N&MA Classification Committee: Proposals 2013-B

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2013-B-1 N&MA Classification Committee pp. 515-523

Make changes to linear sequence in family Mimidae

Description of the problem:

Mimidae is an entirely New World family with a broad distribution, including numerous species found on—or restricted to—Caribbean and Pacific islands. The relationships of these mockingbirds and thrashers have been investigated in many molecular phylogenetic studies over the past two decades, with high congruence among studies in their implications for the major groups within this radiation (Zink et al. 1997, 1999, 2001; Zink and Blackwell-Rago 2000; Hunt et al. 2001; Rojas-Soto 2003; Sgariglia and Burns 2003; Barber et al. 2004; Arbogast et al. 2006; Zuccon et al. 2006; Lovette and Rubenstein 2007; Hoeck et al. 2010).

Previous Check-List revisions have incorporated much of this new information and addressed species and generic limits within the Mimidae. There is some ongoing uncertainty about species limits in the *Mimus polyglottos/gilvus* complex (the Northern and Tropical mockingbirds) and within the Galapagos Mockingbird complex, but there is insufficient new information at present to propose new changes to their species-level classifications. This proposal therefore considers only the linear sequence of the currently recognized Mimidae species and genera.

New information:

One reason that this wealth of new phylogenetic information has not yet led to a change to the linear sequence of the Mimidae is that the many earlier phylogenetic studies each included only a subset of the relevant taxa, as those investigations were usually motivated by questions about population- and species-level relationships within Mimidae subgroups, or by questions about the biogeography of particular spatial regions. The most recent phylogenetic study (Lovette et al. 2012) of the Mimidae was the first to include representatives of all currently recognized Mimidae species, and it forms the primary basis for the new linear sequence proposed here, although the many earlier studies also deserve credit for suggesting and supporting these relationships.

This most recent and most inclusive phylogenetic study was based on both mitochondrial DNA and nuclear DNA (three unlinked autosomal introns) sequence markers. Given the relatively shallow level of divergence in the Mimidae, the greatest phylogenetic signal came from the mitochondrial dataset, but separate reconstructions based on each nuclear marker supported (or did not conflict with) the genus-level clustering and topology seen in the mtDNA-based reconstructions (Lovette et al. 2012).

Recommendation:

Change the linear sequence of the species and genera (extralimital species are placed in brackets) within the Mimidae to:

Genus *Melanotis*
    Species *caerulescens*
    Species *hypoleucus*
Genus *Melanoptila*
    Species *glabrirostris*
Genus *Dumetella*
    Species *carolinensis*
Genus *Ramphocinclus*
    Species *brachyurus*
Genus *Allenia*
    Species *fusca*
Genus *Margarops*
    Species *fuscatus*
Genus *Cinclocerthia*
    Species *ruficauda*
    Species *gutturalis*
Genus *Toxostoma*
    Species *curvirostre*
    Species *ocellatum*
    Species *rufum*
    Species *longirostre*
    Species *guttatum*
    Species *bendirei*
    Species *cinereum*
    Species *redivivum*
    Species *lecontei*
    Species *crissale*
Genus *Oreoscoptes*
    Species *montanus*
Genus *Mimus*
    [Species *saturninus*]
    [Species *triurus*]
    [Species *dorsalis*]
    [Species *thenca*]
    [Species *patagonicus*]
    Species *gundlachii*
    [Species *parvulus*]
    [Species *trifasciatus*]

[Species *macdonaldi*]
[Species *melanotis*]
[Species *longicaudatus*]
Species *graysoni*
Species *gilvus*
Species *polyglottos*

Literature cited:

Arbogast, B. S., Drovetski, S. V., Curry, R. L., Boag, P. T., Seutin, G., Grant, P. R., Grant, B. R., Anderson, D. J. 2006. The origin and diversification of Galapagos mockingbirds. Evolution 60, 370-382.

Barber, B. R., Martinez-Gomez, J. E., Peterson, A. T. 2004. Systematic position of the Socorro mockingbird *Mimus graysoni*. J. Avian Biol. 35, 195-198.

Hoeck, P. E. A., Bollmer, J. L., Parker, P. G., Keller, L. F. 2010. Differentiation with drift: a spatio-temporal genetic analysis of Galapagos mockingbird populations (*Mimus* spp.). Phil. Trans. R. Soc. B 365, 1127-1138.

Hunt, J. S., Bermingham, E., Ricklefs, R. E. 2001. Molecular systematics and biogeography of Antillean thrashers, tremblers, and mockingbirds (Aves: Mimidae). Auk 118, 35-55.

Lovette, I.J., Rubenstein, D.R. 2007. A comprehensive molecular phylogeny of the starlings (Aves: Sturnidae) and mockingbirds (Aves: Mimidae): congruent mtDNA and nuclear trees for a cosmopolitan avian radiation. Mol. Phylogenet. Evol. 44, 1031-1056.

Lovette, I. J., B. S. Arbogast, R. L. Curry, R. M. Zink, C. A. Botero, J. P. Sullivan, A. L. Talaba, R. B. Harris, D. R. Rubenstein, R. E. Ricklefs, and E. Bermingham. 2012. Phylogenetic relationships of the mockingbirds and thrashers (Aves: Mimidae). *Molecular Phylogenetics and Evolution* 63, 219-229.

Rojas-Soto, O. R. 2003. Geographic variation of the curve-billed thrasher (Toxostoma curvirostre) complex. Auk 120, 311-322.

Sgariglia, E. A., Burns, K. J. 2003. Phylogeography of the California Thrasher (*Toxostoma redivivum*) based on nested-clade analysis of mitochondrial-DNA variation. Auk 120, 346-361.

Zink, R.M., Blackwell-Rago, R.C., 2000. Species limits and recent population history in the Curve-billed Thrasher. Condor 102, 881-886.

Zink, R.M., Blackwell, R.C., Rojas-Soto, O. 1997. Species limits in the Le Conte's Thrasher. Condor 99, 132-138.

Zink, R.M., Dittmann, D.L., Klicka, J., Blackwell-Rago, R.C., 1999. Evolutionary patterns of morphometrics, allozymes, and mitochondrial DNA in thrashers (genus *Toxostoma*). Auk 116, 1021-1038.

Zink, R.M., Kessen, A.E., Line, T.V., Blackwell-Rago, R.C., 2001. Comparative phylogeography of some aridland bird species. Condor 103, 1-10.

Zuccon, D., Cibois, A., Pasquet, E., Ericson, P.G.P. 2006. Nuclear and mitochondrial sequence data reveal the major lineages of starlings, mynas and related taxa. Mol. Phylogenet. Evol. 41, 333-344.

Submitted by: Irby Lovette, Cornell University

Date of proposal: 27 Feb 2013

2013-B-2 N&MA Classification Committee pp. 404-405

Split Nutting's Flycatcher into two species: *Myiarchus nuttingi* and *Myiarchus flavidior*

Description of the problem:

The traditional Nutting's Flycatcher *Myiarchus nuttingi* is ostensibly resident from Mexico to Costa Rica. Largely on the basis of voice, Lanyon (1961) ended the long-standing debate that Nutting's was a separate species from Ash-throated Flycatcher. Howell (2012) drew attention to marked differences in voice and habitat, and subtle differences in plumage, within Nutting's Flycatcher: the Pacific-slope taxon *flavidior* (ranging from Chiapas, Mexico, to northwest Costa Rica) is quite distinct from other subspecies. Within the genus *Myiarchus* these differences are considerable, and greater than among other taxa of flycatchers recognized as full species.

New information:

Sari & Parker (2012:9, excerpted below, and their Fig. 2) found that Nutting's Flycatcher was paraphyletic. Unfortunately, given their ambiguous use of English, it is unclear whether they paid attention to subspecies (assuming such data were available on specimen labels), but they nonetheless found a distinct break within Nutting's, apparently between *nuttingi* and *flavidior*:

"In our phylogeny, M. nuttingi was represented by two independent lineages, one with samples from Costa Rica (id numbers CR6, CR13, and CR15), and another from specimens collected in San Salvador (KUNHM collection – id numbers 9314, 9281, and 9288; Figs. 2 and 3). Three subspecies are currently recognized for M. nuttingi (Lanyon, 1961), and the lineages found here might represent two of them, M. n. flavidior in El Salvador and M. n. nuttingi in northwestern Costa Rica, where both races co-occur (Lanyon, 1961). We did not find support for the monophyly of M. nuttingi, so taxonomic revision, delimitation of contact zones, and studies of genetic introgression between races of M. nuttingi would be important for the confirmation of their status as subspecies. Based on morphological and vocal characters, M. nuttingi has been considered closely related to M. cinerascens (see Lanyon, 1961), but we found that these two species are not sisters. Instead, the closest relative of M. cinerascens is M. crinitus from the southeastern US."

Beyond vocal, ecological, morphological, and apparent genetic differences, a consideration of biogeography further supports the break of *flavidior* as a distinct species.

English Names

Myiarchus flycatchers are rather uniform in appearance, and descriptive names are not much use. No especially apt geographic or habitat-based names seem appropriate in this case.

Howell (2012) suggested retaining the name Nutting's Flycatcher *M. nuttingi* for birds from northern Mexico to Central America, and naming the cryptic species as Ridgway's Flycatcher *M. flavidior*, "in recognition of Robert Ridgway's perspicacity in discerning that Nutting's Flycatcher was a species distinct from Ash-throated—an insight that required 80 years before it became universally accepted." (While Ridgway's Flycatcher was used many years ago for a subspecies of Northern Beardless Tyrannulet, that name is unlikely to be resurrected, and certainly isn't widely known or recent.)

An alternative is to not use Nutting's Flycatcher for either species, a generally desirable course when both taxa are migratory but less of a concern when both are resident and unlikely to occur together, as here. However, should this course be followed, a new English name would needed for *nuttingi*, and "Nutting's Flycatcher" could be used in cases where specific identity is uncertain, as in using Western Flycatcher for Pacific-slope Flycatcher/Cordilleran Flycatcher or Winter Wren for Western Winter Wren/Eastern Winter Wren.

Given that English and scientific patronyms do not need to be the same, naming the northern birds as Phillips's Flycatcher *M. nuttingi*, in recognition of Allan R. Phillips and his lifetime's work on Mexican birds, would seem a suitable alternative. Although relationships between Phillips and some members of the AOU were not always as cordial as they might have been, using this name might signal a sense of moving on and leaving the past behind.

Another possibility is Lanyon's Flycatcher *M. nuttingi*, in recognition of Bud's work on the genus and family as a whole. However, the subspecies of Yucatan Flycatcher *Myiarchus yucatanensis lanyoni* named after Lanyon might be a problem; endemic to Cozumel Island, it may be extinct (killed by the introduced boa constrictors that have decimated many bird populations on the island, including the thrasher), but one day it might be recognized as a species based on post hoc genetics; who knows?

In conclusion, I suggest retaining Nutting's Flycatcher (or using Phillips's Flycatcher) for *M. nuttingi*, and using Ridgway's Flycatcher for *M. flavidior*:

Nutting's Flycatcher (subspecies *inquietus* and *nuttingi*) ranges from Mexico through the interior and adjacent Pacific Slope of northern Central America to northwest Costa Rica

Ridgway's Flycatcher (monotypic *flavidior*) ranges in Pacific Slope lowlands from Chiapas, Mexico, south to northwest Costa Rica.

Literature cited:

Howell, S. N. G. 2012b. M-M-M-Maybe you just ain't seen Nutting yet? *Neotropical Birding* 10:14-17.

Lanyon, W. E. (1961) Specific limits and distribution of Ash-throated and Nutting's flycatchers. *Condor* 63:421–449.

Sari, E. H. R., & P. G. Parker. 2012. Understanding the colonization history of the Galápagos flycatcher (*Myiarchus magnirostris*). *Molecular Phylogenetics and Evolution* (2012), doi:10.1016/j.ympev.2011.10.023

Submitted by: Steve N. G. Howell, PO Box 423, Bolinas, CA 94924, USA.

Date of proposal: 21 Mar 2013

Add Common Moorhen *Gallinula chloropus* to the AOU Checklist

Description of the problem:

As discussed in Withrow and Schwitters (2012), a recent taxonomic decision by NACC (Chesser et al. 2011) split New World Common Gallinule *Gallinula galeata* from Old World Common Moorhen *G. chloropus* based on differences in vocalizations (Constantine et al. 2006) and in mitochondrial DNA (Groenenberg et al. 2008). The immediate result of this split was the tacit replacement on AOUCL of Common Moorhen *G. chloropus* with, instead, Common Gallinule *Gallinula galeata*. Thus, for at least a brief period, the species Common Moorhen *G. chloropus* has ceased in fact to exist on the AOUCL.

New Information:

At almost the same time as this split was formalized, a moorhen/gallinule was collected at Shemya Island, Aleutian Islands, by Michael T. Schwitters, a bird that Jack J. Withrow and Schwitters (2012) have demonstrated is an example of the Old World form and thus – practically before there was time to institute the changes above – would add Common Moorhen *Gallinula chloropus* to the list of birds known to have occurred naturally within the political limits of North America.

Recommendation:

Add Common Moorhen *Gallinula chloropus* to the AOU Checklist as a species of accidental occurrence.

Literature cited:

Chesser, R. T., Banks, R. C., Barker, F. K., Cicero, C., Dunn, J. L., Kratter, A. W., Lovette, I. J., Rasmussen, P. C., Remsen, J. V. Jr., Rising, J. D., Stotz, D. F., and Winker, K. 2011. Fifty-second supplement to the American Ornithologists' Union *Check-list of North American Birds*. Auk 128:600–613.

Constantine, M., and The Sound Approach. 2006. The Sound Approach to Birding: A Guide to Understanding Bird Sound. Sound Approach, Dorset, England.

Groenenberg, D. S. J., Beintema, A. J., Dekker, R. W. R. J., and Gittenberger, E. 2008. Ancient DNA elucidates the controversy about the flightless island hens (*Gallinula* sp.) of Tristan da Cunha. PLoS One 3(3):e1835; doi:10.1371/journal.pone.0001835.

Withrow, J.J., and M.T. Schwitters. First American record of the Common Moorhen (*Gallinula chloropus*) confirmed by molecular analysis. Western Birds 43:259-265.

Submitted by: Dan Gibson
Date of proposal: 21 Mar 2013

Merge all North American rosy-finches into *Leucosticte tephrocotis*

Description of the problem:

The current (7th edition) of the Check-list recognizes 3 species of *Leucosticte*, *L. tephrocotis* (Gray-crowned Rosy-Finch), *L. atrata* (Black Rosy-Finch, and *L. australis* (Brown-capped Rosy-Finch). On the basis of their DNA sequence analysis, Drovetski *et al.* (2009) suggest that these 3 taxa be merged into a single polytypic species, *L. tephrocotis*.

Largely based on the work (mostly unpublished) of R. E. Johnson (1973), the most recent Check-list (1998) recognizes 3 specific-level North American *Leucosticte*. Johnson presented phenotypic data on many populations of rosy-finches. These birds show much interpopulational variation in several geographically separated taxa, with limited apparent hybridization among them (see below). The AOU (and others) have recognized three of these taxa as distinct species (on what basis I know not, but presumably following Johnson's monograph): *L. tephrocotis* (*s.s.*) (Gray-crowned Rosy-Finch), *L. atrata* (Black Rosy-Finch), and *L. australis* (Brown-capped Rosy-Finch). These are largely geographically and phenotypically distinct.

New information:

Drovetski *et al.* examined sequence data from 201 individuals of the named North American taxa as well as from 3 Asian taxa, with Mongolian Finch (*Bucanetes*), Bullfinch (*Pyrrhula*) and Pine Grosbeak (*Pinicola*) tissue for outgroup comparison. They also did a multivariate factor analysis on Johnson's data (8 measures of size & coloration; North American samples of rosy-finches only). These features (bill length, bill depth, tarsometatarsus + middle toe length, wing length, tail length, dominant wave length [hue], brightness, and purity [chroma] of the breast with taxa assumed rather than defined geographically), all would seem to me to reflect local adaptation rather than historical relationships. Moreover, these analyses omitted those plumage characters (i.e., face pattern) formally used to diagnose the taxa (Remsen 2010). [Note: Drovetski et al 2009 write: "Locality groupings were assumed *a priori* to represent particular taxa rather than analyzing geographically spaced samples designed to text taxonomic limits." Black and Brown-capped rosy-finches are monotypic, but Gray-crowned is separated into several subspecies. I assume that each subspecies for which there were data available was considered to be a different "locality grouping," but am not sure just what this means. Were only breeding birds considered? This may just simply be a case of quantifying preconceptions, but in any event this analysis would seem to mean little.]

From the factor analysis of the size and color data, the first factor accounted for 66.9% of the total variance, and clearly separated the large Bering Sea birds from the smaller continental ones; Factor 2 (color; 15.6%) separates Black Rosy-Finch from the others (if they were considered to be "blacks" because they were darker than the others, then this is not surprising).

The analysis of the ND2 sequences resulted in a single tree, with 4 clades: a single N. A. clade and also an Asian clade, as well as Brandt's Mountain Finch (*L. brandti*), and Hodgson's Mountain Finch (*L. nemoricola*) clades. There is no structuring of the N. A. haplotypes; they are not reciprocally monophyletic, and the combined variability in all N.A. taxa was less than in two of their Asian congeners. AZ-specific interon (ACO119) and autosomal coding locus (MC1R) gave little additional phylogenetic information.

AMOVA (measurements and color) revealed that division into the 3 currently recognized species did not account for a significant portion of the observed genetic variation (among localities = 28.7%; among individuals = 55.4%).

Drovetski *et al.* concluded (2009:444) "(t)he patchy distribution of Rosy-finches in North America obscures the clinal variation of their phenotypic differences by creating breaks along the continuum.... [W]hen all specimens are considered in the geographic context without *a priori* taxonomic assignment, most of [the] apparently discrete variation can be explained by island vs. mainland populations, between sexes, and by a limited cline."

Recommendation:

The single North American species hypothesis is consistent with both the molecular and phenetic analyses summarized here. Natural hybridization occurs between Gray-crowned and Black rosy-finches apparently rather frequently (5 of the 201 specimens used in these DNA analyses were designated as "hybrids" in the caption of their Figure 2 [I have no idea of whether or not this was a randomly collected sample, or what hybrids were thought to be involved]). Gray-crowned and Black hybridize in Montana and Idaho where their ranges overlap. Brown-capped Rosy-Finches hybridize with Brown-capped Rosy-Finches in the Bitterroot Mountains in Idaho and Montana. The vocalizations among the taxa of rosy-finches are said to be "similar." Thus, I can accept the single-species interpretation, but I do so while admitting little field exposure with rosy-finches (and none with Blacks), and don't think that any of the data presented in Drovetski et al. presented are particularly compelling. I look forward to the comments of those of you who know rosy-finches better.

If this change is made, I think that a new vernacular name would be helpful to differentiate between Gray-crowned *sensu stricto* and *sensu lato*. I offer **American Rosy-Finch** as an option, also suggested by our 1998 Check-list.

Literature cited:

Drovetski, S. V., Zink, R. M., and Mode, N. A. 2009. Patchy distribution belies morphological and genetic homogeneity in rosy-finches. *Molecular Phylogenetics and Evolution* 50:437-445.

Johnson, R. E. 1973. The biosystematics of the avian genus *Leucosticte* in North America. Unpubl. Ph.D. thesis. Univ. California, Berkeley.

-----. 2002. Black Rosy-Finch (*Leucosticte atrata*). *In* The Birds of North America, No. 678 (A. Poole and F. Gill, eds.). The Birds of North America, Inc., Philadelphia, PA).

-----, Hendricks, D. L. P., and Hunter, K. B. 2000. Brown-capped Rosy-Finch (*Leucosticte australis*). *In* The birds of North America, No. 536 (A. Poole and F. Gill, eds.). The Birds of North America, Inc., Philadelphia, PA.

Macdougall-Shackleton, R. Johnson, R. E., and Hahn, T. P. 2000. Gray-crowned Rosy-Finch (*Leucosticte tephrocotis*). *In* The Birds of North America, No. 559 (A. Poole and F. Gill, eds). *In* The Birds of North America, Inc., Philadelphia, PA.

Remsen, Jr., J. V. 2010. Subspecies as a meaningful taxonomic rank in avian classification. *A. O. U. Ornithol. Monogr. No. 67*.

Submitted by: Jim Rising

Date of proposal: 26 Mar 2013

Change the linear sequence of *Haemorhous* finches

Description of the problem:

Until recently, the American “purple” finches were placed in the genus *Carpodacus*, with the Eurasian species of these. Recently, we moved the American *Carpodacus* to *Haemorhous*, to include the endemic taxa *purpureus, cassinii* and *mexicanus*. These American taxa are apparently a monophyletic group (Arnaiz-Villena *et al.* 2007), arranged in this sequence (i.e., Purple, Cassin's, House) in the AOU Check-list (1998).

New information:

Smith *et al.* (2013) produced a phylogeny of *Haemorhous* based on one mitochondrial marker (NADH) and five nuclear markers (beta-fibrinogen interon 5, βFib5; eukaryotic translation elongation factor 2, EEF2; muscle-specific kinase receptor, Musk; ornithine decarboxylase, ODC; and rhodopsin, Rho1). They used *Spinus pinus* as an outgroup. Their results suggest a sequence of House Finch, Cassin’s Finch, and Purple Finch [(siskin)((House)(Cassin’s, Purple))]. From what I can see, I would bet that more than one named subspecies of House Finch was involved (AZ, NM, MX – could all be the same, but MX could be a whole bunch of things) – but not a matter for our concern here.

Recommendation:

Based on these data, House, Cassin's, Purple appears to be a reasonable sequence. My suspicion is that the original sequence was set in an *ad hoc* manner, but perhaps was based on some data or hypothesis.

Literature cited:

Arnaiz-Villena, et al. 2007. *Acta Zool. Sinica* 53:826-834.
Smith, B. T., Bryson, Jr., R. W., Chua, V., Africa, L., & Klicka, J. 2013. Speciational history of North American *Haemorhous* finches (Aves: Fringillidae). *Mol. Phylo. Evol.*, 66:1055-1059.
Zuccon, D. et al. 2012. *Mol. Phylo. Evol.,* 62:581-596.

Submitted by: Jim Rising

Date of proposal: 11 Apr 2013

Change the citations for nine species described by Thomas Say

Description of the problem:

Numerous bird species were described by Thomas Say based on collections made during explorations to the Rocky Mountains under the command of Major Stephen H. Long during 1819 and 1820. A record of the results of these explorations, *Account of an expedition from Pittsburgh to the Rocky Mountains*, compiled by Edwin James, was published in both Philadelphia and London in what has heretofore been accepted to be early 1823. The authority for the bird species described in this publication has generally been cited as “Say, 1823, in Long, Exped. Rocky Mount.” (e.g., AOU 1998).

New information:

Woodman (2010) presented evidence that the Philadelphia edition of the account was available and for sale in December 1822 (whereas the London edition was not available until February 1823), thus necessitating a change of dates of description from 1823 to 1822. Woodman also noted that James is the primary editor of the *Account*, which was compiled from the notes of several of the expedition’s members, and that confusion over authorship is due in part to paraphrasing of the title as *Major Long’s Exploratory Travels to the Rocky Mountains.*

Recommendation:

I recommend that the authority for the following species be changed from “Say, 1823, in Long, Exped. Rocky Mount.” to “Say, 1822, in James, Acct. Exped. Rocky Mount.”:

Dendragapus obscurus (Dusky Grouse)
Limnodromus scolopaceus (Long-billed Dowitcher)
Patagioenas fasciata (Band-tailed Pigeon)
Tyrannus verticalis (Western Kingbird)
Salpinctes obsoletus (Rock Wren)
Oreothlypis celata (Orange-crowned Warbler)
Chondestes grammacus (Lark Sparrow)
Passerina amoena (Lazuli Bunting)
Spinus psaltria (Lesser Goldfinch)

Literature cited:

James, E. (compiler). 1822. *Account of an expedition from Pittsburgh to the Rocky Mountains, performed in the years 1819 and '20, by order of the Hon. J. C. Calhoun, Sec'y of War: under the command of Major Stephen H. Long. From the notes of Major Long, Mr. T. Say, and other gentlemen of the exploring party. Compiled by Edwin James, botanist and geologist for the expedition. In two vols. – with an atlas.* Philadelphia: H. C. Carey and I. Lea.

Woodman, N. 2010. History and dating of the publication of the Philadelphia (1822) and London (1823) editions of Edwin James's *Account of an expedition from Pittsburgh to the Rocky Mountains*. Archives of Natural History 37:28-38.

Submitted by: Terry Chesser

Date of proposal: 17 Apr 2013

Transfer *Terenura callinota* to the genus *Euchrepomis*

Note: This is a modified version of SACC proposal 557, which passed unanimously.

Description of the problem:

Traditional classifications place six species in the genus *Terenura*. Cabanis and Heine (1859–1860) named the genus *Terenura* for the species *Myiothera maculata* (Wied, 1831) of southeastern Brazil, which had been placed in *Formicivora* by Sclater (1858). The species *Formicivora callinota* (Sclater, 1855) was subsequently included in *Terenura* by Taczanowski and von Berlepsch (1885) without comment, but perhaps because Sclater noted in his original description of *callinota* that ‘‘it [*callinota*] must be placed next to the Brazilian *Formicivora maculata* . . . with which it agrees in form and style of plumage.’’ This rationale apparently led to the association of *callinota* with *maculata*. Subsequent classifications continued to place both in *Terenura*, along with four additional species described later, all noted as close relatives of *callinota* and all described in *Terenura*: *T. humeralis* (Sclater and Salvin, 1880), *T. spodioptila* (Sclater and Salvin, 1881), *T. sharpei* (von Berlepsch, 1901), and *T. sicki* (Teixeira and Gonzaga, 1983). At least two phenotypic groups have been recognized within the genus based on plumage differences: the ‘‘streaked-headed’’ group consisting of *T. maculata* and *T. sicki*, and the ‘‘standard’’ *Terenura* consisting of the remaining four species (Ridgely and Tudor, 1994).

Molecular studies showed *Terenura sharpei* and *T. humeralis* to be the sister group to all other Thamnophilidae (Bravo et al., 2012; Brumfield and Edwards, 2007; Irestedt et al., 2004; Moyle et al., 2009), but the lack of samples of the type species of the genus, *T. maculata*, prevented conclusions regarding the monophyly of *Terenura* and its phylogenetic placement.

New information:

Results from a subset of taxa from a densely sampled molecular phylogeny of the Thamnophilidae (including 214 of 220 species) confirmed that *Terenura* is not monophyletic (Bravo et al. 2012). *Terenura callinota*, *T. sharpei*, *T. humeralis*, and *T. spodionota* are not related to the type species of the genus, *T. maculata*, and form a clade that is sister to all other members of the family. *Terenura maculata* is related to the genus *Myrmotherula*. Because no other generic name is available for *callinota*, *sharpei*, *humeralis*, and *spodionota,* Bravo et al. (2012) erected the name *Euchrepomis* for these four species in recognition of the bright yellow or bright orange-rufous coloration of the lesser secondary coverts of the males. They showed that *Euchrepomis* is genetically, morphometrically, and vocally diagnosable from all similar antwrens.

Recommendation:

We recommend a “YES” vote to recognize the recently described genus *Euchrepomis* for “*Terenura*” *callinota* (Rufous-rumped Antwren).

Literature cited:

Bravo, G. A., J. V. Remsen, Jr., B. M. Whitney, and R. T. Brumfield. 2012. DNA sequence data reveal a subfamily-level divergence within Thamnophilidae (Aves: Passeriformes). Molecular Phylogenetics and Evolution 65:287–293.

Cabanis, J., Heine, F. 1859–1860. Verzeichniss der ornithologischen Sammlung des Oberamtmann Ferdinand Heine auf Gut St. Burchard vor Halberstadt. Mit krtischen Anmerkungen und Beschreinbung der neuen Arten systematisch bearbeitet. Museum Heineanum 2, pp. 1–175.

Sclater, P.L. 1855. Descriptions of some new species of ant-thrushes (Formicariinae) from Santa Fé di Bogota. Proc. Zool. Soc. London 23, 88–90.

Sclater, P.L. 1858. Synopsis of the American ant-birds (Formicariidae). Part II., Containing the Formicivorinae or antwrens. Proc. Zool. Soc. London 26, 232–271.

Sclater, P.L., Salvin, O. 1880. On new birds collected by Mr. C. Buckley in eastern Ecuador. Proc. Zool. Soc. London 48, 155–161.

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Teixeira, D.M., Gonzaga, L.P., 1983. A new antwren from northeastern Brazil. Bull. Br. Ornithol. Club 103, 133–135.

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[Other references in SACC Literature]

Submitted by: Gustavo A. Bravo and Van Remsen

Date of proposal: 19 Apr 2013

2013-B-8 N&MA Classification Committee pp. 352-353, 355, 480-481

Split South American endemics *Automolus rufipectus, Dendrocincla turdina,* and *Troglodytes cobbi* from checklist species *A. rubiginosus, D. fuliginosa,* and *T. aedon*, respectively

The South American Checklist Committee has passed proposals to split three endemic South American taxa from the widespread species with which they were previously considered conspecific: (a) *Automolus rufipectus* of the Santa Marta Mountains of Colombia was split from the widespread species *A. rubiginosus* (SACC proposal 394), (b) *Dendrocincla turdina* of eastern Brazil, northeastern Argentina, and eastern Paraguay was split from the more widespread species *D. fuliginosa* (SACC proposal 540), and (c) the Falkland Islands endemic *Troglodytes cobbi* was separated from the widespread species *T. aedon* (SACC proposal 526). Further background information on and discussion of these changes can be found on the SACC website at
http://www.museum.lsu.edu/~Remsen/SACCprop394.html,
http://www.museum.lsu.edu/~Remsen/SACCprop540.html,
and http://www.museum.lsu.edu/~Remsen/SACCprop526.html.

Recommendation:

These changes involve simple splits of single South American forms from species on the North American checklist and would result only in changes to the extralimital parts of the distributional statements of these species. I recommend that these changes be endorsed based on the principle of deferring to SACC for treatment of strictly South American taxa.

Submitted by: Terry Chesser

Date of proposal: 19 Apr 2013

Change the English name of *Thamnophilus atrinucha* to Black-crowned Antshrike

Thamnophilus atrinucha, *T. stictocephalus, T. sticturus, T. pelzelni*, and *T. ambiguus* were formerly considered conspecific with *T*. *punctatus*, with the broad species known as "Slaty Antshrike." These species are now recognized as distinct and their English names are now variations of Slaty-Antshrike, including Western Slaty-Antshrike for *T. atrinucha*. The genetic data of Brumfield & Edwards (2007), however, indicated that *T. atrinucha* belongs to a separate group within *Thamnophilus* than other members of the Slaty-Antshrike complex, a finding buttressed by a comprehensive genetic study of the Thamnophilidae by Bravo (2012). The English name of *T. atrinucha* should be changed to reflect the fact that it does not belong to this species group.

After considering several proposed English names, including Western Antshrike (rejected because this name was used previously for *Dysithamnus occidentalis*) and Black-naped Antshrike (a translation of the scientific name, but rejected because of possible confusion in that the black of the crown only extends to the upper center of the nape, rather than the entire nape) the SACC approved changing the name to Black-crowned Antshrike. This English name has not been used previously and provides an accurate description of the species. Comments on SACC proposal 570, to change the English name to either Black-crowned Antshrike or Black-naped Antshrike, are appended below

Recommendation:

We recommend that the checklist committee follow SACC in adopting Black-crowned Antshrike as the English name for *Thamnophilus atrinucha*.

Literature cited:

Bravo, G.A. 2012. Phenotypic and niche evolution in the antbirds (Aves, Thamnophilidae). Ph.D dissertation. Louisiana State University. Baton Rouge.

Brumfield, R. T., and S. V. Edwards. 2007. Evolution into and out of the Andes: a Bayesian analysis of historical diversification in *Thamnophilus* antshrikes. Evolution 61:346–367.

Comments on SACC proposal 570:

Comments from Remsen: “YES. As long as we are inventing a name, let’s go for the more accurate one. Matching an inaccurate scientific name has no value, in my opinion.”

Comments from Stiles: “YES. Like Van, I prefer the most descriptive name and given that most or all of the other distinctly “Black-capped” antshrikes are cis-Andean, this name doesn’t create overmuch confusion for this trans-Andean species!”

Comments from Zimmer: “YES, to change the English name of *T. atrinucha* to “Black-crowned Antshrike”. As has already been established, use of the hyphenated group name of “Slaty-Antshrike” is no longer appropriate. Use of “Western” Antshrike would promote confusion with the name previously used for *Dysithamnus occidentalis*. Neither “Northern Antshrike” or “Trans-Andean Antshrike” or “Central American Antshrike” really works either, since those names could just as easily describe the distributions of other species of antshrikes. I think that this is one of those instances where a descriptive, morphological-based name is better and more informative than a geographically based name (which I generally prefer). In this specific case, I agree that “Black-crowned” is more accurate, and therefore, preferable to “Black-naped” as a modifier.”

Comments from Schulenberg: “Either "Black-crowned" or "Black-naped" seem fine to me. Something should be done about all the "xxx Slaty-Antshrikes", but that's another issue.”

Submitted by: Mort Isler and Terry Chesser

Date of proposal: 24 Apr 2013

Split (a) *Schiffornis veraepacis* and (b) *S. stenorhyncha* from *Schiffornis turdina*

Description of the problem:

AOU-SACC Proposal 505 proposed splitting Thrush-like Manakin *Schiffornis turdina* into two to seven species. The proposal passed, with the committee adopting the taxonomic arrangement proposed in Donegan et al. (2011), who advocated a five-way split along different geographical lines over the approach of Nyari (2007). Information on the background and classification history is set out in proposal 505, which is repeated in full below.

For AOU-NACC purposes, the proposal that was considered by AOU-SACC can be greatly simplified into two sub-parts.

The original AOU-SACC sub-proposal A is relevant here too and reads as follows: split *veraepacis* (with *dumicola, rosenbergi, buckleyi, acrophites, aenea* and *olivacea*), from *turdina* (with *wallacii, amazonum, intermedia, steinbachi, stenorhyncha* and *panamensis*). Taxa *dumicola* (of the *veraepacis* group) and *stenorhyncha* (sister to eastern races for which the name *turdina* is senior) are sympatric in central Panama (Ridgely & Gwynne 1989) and differ in voice and plumage. Recordings of the two vocal types that these names represent (by Ken Allaire, Mike Nelson and others from Panama; see sonograms in Donegan et al. 2011) overlap geographically, confirming sympatry or elevational parapatry. According to recordist notes, the two do not respond to playback of one another. Separately, subspecies *aenea* (east slope Ecuador) replaces the Amazonian lowland groups (*amazonum*) by elevation in the Andes-Amazon interface. This split is an easy one under a BSC approach, mandated by two instances of sympatry, but it produces two vocally non-cohesive species with weird distributions, one of which is polyphyletic. It is strongly recommended that this proposal be accepted as a starter, in the context of the following sub-proposal.

[As an additional aside, Matt Miller has subsequently indicated that the two groups (*veraepacis* and *xxxx* (name depends on outcome of sub-proposal B) replace one another by elevation in some parts of Panama. The existing AOU checklist also suggests that this is the case but called for further research, particularly into voice in South America.]

The original AOU-SACC sub-proposal B is relevant here too and reads as follows: split *stenorhyncha* (with *panamensis*) from *turdina* (with *wallacii, amazonum, steinbachi* and *intermedia*). These populations are allopatric sister populations. Nonetheless, this is a straightforward split in light of them having perhaps the strongest pairwise vocal differences of any two groups in *Schiffornis*, which are equivalent to those between sympatric populations. This sub-proposal, if accepted, clears up some of the strange distribution caused by A and deals with vocal non-cohesiveness of the eastern group. The plumage differences between the three species split by proposals A and B together are also very strong, as illustrated by photographs of examples of the three taxa from Colombia in Donegan et al. (2011). Daniel Cadena fairly criticised Nyári (2007)'s split of this taxon because it was not based on recordings from different biogeographic regions within the range of *stenorhyncha* (including '*panamensis*'). In Donegan et al. (2011), we presented or studied sonograms of birds from near the type locality of *stenorhyncha* (Falcon, Venezuela), the Merida Andes (including near the southernmost extent of the East slope population), the Magdalena valley, Serranía de San Lucas (Cauca valley border, it does not go further south in this region) and eastern Panama (near the type locality of "*panamensis*"), confirming the consistency of the song throughout the range of Nyári (2007)'s proposed species *stenorhyncha*.

As in AOU-SACC proposal 505, these two proposals come very highly recommended for acceptance and should be regarded as uncontroversial under any species concept. AOU-SACC had to deal with a series of other more difficult splits that AOU-NACC need not trouble itself with because the taxa are extralimital and splits do not affect naming.

New information:

Nyari's (2007) phylogeny and Donegan et al.'s (2011) vocal analysis mandate a revision of this species, as set out in AOU-SACC Proposal 505.

Recommendation:

Instead of just *S. turdina*, two species would be recognised, Western Schiffornis *S. veraepacis* (if sub-proposal A passes) and Russet-winged Schiffornis *S. stenorhncha* (if sub-proposal B passes). The accounts for the *veraepacis* group in the checklist would be carried to a new *S. veraepacis* account and for the *turdina* group to a new *S. stenorhyncha* account. Existing discussions of uncertainty of species limits in the group would be amended. Reference should be added to the works of Nyari (2007) and Donegan et al. (2011).

Note on English names:

AOU-SACC Proposals 543 and 543A-C on English names indicated "Western" and "Russet-winged" to be the most popular names amongst committee members and other persons polled. [Note from Chesser: This is still under debate among SACC members. This is a largely South American species or species group, and I suggest that we simply adopt the English names accepted by the SACC when it has reached a decision.]

Literature cited:

Donegan, T. M., Quevedo, A., McMullan, M. & Salaman, P. 2011. Revision of the status of bird species occurring or reported in Colombia 2011. *Conservación Colombiana* 15: 4-21. http://www.proaves.org/IMG/pdf/CC15/Conservacion_Colombiana_15_4-21.pdf
Nyári, Á. S. 2007. Phylogeographic patterns, molecular and vocal differentiation, and species limits in *Schiffornis* turdina (Aves). *Molecular Phylogenetics & Evolution* 44: 154-164.
Ridgely, R. S., and J. A. Gwynn. 1989. Birds of Panama. Princeton Univ. Press, Princeton.

Submitted by: Thomas Donegan, Fundación ProAves.

Date of proposal: 25 Apr 2013

SACC Proposal 505: Split Thrush-like Manakin *Schiffornis turdina* into any of two to seven species

Background: In Proposal 327, *S. turdina* was proposed to be split into five species, based on Nyári (2007). Although several committee members voted in favour of this treatment (and all were in favour of some splitting), the proposal was rejected, there has been no follow up proposal and *S. turdina* remains a single species on the AOU-SACC list. A large number of archived and published sound recordings are now available of greater *turdina* in published recording compilations and online resources such as xeno-canto. In Donegan et al. (2011), we re-examined Nyári (2007)'s recommendations for purposes of the Colombian checklist and recent Spanish language field guide (McMullan et al. 2011) in light of the additional sound recordings available today. Photographs of birds representing the three different Colombian populations were also presented. This proposal gives the AOU-SACC the opportunity to reconsider this over-lumped group once again.

Summary of Proposal: Nyári (2007) presented molecular data and some sonograms, as discussed in proposal 327. The present species '*turdina*' is a complex group obviously constituting several biological species, with several distinctive vocal types and some instances of sympatry. In Donegan et al. (2011), we studied geographic variation in voice, the type localities of various of the names and priority issues. Committee members who wish to consider the issues in more detail may wish to consult the paper, alongside Nyári (2007)'s maps and phylogeny. We presented a series of sonograms and studied recordings with a broader geographical sample for recognised subspecies. We differed from Nyári (2007) in a handful of aspects, but the bulk of Nyári (2007)'s proposals were supported by additional materials now available.

The *turdina* group needs splitting even under the most conservative of species concepts. Although this aspect was not highlighted by Nyári (2007), two of the taxa in the *turdina* group are sympatric in Central Panama (Ridgely & Gwynne

1989) and two others of them replace one another by elevation in the Amazon-Andes interface of Ecuador and Peru (Ridgely & Greenfield 2001, Krabbe & Nilsson 2003). Moreover, various other populations are vocally distinct. This proposal should therefore involve a discussion of the extent and manner in which one ought to go about cutting off various limbs in this group so as to produce a set of species which are vocally cohesive or, for those interested in such matters, monophyletic (rather than a question of whether sub-division is warranted at all). In Donegan et al. (2011), we proposed recognising the following species (using Nyári 2007's vernacular names):

1. Thrush-like Schiffornis *S. turdina* (provisionally including subspecies *steinbachi, amazonum, wallacii* and *intermedia*) of the Amazon region and Atlantic forest, including the Amazonian region of Colombia.

2. Slender-billed Schiffornis *S. stenorhyncha* (including *panamensis*) of the Tacarcuna region of Panama and Colombia, Magdalena valley and Central, East and Merida Andes of Colombia and north-western Venezuela.

3. Brown Schiffornis *S. veraepacis* (including *dumicola, rosenbergi, "buckleyi"* and *acrophites*) of the Chocó from northernmost Peru through Ecuador to Colombia and Central America from northern/western Panamá northwards.

4. Foothill Schiffornis *S. aenea* of the western Amazon region of Ecuador and Peru.

5. Olivaceous Schiffornis *S. olivacea* of the Guianan shield.

The table below summarises the range of named populations and other proposed treatments:

Population name	**Range**	**Nyári Vocal group**	**Nyári's molecular group**	**Nyári's PSC approach**	**Nyári's BSC approach**	**Nyári's SACC proposal**	**Donegan et al. 2011 BSC approach**
veraepacis, dumicola	Central America (S Mexico and Belize to N/W Panama)	A	1	*veraepacis*	*veraepacis*	*veraepacis*	*veraepacis*
rosenbergi / buckleyi / acrophites	Chocó of Colombia and N Ecuador	A	3	*rosenbergi [/ veraepacis]*	*veraepacis*	*veraepacis*	*veraepacis*

olivacea	Guianan shield of Venezuela to Suriname, N. Brazil	A	7	*olivacea*	*veraepacis*	*olivacea*	*olivacea*
aenea	West Amazon in Ecuador and Peru	B	4	*aenea*	*aenea*	*aenea*	*aenea*
stenorhyncha / panamensis	Northern Colombia, Venezuela, S/E Panama	E	2	*stenorhyncha*	*stenorhyncha*	*stenorhyncha*	*stenorhyncha*
turdina, intermedia	Atlantic forest region of Brazil	D	6	*turdina*	*turdina*	*turdina*	*turdina*
steinbachi	Southern Amazonia in Peru, Bolivia, Brazil	C	6	*amazonum*	*amazonum*	*amazonum*	*turdina*
wallacii	Para, Brazil and surrounding region	A/C	6/7	*amazona / olivacea*	*amazonum / veraepacis*	*amazonum / olivacea*	*turdina*
amazonum	Northern Amazonia in Colombia, Venezuela, Peru, Bolivia, Brazil.	C	5	*amazonum*	*amazonum*	*amazonum / turdina*	*turdina*

Sub-proposals: This proposal set is split into various cumulative sub-proposals, such that if the SACC disagrees with Donegan et al. (2011)'s treatment or wishes to adopt some other arrangement, it can stop at an earlier or later stage of limb separation and still come up with a new taxonomy.

A: Split *veraepacis* (with *dumicola, rosenbergi, buckleyi, acrophites, aenea* and *olivacea*), from *turdina* (with *wallacii, amazonum, intermedia, steinbachi, stenorhyncha* and *panamensis*). Taxa *dumicola* (of the *veraepacis* group) and *stenorhyncha* (sister to eastern races for which the name *turdina* is senior) are sympatric in Central Panama (Ridgely & Gwynne 1989) and differ in their voice and plumage. Recordings of the two vocal types that these names represent by Ken Allaire, Mike Nelson and others from Panama (of which sonograms were presented in Donegan et al. 2011) overlap geographically, confirming sympatry or elevational parapatry. According to recordist notes, the two do not respond to playback of one another. Separately, subspecies *aenea* (east slope Ecuador) replaces the Amazonian lowland groups (*amazonum*) by elevation in the Andes-Amazon interface. This split is an easy one under a BSC approach, mandated by two instances of sympatry, but it produces two vocally non-cohesive species with weird distributions, one of which is polyphyletic. It is strongly recommended that this proposal be accepted as a starter, in the context of the next following sub-proposals.

B. Split *stenorhyncha* (with *panamensis*) from *turdina* (with *wallacii, amazonum, steinbachi* and *intermedia*). These populations are allopatric sister populations. Nonetheless, this is a straightforward split in light of them having perhaps the strongest pairwise vocal differences of any two groups, which are equivalent to those between sympatric *Schiffornis*. This proposal, if accepted, clears up some of the strange distribution caused by A and deals with vocal non-cohesiveness of the eastern group. The plumage differences between the three species split by proposals A and B together are also very strong, as illustrated by photographs of examples of the three taxa from Colombia in Donegan et al. (2011). Daniel Cadena fairly criticised Nyári (2007)'s split of this taxon on account of it not being based on recordings from different biogeographic regions within the range of *stenorhyncha* (including '*panamensis*'). In Donegan et al. (2011), we presented or studied sonograms of birds from near the type locality of *stenorhyncha* (Falcon, Venezuela), the Merida Andes (including near the southernmost extent of the East slope population), the Magdalena valley, Serranía de San Lucas (Cauca valley border, it does not go further south in this region) and eastern Panama (near the type locality of "*panamensis*"), confirming the consistency of the song throughout the range of Nyári (2007)'s proposed species *stenorhyncha*.

--- These first two proposals come very highly recommended for acceptance and should be regarded as uncontroversial under any species concept. There now follow two further splits of allopatric populations which Nyári proposed making in his SACC proposal and with which Donegan et al. (2011) agreed. ---

C. Split *aenea* (monotypic) from *veraepacis* (with *dumicola, rosenbergi, buckleyi, acrophites*; and *olivacea* if D fails). *S. aenea* is the Andean East slope population in Ecuador and Peru. It may also extend in range into southern Colombia, although there are no records there yet. The two populations subject to proposal C are apparently allopatric sister populations which straddle the Andes. When split, they appear as mutually monophyletic (c.3% mtDNA difference). Rejecting this split does not therefore produce polyphyly or paraphyly. *S. aenea* is however vocally rather different from *veraepacis*, leading to various authors such as Ridgely & Tudor (2001) and Krabbe & Nilsson (2003) noting that more than one species may be involved (see sonograms in Donegan et al. 2011). The differences are not as great as those shown by *stenorhyncha* (Proposal B), with various of the note shapes of the *aenea* song having equivalents in a different order in the songs given by the *veraepacis* group, but the differences in note shape or order of note shape are consistent and diagnosable. We preferred to spit *aenea*, based on vocal differences, molecular data and their distributions -

which straddle the Andes in a very high part of the mountain range. Taken together, these factors put the burden of proof on those who would have these two lumped.

D. Split *olivacea* (monotypic) from *veraepacis* (with *dumicola, rosenbergi, buckleyi* and *acrophites*; and *aenea* if C fails). This is the troubling Guianan shield population. This proposed split was subject to a differing treatment by Nyári (2007) in his BSC interpretation (lumped) versus his SACC proposal and PSC interpretation (split). *olivacea* is vocally very similar to the *veraepacis* group, with no differences elucidated by Nyári (2007). In Donegan et al. (2011), we noted small differences in the extent of the upturn of the main note. In the previous proposal, there was some speculation as to whether the vocal similarities here are a result of limited divergence or convergence, although most committee members seemed in favour of splitting this taxon. It is basal to all other current *turdina* in the Nyári (2007) phylogeny, showing 9%+ mtDNA differentiation from all other taxa. In Donegan et al. (2011), we presented additional information suggesting that *olivacea* is indeed the correct name for this population, as provisionally treated by Nyári (2007). Not splitting *olivacea* produces a *veraepacis* group which is polyphyletic and which has a strange distribution. Because Proposal A should not be regarded as optional (and B and C are strongly recommended), it would be reasonable also to make this split.

--- We went this far in Donegan et al. (2011) and did not adopt any further splits. ----

E. Split *amazonum* (with *wallacii*) from *turdina* (with *intermedia* and *steinbachi*). This is the “North Amazon vs. South Amazon and Atlantic forest” split that Nyári (2007) proposed. There is clearly vocal variation in the southern part of the greater ‘*turdina*’ range, but this split raises various difficulties, some of which were discussed by Doug Stotz and Van Remsen as being unfavourable factors towards adopting the treatment in the previous proposal. Further research indicates that *turdina* in the subspecies sense (South Atlantic forest) resembles *amazonum* (North Amazon) vocally; whilst *intermedia* (North Atlantic forest) generally resembles assumed *steinbachi* (south Amazon of Bolivia to Peru) vocally. All these eastern and southern populations are monophyletic when taken together. Their songs are all generally comprised of longer notes than the other species, differing among one another in the shapes of up or down-turns at the start of end of particular notes (see sonograms in Donegan et al. 2011). These vocal differences exceed those shown by *olivacea* but do not reach the differentiation shown by *aenea*. Given the scope of our paper (Colombia), a detailed examination of the Bolivian and Brazilian types, their localities and sound recordings on different sides of major Amazonian rivers was out of scope, but could be recommended for further research. This split or other possible treatments for the southern populations may be warranted but lumping them does not cause paraphyly or polyphyly, nor does it produce a vocally uncohesive group or preclude further studies from taking place. We did not recommend adopting this split for the time being but further research is clearly warranted.

--- Nyári (2007) went this far and did not adopt any further splits. ---

F. A further possible alternative for the Eastern taxa based on tentative vocal data would be to split *turdina* (with *wallacii* and *amazonum*) from *intermedia* (with *steinbachi*). This produces two species of strange distribution and it is unknown how this would hang with molecular data, owing to the lack of sampled individuals from very close to the type localities of some of these names in Nyári (2007). We did not take this step, but SACC members with greater familiarity with southern Amazonian and Atlantic forest birds may wish to comment on this option or consider it, for completeness, as an alternative to E.

G. Split *veraepacis* (with *dumicola*) from *rosenbergi* (with *buckleyi* and *acrophites*). Proposals A-C, if accepted, result in *veraepacis* including two disjunct populations, one in the Chocó-Tumbes and another broadly in Central America. The ranges of the two are bisected in the Tacarcuna region to southern/eastern Panama by that of *stenorhyncha*. The *veraepacis* and *rosenbergi* groups are apparently mutually monophyletic, although with low molecular differentiation (0.8% mtDNA) and with small vocal differences. This would be a possible split under some species concepts (e.g. PSC) but we did not adopt it. Tentative differences in secondary calls (based on only a single Central American recording of the secondary song) should be regarded as a matter for further research. As with the possible Amazonian splits, not adopting this treatment does not preclude further studies of these birds.

<u>Recommendation</u>: A resolute YES to A and B; YES to C and D for the reasons set out in Donegan et al. (2011) and Nyári (2007). NO to E, F and G for the time being, with a note that further research could shed light on variation among these populations and that other splits may be warranted in the future.

<u>English names</u>: If any of these proposals pass, then the English names suggested by Arpad Nyári in Proposal 327 (see above) would be adopted. If anyone prefers a different vernacular name for a narrower *turdina*, then they can raise a separate proposal on that issue.

<u>References</u>:

Donegan, T.M., Quevedo, A., McMullan, M. & Salaman, P. 2011. Revision of the status of bird species occurring or reported in Colombia 2011. *Conservación Colombiana* 15: 4-21. http://www.proaves.org/IMG/pdf/CC15/Conservacion_Colombiana_15_4-21.pdf

Nyári, Á. S. 2007. Phylogeographic patterns, molecular and vocal differentiation, and species limits in *Schiffornis* turdina (Aves). *Molecular Phylogenetics & Evolution* 44: 154-164.

Other references are cited in these papers.

Thomas Donegan, October 2011

<u>Comments from Robbins</u>: "YES to subproposals A (*veraepacis*), B (*stenorhyncha*), C (*aenea*), and D (*olivacea*). Nyari's genetic and preliminary vocal data clearly established that these taxa should be elevated to species level; Donegan et al.'s (2011) more in-depth vocal analyses corroborated those results. Undoubtedly, recognition of additional species will be warranted when more detailed information becomes available."

<u>Comments from Stiles</u>: "This proposal would split *Schiffornis turdina* into at least four species, based primarily upon data from the genetic analysis of Nyári and a more detailed analysis of vocalizations by Donegan. The latter are a definite step forward and incline me to agree with the proposal, although given the abundant material available, I would have felt more comfortable if statistical analyses of the vocalizations had been performed. However, with the data at hand I agree that the burden of proof has now shifted onto those who would retain a single species. I have now had a chance to examine the Colombian material here and have obtained information on the plumage differences of at least the Colombian taxa, and this leaves me with strong doubts regarding the English names proposed by Nyári – given the rather subtle differences between most taxa and the uncertainty regarding the limits of some distributions, I feel that appropriate and descriptive English names may be important, and will make a series of suggestions below (which might warrant a separate proposal). For now, regarding the splits proposed by Donegan:

> A. Split the *veraepacis* group from a broad *turdina*: YES. The sympatry of the two groups in central Panamá, plus differences in vocalizations and evidence that the two do not respond to each other's songs makes this split mandatory.
>
> B. Split the eastern *amazonum* group from the northern *stenorhyncha* group: YES. Both the genetic data and information from vocalizations, as well as biogeography, make this split logical and desirable.
>
> C. Split *aenea* from the veraepacis group: YES. Although the genetic data are not quite as definitive, both vocalizations and biogeography seem better addressed by splitting.
>
> D. Split *olivacea* of the Guyana Shield from the *veraepacis* group: YES. This split will avoid massive polyphyly in the genus as a whole and makes good biogeographical sense as well.

"I agree with Donegan that although some further splits could be made, especially in the *amazonum* group, more data are required. The split of *rosenbergi-acrolphites* from *veraepacis* has no genetic backing and mainly reflects the gap in distributions, which might be better explained by historical factors. For now, the four splits suggested are sufficient and desirable.

"Now, for the messy part: English names. Because the splits have as yet not been "officially" adopted, it seems appropriate to make these suggestions now. To begin with, I feel strongly that the name "Brown Schiffornis" for the *veraepacis* group (species) is singularly inappropriate. The two Colombian races (*acrolophites* and *rosenbergi*) are much the greenest of all the taxa. By far the brownest of all is *stenorhyncha* (which is also not the "slenderest-billed"; that distinction belongs to *amazonum*). Hence, I would propose that the name "Brown Schiffornis" be applied to *stenorhyncha* (or "Brownish Schiffornis" if one wishes to avoid confusion with Nyari's names). "Greenish Schiffornis" could then be applied to *veraepacis*. I make this latter suggestion a bit tentatively, as I do not have material of the northern races of the latter for direct comparison, although my description and the plate in the Costa Rican guide (as well as the descriptions in Ridgway) emphasize olive-green to olive-brown tones. (The name "olivaceous" would also be appropriate, but is perhaps best reserved for *olivacea* of the Guyana Shield – although for the latter "Guianan Schiffornis" would also be appropriate). Aside from its overall brownish coloration, the most trenchant plumage characters of *stenorhyncha* are the decidedly rufescent color of the wings and the sharp division of the brownish to olive-brown breast band and the grayish-olive lower breast and belly; it is also the largest taxon. Hence "Rufous-winged" or "Grayish-bellied" would not be inappropriate for this taxon. I note here that none of our series of *stenorhyncha* show the clear gray belly of the bird in Donegan's photo: given the yellowish coloration of the basal tomia and gape, I suspect that his bird was young – I suspect that young birds in this genus in general are brighter and more contrasty than adults, though few of our specimens are reliably aged. Actually, the belly in *amazonum* is also grayish olive and in some is grayer than in most *stenorhyncha*, although the contrast with the breast is not nearly so sharp. The wing in *amazonum* is a darker, duller brown than in *stenorhyncha* so "Brown-winged Schiffornis" would emphasize this difference, but I think that "Amazonian Schiffornis" is certainly simpler and appropriate, and could be kept for *amazonum* should this group be split further - although such a split (or splits) seem problematic for the present.

Comments from Stotz: "YES on subproposals A, B, C, and D. NO on subproposals E, F, and G. I am much more comfortable with this proposal than the previous proposal. We will probably have to split other taxa farther down the line, but this is a reasonable first step on this complicated group. I agree with Gary that we need to think about English names. However, his suggestion of "Greenish" for *veraepacis* won't work. Greenish Schiffornis is the name of *Schiffornis virescens* of SE Brazil. I think that it would be a mistake to use Olivaceous for *veraepacis* with *S. olivaceus* being one of the names. So I don't have a good alternative in mind, but I agree with Gary that Brown would not be a great choice. I would go for Brownish for *stenorhyncha*, and while I am okay with Olivaceous for *S. olivaceus*, I think we would be better served to adopt a geographic modifier Guianan for that species. One further issue is a name for the reduced *turdina*. Given the complications in that group, and how much of the *turdina* (sensu lato) has been carved off from it. I think we absolutely have to have an alternate English name for this new *turdina*. Unfortunately I have not come up with a great answer. These are really dull birds. My only thought is Southern Schiffornis."

Comments from Pérez: "YES to subproposals A, B, C, and D. I think molecular, vocal and distributional data support this awaited treatment for *S. turdina*. NO to subproposals E, F and G; more in-depth treatments, as suggested by Donegan et al. (2011), would likely provide information for further splitting."

Comments from Jaramillo: "YES – accept A, B, C, and D. I am not comfortable going further with it, particularly as vocal data are relatively common and further separations require a higher level of scrutiny. Having said this, once we determine which forms the committee has decided to split off, or not, I think we need a separate simple proposal on the English names."

Comments from Nores: "YES to subproposals A (*veraepacis*), B (*stenorhyncha*), C (*aenea*), and D (*olivacea*). I consider that the molecular and vocal data support this treatment. However, I did not find either in the proposal or the Nyári´s paper a clear relationship between the new species and the regions 1-7 of the phylogeographic tree of Nyári. How much easier would have been to interpret the Nyári´s tree if he would have put the name of the subspecies or species next to the name of the region.

Comments from Remsen: "YES to subproposals A (*veraepacis*), B (*stenorhyncha*), C (*aenea*), and D (*olivacea*). Echoing the comments of others, there is good evidence for at least 5 species, and more will likely come from additional analyses. By the way, I strongly agree with Manuel's comment on the poor labeling in Nyári´s tree – just one more example of how poorly edited *MPE* is.

"English names are a real problem. Ridgway and Hellmayr both used "Olivaceous" for *S. olivacea*, so I would favor retaining that one. Gary's comment on "Brown" for veraepacis is correct – "Brown" is perhaps the worst name possible for this species, even though used by Ridgway and Hellmayr. Ridgway used "Russet" for stenorhyncha, but I do think Gary's name is better. Hellmayr used "Slender-billed", but no reason to perpetuate that if inaccurate. Also, Doug's comments are also correct – "Greenish" is not available, and we really cannot retain "Thrush-like" for a dramatically diminished *S. turdina* (and besides, it's a poor name, species epithet not withstanding). So, I am installing some temporary English names based on Gary's recommendations and will be appointing someone to make a formal proposal to examine carefully the English names before they get any traction."

Split *Myrmeciza zeledoni* from *M. immaculata*

Description of the problem:

AOU-SACC Proposal 541 proposed splitting Western Immaculate Antbird *Myrmeciza zeledoni* from Immaculate Antbird *M. immaculata*. In AOU-SACC Proposal 568A, "Zeledon's Antbird" was adopted instead of Immaculate Antbird as the English name for *M. zeledoni*.

New information:

Donegan's (2012) study of voice, biometrics and plumage, based on fieldwork and specimens, requires species limits to be reconsidered, as discussed below in AOU-SACC proposal 541.

Recommendation:

Only *zeledoni*, not *immaculata*, as reconstituted, occurs in the AOU-NACC area. As a result, if this proposal passes, the name of Immaculate Antbird *M. immaculata* would change to Zeledon's Antbird *M. zeledoni.* Reference should be added to Donegan (2012).

Literature cited:

Donegan, T. M. 2012. Geographical variation in Immaculate Antbird *Myrmeciza immaculata*, with a new subspecies from the Central Andes of Colombia. *Bull. Brit. Orn. Cl*. 132: 3–40.

Submitted by: Thomas Donegan, Fundación ProAves.

Date of proposal: 25 Apr 2013

Some extraneous comments on the approach of the AOU-SACC to the work of the author in past proposals have been deleted from the original proposal, which is otherwise set out verbatim below.

SACC Proposal 541: Split *Myrmeciza zeledoni* from *M. immaculata*

Proposal: This proposal, if it passes, would result in an additional *Myrmeciza* being recognised based on a recent publication.

Discussion:

Donegan (2012) published a detailed study of available type specimens, plumages, voice and biometrics of *M. immaculata*. Species limits were not the central point or objective when work on the paper started – that instead being subspecies limits and the undescribed, vocally divergent Colombian Central Andes population. However, data showing present species limits to be misled came up and, unfortunately, could not be ignored in a serious taxonomic review like this. We included this split in the Colombian field guide, Spanish language version (McMullan et al. 2011) after sharing the MS with co-authors, among only 3-5 considered deviations from SACC.

The two groups for which species rank is proposed here meet the Isler tests for vocal differentiation (including when subspecies within the groups are subject to cross-pairwise comparisons, not just the species groups). That is to say, there are multiple fully diagnosable vocal differences, which in number exceed the differences observed between sympatric Thamnophilidae.

M. zeledoni and *M. immaculata* are also diagnosable through a range of both male and female plumage characters, with significant but non-diagnosable differences in biometrics and distributional patterns suggesting ecological differentiation. The plumage differences (in carpal patch extent in both sexes, the shade of brown in upperparts and underparts of females and lores feathering) greatly exceed those between some known-to-be-good antbird species occurring in the same forests of Colombia (e.g. *C. parkeri/tyrannina*) and other recently proposed splits accepted by this committee, such as the Xingu Scale-backed Antbird. Ridgway (1909) made this split (and also split *zeledoni* from *macrorhyncha*) on this basis.

As summarised in the abstract: "*Available data … support splitting Immaculate Antbird into two species, under any modern version of the Biological Species Concept. Western Immaculate (or Zeledon's) Antbird M. zeledoni inhabits foothills and mountains from Costa Rica southwards, and includes M. z. macrorhyncha of southern Panama to Ecuador. (Andean) Immaculate Antbird M. immaculata occurs in the Central, East, Perijá and Mérida Andes of Colombia and Venezuela (and includes the new subspecies). Vocal differences exceed those between parapatric Goeldi's Antbird M. goeldii and White-shouldered Antbird M. melanoceps and those between sympatric thamnophilids in other genera.*"

This proposal is not novel in that Ridgway (1909) also split them (and he split *zeledoni* from *macrorhyncha/berlepschi* too). The proposal in Donegan (2012) is a minimalist treatment in that *macrorhyncha* is also diagnosable vocally from *zeledoni* based on its more equal-length first note of male songs. New taxon *concepcion* shows small subjective note shape differences from *immaculata* in both song and call and is near-diagnosable (over 90% by actual data but not 97.5% using t-distributions) by a quantitative vocal measure (song speed). As a result, some of the other taxa proposed for subspecies rank by Donegan (2012) present similar situations to the borderline split of *M. palliata* which was recently approved by this committee. Both *concepcion* and *macrorhyncha* appear to be good phylogenetic species and more ardent splitters would go further than just separating [*zeledoni+macrorhyncha*]. This proposal therefore reflects a conservative approach. PSC / evolutionary species concept advocates might recognise four species. The two split groups recognised in this proposal are vocally and morphologically cohesive whilst mutually highly differentiated.

Because *zeledoni* and *berlepschi* are contemporaneously described, and because the latter name may resurface with generic revisions, *zeledoni* was selected as the name that should apply by explicit first reviser action.

…

<u>Vernacular names</u>: Western Immaculate Antbird and Andean Immaculate Antbird should be adopted on passing of the proposal, being preferred for reasons stated in the paper. A separate proposal on vernacular names will be raised in the event that this proposal passes, to change the former's name to Zeledon's Antbird (retaining Immaculate for a more restricted *immaculata*). In the event that this proposal fails, a series of proposals for various antbirds currently ranked as species to be lumped or re-lumped for consistency would have to be presented: including just in this genus *M. goeldii* into *M. melanoceps* and both *M. palliata* and *M. berlepschi* back into *M. laemosticta*.

<u>References</u>:

Donegan, T. M. 2012. Geographical variation in Immaculate Antbird *Myrmeciza immaculata*, with a new subspecies from the Central Andes of Colombia. *Bull. Brit. Orn. Cl*. 132: 3–40.

Other references are cited in the above paper.

Thomas Donegan, August 2012

<u>Comments solicited from Mort Isler</u>: "I should start by revealing that I was a reviewer for the *BBOC* on what became the Donegan 2012 paper. The paper has a wealth of valuable information regarding the *Myrmeciza immaculata* group, but I will confine my remarks to the taxonomic recommendations. After reexamining the paper, I continue to support the author's recommendation that *M. immaculata* and *M. zeledoni* be considered distinct species based primarily on the multiple vocal characters that distinguish their loudsongs. Although I did not attempt to replicate the analysis, I spot-checked the vocal character measurements of the author, and I found them to be altogether consistent. I might also add for future consideration that the data supporting subspecies within each of the species is also appears well founded.

"One problem. The published version of the paper states that vocalizations of the two species differ diagnostically "in the note shape of single-note calls". This statement was not in the manuscript that I reviewed, which only stated, "there are small differences in the note shape of single-note calls". After reviewing spectrograms, I conclude that the single-note calls of the two species are so similar that “blind tests” by non-participants (not mentioned in the methodology) would be required to come to the determination that they differ diagnostically in note shape. Moreover, the difference in shape is not made explicit in the paper except to state that in *zeledoni “the up-down stroke is thicker and longer at peak in most recordings”,* which I believe, are not descriptions of note shape but are measurable characters. (The Xeno-canto commentary mentioned in the proposal also speaks of a more rounded note shape.) This weakness in the paper is unfortunate and should not detract from the otherwise excellent analysis and the taxonomic conclusions.”

“I also mention that when I reviewed the proposal, I recommended to Thomas that he use ”Zeledon’s” and “Immaculate” as English names for the two species, at least partially because *zeledoni* occupies portions of the Western Andes of Colombia and Ecuador so “Andean Immaculate Antbird” for *immaculata* isn’t completely appropriate. It also seems like a good idea to recognize the contributions of Zeledon.”

Comments from Thomas Donegan: “I'd like to thank Mort Isler for taking the time to look through a long paper again, for his thoughtful comments above and also in the peer review process, as well as in making various otherwise un-archived sound recordings available. These sorts of taxonomic reviews can only proceed with collaboration; and he and others made available many sound recordings without which sample sizes would have been lower and this paper's conclusions would not have had the same statistical support. I also agree with his comments substantively.

“Whilst I am uneasy with making aspects of the peer review process a matter of public record, the following further observations should probably be made. In Appendix 3D, below the table on page 34 of the published paper, it states: "*macrorhyncha* and *zeledoni*: up-down stroke, thicker and longer at peak in most recordings". In Appendix 3D on page 32 of the submitted MS, it stated: "*macrorhyncha* and *zeledoni*: up-down stroke, thicker at peak in most recordings". In Appendix 4 (of both the published and submitted MS), it states "*σ*NS(4)" under each cell comparing members of the *zeledoni* group and *immaculata* group. This denotes subjective diagnosability of note shapes of single note songs based on available recordings but without statistical analyses. These cell entries did not change from submission to publication. The only material change is that in the species limits section of the published paper, subjective differences in note shape - previously noted only in appendices - were mentioned within a list of differentiating characters.

“Methods for analysing note shape involved writing down a description for each vocalisation analysed, alongside the mensural data, as part of the process when each vocalisation was analysed. This gave raw data for the basis of textual note shape descriptions. When writing up the MS, I then lined up all available sonograms of all taxa for single notes in order to verify the descriptions and differences. This verification process was repeated at least thrice more for single note shapes: once when selecting sonograms, when I used the opportunity to double-check descriptions; at least once when writing the diagnosis section of the new subspecies (this was itself checked a few times); and a third time after having more recently commented on Chaves et al. (2010)'s approach to note shape variables, to double-check for consistency of approach with my comments and other studies. There is no blind test involved, but there were at least 4 self-verifications after initial data collection. None of this checking and double-checking is mentioned in the methods simply because anyone publishing a major revision should be expected to check, double-check and double-double-check their results.

“I agree that more research could be done into differences in single note calls using statistics and blind tests. I decided against spending more time on this because there were only 2 recordings available of this sort of vocalisation for four key populations (Merida, East Andes, Central Andes and the nominate population of *zeledoni*). It is doubtful that statistical tests of diagnosability or even t-tests of average difference could be "passed" with samples of this size even if some truly diagnostic variable could be identified given (i) that data were compared not between putative species at a macro-level but at subspecies and population level, and (ii) the high numerical value that t-distribution tables give with such low degrees of freedom. I therefore took only basic frequency and length data for such calls. There are some small apparent non-diagnosable differences in acoustic frequency between these calls evident when one looks at the raw data, but little by way of statically backed differentiation was found for these sorts of vocalisations. Of course, small sample sizes also mean that conclusions about note shape variation are tentative and provisional. But rather than a weakness or shortcoming in the paper, this is more a function of a non-ideal data set for this particular sort of rare vocalisation and an author focusing analysis on the types of vocalisations and variables with greater samples that are more likely to produce useful results.

“Based on samples available, the *zeledoni* group (for which there is a much greater sample of subspecies *macrorhyncha*) have a more "n", less "chevron" shaped single note, with a thicker piece of noise of more or less stable frequency at the peak of the note, compared to a narrower / more pointed / faster and less stable peak in *immaculata* group as a whole. The wording in the paper and XC summary sought to capture this difference more concisely, and did so accurately. The small differences can be seen from Figure 5 (compare Figs. 5A-C of *zeledoni* group with Figs. 5D-H of *immaculata* group). Committee members who are interested in this issue should look at the published sonograms and can make their own mind up as to what they think of the observed differences. Time will tell whether these observed differences can also be supported statistically or remain true when greater samples of some taxa are considered.

“As suggested by Mort Isler in his comments, I would like to *really emphasise* that no "hat" should be "hung" on single call note shape differences in the species-level taxonomic proposals set out in the paper. Sample sizes for male loudsongs (and indeed other songs and calls and some other populations' single-note calls) were considerably larger than n=2. See the data in Appendix 3 for sample sizes and in Appendix 4 for the full details of statistical tests passed for different variables. The two proposed species groups subject to this proposal differ from one another diagnosably in several more variables than is the standard species benchmark for antbirds, even disregarding any putative differences in the note shape of single note calls.

“As for vernacular names, the preference for "Western Immaculate Antbird" (*zeledoni*) and "Andean Immaculate Antbird" (*immaculata*) is for two reasons. First, there are probably issues with restricting "Immaculate" to a species whose range does not coincide with the region where probably most birders have seen these (western Ecuador and Costa Rica, where *zeledoni* in the species sense occurs). The *zeledoni* group occurs to the west of *immaculata*. Whilst *zeledoni* does itself also occur in the western cordillera and slope of the Andes and achieves similar elevations to *immaculata*, a split *immaculata* is restricted to Andean slopes mostly at 800-2000 m, which is an unusual distribution for a thamnophilid antbird. (In Colombia, Parker's Antbird, Rufous-winged Antwren, various *Dysithamnus* and Unicolored Antshrike are others that spring to mind as truly Andean in distribution; this compares to many tens of lowland antbirds.) Secondly, this suggestion is based on a personal bias against using patronyms generally where possible.

“Sorry for boring you all with the length of this comment. As they say over here: "I'll get my coat."

Comments from Pacheco: “YES. Bem corroborado por evidências vocais bem manejadas na análise.”

Comments from Zimmer: “YES. The vocal differences are, I believe, of a number and degree consistent with a ranking of separate species. I would echo Mort Isler’s comments regarding the wealth of information provided in Donegan (2012) supporting this split — the paper does not suffer from any lack of detail. …”

Comments from Stiles: “YES. The vocal differences are sufficiently solid and morphological differences, though rather subtle, are also consistent with treating *zeledoni* as a separate species from *immaculata*; the split makes biogeographical sense as well. However, I strongly endorse Isler’s suggestions regarding English names: Immaculate for *immaculata* and Zeledón’s for *zeledoni*. Unlike the hapless Schiff, Zeledón was a pioneering Costa Rican ornithologist (and friend of Ridgway) who made important collections that formed the nucleus of the Museo Nacional de Costa Rica’s collection, which I have used on numerous occasions - and I can testify to the quality of Zeledon’s skins!”

Comments from Jaramillo: “YES – This seems uncontroversial and clear cut. Regarding names, Zeledon’s sounds good to me. But create a unique name for *immaculata* – otherwise this creates problems in today’s world. In particular many observers are using e-bird as a primary way to record data, and unique names will decrease confusion in systems such as this.”

Comments from Remsen: “YES …”

Comments from Nores: “YES. The vocal differences are sufficiently important and morphological differences, to a lesser degree, are also consistent with treating *zeledoni* as a separate species from *immaculata*.”

SACC Proposal 568: Change English names of Immaculate Antbirds

With the passing of Proposal 541, the names "Western Immaculate-Antbird" and "Andean Immaculate-Antbird" were adopted for *Myrmeciza zeledoni* and *M. immaculata* respectively. Vernacular names were discussed in Donegan (2012) as follows:

"Although some commentators prefer new names for components of split species (e.g., Remsen et al. 2012), the name ‘Immaculate Antbird’ remains appropriate even for a split *M. immaculata*, being a direct translation of the species’ scientific name. Cory & Hellmayr (1924) used two uninspiring patronyms for the other group: Zeledon’s Ant-catcher for M. i. zeledoni and Berlepsch’s Ant-catcher for *M. i. berlepschi*. No other vernacular names appear ever to have been used. ‘Berlepsch’s Antbird’ would confuse with Stub-tailed Antbird *M. berlepschi* and the name *berlepschi* is currently replaced by *macrorhyncha*. However, Zeledon’s Antbird would be available.

“Because patronyms convey little information about birds to their main users—birdwatchers—possible alternative names for *M. zeledoni* require consideration. These antbirds do not lend themselves to plumage-based names due to their strong sexual dichromatism. No obvious plumage patterns unite both sexes and the various populations of the new species. Males are uniform black, and females uniform brown, but Uniform Antshrike (*Thamnophilus unicolor*) could confuse and ‘White-shouldered Antbird’ is already used for *M. melanocep*s. A good morphological-based name for *zeledoni* is therefore elusive. Similarly, there is no geographic name available to describe the region from western Ecuador to Costa Rica. Arguably the best approach is to use Western Immaculate Antbird (*zeledoni*) and Andean Immaculate Antbird (*immaculata*). They are clearly related and both have been known as Immaculate Antbirds for a long time."

In response to some calls for "Zeledon's Antbird", I further noted as follows in Proposal 541:

"… the preference for "Western Immaculate Antbird" (*zeledoni*) and "Andean Immaculate Antbird" (*immaculata*) is for two reasons. First, there are probably issues with restricting "Immaculate" to a species whose range does not coincide with the region where probably most birders have seen these (western Ecuador and Costa Rica, where *zeledoni* in the species sense occurs). The *zeledoni* group occurs to the west of *immaculata*. Whilst *zeledoni* does itself also occur in the western cordillera and slope of the Andes and achieves similar elevations to *immaculata*, a split *immaculata* is restricted to Andean slopes mostly at 800-2000 m, which is an unusual distribution for a thamnophilid antbird. (In Colombia, Parker's Antbird, Rufous-[rumped] Antwren, various *Dysithamnus* and [Uniform] Antshrike are others that spring to mind as truly Andean in distribution; this compares to many tens of lowland antbirds.) Secondly, this suggestion is based on a personal bias against using patronyms generally where possible."

A map showing the two species' distributions in Colombia is set out below, copied from Donegan (2012). *M. zeledoni* extends further south in Ecuador to around the Chocó / Tumbes interface (subspecies *macrorhyncha/berlepschi*) and also north into highlands of Costa Rica (subspecies *zeledoni*).

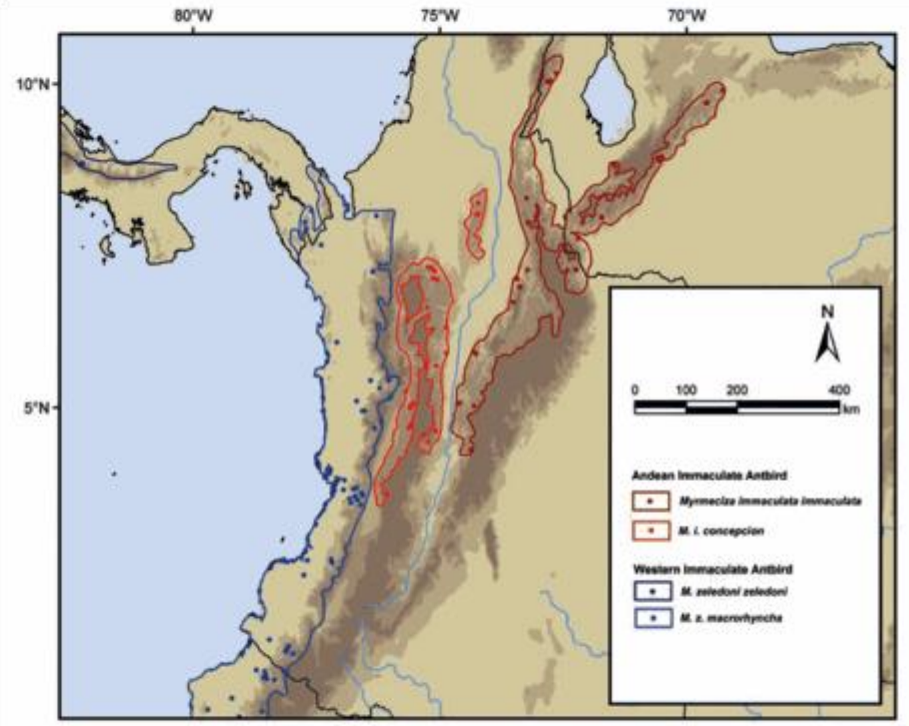

Figure 9. Map showing distribution of *M. immaculata* taxa in Colombia and surrounding regions, based on the new taxonomy established in this paper.

An opportunity is now presented to adopt different names, which some committee members indicated they would prefer. Some committee members also expressed a preference for re-naming *immaculata* as something else if *zeledoni* becomes Zeledon's. This proposal is split into various parts as follows:

A: Change name of *M. zeledoni* from "Western Immaculate-Antbird" to "Zeledon's Antbird".

B: Change name of "Andean Immaculate-Antbird" to something else. Options would include simple "Immaculate Antbird" or "Lafresnaye's Antbird". If there is any swell of support for these or another name or better idea, then this can be dealt with in a follow-up proposal or sub-proposal.

Thomas Donegan, November 2012

Comments from Remsen: “A. YES. B. YES. Anything to get rid of these awkward compound names is good, in my opinion. Although patronyms are not popular, I like them when they highlight the history of ornithology, and certainly when descriptive names are of minimal or no use.”

Comments from Robbins: “Given that there are no obvious good choices, I’ll support both A & B of Donegan’s proposal.”

Treat *Thalurania fannyi* and *Thalurania colombica* as conspecific

Description of the problem:

AOU-SACC Proposal 558 proposed to lump these Woodnymph species. This proposal is set out below.

New information:

Data presented in Donegan (2012), as discussed in AOU-SACC proposal 558.

Recommendation:

The name of the Green-crowned Woodnymph *T. fannyi* would change back again to Crowned Woodnymph *T. colombica*, undoing the split of Escalante-Pliego & Peterson (1992). Reference should be added to Donegan (2012).

Literature cited:

Donegan, T.M. 2012. Range extensions and other notes on the birds and conservation of the Serranía de San Lucas, an isolated mountain range in northern Colombia. *Bull Brit. Orn. Cl.* 132: 140-161.

Escalante-Pliego, P. & Peterson, A. T. 1992. Geographic variation and species limits in Middle American woodnymphs (*Thalurania*). *Wilson Bull*. 104: 205–219.

Submitted by: Thomas Donegan, Fundación ProAves.

Date of proposal: 25 Apr 2013

SACC Proposal 558: Re-lump *Thalurania fannyi* and *T. colombica* into Crowned Woodnymph *T. colombica*

Proposal: This proposal, if it passes, would result in *T. colombica* being removed from the AOU-SACC list and lumped, a prevailing treatment in publications before the 1990s.

Discussion:

The split of *fannyi* from *colombica* by Escalante-Pliego & Peterson (1992) was based on differences in crown coloration and under a phylogenetic species concept. Recent data shows that the distribution of *Thalurania* morphotypes in Colombia is a rather more complex matter than would have been evident from materials available in the mid-1990s. As hinted by some comments in proposal 137 (http://www.museum.lsu.edu/~Remsen/SACCprop137.html), this proposed split, which is currently accepted by SACC, requires reconsideration.

Donegan (2012) recently summarised the situation of these species in Colombia, the only country in which Escalante-Pliego & Peterson (1992)'s two species supposedly co-occur. [Localities mentioned in the first two sentences are from Serranía de San Lucas, an isolated massif north of the Central Andes. Anorí and Samaná are in the north of the adjacent main Central Andes range.]:

> ""**WOODNYMPHS** *Thalurania* sp.
> Trapped previously by Salaman *et al*. (2002a) at La Punta (5) and La Teta Resort (12) and
> others mist-netted at Santa Cecilia (11, 2†) where sound-recorded in the hand or on release (XC99512-13, 104416-17) and while foraging (XC104452). All males at Santa Cecilia had a spot of purple feathers slightly behind the centre of the otherwise green crown (Fig. 5). In this feature, they are intermediate between Purple-crowned Woodnymph *T. colombica* of the East Andes and Green-crowned Woodnymph *T. fannyi hypochlora* of the West Andes and adjacent lowlands, which were previously treated together as 'Crowned Woodnymph'. ICN specimens collected at Anorí similarly possess a small purple forehead spot. Escalante-Pliego & Peterson (1992) noted that '*One to several violet feathers at the rear edge of the forecrown are observed in most Panamanian specimens*'. To this should now be added specimens from San Lucas and the northern Central Andes (F. G. Stiles *in* Remsen *et al*. 2012, proposal 137). *T. fannyi* and *T. colombica* were split largely on the basis of crown coloration in males (Escalante-Pliego & Peterson 1992). In the latter study, purple-crowned birds were considered restricted to the Santa Marta Mountains and Central Andes of Colombia, with green-crowned birds in the West Andes and adjacent lowlands. We now know that purple-crowned birds occur in the East Andes (Donegan *et al*. 2007), with green-crowned birds in the West Andes and purple-and-green-crowned birds in the northern Central Andes and San Lucas. At río Samaná, Caldas (05°25'39"N, 75°01'07"W), purple-and-green crowned males also occur, as do males with almost no purple in the crown (M. Slaymaker in litt. 2012). Green-and-purple-crowned males from Panama are generally assigned to *T. f. fannyi*, with pure green-crowned *hypochlora* in the Colombian Chocó and West Andes. Treatment of *T. colombica* and *T. fannyi* as separate species requires revision in light of the known distribution of morphotypes in Colombia, identical female plumages and similar vocalisations throughout the Colombian Andes."

We have more recently seen the description (recently rejected by this committee) of *Thalurania nigricapilla* (see proposal 472: http://www.museum.lsu.edu/~Remsen/SACCprop472.html). The main distinguishing feature cited in the diagnosis section for the *nigricapilla* description was also in crown coloration. Most committee members accepted the proposition in the proposal that individual variation in this feature should be investigated further before recognising this species.

These two proposed woodnymph species' calls sound pretty similar (click on below links, then on "Sonograms", if these do not immediately come up on your browser):

http://www.xeno-canto.org/browse.php?query=Green-crowned Woodnymph (Thalurania fannyi) 8&pagenumber=&order=taxonomy&view=3

http://www.xeno-canto.org/browse.php?query=Violet-crowned Woodnymph (Thalurania colombica) 9&pagenumber=&order=taxonomy&view=3

Females are indistinguishable. In biometrics and plumage features other than adult male crown coloration, they are also similar, based on my data and experience from mist-netting populations in all three Andean cordilleras of Colombia. Mostly green-crowned birds now seem to be replaced by purple-crowned birds at some point in the mid-Central Andes (rather than this being between-Cordillera variation). Some "good" species show North/South distribution splits in the Central Andes, but they tend to be of higher elevation than *Thalurania*.

It bears note in terms of assessing this proposal that although Escalante-Pliego & Peterson (1992) was published some time ago, many birders active in Colombia were unaware of the split until relatively recently, due to near-universal use of Hilty & Brown (1986)'s field guide until the mid-2000s at least. The Restall and McMullan field guides split these, but they were published in 2006 and 2010/2011 respectively. The *status quo* versus novel nature of a split treatment is therefore somewhat moot in the country where the two occur together.

Now arguing against a change in treatment, one could adopt a similar viewpoint here to that of some committee members in proposal 173 (http://www.museum.lsu.edu/~Remsen/SACCprop173.html) on *Zimmerius chrysops*. There, new distributional data were thought by some not to be sufficient to show current species-level taxonomy to be misled, but just to demonstrate that previous assumptions as to the distributions of species required reconsidering. An approach against lumping these two species would regard the SACC baseline as only to be amended following a detailed peer-reviewed publication including a detailed vocal and/or molecular study specifically on topic, refuting the current treatment. No such study post- Escalante-Pliego & Peterson (1992) is available and a detailed molecular and vocal investigation would indeed be helpful and welcomed.

Although they were described very close together in time, the name *colombica* (Bourcier, 1843) apparently has priority over the name *fannyi* (Delattre & Bourcier 1846). The vernacular name of this hummingbird if lumped would revert to "Crowned Woodnymph". Vernacular names for this group were previously discussed in Proposal 303 (http://www.museum.lsu.edu/~Remsen/SACCprop303.html).

Finally, I'd like to stress that Escalante-Pliego & Peterson (1992)'s study includes a lot of extremely valuable data and analysis on this genus, especially as regards plumage variation and the Central American populations. The doubts expressed here as to their proposed treatment for the Colombian taxa are based in part on recent data not available to these authors and perhaps in part on a different approach to species limits. This proposal should therefore not be taken as a criticism of their work overall, which is a valuable and appreciated contribution to the ornithological literature.

References:

Donegan, T.M. 2012. Range extensions and other notes on the birds and conservation of the Serranía de San Lucas, an isolated mountain range in northern Colombia. *Bull Brit. Orn. Cl.* 132: 140-161.

Escalante-Pliego, P. & Peterson, A. T. 1992. Geographic variation and species limits in Middle American woodnymphs (*Thalurania*). Wilson Bull. 104: 205–219.

Other papers mentioned are cited in the above.

Thomas Donegan, October 2012

Comments from Stiles: "YES. Although I haven't really gotten serious (yet?) about putting all my *Thalurania* data together, I am firmly convinced that there is no solid basis for splitting *fannyi* from *colombica.* To begin with, I should note that the original separation by Peterson et al. involved a bit of circular reasoning: they started out with the question of whether the green- and purple-crowned male birds were separable in multivariate space, then concluded from a PCA that they were - but mostly on the basis of crown color! They effectively negated Zimmer's statement that blue feathering at the rear of the crown in *fannyi* decreased southwards from the zone of closest approach to *colombica*; in this I find that they were correct. However, the small blue feathers bordering the crown are not the feathers of interest here: Thomas (and I) refer to the larger, more brilliant display feathers of the crown itself, and among these occurs the mixture of green and purple feathers in the N end of the Cordillera Central (Antioquia; Andrés Cuervo specimens) and the Serranía de San Lucas (Bolívar); males of the Magdalena Valley and the Cordillera Oriental are purple-crowned. Thus, a considerable zone of intermediacy exists between *colombica* and *fannyi* based on male crown color. Even more convincing is the situation in females, in which geographic trends in coloration exist but do not support a split along the lines of male crown color. Had Peterson et al. looked at females (notably features like unicolored vs. bicolored grey underparts, blue vs. green shoulders, etc.) they might well have reached a different conclusion.

Comments from Remsen: "YES. I am convinced by Donegan's proposal and by Gary's comments."

Comments from Pacheco: “YES. Pelas mesmas razões acima.”

Comments from Nores: “YES. I find Donegan’s proposal very convincing and Gary’s comments very useful. By coloration and especially by distribution with a zone of hybridization it appears evident that they are subspecies.”

Comments from Robbins: “YES, based on both Thomas Donegan and Gary Stiles’s comments.”

Comments from Zimmer: “YES, for reasons stated by both Thomas and Gary.”

Comments from Pérez-Emán: “YES. Data presented by both Thomas Donegan and Gary show that there is no basis to consider these two taxa as different species. It would be interesting to study the potential hybridization zone at the northern end of the Cordillera Central of Colombia, a pattern that might be found to be similar in other codistributed taxa.”

Additional comment of the author in response to some of the AOU-SACC members' comments. It is not sensible to jump to the conclusion immediately that there is hybridisation occurring here. It is clear that (i) the ranges of purple-crowned and green-crowned birds in Colombia are much more complex than was set out in Escalante-Pliego & Peterson (1992), as discussed above; (ii) there are intermediate (green-and-purple crowned) birds in San Lucas and Anorí, between the supposed range of green versus purple crowned birds, but this seems to be a stable plumage state in the region; and (iii) female plumages and voices do not vary geographically consistent with male crown coloration. It remains to be seen whether the northern Central Andes population represents an undescribed subspecies with intermediate plumage features as opposed to a result of active hybridisation. Molecular studies could helpfully address this issue. Either way, based on this data and other consideration (vocal, female plumages), the split of these species no longer seems supported.