anoles, all or most of which G apparently either did not read or did not understand. A reading and understanding of those categories (especially that of EA) should have previously answered some of the instances where I believe G erred. Also, it appears that G did not consult McCranie (2011) for information on those EA categories as suggested by M&K. If G had carefully read those distributional categories, especially that on EA, he probably would not have written his naive statements about "undisturbed broadleaf rainforest" (UBR hereafter) as those that appear on p. 656 regarding *Norops wampuensis*. In addition, G appears to not have fully understood what M&K (p. 203) wrote about "extreme differences in habitat" between *N. tropidonotus* and *N. wampuensis*. M&K were largely referring to the nighttime sleeping habit differences between those two nominal forms. M&K also wrote that those two species were extremely closely related and might even be conspecific (but that suggestion *was not mentioned* [emphasis mine] by G in his critique about the M&K systematics).

Norops wampuensis, as currently understood, is restricted to the UBR in northeastern Honduras as discussed in M&K and in McCranie (2011; also, as undisturbed lowland forest), and *N. tropidonotus* has never been found in those forests. M&K (p. 182) also wrote that *N. tropidonotus* "is most common in open situations," thus certainly not in closed canopy UBR. Those forests refer to portions of EA 15, 21, 22 (especially that of 21). Those majestic UBR do not occur (and probably never did, at least in the history of mankind) outside of the Mosquitia region of northeastern Honduras southward, except with an arm historically reaching the slopes and lowlands of the Montaña del Malacate area northeast of the Sierra de Agalta in the headwaters of the ríos Patuca and Paulaya. It seems clear G did not read or understand any of the EA sections in, or referred to by M&K, and that he confused the majestic UBR of EA 21, 15, and 22 (also see McCranie 2007) with those currently at best rather old second growth forest around Trujillo westward "based on [his] personal experience in both Mexico and Honduras" (p. 656). However, none of his experience includes those UBR forests in EA 21, 15, and 22. Those Trujillo and westward forests (best developed near La Ceiba) are in EA 26 and 27 in M&K and McCranie (2011) and have significantly different amphibian and reptilian species compositions, a fact lost on G. On my most recent trip to EA 21 in July-August 2015, I spent two plus weeks traveling by motorboat up the Río Warunta and walking around various campsites set up in UBR along that river. I have spent nearly a year's real time since 2001 working those Warunta-Rus Rus UBR, with rarely even seeing another human from outside my group, and as of 2015 only two of my about 30 campsites had been deforested by the *ladino* invasion. Yet, in all that time, I have touched only a tiny fraction of those forests. Want to feel small, then enter those UBR and realize they extend further than you could probably walk without unlimited time available. It is insulting for someone without any experience in those forests, to tell me "there is likely to be very little undisturbed broadleaf rainforest [UBR] left in the country" (p. 656). To help convey the distributional importance of UBR in EA 21, 22, 15 of Honduras, 22 of the 136-snake species (McCranie 2011) and four of the 39 anole species (M&K) do not occur in Honduras outside of those UBR in EA 21, 15, and 22 (see McCranie 2011, 23–24 for a discussion of the various types of vegetation in those areas; also see McCranie 2007).

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G (p. 656) also makes the statement regarding various anoles that "Juveniles and even adult females are often remarkably similar between species." That might be true, but only at best superficially, or to someone not familiar with various species of anoles, not as G apparently believes. How can that undocumented they all look-alike statement appear in the scientific literature concerning anoles again? A similar statement first appeared in Poe (2013), without mentioning the extremely outstanding scale character differences among females of the species being used as examples. If one examines the scale characters of the head and body of an anole under a microscope, or a hand-held lens in the field, they are surprisingly easy to assign to a species, at least in my experience with the anoles of Honduras and various introduced species in south Florida. Females of those anoles simply do not all look-alike, as some colleagues want us to believe. Scale characters, along with other morphological characters (i. e. dewlap color and size in males [mentioned by G] and some females; body sizes and proportions), are clearly usable to enable an experienced anole worker to place species in a phenetic-based species group, or as a quick aid in identifying anole species. So, who can say, and even apparently believe, that scale characters are not informative as an aid in identifying females of various species, or in using the latest techniques (for both sexes) to perform combined morphological and molecular data based phylogenetic analyses, and all said without providing supporting data, and mostly from one small group of colleagues who apparently do not want to do sufficient work to gather morphological data.[Note added in proof. While this manuscript was in press, Poe et al. (2017; Systematic Biology) published a phylogenetic analysis that included some morphological data. Unfortunately, a perusal of that new publication indicates that those morphological data were gathered largely by Ernest Williams many years ago (also see Poe, 2004; Herpetological Monograph 18, who had indicated those data were largely from Williams)]. No scientist can simply *assume* (emphasis mine) those characters are uninformative in a phylogenetic analysis until someone takes the time to gather sufficient data to perform a combined morphological and genetic dataset-based phylogenetic analysis using the latest techniques. Using only genetic data to perform a phylogenetic analysis is not using all data available. Ignoring those gatherable morphological data and instead saying they are *uninformative* (emphasis mine) and insinuating they are of no use in identifying anoles, all without attempting to provide sufficient supportive data, is simply unprofessional and should not be condoned in modern systematics. Morphological data from the anole species occurring in Honduras, could in no way, be proven to be uninformative.

Another problem with the G review is that he suggests several taxonomic changes, again without providing any supportive data. Honduras is one of the leaders among the seven Central American countries in having a relatively high percentage of herpetofaunal endemics. As McCranie (2015) noted, part of the reason behind those relatively high percentages of Honduran endemic species is the amount of dedicated fieldwork followed by systematic study of those field collected specimens that has occurred for over 30 years. Surprisingly, that dedicated work drew comments from G that could be interpreted as veiled criticism. Instead of criticizing colleagues who are making efforts to understand the species limits in Central American anoles, colleagues should offer the herpetological community results of their own work and studies. G (p. 656), again without any supporting data to back his taxonomic suggestions, appears to be questioning that dedicated work and the validity of several *Norops* species described resulting from that Honduran fieldwork, by writing "why are there endemic species related to [*Norops*] *laeviventris* in Honduras." G also wrote "*Anolis* [*Norops*] *laeviventris* is currently considered to be a widespread species (from Panama to Mexico) [but by whom; certainly, not by all colleagues] and the presence of diagnosable microendemic species of this form only within Honduras seems odd." The odd thing about that reasoning and G's repeated lack of supporting data (thus presumed data) appear to be "where are the anole researchers in those other countries and why have they not collected and studied various isolated montane populations of *N. laeviventris*? G also questioned the validity of *N. morazani* and *N. rubribarbaris*, among other species "presumed [not scientifically informative] to be isolates of more widely distributed species" (p. 656). The two just mentioned species are members of the *N. crassulus* species complex (all of which G apparently presumes to be the single species *N. crassulus;* another presumption not shared by all colleagues), with its montane distribution and many isolated populations. Phylogenetic studies (J. Townsend, pers. comm.), using both molecular and morphological data, show that the *N. crassulus* complex represents a group of multiple valid species, including *N. morazani* and *N. rubribarbaris*, that G presumed otherwise. Certainly, combined molecular and morphological data based phylogenetic analyses have been successfully use to understand systematic relationships for quite a few other lizard groups. So, why not use those combined data sets to perform phylogenetic analyses to understand those relationships within the already shown to be monophyletic anole family Dactyloidae? Hiding behind presumptions is certainly not scientifically the best method to use.

The G attitude is apparently that what he believes about anole systematics is correct (documented or otherwise) and the thinking of any anole colleague is incorrect if it conflicts with his belief, seems to reflect a bit of animosity based on a self-conceived territorial invasion of his own group's ownership of anole systematics, and is somewhat annoying to someone who has "light-years" of more experience and on-hand research with the anoles in question than does G. The

current trend in anole systematics of criticizing a colleague's work, instead of offering your own comparative results, and using undocumented presumptions, should be condemned. Another anole thought is that the most annoying peer review I have ever received was an anonymous review regarding a manuscript proposing a new species of *Norops* from Honduras, that seems certain to have been written by a member of that small group wanting to force their views of anole systematics on the entire herpetological community. Constructive criticism is good for science, but the same cannot be said about presuming scientific results and preparing animosity-based peer reviews.

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