

## N&MA Check-list Committee Proposals 2008-B

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**Proposal: Transfer *Piranga*, *Habia*, and *Chlorothraupis* to Cardinalidae**

[Note from Remsen: this proposal passed unanimously, and SACC members' comments appended]

Effect on NACC: This would transfer three genera that we have already excised from Thraupidae to the Cardinalidae.

Background: NACC classification has already removed these three genera from their traditional home in the Thraupidae to Incertae Sedis. The SACC classification (online) has the following footnotes:

2. [*Piranga*] There is strong genetic evidence that the genus *Piranga* belongs in the Cardinalidae (Burns 1997, Klicka et al. 2000, Yuri and Mindell 2002, Burns et al. 2003, Klicka et al. 2007). Proposal to transfer to Cardinalidae passed.

5. [*Habia*] Genetic data (Burns 1997, Burns et al. 2002, 2003, Klicka et al. 2000, 2007) indicate that the genus *Habia* does not belong in the Thraupidae, but in the Cardinalidae. Proposal to transfer to Cardinalidae passed.

6. [*Chlorothraupis*] Genetic data (Burns 1997, Burns et al. 2002, 2003, Klicka et al. 2000, 2007) indicate that the genus *Chlorothraupis* does not belong in the Thraupidae, but in the Cardinalidae. Similarity in behavior to *Habia* had been noted previously by Willis (1966). Proposal to transfer to Cardinalidae passed. Klicka et al. (2007) found that *Habia* is paraphyletic with respect to *Chlorothraupis*, with *H. rubica* closer to *Chlorothraupis* than to *H. fuscicauda* + *H. gutturalis*.

New information: Klicka et al. (2007) confirmed these findings with broader taxon-sampling. Their combined analysis included 102 genera of tanagers, emberizines, and cardinalines. Because the three genera above have been sampled in most of the previous genetic work, with similar results, they are treated here as a package. [Klicka et al. (2007) also found that *Granatellus* is embedded in Cardinalidae but *Saltator* is not cardinaline, and made other taxonomic recommendations, such as a merger of *Habia* and *Chlorothraupis*, but these are best dealt with in separate proposals.] The genetic sampling consisted of 2281 bp of two mitochondrial genes, ND2 and cyt-b ... a nice sample.

The critical node (#1 in their Fig. 1) that places these three genera within a group that also consists of *Cardinalis*, *Caryothraustes*, *Periporphyrus*, *Rhodothraupis*, *Pheucticus*, *Granatellus*, *Cyanocompsa*, *Amaurospiza*, *Cyanoloxia*, *Passerina*,

and *Spiza* has strong support (100% Bayesian, 78% MP bootstrap, 92% ML bootstrap); see the MS for additional details.

Analysis and Recommendation: mtDNA is widely considered a reliable predictor of phylogeny at these levels of taxonomy, and certainly these data sets represent the first truly scientific estimates of the phylogeny and classification of this group. As hard as it will be for some people, North Americans at least, to accept that what they think of as "tanagers" are not true tanagers, the genetic data leave no option to but to transfer them to Cardinalidae. Although Klicka et al. (2007) consistently found support for a Thraupidae-Cardinalidae sister relationship, that node does not have strong support. Therefore, merging Cardinalidae into Thraupidae cannot be just justified. Klicka et al. (2007) treated these families as well as parulids, icterids, and emberizids as tribes of Sibley & Ahlquist's massive Emberizidae, and once we get more confidence in estimating divergence times and hopefully tying family rank to absolute age of the lineage, then in fact they may all end up in one family. But that is not relevant to group monophyly and our current classification, which ranks them as families.

To comfort those who might be disturbed by such a radical change, note that *Piranga* shares with *Pheucticus* and some *Passerina* complex age and seasonal plumage changes and complex songs that are rare or unknown in core Thraupidae. The resemblance in plumage between *Cardinalis* and *Habia cristata* is not the coincidence that we once thought. Having placed all the cardinalines sensu Klicka et al. in a separate synoptic series section in our collection, I am impressed by the overall phenotypic similarities in plumage patterns, subtle shades, and texture; these sort of fuzzy things don't count for anything, of course, but I speculate that had earlier ornithologists not been so mesmerized by bill shape differences in their classifications and focused more on patterns and textures that the "new" Cardinalidae wouldn't be so radical.

I recommend a YES vote on this one -- we've had these genera dangling in Incertae Sedis limbo waiting for just one more data set, and those data have arrived.

References: (see SACC Biblio for rest)

KLICKA, J., K. BURNS, AND G. M. SPELLMAN. 2007. Defining a monophyletic Cardinalini: A molecular perspective. *Molecular Phylogenetics and Evolution* 45: 1014–1032.

Van Remsen (in consultation with Kevin Burns and John Klicka), May 2008

Note: This move will undoubtedly cause some major anxiety among those concerned with making English names "perfect." Rather than consider changing the names to something besides "Tanager", I recommend considering the name "tanager" to refer to an ecomorph (intermediate bill shape between warbler and

finch), just like "sparrow," "grosbeak," "finch," "warbler," "chat," "flycatcher," and so forth, rather than to refer to a taxonomic group, with "tanager" the name used for those with beak morphology intermediate between "finch" and "warbler."

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Comments from Robbins: "YES, for transferring these three genera from Incertae Sedis status to the currently recognized family Cardinalidae. As an aside, I fully support changing the English names of these taxa to reflect their true relationships. Lets call all the *Piranga* "tanagers", Scarlet *Piranga*, Hepatic *Piranga*, etc. In my opinion, there is no difference in using that as an English name as there is in using Euphonia or a host of others. Now is the time to correct these misnomers."

Comments from Stiles: "YES. This transfer is clearly mandated by the data. I really don't have a strong opinion on the English names. I see no problem with continuing to call them tanagers in the generic sense; although Mark's idea of calling them "Pirangas" is not bad, I suspect that the rather enormous inertia of 150+ years of "tanagers" is enough to tip the balance towards conservatism in this case."

Comments from Stotz: "YES. This has been in the works for a while, and this dataset seems to make it clear that these genera all belong in Cardinalinae. In terms of the English names, I am in favor of leaving them be, at least for the time being. Once everything is moved around the Thraupidae is going to be full of things not called tanagers and there will be tanagers scattered through various families. We have grosbeaks, buntings and finches already in multiple families, and seem to have survived. I think we can survive the name tanagers not providing much phylogenetic information."

Comments from Zimmer: "YES. The genetic data seem clear. With respect to English names, I like Mark's suggestion because it cleans up a messy situation, but I would have to agree with Doug that we are probably better off waiting on any changes given that there is bound to be more upheaval."

Comments from Schulenberg: "YES. I don't have any problems with leaving all with "tanager" as part of the English. As Van says (for once I agree with Van on English names - take note!), we long ago accepted that many other group names have no phylogenetic meaning. I'd like to see if this same approach can be accepted for ex-thraupid tanagers."

Comments from Nores: "YES. El análisis genético muestra bien esto, pero yo sería de la idea que en caso como estos en que existe un análisis confiable, como parece ser el de Klicka et al., y que todas las propuestas están basadas en ese análisis, se hiciera una sola propuesta con todos los cambios. Por ejemplo .... 319, 320, 321."

### Transfer *Granatellus* to Cardinalidae

[Note from Remsen: this proposal passed unanimously, and SACC members' comments appended]

Effect on NACC: This would transfer a genus from the Parulidae to the Cardinalidae.

Background: SACC classification has already removed *Granatellus* from Parulidae and placed it in Cardinalidae, with the following footnote:

33. Recent genetic data (Lovette & Bermingham 2002) show that the genus *Granatellus* is not a member of the Parulidae (but true relationships uncertain, perhaps closest to Cardinalidae); Lowery & Monroe (1968) suspected that it did not belong in the Parulidae, and Meyer de Schauensee (1966) suspected that it belonged in the Thraupidae. Storer (1970a) suspected that plumage similarities between *Granatellus* and *Rhodinocichla* suggested a close relationship between the two. Genetic data (Klicka et al. 2007) indicate strong support for placement in the Cardinalidae. Proposal passed to place in Cardinalidae.

New information: Klicka et al. (2007) with broader taxon-sampling, including all three *Granatellus* (1 extralimital to NACC), confirmed what Lovette & Bermingham (2002) had suspected from their analyses. Klicka et al.'s analysis included 102 genera of tanagers, emberizines, and cardinalines. The genetic sampling consisted of 2281 bp of two mitochondrial genes, ND2 and cyt-b ... a nice sample.

The critical node (#1 in their Fig. 1) that places *Granatellus* within a group that also consists of *Piranga*, *Habia*, *Chlorothraupis*, *Cardinalis*, *Caryothraustes*, *Periporphyrus*, *Rhodothraupis*, *Pheucticus*, *Cyanocompsa*, *Amaurospiza*, *Cyanoloxia*, *Passerina*, and *Spiza* has strong support (100% Bayesian, 78% MP bootstrap, 92% ML bootstrap); see the MS and Proposal 318 or additional details.

Analysis and Recommendation: mtDNA is widely considered a reliable predictor of phylogeny at these levels of taxonomy, and certainly these data sets represent the first truly scientific estimates of the phylogeny and classification of this group. Two independent data sets now place *Granatellus* within this group. There are no contrary data, and *Granatellus* has been placed within Parulidae in the past largely on the basis of its small body size and bill ... in other words, there are essentially no scientific data for its placement in Parulidae or any other family.

Klicka et al.'s phylogeny placed *Granatellus* sister to a monophyletic group that consists of the "blue" cardinalines (*Passerina*, *Cyanocompsa* etc.) + *Spiza*. Sister to the *Granatellus* + Blue group, is *Pheucticus*, and as Klicka et al. noted, *Granatellus* shares red and black plumage with *Pheucticus ludovicianus*. Relationships among *Pheucticus*, *Granatellus*, and the Blue group are weakly defined, however, and the possibility remains that *Pheucticus* and *Granatellus* could be sisters. Our current linear sequence will need to be modified to group, at least until those deeper nodes are better-resolved, *Pheucticus*, *Granatellus*, and the Blue group. Thus, placement in our current sequence will be only temporary; placing *Granatellus* after *Pheucticus* (if this proposal passes) is perhaps the best temporary solution.

I recommend a YES vote on this one -- for the first time, data rather than general impressions can be used to place *Granatellus* in a phylogenetic classification.

References:

KLICKA, J., K. BURNS, AND G. M. SPELLMAN. 2007. Defining a monophyletic Cardinalini: A molecular perspective. *Molecular Phylogenetics and Evolution* 45: 1014–1032.

LOVETTE, I. J., AND E. BERMINGHAM. 2002. What is a wood-warbler? Molecular characterization of a monophyletic Parulidae. *Auk* 119: 695-714.

Van Remsen (in consultation with Kevin Burns and John Klicka), May 2008

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Comments from Stiles: "YES. Again, the change is clearly mandated by the genetic data - they were decidedly odd "warblers" in any case, and "chat" is a sufficiently nonspecific English name that I see no need to tinker with it!"

Comments from Stotz: "YES. Always a weird warbler, good to get clear placement somewhere."

Comments from Zimmer: "YES. Independent data sets strongly support the change, with an absence of any conflicting data. As an aside, I've always thought there was a *Passerina*-like quality and pattern to calls and songs of the various *Granatellus* species, so Klicka et al.'s findings regarding the relationships of the "blue group" to *Granatellus* makes sense from the standpoint of vocal characters as well."

Comments from Robbins: "YES, there is strong genetic support for placing *Granatellus* within the Cardinalidae. Although this genus always seemed out of place within the Parulidae, I would never have guessed that it was in a clade with *Piranga*, *Habia*, et al."

Comments from Pacheco: "YES. Os resultados convergentes apresentados por dados oriundos de trabalhos genéticos recentes confirmam de maneira mandatória a suspeita iniciada nos anos 1960's."

### Transfer *Amaurospiza* to Cardinalidae

[Note from Remsen: this proposal passed unanimously, and SACC members' comments appended]

Effect on NACC: This would transfer a genus from Emberizidae to the Cardinalidae.

Background: NACC classification currently places *Amaurospiza* in the Emberizidae. SACC has moved it to Cardinalidae, with the following footnote:

37. Although linear classifications traditionally place *Amaurospiza* near *Oryzoborus* and *Sporophila* (e.g., Hellmayr 1938, Meyer de Schauensee 1970, Paynter 1970a), plumage pattern and habitat suggests a relationship to *Cyanocompsa* and *Passerina* in the Cardinalidae (Paynter 1970a). *Amaurospiza* was placed between *Sporophila* and *Cyanospiza* (= *Passerina*) by Ridgway (1901), who proposed a close relationship to *Cyanospiza*. Beecher (1953) and Tordoff (1954) used morphological characters to propose that *Amaurospiza* belonged in the Emberizidae and was thus not close to the cardinalines. Genetic data (Klicka et al. 2005) now confirm that *Amaurospiza* belongs on the Cardinalidae, as originally proposed by Ridgway. Proposal passed to transfer to Cardinalidae.

Thus, Ridgway had it right (as was usually the case) and Paynter was tempted to place it with *Passerina*.

New information: Klicka et al. (2007) with broad taxon-sampling, but only one *Amaurospiza* (*A. concolor*) confirmed what Ridgway and Paynter had suspected. Klicka et al.'s analysis included 102 genera of tanagers, emberizines, and cardinalines. The genetic sampling consisted of 2281 bp of two mitochondrial genes, ND2 and cyt-b ... a nice sample.

The critical node (#1 in their Fig. 1) that places *Amaurospiza* within a group that also consists of *Piranga*, *Habia*, *Chlorothraupis*, *Cardinalis*, *Caryothraustes*, *Periporphyrus*, *Rhodothraupis*, *Pheucticus*, *Cyanocompsa*, *Granatellus*, *Cyanoloxia*, *Passerina*, and *Spiza* has strong support (100% Bayesian, 78% MP bootstrap, 92% ML bootstrap); see the MS and SACC Proposal 318 or additional details. Further, they found strong support for *Amaurospiza* forming a monophyletic group with *Cyanocompsa cyanoides*, *C. brissonii*, and *Cyanoloxia* (with *Cyanocompsa parellina* outside that group); thus, *Amaurospiza* forms a cozy cluster with a group of similarly plumaged (blue males and rufescent females), tropical seed-crushers.

Analysis and Recommendation: mtDNA is widely considered a reliable predictor of phylogeny at these levels of taxonomy, and certainly these data sets represent the first truly scientific estimates of the phylogeny and classification of this group. *Amaurospiza* is deeply embedded in this group, and the phenotypic signal for this is also strong. Thus, I recommend a YES vote on this one.

References:

BEECHER, W. J. 1953. A phylogeny of the oscines. Auk 70: 270-333.

KLICKA, J., K. BURNS, AND G. M. SPELLMAN. 2007. Defining a monophyletic Cardinalini: A molecular perspective. Molecular Phylogenetics and Evolution 45: 1014–1032.

[See SACC Literature Cited for others]

Van Remsen (in consultation with Kevin Burns and John Klicka), May 2008

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Comments from Stiles: "YES. Again, the change is clearly mandated and makes good sense phenotypically as well. It is also reassuring to see Cardinalidae taking shape as a coherent family."

Comments from Zimmer: "YES. Genetic data are convincing, and coincide nicely with morphology (including plumage patterns of both male and female). I would note that vocal characters of *Amaurospiza* also fit nicely with *Cyanocompsa*."

Comments from Robbins: "YES, the unequivocal genetic data, in concert with the plumage morphology of male and female, make this a logical decision."

Comments from Pacheco: "YES. Os dados moleculares enfim corroboram a prévia sugestão. Concordo com Kevin que vocalmente há uma boa similaridade entre o repertório de *Amaurospiza* e *Cyanocompsa*."

**Remove *Saltator* from Cardinalidae**

[Note from Remsen: this proposal passed unanimously, and SACC members' comments appended]

Effect on NACC: This would transfer one genus from Cardinalidae to Incertae Sedis.

Background: NACC classification currently places *Saltator* in the Cardinalidae. The genus *Saltator*, although itself widely suspected of being polyphyletic, has always been placed with the cardinalines. I do not know what the basis for that was.

New information: Klicka et al.'s (2007) analysis included 102 genera of tanagers, emberizines, and cardinalines. The genetic sampling consisted of 2281 bp of two mitochondrial genes, ND2 and cyt-b ... a nice sample. Not only is *Saltator* not in the Cardinalidae but there is support for placement within the Thraupidae. The critical node (#2 in their Fig. 1) for that placement has strong support (> 95% Bayesian). That node places *Saltator* (plus extralimital *Saltatricula*) as sister to the rest of the tanagers. [Extralimital *Saltator rufiventris* is not a *Saltator* but is deeply embedded within the Thraupidae].

Analysis and Recommendation: mtDNA is widely considered a reliable predictor of phylogeny at these levels of taxonomy, and certainly these data sets represent the first truly scientific estimates of the phylogeny and classification of this group. There is no support for retaining this genus in Cardinalidae, and I am unaware of whatever rationale was used originally for the placement of *Saltator* there (and when you look, as in our synoptic series, at the true cardinalines and *Saltator*, one cannot see any phenotypic signal that suggests a relationship).

For me, the only question is whether to place them within Thraupidae or leave them as Incertae Sedis. Given that their placement in Thraupidae is based on one node and one study (and no nuclear DNA analyzed so far), and given that Burns, Klicka et al. will undoubtedly be publishing more on Thraupidae and *Saltator*, I suggest a cautious approach by placing them temporarily as Incertae Sedis, with all appropriate footnotes indicating their likely relationship to Thraupidae. Thus, I recommend a YES vote on their deportation to Incertae Sedis. If someone wants to submit a separate proposal for direct placement in Thraupidae, please do so. Note that SACC comments were almost unanimously in favor of placement in Thraupidae.

References:

KLICKA, J., K. BURNS, AND G. M. SPELLMAN. 2007. Defining a monophyletic Cardinalini: A molecular perspective. *Molecular Phylogenetics and Evolution* 45: 1014–1032.

[See SACC Literature Cited for others]

Van Remsen (in consultation with Kevin Burns and John Klicka), May 2008

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Comments from Cadena: "YES to moving them out from Cardinalidae, but NO for placing them incertae sedis. I think the data are sufficient to place *Parkerthraustes* and *Saltator* in the Thraupidae. Figure 1 in Klicka et al. shows a strongly supported clade that includes the Thraupidae, *Saltator*, and *Parkerthraustes*. Unless one wants to create a new family for *Saltator*, which doesn't appear sensible, the only alternative that is consistent with our current understanding of phylogeny is to treat all the members of the clade as representatives of a single family, Thraupidae. It is important to note that Klicka et al. 2007 state that "our data cannot rule out the hypothesis that they [*Saltator*] are a sister clade to the Cardinalini", a conclusion which is based on a non-significant SH test of topologies. This statistical test is known to be conservative, and the other way to look at this is that posterior probability support for a clade formed by *Saltator* and the Cardinalini is less than 0.05, which means that using a Bayesian criterion, one can reject that hypothesis of relationships. In sum, I think we have enough data to support moving these taxa to the Thraupidae. I concur with Alvaro's recent comment in that we tend to overuse the incertae sedis "rank", and that we should go with the best available phylogenetic evidence."

Comments from Stiles: "YES on transferring these two genera out of Cardinalidae (where I suspect that their massive bills had been the main argument for placing them there). However, I agree with Daniel that current evidence is sufficient for placing them in Thraupidae, probably at the beginning of that family as they are evidently sister to the rest. The lack of sexual dichromatism is also a feature shared with most "true" tanagers but not with most (all?) cardinalines. Like Daniel, I see nothing useful to be gained by either leaving the saltators in "incertae sedis" or erecting a separate family for them."

Comments from Stotz: "YES. While tempted to move them all the way to Thraupidae, it seems like there is some question as to the specific placement within that family."

Comments from Zimmer: "YES for moving both genera out of Cardinalidae, and into Thraupidae. I find Daniel and Gary's arguments against temporary placement as incertae sedis compelling."

Comments from Robbins: "YES to both removing these from Cardinalidae and placing them in Thraupidae."

Comments from Pacheco: "YES. Igualmente (a #320), a partir das informações apresentadas, sou favorável a remoção deste dois gêneros de Cardinalidae e sua inserção em Thraupidae."

**Recognize the genus *Dendroplex* Swainson 1827 (Dendrocolaptidae) as valid**

[Note from Remsen: this proposal passed SACC 7-3, and SACC members' comments appended]

Effect on AOU CL: this would transfer polytypic *Xiphorhynchus picus* to *Dendroplex*.

Background: Since 1951, authors (Peters 1951, Clements 2000, Marantz et al. 2003, Dickinson 2003) have placed the Straight-billed (*X. picus*) and Zimmer's (*X. kienerii*) woodcreepers in the genus *Xiphorhynchus*, even though earlier authors classified them in the genus *Dendroplex* (Sclater 1890, Hellmayr 1925, Zimmer 1934, Todd 1948). The original characterization of *Dendroplex* (1827: 354) provided only a brief diagnosis of the new taxon, and no reference to a type species. Ten years later, the same author (Swainson 1837: 313-314) provided essentially the same diagnosis of the original description, but this time it was accompanied by an illustration showing the straight culmen and lateral compression of the type species. However, at the end of the characterization, Swainson added: "The scansorial type *D. guttatus* Spix i, 91, f. 1", which refers to figure 1 of plate 91 in Spix (1824), thereby satisfying the requirements of ICZN for type species designation by subsequent monotypy (ICZN 1999).

Subsequently, Hellmayr (1925: 288) pointed out that Swainson's diagnoses of 1827 and 1837 and bill outline correspond to the characters of the Straight-billed Woodcreeper (originally described as *Oriolus picus*), although the only species mentioned (*D. guttatus* Spix i, 91, f. 1), "belongs to the genus *Xiphorhynchus* Swainson". Following Hellmayr (1925), Peters (1951: 36) recognized that "*D. guttatus* Spix i, 91, f. 1" depicts in fact a bird now known as *Xiphorhynchus ocellatus* (Spix 1824), and stressed that under Opinion 65 (Schenk & McMasters 1948: 54) the case of misidentification had to be formally presented to the ICZN for ruling, and that until a decision was reached, *Xiphorhynchus ocellatus ocellatus* = *Dendrocolaptes ocellatus* Spix 1824 continued to be the type of *Dendroplex*.

New information: Aleixo (2002) showed with high statistical support that the genus *Xiphorhynchus* (sensu Peters 1951) is paraphyletic, and that the sister taxa *X. picus* and (extralimital) *X. kienerii* are the only species which do not belong in a clade with the remaining *Xiphorhynchus* species; actually, all phylogenetic hypotheses obtained strongly indicated that *X. picus* and *X. kienerii* belong to a separate clade not nested within any of its closely related and apparently monophyletic genera *Campylorhamphus*, *Lepidocolaptes*, or *Xiphorhynchus* (Aleixo 2002). Thus, he suggested that they might be grouped in a different genus, in which case the name *Dendroplex* Swainson, 1827, would be available if problems with its type species designation were resolved. Because

the latest (fourth) edition of the International Code of Zoological Nomenclature (ICZN 1999) now allows a misidentified type species to be set aside without the requirement of a ruling from the Commission, Aleixo et al. (2007) proposed the conservation of *Dendroplex* Swainson, 1827, as a valid taxon. According to them, the following lines of evidence support Hellmayr's (1925) interpretation that Swainson's (1837) identification of "*D. guttatus* Spix, i, 91, f. 1" as the type was a case of misidentification, and that *D. picus* = *Oriolus picus* J. F. Gmelin 1788 was the taxon upon which Swainson actually based *Dendroplex*:

1) Swainson (1827: 354) himself was unsure whether the taxon on which he based *Dendroplex* had been described or not. Ten years later, when he next cited *Dendroplex* (Swainson 1837: 313-314), the original diagnosis was maintained and even illustrated in detail (see Fig. 1), but "*D. guttatus* Spix, i, [pl.] 91, f. 1." was mentioned as belonging to the genus apparently in accordance with Lesson (1830: 313), who a few years before explicitly equated "*D. guttatus* Spix, pl. 91" with "*Oriolus picus* Gm"

2). There is a significant discrepancy between the levels of detail and resolution of the bill outline presented in figure 281e of Swainson (1837: 313) as diagnostic of *Dendroplex* (see Fig. 1) and that of *D. guttatus* as illustrated in Spix's plate, chosen by Swainson (1837) as the type of *Dendroplex*. While the latter illustration is poor in resolution and depicts a bird which in fact resembles several taxa currently classified in the genus *Xiphorhynchus*, figure 281e of Swainson (1837), on the other hand, is very well resolved and refers unambiguously to the only species in the entire family Dendrocolaptidae known to this day to possess such a bill shape: *X. picus* = *Oriolus picus* J. F. Gmelin, 1788 (Marantz et al. 2003).

Overall recommendation: those taxa originally described or classified as *Dendroplex* according to Gray (1840), Sclater (1890), Hellmayr (1925), Zimmer (1934), and Todd (1948), but later transferred to *Xiphorhynchus* by Peters (1951), should be returned to *Dendroplex*, which will contain only two distinct sister biological species: the polytypic *Dendroplex picus* and the monotypic *Dendroplex kienerii*, as delineated by Marantz et al. (2003).

#### Literature cited:

Aleixo, A., S. M. S. Gregory & J. Penhallurick. 2007. Fixation of the type species and revalidation of the genus *Dendroplex* Swainson, 1827 (Dendrocolaptidae). Bull. B. O. C. 127: 242-246.

Aleixo, A. 2002. Molecular systematics and the role of the "várzea"- "terra-firme" ecotone in the diversification of *Xiphorhynchus* Woodcreepers (Aves: Dendrocolaptidae). Auk 119: 621-640.

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Alex Aleixo, May 2008

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Comments from Nores: "YES. Tanto el análisis molecular que muestra a *picus* y a *kienerii* como un género diferente de *Xiphorhynchus*, como las razones para resurgir el género *Dendroplex*, están para mi muy bien fundamentadas."

Comments from Cadena: NO. The proposal states that Aleixo (2002) demonstrated that *Xiphorhynchus* is paraphyletic and that *X. picus* and *X. kienerii* belong to a separate clade with "high statistical support", an assertion that I think is somewhat inaccurate. It is true that the analyses convincingly showed *Xiphorhynchus* to be paraphyletic (in the sense that *X. fuscus*, then included in *Lepidocolaptes*, is nested within it), but support for the finding that *picus* and *kienerii* are more closely related to other woodcreeper genera is far from strong. Figure 1 in Aleixo (2002) is the strict consensus of the most parsimonious trees; here, the clade formed by *picus* and *kienerii* occupies an unresolved position, which implies that one cannot reject the hypothesis that these two taxa form a clade that is sister to the rest of *Xiphorhynchus*, a situation that would not require a taxonomic change. Figure 2 is a maximum likelihood tree in which *picus* and *kienerii* form a clade that seems to be allied with other woodcreeper genera and not with the remainder of *Xiphorhynchus*. However, the relevant node here has less than 50% bootstrap support, so again one cannot reject the hypothesis that these two species are sister to the rest of *Xiphorhynchus*. Figure 4 is a Bayesian tree, and here again *picus* and *kienerii* form a clade that does not appear to be sister to the rest of *Xiphorhynchus*, but the posterior probability of the relevant node is only 64%, which again is far from significant. In sum, the data are suggestive but not conclusive, so I'd rather wait until the new comprehensive studies of Furnariid phylogeny based on more genes (i.e. Brumfield et al., Moyle et al.) are completed to decide whether this proposed change is warranted."

Additional comments from Aleixo: "Cadena is right in his interpretation that the clade *picus* / *kienerii* cannot be probabilistically or parsimoniously completely ruled out as the sister clade of all remaining *Xiphorhynchus* species plus ex-*Lepidocolaptes fuscus* in which case, according to his opinion, the recognition of *Dendroplex* as a separate genus from *Xiphorhynchus* would not be justified. Even though none of the phylogenies presented in Aleixo (2002) show *picus* / *kienerii* and the remaining *Xiphorhynchus* species as sister taxa (in fact ML and Bayesian estimates suggest a completely different topology), strong statistical support falsifying this relationship is also lacking. Nonetheless, what all phylogenies presented in Aleixo (2002) did show with strong statistical support, was that the *picus* / *kienerii* clade cannot be grouped probabilistically or

parsimoniously within the remaining *Xiphorhynchus* species. Considering that this study sampled all known species of *Xiphorhynchus* (by far the largest woodcreeper genus), those results strongly support the evolutionary independence of the *picus* / *kienerii* clade irrespective of the resolution concerning its sister clade. After all, is there a completely objective way to delimit genera using phylogenies? For example, it could be also argued that the phylogenies presented in Aleixo (2002) support the recognition of two genera within the "true *Xiphorhynchus*", since two well supported clades were consistently recovered by all phylogeny estimates.

When this is contrasted with the taxonomic history involving taxa of the *picus* / *kienerii* clade, it becomes more apparent that the results showed in Aleixo (2002) finally provide a phylogenetic basis for the distinct treatment those taxa received originally when grouped under the genus *Dendroplex* by taxonomists such as C. E. Hellmayr and J. T. Zimmer. In fact, the history of how *Dendroplex* was suppressed makes it evident that in fact no hard evidence has ever been presented to lump it with *Xiphorhynchus* in the first place! The genus *Dendroplex* was used for both *picus* and *kienerii* (back then *necopinus*) until 1948 when Todd (1948) moved *kienerii* (but not *picus*) to *Xiphorhynchus*, and this was done in just a single sentence (!), which I transcribe below:

"I am convinced that the resemblance, close as it is, between *necopinus* and *Dendroplex* is superficial and fortuitous, so to speak, and not indicative of congeneric affinity."

In Peters (1951), *picus* was finally moved to *Xiphorhynchus* solely on the basis of the nomenclatural issues surrounding the name *Dendroplex*, which Aleixo et al. (2007) finally helped to solve. Thus, the only "evidence" conflicting with the independent taxonomic treatment which *picus* / *kienerii* have historically always received in the genus *Dendroplex* was Todd's sentence arguing for the paraphyly of *Dendroplex*, which as Aleixo (2002) showed, has absolutely no phylogenetic basis.

In sum, what seems really key to the discussion is that the phylogenies presented in Aleixo (2002) strongly support the evolutionary independence of *picus* / *kienerii* from *Xiphorhynchus*, as recognized early on by their assignment to a different genus. Therefore, the bulk of all available evidence (traditional taxonomy + molecular phylogeny) favors the recognition of *Dendroplex* rather than their lumping with *Xiphorhynchus*."

Additional comments from Cadena: "I appreciate all the points made by Alex and he is absolutely right that one cannot positively (i.e. statistically) demonstrate that *picus* and *kienerii* are not more closely allied to other woodcreepers than to other taxa currently placed in *Xiphorhynchus*. That said, the important issue here is that there is a hypothesis about relationships that is implicit in our current taxonomy (i.e. that *picus* and *kienerii* belong in a clade with the rest of

*Xiphorhynchus*), and the data in Aleixo (2002) do not allow rejecting such hypothesis. Therefore, because we strive to maintain a stable classification, I argue that this change is not (yet) well-substantiated because it is not based on evidence that allows falsifying our current hypothesis, so I see no need to change. Again, it is true that one cannot "prove" that our current hypothesis is correct (which is true for essentially all scientific hypotheses), but unless there is evidence to reject it, I maintain that we should not modify our classification. If we were starting to classify woodcreepers from scratch, I would probably go for Alex's suggestion, but given that we need to work based on an existing classification, I believe we need to be conservative and accept only changes that are strongly supported by published evidence. And, with all due respect, I insist that the assertion that "the phylogenies presented in Aleixo (2002) strongly support the evolutionary independence of *picus/kienerii* from *Xiphorhynchus*" is not correct. The standard way to assess "strong" support for phylogenetic relationships is clade support values (bootstrap proportions and posterior probabilities) and statistical tests of topologies. Clade support values in this case are not strong, and test of topologies have not been conducted (and would very likely not reject the monophyly of *Xiphorhynchus* as currently defined). In sum, let's wait for the detailed phylogenetic analyses that are underway; I predict Alex will be right at the end, but the evidence to substantiate his proposed change is simply not out there yet."

Additional comments from Aleixo: "I am not arguing that the phylogenetic position of *picus / kienerii* is well resolved, but again, nodal support indices indicate with a high degree of confidence that they are not nested within a clade grouping all remaining species of *Xiphorhynchus* (all phylogenies shown in Aleixo 2002 clearly show that). Whether or not you think that this degree of evolutionary independence is enough to delimit different genera ends up being a matter of taste. My main point is actually that the current hypothesis about relationships implicit in our current taxonomy (i.e. that *picus* and *kienerii* belong in a clade with the rest of *Xiphorhynchus*), to use Cadena's words, results from bad taxonomy practice in the first place, and that in a way is totally independent from the phylogenetic evidence available nowadays. In fact, if it were not for Peters' (1951) comments on the nomenclatural issues preventing the use of *Dendroplex*, this would be exactly the "null-hypothesis" that would be implied by current taxonomy. In other words: *Dendroplex* was submerged into *Xiphorhynchus* due to a nomenclatural issue, not as the result of a taxonomic review. As far as the only "hard-data" reviews to date on *picus / kienerii* are concerned (Hellmayr 1925, Zimmer 1934, Aleixo 2002), they all point towards the evolutionary / taxonomic independence between those two biological species and the remaining species of the genus *Xiphorhynchus*. Thus, when Aleixo et al. (2007) "rescued" the nomenclatural validity of *Dendroplex*, they restated the original null-hypothesis of evolutionary relationships implied by taxonomy before the nomenclatural impediment discussed by Peters (1951) was put into effect. Furthermore, having *picus / kienerii* grouped under *Dendroplex* is more consistent with nomenclatural stability than have them grouped under

*Xiphorhynchus*, since the former treatment had been in place for nearly 150 years before its replacement. With that said, the molecular data shown in Aleixo (2002) is not at odds with the proper taxonomic null hypothesis that should be evaluated when considering *picus* / *kienerii*, i.e., their placement in a separate genus from *Xiphorhynchus*."

Comments from Stiles: "YES, despite the caveats of Daniel. It clearly doesn't belong in *Xiphorhynchus*, and whether or not to split the latter is not an issue at present."

Comments from Robbins: "YES. This really comes down to one's choice in whether the uniqueness of *picus* and *kienerii* merit generic distinction from other species currently treated as *Xiphorhynchus*. I could go either way on this, but given that these taxa were originally placed in *Dendroplex* and the subsequent transfer of them into *Xiphorhynchus* was based solely on superficial plumage similarities, I vote "yes" on this proposal."

Subsequent Comments from Robbins: "Change to NO. I was on the fence on whether to recognize *Dendroplex*. I'm quite copasetic with following Curtis's suggestion of waiting on making a change."

Comments from Stotz: "YES. While I recognize Daniel's argument that the data presented by Aleixo and company does not absolutely demonstrate that *Xiphorhynchus* would be paraphyletic if it included, it does suggest that would in fact be the case. Given that the lumping of *Dendroplex* into *Xiphorhynchus* was done on such weak grounds in the first case, I think splitting them off, even though they might be sister to *Xiphorhynchus* makes sense."

Comments from Remsen: "NO. Tough decision. Although I appreciate Alex's excellent point that the only reason that the status quo keeps these species in *Xiphorhynchus* is from an error in nomenclature, I think that it is necessary to demonstrate that keeping the two *Dendroplex* in *Xiphorhynchus* would beyond a doubt make this a paraphyletic genus. Existing genetic data are suggestive but not conclusive. Until additional data fortify the hypothesis that *Dendroplex* is closer to other genera, I see no reason to place them in a separate genus. I am unimpressed with any subjective rationale as to why this group of birds differs in any meaningful way from *Xiphorhynchus* in terms of plumage, voice, and morphology; bill shape by itself, one of the most plastic characters in bird morphology, is not a valid delimiter of generic boundaries."

Comments from Zimmer: "YES, for reasons stated by Alex in the proposal, and by Mark, Gary and Doug."

Comments from Pacheco: "YES. Os argumentos apresentados pelo Alex, são ao meu ver, apesar das opiniões respeitáveis de Cadena e Van, suficientes e

consistentes para o reconhecimento de *Dendroplex* – no arranjo proposto – como válido."

Comments solicited from Curtis Marantz: "This is a difficult situation for two reasons. First, as Daniel pointed out, the genetic evidence published to date is equivocal regarding the need for recognizing the two *Dendroplex* as separate from *Xiphorhynchus*. I would probably be more strongly swayed by the evidence already published had there not been a project underway looking in depth at Furnariid phylogeny (including the Dendrocolaptidae / Dendrocolaptinae). Under the assumption that this complex will be studied more extensively in the near future I would recommend waiting to see if the situation is better resolved before making any changes. Secondly, I have looked into the nomenclatural issues discussed by Aleixo et al. (2007) and must admit that they are most complex. It indeed appears that Swainson (1837) erred when he chose *D. guttatus* as the type for *Dendroplex* despite the fact that his figure 281e indeed depicts *X. picus* and only *X. picus*. Although I am far from an expert on nomenclatural issues I would think that common sense would dictate that, if at all possible, the best course of action to take, if indeed the recognition a separate genus is warranted, may be to abandon the name *Dendroplex* altogether and propose a new name. This would seem reasonable given not only the confused past surrounding *Dendroplex* but also because the bill characters first used by Swainson to diagnose *Dendroplex* do not really apply to *kienerii*, which has a somewhat different bill shape."

Comments from Jaramillo: "YES - I am swayed by Alex's reasoning, and going a bit on gut feeling here. There is room for a certain amount of subjectivity in these decisions on genera, and I will be honest about it."

**Change rank and sequence of Galliform families**

Our present (AOU 1998) listing of galliform taxa is:

Family Cracidae  
Family Phasianidae  
    Subfamily Phasianinae  
    Subfamily Tetraoninae  
    Subfamily Meleagridinae  
    Subfamily Numidinae  
Family Odontophoridae

Aside from recognition of New World Quail as a family, this goes back to 1983, probably farther.

Many other works (Sibley and Ahlquist 1990, Dickinson 2003) recognize Numididae as a family, and other sequences are common. Many do not use subfamilies.

New Information—

Cox et al. (2007) used a combined data set of five nuclear loci and three mitochondrial regions to figure out the relationship of New World Quail (NWQ) to other galliforms, reviewing several classifications. They came up with support for NWQ being a clade basal to Phasianids, excluding Numidids, which were basal to both. This would lead to a sequence listing of:

Family Cracidae  
Family Numididae  
Family Odontophoridae  
Family Phasianidae

Cox et al. (2007) did not use subfamilies, and their data suggest that *Meleagris* is embedded in Phasianidae, contrary to some views (Sibley and Ahlquist 1990) that it is a grouse. However, that was not the thrust of their study, so we can ignore subfamilies and leave ours as is.

I propose that we elevate the Guinea Fowl to family level and change our sequence of families to follow Cox et al. This would result in the following arrangement:

Family Cracidae  
Family Numididae  
Family Odontophoridae  
Family Phasianidae  
    Subfamily Phasianinae

Subfamily Tetraoninae  
Subfamily Meleagridinae

Cox, W. A., R. T. Kimball, and E. L. Braun. 2007. Phylogenetic position of the New World Quail (Odontophoridae): Eight nuclear loci and three mitochondrial regions contradict morphology and the Sibley-Ahlquist tapestry. *Auk* 124:71-84.

Richard C. Banks, 9 April 2008 Revised 27 June 2008

**Lump *Myiobius sulphureipygius* into *Myiobius barbatus***

Effect on North American checklist: This would change the name of *Myiobius sulphureipygius* to *Myiobius barbatus*. It would require changes in the range description and notes.

**History:**

More details on the history of the taxonomic treatment of this complex are below in the South American Checklist Committee proposal. Basically, Hellmayr (1927) treated *sulphureipygius* and *barbatus* as distinct species. Ever since, the Central American literature has followed that treatment, including AOU 6<sup>th</sup> (1983) and 7<sup>th</sup> (1998) editions. However, Zimmer (1939) concluded that *sulphureipygius* was best treated as conspecific with *barbatus* and most South American works have followed that treatment.

The South American Checklist Committee recently considered a proposal on this complex, as well as a companion proposal on the *M. atricaudus* complex, and voted to maintain the status quo of both groups as widespread polytypic species under the names *barbatus* and *atricaudus*. This proposal for the North American committee is to provide an opportunity to bring the two committees into agreement on this issue.

New Information: There is no new information on this complex relating to the specific status of the various taxa in *Myiobius*.

Recommendation: My recommendation is to vote YES on this proposal. This is an odd situation. South American and Central American treatments of this complex have a long history of being at odds. There is no firm evidence to support either treatment. There are no published analyses of voice, there is no published genetic information, and not even a serious analysis of plumage distinctions.

My argument for lumping *sulphureipygius* into *barbatus* has three pieces. First, it would standardize treatment between the North American and South American committees. This is not a requirement, but is a positive thing if possible.

Second, in this complex and *M. atricaudus*, there are other taxa that in terms of plumage are at least as well-differentiated as *sulphureipygius* and *barbatus*, namely *mastacalis* in the *barbatus* complex and *ridgwayi* in the *atricaudus* complex. It seems like maintaining *sulphureipygius* as a distinct species while not treating these other taxa as even "groups" is a very uneven treatment. To me, the only logical treatments with current data would be 2 species, *barbatus* and *atricaudus*, or five (*sulphureipygius*, *barbatus*, *mastacalis*, *ridgwayi*, and *atricaudus*). I think this strongly enough, that if the committee chooses to maintain *sulphureipygius*, I will provide proposals to split these other taxa,

despite the lack of strong data to support their split and the fact that they are extralimital.

Third, *Myiobius* are not very vocal, but voice descriptions are usually similar. For example, Birds of Ecuador, which recognizes two species (explicitly following AOU), says for both “foraging birds occasionally give a sharp “psik”. HBW gives song descriptions for *M. sulphureipygius*, but no descriptions for *barbatus*. I cannot find any song description for *barbatus* (or *mastacalis*) anywhere in the literature.

Overall, I would suggest that if we were starting from scratch, instead of from tradition, we would never split *sulphureipygius* and *barbatus*.

The strongest arguments for maintaining the status quo in Central America are two. First, that it is the status quo, which is usually a fairly strong argument, but is hurt in this case by being a local status quo. The second argument is that given that the very similar *Myiobius atricaudus* is definitely a different species (being broadly sympatric and sometimes syntopic), the plumage variation between *sulphureipygius* and *barbatus* is at least as strong as between these distinct species. I am sympathetic to this argument, but it really applies not just to *sulphureipygius*, but also to *mastacalis* in the *barbatus* complex and *ridgwayi* in the *atricaudus* complex.

SACC proposal 342 to split *Myiobius sulphureipygius* and/or *Myiobius mastacalis* from *Myiobius barbatus*

Effect on South American checklist: This would split *Myiobius barbatus* into two or three species.

Background:

SACC currently treats *Myiobius barbatus* as including *sulphureipygius*, while the North American committee treats *sulphureipygius* as a distinct species (AOU 1998). A form in southeastern Brazil (*mastacalis*) is treated as a group that could be specifically distinct in several references, and at least a couple I have seen say that it is sometimes treated as distinct, but I have not found any references that actually do treat it as a separate species (Actually Wikipedia lists *mastacalis* as a separate species, but when you click on the common name (Yellow-rumped Flycatcher) it takes you to an Asian species [more on that later]).

Hellmayr (1927) treated trans-Andean *sulphureipygius* as a distinct species from *barbatus*. Zimmer (1939) concluded that “It seems probable, therefore, that *sulphureipygius* and *aureatus* deserve inclusion in the *barbatus* group.” His main basis for this was, in fact, the similarity between *aureatus* (the South American subspecies of the *sulphureipygius* group) and *mastacalis* (the SE Brazilian subspecies). He noted that *aureatus* and *barbatus* approached one another without signs of intergradation. However, they are across a range of Andes from one another. Since Zimmer there has been a diversity of treatments.

References treating *sulphureipygius* and *barbatus* as conspecific include Hilty and Brown 1986, Meyer de Schauensee 1966, 1970, Pinto 1944, Ridgely and Tudor 1994, Sibley and Monroe 1990, Traylor 1979, Zimmer 1939.

References splitting *barbatus* and *sulphureipygius* as separate species: Hellmayr and Cory 1927, Hilty 2003, Ridgely and Greenfield 2001, AOU 1983, 1998, Farnsworth and Lebbin 2004. Basically all of the Central American literature back at least until Blake (1953) treats the two as separate except for the 2<sup>st</sup> edition of *Birds of Panama* (Ridgely and Gwynne 1989).

Analysis: There are really two current treatments that are widely applied. Most South American literature lumps everything into *barbatus*, while Central American literature splits out *sulphureipygius*. The almost 50/50 split in how these taxa are treated is a reflection of the fact that there are essentially no data that support either treatment. *Myiobius sulphureipygius* and *M. barbatus* are allopatric and show low levels of plumage and morphometric divergence. They are not very vocal and most references either don't mention voice or give a transcription that seems like it could be interpreted as indicating that they sound the same. Overall the morphological distinctiveness of trans-Andean *sulphureipygius* and eastern Brazilian *mastacalis* from Amazonian *barbatus* seems roughly equivalent, so it would seem like treatment as a single species or three species are the most logical options, although historically the splitting of *mastacalis* has not been a common treatment (or even a rare treatment?).

One weak piece of evidence that applies to the *mastacalis/barbatus* question is that Traylor (1979) indicates that birds from western Mato Grosso are intermediate between *mastacalis* and the subspecies *insignis* (part of the *barbatus* group) of southeastern Amazonia. He suggests that they might be intergrades between *mastacalis* and *insignis*. I assume that this refers to two specimens discussed by Zimmer (1939), one of which he assigns to *insignis* and the other which he indicates "agrees better with *mastacalis*," although it is unusually large (for any *Myiobius*). Given that both these specimens are from Amazonian drainages and *mastacalis* as far as I can tell is not even in the Parana drainage, that these birds are really indicative of intergradation between *insignis* and *mastacalis* seems doubtful to me.

One weak piece of evidence that might suggest splitting the taxa within *barbatus* is the fact that *barbatus* and the very similar *atricaudus* are broadly sympatric in all three major forest realms. The Amazonian populations of these two species are very similar, such that specimens have been routinely misidentified. The species tend to replace one another in a patchwork, however, there are localities from which both species are known. If these taxa act as distinct species, it might indicate that the morphologically more divergent forms in Central America and eastern Brazil would act as distinct species—maybe.

Recommendation: Ridgely (1976) treats *sulphureipygius* as a distinct species. Later, Ridgely (Ridgely and Gwynne 1989 and Ridgely and Tudor 1994) argues against treating *sulphureipygius* as distinct from *barbatus* essentially because *mastacalis* is as distinctive as *sulphureipygius*. However, in 2001 (Ridgely and Greenfield 2001), he again follows AOU (1998) in splitting *sulphureipygius*. I find I agree with Ridgely – I really don't know what the best approach is. My weak recommendation is to maintain all the forms as a single species given the

unimpressive morphological distinctiveness of all forms in the absence of vocal or genetic evidence suggesting otherwise.

Proposal A to split *sulphureipygius* from *barbatus*. I recommend a NO vote. If we maintain *sulphureipygius* as a subspecies of *barbatus*, I will write a proposal for the North American committee to change their treatment to a single species treatment, although I'm not sure I can see why they would necessarily change. Note that if split, the English name Sulphur-rumped Flycatcher would apply to *sulphureipygius*. Whiskered Flycatcher is usually used for *barbatus*.

Proposal B to split *mastacalis* from *barbatus* I recommend a NO vote. If we split this, there is an English name issue. Meyer de Schauensee (1966) suggests, with the three species treatment, *barbatus* as Whiskered Flycatcher, and Yellow-rumped Flycatcher for *mastacalis*. However, Yellow-rumped Flycatcher is used for the Asian species *Ficedula zanthopygia*. Its use for *Myiobius mastacalis* would seem like a bad idea. Sibley and Monroe (1990) use Bearded Flycatcher for the *barbatus* group and Whiskered for *mastacalis*. I would suggest that if we split this complex, that we follow the English names use by Sibley and Monroe.

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COMMENTS of SACC Committee members:

Comments from Remsen: "NO (both A and B). Until actual data are presented for one or the other treatment, any classification is largely arbitrary. As Doug recommends, I don't think we have a choice but to stick with to status quo until those data appear."

Comments from Nores: "NO (both A and B). Pienso que como en la propuesta 343, lo más apropiado será esperar por nuevas evidencias en vocalizaciones o análisis moleculares. Además, *sulphureipygius* es más similar a *mastacalis* que a *barbatus* (de acuerdo a los dibujos del HBW) así que de separar *sulphureipygius*. debería también separar a *mastacalis* y ponerlos en una misma especie, que en este caso tendría prioridad *mastacalis*. Así que quedaría *Myiobius mastacalis mastacalis* y *Myiobius mastacalis sulphureipygius*."

Comments from Stiles: "YES. This is, as Doug notes, a tough one.. from my Central American-Colombian perspective, I would be tempted to go the other way and vote YES on both - given the often subtle differences in plumage among congeneric tyrannids, including the sympatric *atricaudus* and *barbatus* in *Myiobius* itself plus the considerably greater difference of *sulphureipygius* and the effective lack of a true "status quo", I'd place the three (*barbatus*, *mastacalis* and *sulphureipygius*) in a superspecies and hope that someone does the genetics and gets some good vocal data soon! As Doug notes, the evidence is pretty tenuous whichever way one goes, so my vote is decidedly *sotto voce*."

Comments from Pacheco: "NO to both A and B. Prefiro aguardar uma análise apropriada. As diferenças morfológicas entre os táxons subordinados ao complexo *M. barbatus* são menos abruptas que aquelas observadas no complexo *M. atricaudus*. É sugestivo que J. T. Zimmer não tenha encontrado distinção entre um espécime do "Rio Roosevelt" (interflúvio Madeira-Tapajós) e aqueles da Mata Atlântica (*M. b. mastacalis*)."

Douglas Stotz

3 July 2008

### Change the spelling of Mountain-gem to Mountaingem

Effect on North American Checklist.

This would change the spelling of the English group name of 5 species of hummingbirds in the genus *Lampornis* from Mountain-gem to Mountaingem, namely:

Green-throated Mountaingem *Lampornis viridipallens*

Green-breasted Mountaingem *Lampornis sybillae*

White-bellied Mountaingem *Lampornis hemileucus*

Purple-throated Mountaingem *Lampornis calolaema*

White-throated Mountaingem *Lampornis castaneiventris*

#### History

The Mexican checklist uses Mountain Gem, two words, without a hyphen. Blake (1953), modified this to Mountain-gem, as did Eisenmann (date). Since then, essentially all literature has used Mountain-gem, until Gill and Wright suggested Mountaingem. Although the committee voted not to accept the sweeping series of changes in hyphenation that was suggested in proposal 2007-A-03, which included this change, the committee indicated that it was open to consider changes on a case-by-case basis.

#### Analysis

Under both the IOC guidelines and the rules proposed by Parkes (1978), which in principle this committee follows, Mountaingem without a hyphen appears to be the preferred spelling for this name. Basically, we use a hyphen when the name includes the name of a bird as part of the group name, so Mountain-Tanager. If that name refers to a bird that taxonomically the bird is not actually related to, it would be lower case (so Stone-curlew). We also use hyphens when the name is difficult to pronounce or understand without one, so Foliage-gleaner, White-eye, etc.

In this case, it seems to me that neither provision for using a hyphen applies. There are no birds called "Gems", unmodified. The only other gem is another species of hummingbird, the Horned Sungem from South America. So this is not equivalent to Mountain-Toucan or Mountain-Tanager. It is more like Woodnymph. It also does not seem that Mountaingem is a difficult word to pronounce or understand, so I can't see that exception as applying..

Recommendation: I recommend a YES vote on this. While I recognize this is perhaps the most crucial issue in modern ornithology, I would hope people will not labor over this decision very long.

References:

Eisenmann, E. 1955. The species of Middle American birds. Transactions of the Linnaean Society, New York 7: 1-128.

Parkes, K. C. 1978. A guide to forming and capitalizing compound names of birds in English. *Auk* 95: 324-326.

Douglas Stotz

3 July 2008

**Add *Patagioenas plumbea* (Plumbeous Pigeon) to main list**

*Patagioenas plumbea* is a species with a broad distribution in tropical and subtropical South America. This species is now documented from the Darién region of Panama by sight records (many), song recordings (several, from independent parties) now archived at Cornell, and two specimens collected in 1997 from the Jungurudó range. One skin is now in the Museo de Vertebrados of the Universidad de Panama and one is in the AMNH.

Angehr et al. (2004) thoroughly summarized these various records and described their own sound and specimen vouchers, all of which are diagnostic of *plumbea*. They noted that the presence of *plumbea* in the Darién has been “overlooked until recently due to its close similarity to *P. nigrirostris* and *P. subvinacea*” and suggested that *plumbea* is widespread and perhaps even fairly common in Panama near the Colombia border.

We recommend adding this species to the list on the basis of this information. The account would read:

***Patagioenas plumbea*** (Vieillot). Plumbeous Pigeon.

*Columba plumbea* Vieillot, 1818, Nouv. Dict. Hist. Nat., nouv ed., 26: 358. (Brésil = vicinity of Rio de Janeiro, Brazil.)

**Habitat.**—Tropical Forest.

**Distribution.**—*Resident* in northwestern Colombia and Ecuador east through eastern Panama (Serranía de Jungurudó and Cerro Pirre, Darién), Venezuela, and the Guianas and south to southern Brazil, northeastern Paraguay, and eastern Bolivia.

**Notes.**—something added from SACC list

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**Position on the Check-List** – Dickinson (2003) places *plumbea* after *inornata* (Plain Pigeon) and before *subvinacea* (Ruddy Pigeon). The mtDNA and Fib-intron phylogeny of Johnson et al. (2001) has *inornata* as the well-supported sister taxon of *subvinacea* (though their species sampling of this group is not complete, so these species may not be actual sister-taxa). Recommendation: place *Patagioenas plumbea* immediately before *Patagioenas subvinacea*.

References Cited:

Angehr, G.R., D G. Christian, & K.M. Aparicio. 2004. A survey of the Serranía de Jungurudó, an isolated mountain range in eastern Panama. Bull. B. O. C. 124: 51-62.

Johnson, K.P., S. De Kort, K. Dinwoodey, A.C. Mateman, C. Ten Cate, C.M. Lessells, and D.H. Clayton. 2001. A molecular phylogeny of the dove genera *Streptopelia* and *Columba*. *Auk* 118: 874–887.

Relevant section from Angehr et al. (2004):

**PLUMBEOUS PIGEON** *Patagioenas plumbea* Very common in both lowlands and highlands, and heard calling almost every day. Two birds were tape-recorded (LNS 96086 and 96087) and then collected by shotgun near the base camp on 14 August 1997 (AMNH 834071 and MVUP 2204 respectively). These represent the first specimens from Middle America. Both specimens had pale cream-coloured eyes and grey wing-linings, characteristic of *Patagioenas plumbea* (Gibbs et al. 2001). The very similar Short-billed *P. nigrirostris* and Ruddy Pigeons *P. subvinacea* also occur in Darién. However, both have red or reddish eyes. *P. nigrirostris* has dark greyish-brown wing linings, and in *P. subvinacea* they are cinnamon (Gibbs et al. 2001). These birds gave a distinctive three-note call (*whit-mo-go*), sometimes with the third note slurred (*whit-mo-go'o*). ... GRA also heard the call of this species near the headwaters of the río Jaqué, on the southern flanks of the Serranía de Jungurudó, on 15 January 1996. The species also occurs on the Serranía de Pirre, where it has apparently been overlooked until recently due to its close similarity to *P. nigrirostris* and *P. subvinacea*. On 2 May 1994 GRA heard several birds calling near the north end of the Pirre range above the town of El Real. On 11 April 1995 GRA, D. & L. Engleman and others observed a bird with cream-coloured eyes calling on the trail above Cana on Cerro Pirre, and several others were heard. On 30 March 2001, GRA saw and taperecorded a bird with cream-coloured eyes giving a three-note call on the same trail. P. Coopmans (pers. comm.) heard the species commonly in the 600–1,300 m range at Cana between 30 January–9 February 1992, and tape-recorded it on 2 and 6 February (LNS 60300, 60302, 60333, 60336; catalogued as '*Columba* species'). Coopmans heard the species subsequently at Cana on trips in December 1994–January 1995, February 1996 and March 1998. G. Rompré (pers. comm.) saw a bird with pale eyes and heard it calling at 550 m near the north end of the Pirre range on 21 April 1995. We found Short-billed Pigeon to be rare on the Jungurudó, with one heard calling at 300 m on 11 August 1997, and greatly outnumbered by *P. plumbea*. We did not see or hear Ruddy Pigeon.

George Angehr, Richard C. Banks, Irby Lovette  
28 July 2008

**Add Yellow-breasted Flycatcher (*Tolmomyias flaviventris*) to main list**

**Background:** A species with a broad distribution in tropical lowland South America, now well documented from El Real in the Darien region of Panama by sight records (many); song recordings, including one now archived at Cornell; and one specimen, which is now at LSU.

The publication of these data are in Angehr (2006) *Annotated Checklist of the Birds of Panama*, which is not a peer-reviewed publication, though it is an appropriate venue for reporting these records.

**Recommendation:** That the committee add this species to the Check-List.

**Position on the Check-List** – Dickinson (2003) places *flaviventris* as the last *Tolmomyias*. Recommendation: in our Check-list, place *Tolmomyias flaviventris* immediately after *Tolmomyias assimilis*.

**Distribution:** Locally in eastern Panama (on the lower Río Tuira between El Real and La Palma in Darien) where it has bred and may be expanding, and broadly throughout tropical Amazonian South America.

**References Cited:**

Angehr, G.R. 2006. *Annotated Checklist of the Birds of Panama*. Panama Audubon Society.

**Relevant section from Angehr (2006):****\*Yellow-breasted Flycatcher** *Tolmomyias flaviventris*

Specimen, tape-recordings, and multiple sight records. Evidently colonizing Darien, although so far only known from near El Real. It was first found near this locality on 10 February 1992 by P. Coopmans (pers. comm.), who saw and tape recorded (LNS 60357) a single vocalizing bird. Individuals were observed at four different sites around El Real between 4-7 September 1992 by W. Martinez and D. and L. Engleman. Several individuals were seen near El Real on 8 April 1993 by G. Angehr, G. Seutin, and K. Kaufmann, and a tape recording was obtained by Kaufmann. Two nests were found, both near wasp nests, and Seutin observed a bird entering one of the nests. Individuals were also seen at various localities around El Real, 9-10 April 1995, by D. Engleman, G. Angehr, and others. Two were seen, and one was collected (specimen to be deposited at LSU), at Pirre Uno, 7 km SSW of El Real, on 4 April 2004, by R. Brumfield (pers. comm.).

[Note: The species has now been recorded elsewhere on the lower Río Tuirá, westward to near Río Iglesias and Setegantí (both near La Palma); Euclides Campos (pers. comm. to George Angehr)]

George Angehr and Irby Lovette  
28 July 2008

### **Establish formal network of Regional Consultants for AOU distribution accounts**

Background: The AOUCL is frequently cited as the authoritative source for bird distribution for our area, and our distribution accounts are one of the most useful features of the CL. It is impossible for the CLC to keep up with the changes in knowledge in breeding, winter, and migration distribution of all 2000+ species in the area, much less the deluge of vagrant records. Further, the research interests of CLC members typically do not include broad biogeographic patterns, much less patterns of vagrancy. For this reason, we added Jon Dunn to the CLC as our primary reviewer of distributional information. However, it is still impossible for one person to keep abreast of all the distributional information.

Proposal: Therefore, I propose a system of formal Regional Consultants for distribution who would feed proposed changes in CL distribution to Jon for review. Therefore, Jon's primary role would be to review proposed changes in our statements before they are incorporated by Andy, our editor, into the working draft.

The mechanics would be as follows:

1. Regional Consultants get Word file of CL distribution statements, probably as web downloads, and then propose modifications of them in Track Changes. Of course the RCs would adhere as much as possible to CL style and degree of resolution (i.e., consistency in degree of detail).
2. These files are sent directly to Jon for approval.
3. Jon emails Andy approved/revised statements.
4. Andy plugs them into our draft.

As for the RCs (or whatever we want to call them – let's discuss), ideally each state and province in North America would have its own person. I think that it is reasonable that we can find a competent, careful person from most. Jon would be in charge of seeking them out, and we as a Committee would have final approval. For Middle America and the Caribbean, we may have a harder time finding qualified people for all countries and territories. Bermuda and Greenland also need RCs. For Mexico, we ought to seek people with expertise at the regional/state level, because that country is more complicated than anywhere in our area. We can live with gaps (areas without RCs). CLC members can assist Jon with proposing candidates, especially south of the border, where Jon has less experience.

Each RC would be listed in the printed version, e.g., “George R. Angehr (Panama).” This would give the RCs a vested interest in the project and recognition for their work. It also involves directly many people in an AOU project, one that will benefit from local expertise. There is no reason to have only 1 per region – in fact, having teams would be a good way to bring more local talent into the process, e.g., “George R. Angehr and Mariano Rivera (Panama)”

The mechanics would be as follows:

1. Jon submits lists of potential RCs to the CLC, with a 1-2 sentence summary of qualifications.
2. We vote on them just like we do on proposals.
3. Jon contacts them to see if they are willing (better to do this after approval in case some get voted down). If we get a YES, then Andy can start sending them files.

Van Remsen, 25 August 2008

**Add *Circus buffoni* (Long-winged Harrier) to Appendix**

Angehr (2006) gives two sight records (1995, 2001) for the Long-winged Harrier, *Circus buffoni*, in Panama, where he considers it a vagrant.. The species is widespread in South America. This is sufficient to add the species to our hypothetical list (Appendix).

***Circus buffoni*** (Gmelin). Long-winged Harrier.

*Falco buffoni* Gmelin, 1788, Syst. Nat., 1, p. 277. Based on "Cayenne Ringtail" Latham, 1781, Gen. Synop. Birds 1, p. 91. (Cayenna = French Guiana.)

This widespread South American species is considered a vagrant in Panama by Angehr (2006) on the basis of sight records at Tocumen Marsh, east of Panama City, 28 August 1995, and El Real, Darien, 1 January 2001.

Angehr, G. R. 2006. Annotated list of the birds of Panama. Panama Audubon Society, Panama, Panama.

Richard C. Banks  
3 Sept. 2008

**Add *Tachycineta albiventer* (White-winged Swallow) to Appendix**

Angehr (2006) reports two sight records of this species in Panama and considers it a vagrant or rare migrant. This is sufficient for us to add it to our Appendix.

***Tachycineta albiventer*** (Boddaert). White-winged Swallow.

*Hirundo albiventer* Boddaert, 1783, Table Pl. Enlum., p. 32.  
(Cayenne).

This South American species is considered a vagrant or rare migrant in Darien, Panama by Angehr (2006) on the basis of sight records on 6 July 1996 and 25 August 1997.

Angehr, G. R. 2006. Annotated list of the birds of Panama. Panama Audubon Society, Panama, Panama.

Richard C. Banks  
3 Sept. 2008

**Split *Notharchus hyperrhynchus* from *N. macrorhynchus***

Effect on North American CL: This proposal would change the species name of a species on the NACC list by elevating an extralimital taxon to species rank.

Background: The *hyperrhynchus* subspecies group (consisting of named subspecies *hyperrhynchus*, *cryptoleucus*, and *paraensis*) of *Notharchus macrorhynchus* (the White-necked Puffbird) was formerly considered (e.g. Ridgway 1914, Cory 1919) to represent a separate species from nominate *macrorhynchus*. Peters (1948) lumped these forms (and *swainsoni*) without explanation into a single wide-ranging, polytypic species. Most recent compilations (Meyer de Schauensee 1966, 1970; Sibley & Monroe 1990; Clements 2000) have followed suit, treating *Notharchus macrorhynchus* as a polytypic species consisting of five subspecies that ranged from Mexico to Argentina. The 7th Edition of the AOU Checklist (1998) recognized two subgroups within this species complex: a wide-ranging *macrorhynchus* group (consisting of the taxa *hyperrhynchus*, *cryptoleucus*, *macrorhynchus*, and *paraensis*), and a geographically disjunct subspecies *swainsoni*, which is restricted to the Atlantic Forest of southeast Brazil, eastern Paraguay and northeastern Argentina. In the 7th Volume of the Handbook of the Birds of the World, Rasmussen and Collar (2002) elevated *swainsoni* to separate species status (also followed by SACC: see Proposal #124) and synonymized *cryptoleucus* of El Salvador and Nicaragua with *hyperrhynchus*. These authors recognize three subspecies of *N. macrorhynchus* as follows:

*N. m. hyperrhynchus* (Sclater 1856) - S Mexico south to N & NE Venezuela, and south to Colombia, Ecuador, E Peru, N Bolivia and W Brazil (E to Rio Tapajós and S to Mato Grosso).

*N. m. macrorhynchus* (Gmelin 1788) - extreme E Venezuela, the Guianas, and extreme N Brazil south to the Amazon.

*N. m. paraensis* (Sassi 1932) - lower Amazon Valley in Brazil (Pará east of the lower Rio Tapajós and into N Maranhão).

Rasmussen and Collar (2002) noted that "Races *hyperrhynchus* and *paraensis* markedly distinct from nominate, and together may constitute a separate species." In the Family Account, the same authors remark that "At the same time, however, it should be noted that the nominate race of the White-necked Puffbird in the Guianan region is also distinctive in appearance and possibly in song; thus, further study of the situation is required."

This situation has received surprisingly little attention from ornithologists or birders, given that plumage differences between *hyperrhynchus/paraensis* versus *macrorhynchus* are striking. The former group differs from the latter in having a

much broader white forehead (white in nominate is restricted to a narrow frontlet), a broader white hindcollar, much less extensive black patches on the flanks, and a noticeably larger bill. These differences are well illustrated in HBW Volume 7. The subspecies *paraensis* is similar to *hyperrhynchus* in plumage characters, but has an even longer bill. The plumage differences between *hyperrhynchus/paraensis* and nominate *macrorhynchus* are of the same order as the differences between any of these three forms and *N. tectus* (Pied Puffbird), *N. ordii* (Brown-banded Puffbird), and *N. pectoralis* (Black-breasted Puffbird), and thus, are consistent with species-level plumage differences across the rest of the genus.

Vocal differences are even more *pronounced*, but no published quantitative analysis exists. In my experience, the songs of *hyperrhynchus* and *paraensis* are virtually identical and unvarying throughout their wide distributions. This song is described by Stiles and Skutch (1989) as "a long bubbling trill, at a constant pitch or rising slightly, then falling" and by Hilty (2003) from Iquitos, Peru as "a long, nasal, frog-like trill, prrrrr (up to 15-20 seconds)". Ridgely and Greenfield (2001) described it as "an evenly pitched monotonous trill that lasts 3-5 seconds, sometimes given by both members of a pair". These descriptions fit my own tape recordings of *hyperrhynchus* from Chiapas, Mexico; Pacific Slope of Costa Rica; and lowland E Ecuador; as well as my recordings of *paraensis* from Mato Grosso and Amazonas, Brazil. The song of nominate *macrorhynchus* is very different, and is described from SE Venezuela by Hilty (2003) as "a long series of rapid pree whistles (ca. 30 whistles in 8 seconds) on the same pitch". I have heard nominate birds giving a complex song that begins with a similar series of whistles (as described by Hilty) that then leads into a series of terminal couplets, recalling the songs of *N. ordii*, *N. pectoralis* and *N. swainsoni*. This song is not even remotely like the trill given by *hyperrhynchus/paraensis*. Rasmussen and Collar (2002) described the song of *N. macrorhynchus* (presumably the nominate form, although this is not explicitly stated) as being "a very high weak trill at variable speeds, usually descending, "ui-ui-uiwi-di-dik wi-di-dik wi-di-dik". Oddly, the first part of this description ("high weak trill") seems to refer to the song of *hyperrhynchus/paraensis*, whereas the transcription that follows sounds more like the song of nominate *macrorhynchus*. Hilty (2003) noted the vocal differences between the two groups as follows:

"Song (mid-morning) in Rio Grande, Bolívar, a long series of rapid pree whistles (ca. 30 in 8 sec) on same pitch. At dawn (Iquitos, Peru) a long, nasal, frog-like trill, prrrrrrrr (up to 15-20 secs) on same pitch, given once every 2-5 minutes and by both sexes."

Unfortunately, Hilty obscured the significance of the differences by seemingly suggesting that they may reflect the difference between dawn songs and regular songs. I have taped *hyperrhynchus* and *paraensis* giving the trilled song at all hours of the day (including mid-day), and, conversely, I have heard the complex song of nominate *macrorhynchus* at dawn from atop canopy towers. Time of day has no bearing on the described vocal differences between these taxa.

Analysis: The plumage, biometric (possibly mainly bill length and depth), and vocal differences between nominate *macrorhynchos* and *hyperrhynchus/paraensis* are comparable to the differences between any of these three taxa and the other recognized species in the genus. The distributions of the two groups are seemingly parapatric in Venezuela (between *hyperrhynchus* and nominate) and in Brazil along the Amazon (nominate along north bank, *paraensis* along south bank), with no reported intergradation. I have no doubts that the vocal differences alone would act as isolating mechanisms between these taxa were they to come in contact, and the strong differences in distribution of black and white on the head and face of the two forms would seemingly also act to preclude recognition. Based on both vocal differences and morphological differences, I'm not even certain that *macrorhynchos* and *hyperrhynchus/paraensis* are one another's closest relatives. In song, distribution of black on the head/face, and in its smaller bill, nominate *macrorhynchos* is more reminiscent of *N. ordii* and *N. pectoralis* than of *hyperrhynchus/paraensis*.

The down side of all of this is the absence of any real published analysis of either vocal or morphological characters. However, qualitative descriptions of both types of characters are in the literature, as are good illustrations that reveal the plumage and bill size distinctions. The separation of *hyperrhynchus/paraensis* would also be consistent with the recognition of *N. swainsoni* as a separate species (as treated in Rasmussen & Collar 2002, and as in SACC classification). *N. swainsoni* differs from the rest of the *macrorhynchos* complex to a similar degree (both vocally and morphologically) as does *hyperrhynchus/paraensis* from nominate, the main difference being that there is more published support for the former split.

This split would result in two species: a monotypic *N. macrorhynchos*; and a polytypic *N. hyperrhynchus* (to include *N. h. paraensis*).

Recommendation: I recommend a YES vote on splitting these two groups, in spite of the absence of any real published analysis. These taxa were originally considered separate species, and were subsequently lumped without published justification. I don't think undoing this unjustified lump should be held to a higher standard, but, in any case, a higher standard exists in the form of published qualitative descriptions of vocal, plumage and size differences. Weak as the published justification is, I believe that the distinctions behind it are sound and biologically significant. If this passes, I'll put together a short proposal suggesting an English name.

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Kevin Zimmer

Comments from SACC members:

Comments from Silva: "YES. The differences in plumage are very striking and as far as I know these two species do not present any evidence for intergradation when their ranges meet. Because they have been described as separate species and lumped without any adequate taxonomic review, I fully agree with the proposal in ranking these taxon as two distinct species."

Comments from Stiles: "[NO] This case is like the preceding with one crucial difference: none of the evidence has been published in detail. While I personally believe that Kevin is right, I feel that if we are to maintain our insistence on published evidence, available for independent evaluation (as we have on a number of similar occasions), I must vote NO. (If Kevin wants to do a short note with sonograms etc., might I recommend *Ornitología Colombiana*??)."

Comments from Robbins: "YES, Kevin presents a very cogent argument for treating *hyperrhynchus* as a species."

Comments from Jaramillo: "YES – Instances such as this one are really difficult for me. We are dealing with taxa that were originally described as separate species, were lumped without published analysis, and current information strongly suggests that this undocumented lump was not a good decision. The full detailed analysis explaining why this split is a valid way to deal with these taxa is not published, but the available data seems pretty clear and I strongly suspect that Kevin is right in his analysis. The stickler in me says, vote NO, yet the

pragmatists says vote YES. There are so many hundreds of these taxonomic issues in South America that need to be tackled. Some may only need a few days work to pull together some data and provide a note to a journal, but there are so few people willing to do this type of taxonomic cleaning up that it seems like many of these questions will not be resolved in many, many decades. I think I will forever be flipping back and forth on how to deal with these types of records, and I commend those that are much more clearly thinking and resolute in their stances. In this particular case, I am taking these verbal descriptions of voice as data, and they are published albeit in literature that was not peer reviewed. Even so they are something on which to anchor this decision, the original lump does not seem to be anchored on anything and that troubles me more than splitting with no published analysis, but some data. I do think that publishing in a venue such as *Ornitología Colombiana* is a superb way to get something on these issues in print.."

Comments from Nores: "NO. Aunque Zimmer parece tener razón, no veo que hayan trabajos publicados que justifiquen la separación. Como este caso hay muchos otros en la misma situación."

Comments from Pacheco: "YES. Em minha primeira experiência sonora com a forma nominada de *macrorhynchos* ao norte do baixo Amazonas (Amapá), eu julguei que minhas gravações pudessem representar *ordii*; na medida em que, conhecia antes as vozes distintas de "*macrorhynchus*" *paraensis* de Carajás (sul do Pará). É possível (como sugerido por Kevin) que *macrorhynchos* seja - no escudo Guianense - o representante do grupo relictual *ordii/swainsoni*. Manter *hyperhynchus* e *macrorhynchos* reunidos, como formas alopátricas de uma mesma espécie, é abonar uma decisão inexata, arbitrária e anacrônica de Peters (1948), diante das informações agora disponíveis."

Comments from Remsen: "YES. My usual vote in cases such as this is "no" due to insufficient published information. However, the qualitative descriptions of voices that have been published combined with the absence of any published rationale for the original merger of a taxon ranked at species level by Ridgway and Cory."