

## **Consistency of Taxonomic Treatments: A Response to**

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**Consistency of Taxonomic Treatments: A Response to Remsen (2005).**—Remsen (2005) set out to provide an overview of the implications of two papers published recently in *The Auk*, one treating antbird vocalizations and their use in establishing species limits (Isler et al. 2005), and the other treating molecular variation along a phenotypic cline across part of the range of one antbird species (Brumfield 2005). He then moved on to a more general essay on species concepts, in which he expressed the familiar taxonomic points of view (Mayr 1963) and dismissed offhand our recent work on Mexican bird taxonomy. The first of these points is addressed below by Zink (2006); here, we comment on the ideas expressed by Remsen (2005) about our recent review of Mexican bird taxonomy.

Remsen (2005:407) acknowledges the poor quality of the existing list of avian subspecies as a descriptor of intraspecific variation with the comment that “the roster of formal subspecies, most described before the advent of statistical methods in ornithology, contains many names that refer only to arbitrary points on clines, average differences between populations, or zones of intergradation....” We agree wholeheartedly, having seen time and time again that described subspecies perform poorly in depicting variation within biological species. It is ironic that Remsen does not fault the authors of the papers he reviews for using existing subspecies limits to structure their studies, instead of verifying their appropriateness *a priori*.

In fact, the problem of historical inertia in taxonomies is much broader than Remsen acknowledges. Avian species-level taxonomy, as it stands presently, was largely formed in the first part of the 20th century, on the basis of incomplete series of specimens and prequantitative methods. The influences of the polytypic “biological” species concept (Mayr 1942, 1963) are pervasive, particularly for regions that have seen little recent taxonomic attention (e.g., Asia, Oceania). The result is a species taxonomy that hides or obscures much real variation.

Recent volumes of the American Ornithologists' Union (AOU) check-list (AOU 1983, 1998) have seen the “biological” species concept school recognize this discrepancy, and respond to it by splitting some complexes that have been subjected to detailed study. To provide some examples, numerous polytypic species were split between 1983 and 1998: *Colaptes auratus*, *Aphelocoma coerulescens*, *Chlorostilbon canivetii*, *Ammodramus caudacutus*, *Thalurexia furcata*, *Empidonax difficilis*, *Vireo solitarius*, *Pipilo erythrophthalmus*, *P. fuscus*, *Icterus galbula*, and *Glaucidium minutissimum*, among others. Nonetheless, those groups are simply those that happened to get inspected: very distinct forms remain to be recognized within many of these groups (e.g., *Co. mexicanooides*, *P. macronyx*) or in related complexes (e.g., *Ap. ultramarina* and *Ap. unicolor*, *I. spurius* and *I. graduacauda*). The difference between groups that were split and those that were not is whether a systematic ornithologist carefully documented differences that, for the most part, were already known to exist.

Remsen (2005) flippantly dismisses our recent treatment of Mexican bird taxonomy (Navarro-Sigüenza and Peterson 2004) as relying on a “trust us, we've looked at the specimens” approach. He claims that our approach lacks rigor and recalls the heyday of the antiquated “Peters Checklist” method. Our work was actually based on detailed examination of >350,000 specimens in more than 50 natural history museums worldwide, considering phenotypic variation in a geographic and ecological context (Navarro-Sigüenza et al. 2003). We fully acknowledge that our lists likely include errors and misjudgements; however, the value of our approach is that, for the first time since 1957 (Friedmann et al. 1950, Miller et al. 1957), a fully consistent taxonomy—in which species limits in each clade are based on the same criteria as in all other clades—is available for Mexican birds that can serve as a guide for future research in species-level systematics. Indeed, our conclusions have now been confirmed by a number of published studies, both from our research group

(Peterson and Navarro-Sigüenza 2000, García-Moreno et al. 2004) and from others (Johnson 2004, Kondo et al. 2004, Zink et al. 2005). In fact, these more detailed studies frequently indicate that still more taxa remain to be recognized in the Mexican avifauna, beyond what we proposed. Finally, although Remsen (2005) points to our “lack of rigor” for our “lack of supporting documentation,” we would point out that we studiously cited characters supporting our splits and provided literature citations and a subspecific synonymy—this level of detail certainly exceeds that of recent AOU volumes (AOU 1983, 1998).

If the objective of building taxonomies was to be purely scientific, focused simply on building the “correct” taxonomy for a given clade, the AOU approach would be logical, representing the state of current knowledge for the group. Avian diversity, however, is the focus of intense attention from the conservation community (Bibby et al. 1992, Collar et al. 1994, Stotz et al. 1996, Brooks and Thompson 2001), with species as the base unit upon which conservation strategies are based. In spite of recent opinions to the contrary (FjeldsÅ 2003), taxonomic consistency and taxonomic viewpoint clearly affect conservation priorities (Hazevoet 1996, Peterson and Navarro-Sigüenza 1999). As such, achieving a consistent taxonomy—under whatever viewpoint—is critical to bird conservation. More generally, Remsen’s (2005) blind allegiance to the “biological” species concept is disappointing, when lineage-based concepts have so much more to offer; his dismissal of other points of view, without presenting logical alternatives, does not advance the field.—A. TOWNSEND PETERSON, *Natural History Museum and Biodiversity Research Center, The University of Kansas, Lawrence, Kansas 66045, USA (e-mail: town@ku.edu)*, and ADOLFO G. NAVARRO-SIGÜENZA, *Museo de Zoología, Facultad de Ciencias, Universidad Nacional Autónoma de México, D.F. 04510, México*.

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**Rigor and Species Concepts.**—Within a review of work on avian vocalizations by affiliates of his own institution, Remsen (2005) turned his attention to the debate over species concepts. This topic is very controversial

and has strong advocates on both sides. Thus, readers should take literally Remsen's warning that "this is not the place for another review of species concepts...." Readers will recognize that his "review" does not present a balanced overview of the principal issues in the debate, owing to Remsen's allegiance to the biological species concept (BSC). It is also important to address controversial issues constructively. Here, I respond to his concerns and criticisms and illustrate my opinion that the BSC continues to be a poor choice for organizing our knowledge of biodiversity.

*The debate in a nutshell.*—Remsen believes that if two taxa, diagnosed by some phenotypic or genotypic data, can interbreed to some (unspecified) degree, they must be classified as the same species. This is the crux of the BSC. Under the phylogenetic species concept (PSC), diagnosably distinct taxa with independent evolutionary histories are considered species regardless of whether they are reproductively isolated from other phylogenetic species. Adoption of one or the other concept leads to major differences in our understanding of avian species diversity.

*Importance of interbreeding.*—Remsen perpetuates the notion that advocates of species concepts other than the so-called "biological" species concept (Mayr 1963) consider the phenomenon of reproductive isolation unimportant. In particular, he remarks that "proponents of the PSC [phylogenetic species concept] explicitly denounce the use of interbreeding in classification" (Remsen 2005:406). This does a disservice to the papers he cites, because readers unfamiliar with them will assume incorrectly that he has understood and reported their content accurately and not out of context. Advocates of the PSC have always acknowledged that interbreeding occurs among individuals of the same species, but its existence (actual or presumed) does not justify uniting taxa that are otherwise diagnosable. There is good reason for this, because the ability to interbreed is an ancestral condition (Rosen 1979).

In modern systematics, one does not unite taxa based on their shared possession of an ancestral condition. Apparently because he does not like the outcome, Remsen has decided to ignore this part of phylogenetic systematics and use his own rules. Advocating that we discard this fundamental rule will ensure that non-sister taxa are united by their joint possession