AOS Classification Committee – North and Middle America

Proposal Set 2025-B

21 February 2025

Treat *Camptostoma thyellophilum* as a separate species from Northern Beardless-Tyrannulet *C. imberbe*

Background:

The tyrant flycatcher genus *Camptostoma* currently includes two species: the Northern Beardless-Tyrannulet (*C. imberbe*), ranging from the southern United States to Costa Rica, and the more widespread Southern Beardless-Tyrannulet (*C. obsoletum*), found from Costa Rica to Uruguay. They have long been treated as separate species due to evidence of sympatry without hybridization in Costa Rica (Hellmayr 1927, Slud 1964, Stiles and Skutch 1989).

Two subspecies were described within *C. imberbe*, *C. i. ridgwayi* from Arizona and *C. i. thyellophilum* from Cozumel Island. Both were described based on minute average differences in size and plumage color observed in geographically and numerically limited samples of museum specimens, and both have been synonymized with *imberbe* in the contemporary literature since Fitzpatrick's (2004) overview of geographic plumage variation in the species.

New Information:

A recent analysis of geographic vocal variation in *Camptostoma* identified two vocally distinct populations within *C. imberbe*: one ranging from the southern United States through western Mexico to Costa Rica, and the other on the Yucatán Peninsula and Cozumel Island (Lima and Vaz 2024; Figs. 1, 2, 3). Lima and Vaz (2024) then reinstated *thyellophilum* as the available name for this vocally distinct population on the Yucatán Peninsula and Cozumel Island, redefining the taxon (both its diagnosis and range) based on vocalizations.

Importantly, the vocal differences between *imberbe* and *thyellophilum* are equivalent in magnitude to those between *imberbe* and *C. o. flaviventre* (Lima and Vaz 2024; Figs. 1, 2), which have long been considered separate species based on evidence of sympatry without hybridization in Costa Rica (Hellmayr 1927, Slud 1964, Stiles and Skutch 1989).

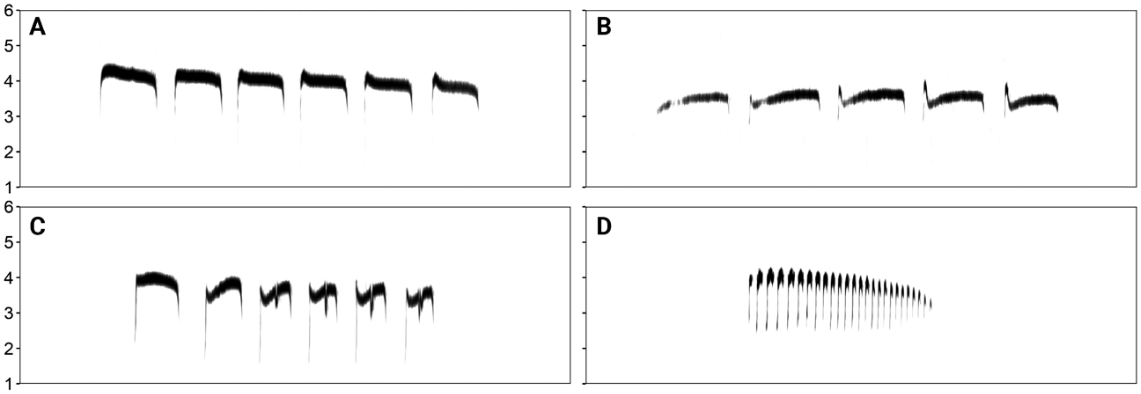


Figure 1. Typical daytime songs of (A) *Camptostoma i. imberbe* (Costa Rica), (B) *C. i. thyellophilum* (Belize), (C) *C. obsoletum flaviventre* (Panama), and (D) *C. o. caucae* (Colombia). From Lima and Vaz (2024).

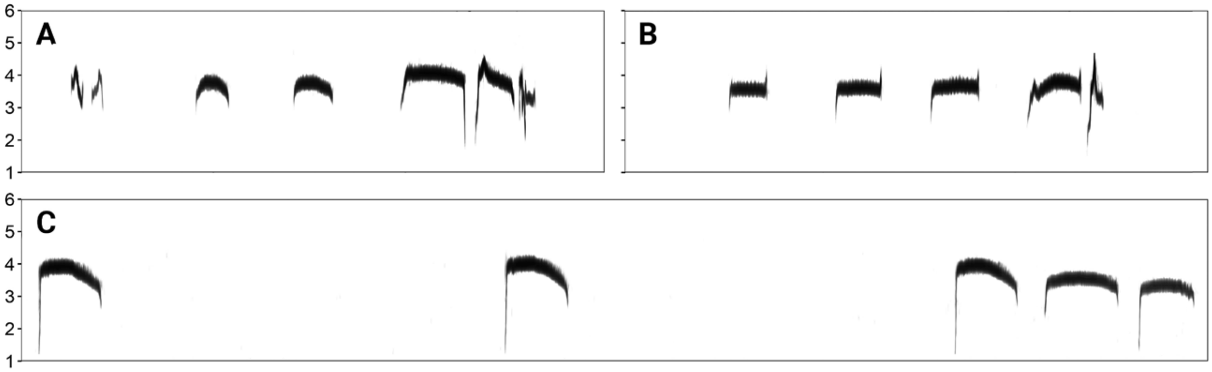


Figure 2. Typical dawn songs of (A) *Camptostoma i. imberbe* (Arizona), (B) *C. i. thyellophilum* (Mexico), and (C) *C. obsoletum flaviventre* (Costa Rica). From Lima and Vaz (2024).

Unfortunately, there was a 350-km sampling gap between the recordings of *imberbe* and *thyellophilum* (Fig. 3), precluding a characterization of their contact zone (Lima and Vaz 2024).

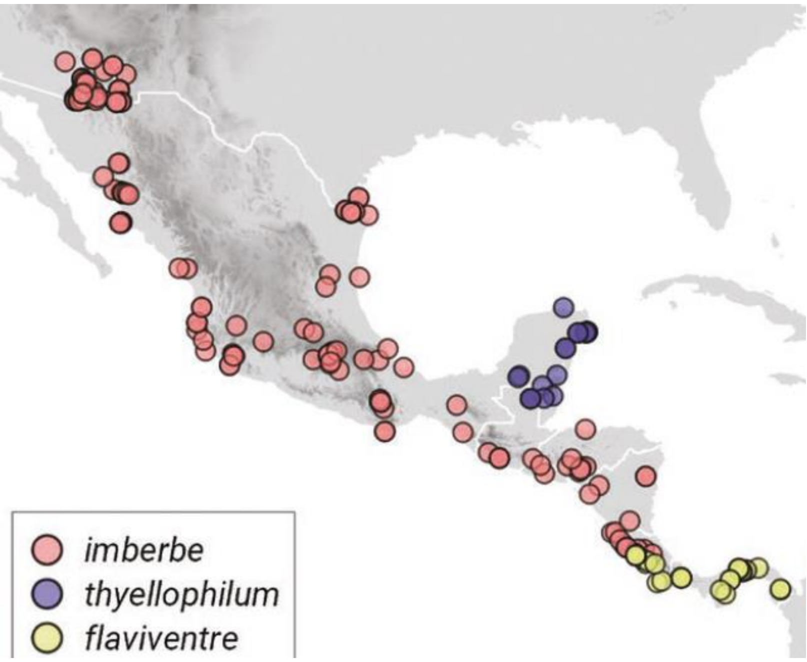


Figure 3. Sampling locations for vocalizations of *Camptostoma i. imberbe*, *C. i. thyellophilum*, and *C. obsoletum flaviventre*. From Lima and Vaz (2024).

According to Fitzpatrick (2004), putative average differences in wing-bar color, rump color, and bill length between *imberbe* and *thyellophilum* (Parkes and Phillips 1999) do not hold water. Although a thorough analysis of geographic variation in plumage and morphometrics might be desirable, any potential differences in these traits are unlikely to be of any relevance in defining biological species limits within this group with conservative plumage and morphology, where vocal differences typically function as the primary basis for species recognition (Lanyon 1963, Stein 1963).

There is little genetic data available for *Camptostoma* tyrannulets in general. One sample of *imberbe* from Nicaragua and one of *thyellophilum* from Campeche, Mexico, were included the latest suboscine phylogeny using genome-wide nuclear markers (Harvey et al. 2020; Fig. 4). Divergence time estimates based on these data suggest that *imberbe* and *thyellophilum* diverged about 2 Mya (Harvey et al. 2020), meaning there is a considerable amount of nuclear divergence between the two compared to many species-level taxa in the Tyrannidae. All this nuclear genetic divergence may plausibly translate into some postzygotic genetic incompatibilities between the two lineages.

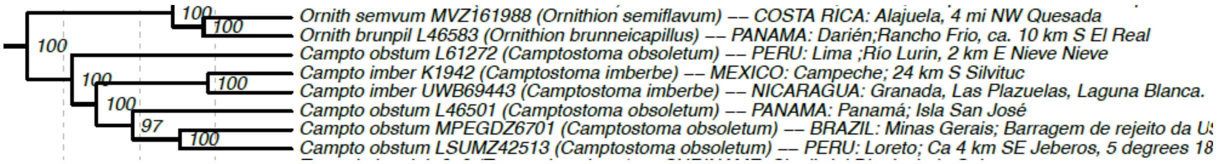


Figure 4. Excerpt from the phylogenetic tree of Harvey et al. (2020), showing relationships within *Camptostoma* and *Ornithion*. Samples of *C. i. imberbe* from Nicaragua and *C. i. thyellophilum* from Mexico are located in the middle of the tree. The vertical dotted line nearest to the species names represents an estimated 2 my of divergence. *Camptostoma obsoletum*, shown here to be polyphyletic, has been proposed to consist of six species (Lima and Vaz 2024; SACC Proposal 1039)

Recommendation:

Assuming that the vocal and genetic differences between them can generate substantial premating and some postzygotic isolation, Lima and Vaz (2024) suggested that *imberbe* and *thyellophilum* are best treated as separate species. Although further sampling of their contact zone is clearly needed to determine the strength of their reproductive isolation, I consider the currently available evidence to be more consistent with a two-species treatment and therefore recommend a YES vote to treat *imberbe* and *thyellophilum* as separate species.

References:

Fitzpatrick, J. W. (2004). Family Tyrannidae (Tyrant-flycatchers). In Handbook of the Birds of the World (J. del Hoyo, A. Elliott and D. Christie, Editors). Lynx Edicions, pp. 170–462.

Harvey, M. G., et al. (2020). The evolution of a tropical biodiversity hotspot. Science 370: 1343–1348.

Hellmayr, C. E. (1927). Catalogue of Birds of the Americas and the Adjacent Islands in Field Museum of Natural History. Volume XIII, Part V. Field Museum Press, Chicago, IL, USA.

Lanyon, W. E. (1963). Experiments on species discrimination in *Myiarchus* flycatchers. American Museum Novitates 2126: 1–16.

Lima, R. D., and R. V. Vaz (2024). Divergence in vocalizations indicates cryptic speciation in *Camptostoma* tyrannulets. Ornithology: ukae058.

Parkes, K. C., and A. R. Phillips (1999). A new subspecies of the Northern Beardless Tyrannulet *Camptostoma imberbe*. Bulletin of the British Ornithologists' Club 119: 59–62.

Slud, P. (1964). The birds of Costa Rica: Distribution and ecology. Bulletin of the American Museum of Natural History 128:1–430.
Stein, R. C. (1963). Isolating mechanisms between populations of Traill's Flycatchers. Proceedings of the American Philosophical Society 107: 21–50.
Stiles, F. G., and A. F. Skutch (1989). A Guide to the Birds of Costa Rica. Cornell University Press, Ithaca, NY, USA.

Submitted by: Rafael D. Lima

Date of Proposal: 7 January 2025

Treat Bran-colored Flycatcher *Myiophobus fasciatus* as three species

Note: This proposal is a modification of SACC proposals 963 and 985. Proposal 963 (species split) passed unanimously (9-0), and the three parts of Proposal 985 (English names) passed 10-0, 10-0, and 6-4. The taxonomic proposal is presented first below, followed by a version of the English names proposal modified for NACC. Comments from SACC on both the taxonomic and English name proposals are placed at the end of the English names proposal.

Background:

The SACC notes read:

> The subspecies *rufescens* of arid western Peru and northern Chile was formerly (e.g., Cory & Hellmayr 1927) considered a separate species from *Myiophobus fasciatus*, but Zimmer (1939c) and Koepcke (1961) reported specimens that showed signs of intergradation between *rufescens* and *M. f. crypterythrus* (cf. Ridgely & Tudor 1994); thus, Meyer de Schauensee (1966) considered them conspecific, and this has been followed by subsequent authors. Jaramillo (2003), however, suggested that *rufescens* should be considered a separate species.

Vocally, the three groups (*fasciatus*, *crypterythrus*, and *rufescens*) are quite distinct, and these distinctions match the marked plumage differences described in the literature. It is remarkable how perfectly similar the calls and songs of birds in the *fasciatus* group are, even when they cover southern Central America and most South America. It is not clear how many of the northern individuals would represent southern migrants (populations in Argentina are complete migrants), but even then, birds giving the full dawn song in southern Central and northern South America sound like those at the southernmost portions of the range. The evidence for the continuity of this vocal time is overwhelming, and contrasts with the clear change to vocalizations of *crypterythrus* and *rufescens*.

For an overview of vocalizations see here:

fasciatus: https://birdsoftheworld.org/bow/species/brcfly1/cur/sounds
crypterythrus: https://birdsoftheworld.org/bow/species/brcfly4/cur/sounds
rufescens: https://birdsoftheworld.org/bow/species/brcfly3/cur/sounds

You will immediately notice that it is easy to track homologous dawn song and diurnal call (or song) between *fasciatus* and *crypterythrus* while at the same time recognizing the obvious differences. The vocalizations of *rufescens* are totally different and have some reminiscence to some *Ochthoeca* voices and duets.

There are two literature reports of intergradation to be dealt with.

Zimmer (1939: page 6) reported a specimen from Pacasmayo, northern La Libertad, Peru that he considered to be "nicely intermediate between *crypterythrus* and *rufescens*, being too pale beneath for typical *rufescens* and too buffy for *crypterythrus*, with pectoral streaking also intermediate. It furnishes additional evidence that the two forms are conspecific". The specimen in question is this one (photos by Paul Sweet):

The specimen is in terrible shape, making it difficult to evaluate in terms of its characters. Beyond that, one specimen, even if an intergrade, does not tell us much. Zimmer, of course a man of his times, did not think in terms of hybrid zones or the incidence of hybridization, but we definitely would take that into account. As for the incidence of introgression, Zimmer also reports specimens of apparently typical *rufescens* in AMNH from Trembladera [= Tembladera], Cajamarca, which is upstream from Pacasmayo; and as Zimmer was aware (he cites these records, without comment), *rufescens* has been collected at two other sites, Paucal and Guadalupe, both in Cajamarca and both a short distance northeast of Cajamarca. So his single, reported intergrade is south of the 'front line' of wherever *rufescens* and *crypterythrus* would be expected to come into contact, and even if an intergrade (which is not clear at all by looking at this specimen), there is no other specimen like it from the same region. Furthermore, there's no indication that Zimmer noticed anything odd or unusual about the geographic distribution of these specimens.

Koepcke (1961: pages 17-18) reported two apparent intergrades from Yantán, Ancash. This is a site almost 300 km (!) south of Zimmer's reported intergrade, and of course there are many specimens of accepted *rufescens* from throughout that intervening area. These two specimens are in Lima, but we have not seen them. One has to wonder if there is some plumage of *rufescens* (juvenile or formative plumage?) that Zimmer, Koepcke, or both were unfamiliar with, and that simply confused them. Certainly it makes no great sense to have uncovered instances of 'introgression' between resident populations at sites that are distant (Zimmer) to very, very distant (Koepcke) from any contact zone.

So where would we expect *crypterythrus* and *rufescens* to meet? The southernmost specimens of *crypterythrus* from west of the Andes that we aware of are from near Taulis, Cajamarca (specimens in Lima, not seen); and the northernmost specimen of *rufescens* we know of is from near Chiclayo, Lambayeque (MVZ, Berkeley; again, not seen). These two localities are at about the same latitude, but Taulis is further inland. Anyway, the turnover between these two taxa must be pretty abrupt, in view of the absence of specimens or sight records of intergrades from this area.

New information:

Genetic data add little to the case. Harvey et al. (2020; see the excerpt from the phylogenetic tree on the next page) included a single sample of *fasciatus* that was sister to *cryptoxanthus* (no samples of *crypterythrus* or *rufescens* included). Note that *Myiophobus* is paraphyletic (type species *fasciatus*).

Recommendation: We recommend the recognition of 3 species, as follows:

1. *Myiophobus fasciatus* (including Central American *furfurosus* and all South American subspecies east of Andes)
2. *Myiophobus crypterythrus* (monotypic; SW Colombia in Nariño south to NW Peru in Piura and Marañon Valley)
3. *Myiophobus rufescens* (monotypic: NW Peru in Lambayeque south to N Chile)

In the SACC voting, a YES vote was for recognizing three species, whereas a NO vote was for either retaining broad *M. fasciatus* or for splitting just *rufescens* (leaving *crypterythrus* with *M. fasciatus* until additional data are published). We'll adopt the same arrangement for NACC.

Thus, vote YES for recognizing the three species specified above, or NO for recognizing one (*M. fasciatus*) or two (*M. fasciatus* and *M. rufescens*) species. If voting NO, specify whether you prefer one or two species. (SACC voted unanimously for three species.)

Excerpt from phylogenetic tree of Harvey et al. (2020):

References:

Cory, C. B., and C. E. Hellmayr. 1927. Catalogue of birds of the Americas. Field Museum Nat. History Publ., Zool. Ser., vol. 13, pt. 5.

Harvey, M. G., G. A. Bravo, S. Claramunt, A. M. Cuervo, G. E. Derryberry, J. Battilana, G. F. Seeholzer, J. S. McKay, B. C. Faircloth, S. V. Edwards, J. Pérez-Emán, R. G. Moyle, F. H. Sheldon, A. Aleixo, B. T. Smith, R. T. Chesser, L. F. Silveira, J. Cracraft, R. T. Brumfield, and E. P. Derryberry. The evolution of a tropical biodiversity hotspot. Science 370: 1343-1348.

Jaramillo, A. 2003. Birds of Chile. Princeton University Press, Princeton, New Jersey.

Koepcke, M. 1961. Birds of the western slope of the Andes of Peru. American Museum Novitates 2028: 1–31.

Meyer de Schauensee, R. 1966. The species of birds of South America and their distribution. Livingston Publishing Co., Narberth, Pennsylvania.

Ridgely, R. S., and G. Tudor 1994. The birds of South America, Vol. 2. University Texas Press, Austin.

Zimmer, J. T. 1939. Studies of Peruvian birds, No. 31. Notes on the genera *Myiotriccus*, *Pyrrhomyias*, *Myiophobus*, *Onychorhynchus*, *Platyrinchus*, *Cnipodectes*, *Sayornis*, and *Nuttallornis*. American Museum Novitates 1043: 1-15.

Submitted by: J. I. Areta & T. S. Schulenberg,

Date of Proposal: February 2023, modified by R. T. Chesser on 21 January 2025

English Names for NACC:

If the proposal to modify species limits in this group passes, then new English names are needed for the component species. Actually, one of them, *M. fasciatus*, the only one that occurs in the NACC area, does not need to change because of strong asymmetry in the distribution and familiarity – see SACC guidelines for English names, Section C2, which were based on the guidelines adopted by NACC.

Below is the SACC proposal followed by votes and comments. The voting results are online here. A and C passed unanimously 10-0. B passed 6-4. As a result, SACC names are now:

M. fasciatus: Bran-colored Flycatcher
M. *crypterythrus*: Mouse-gray Flycatcher (extralimital)
M. *rufescens:* Rufescent Flycatcher (extralimital)

Naturally, I recommend NACC follow SACC on this one. Certainly, retaining Bran-colored for the species in the NACC area should not be controversial.

Submitted by: Van Remsen

Date of Proposal: 7 January 2025

SACC Proposal 985

Establish English names for (A) *Myiophobus fasciatus*, (B) *M. crypterythrus*, and (C) *M. rufescens*

With passage of SACC Proposal 963 (Split *Myiophobus fasciatus* into three species), we need to formally establish English names for the three newly recognized species. The only mention of English names in the proposal was a note that I appended:

"Note from Remsen on English names: if this passes, a separate proposal would be needed, with the starting point presumably the names already adopted in the BLI/BOW accounts, e.g. retaining Bran-colored Flycatcher for widespread *M. fasciatus*, and adopting Mouse-gray Flycatcher for *M. crypterythrus* and Rufescent Flycatcher for *M. rufescens*."

Del Hoyo and Collar (2016) ["BLI"] treated the three as separate species, and introduced the use of the three English above. Note that Birds of the World/Clements/eBird already instituted this spilt without SACC or NACC having considered the taxonomic split, much less the English names.

A. *M. fasciatus*

BLI retained “Bran-colored” for narrowly defined *M. fasciatus*, and this is consistent with AOS/NACC guidelines on the English names when one of the daughters has a vastly larger distribution and associated literature than do the other daughters (rather than coin new names for all the daughters.). Whether any of us could reliably identify the color “bran” remains to be seen, but that’s the long-standing, memorable name.

- A YES vote on means you favor Bran-colored, which we recommend. A NO vote is for something else (and will elaborate).

B. *M. crypterythrus*

The derivation of “Mouse-gray” for *M. crypterythrus* is not so clear. We assume this was a BLI-created name. Ridgely and Greenfield (2001) mentioned *crypterythrus* as having “dull grayish-brown upperparts” with respect to the cis-Andean nominate *fasciatus*, but because separate species status was not anticipated at that time, no English name was suggested. Indeed, it is a dull grayish brown species, but so are many other small tyrannids. Given the nearly impossible task of creating unique plumage-based names for the 100+ small tyrannids, we don’t think it’s worth exploring whether *crypterythrus* is indeed actually “mouse-gray” or how many other small tyrannids to which that name might apply. This name is catchy and memorable, reasonably descriptive, has a 7-8 year track record of use in BLI and about a year of use from the bully-pulpit of eBird.

One of us (Alvaro) thought of a name that we regard as better than Mouse-gray and is at least a viable competitor. Cory and Hellmayr (1927) treated *crypterythrus* as a subspecies of *M. fasciatus* and called it “Western Banded Flycatcher”; they used “Banded Flycatcher” for nominate *fasciatus*, and “Something Banded Flycatcher” for 3 of the other 4 subspecies. In the field and in specimens, the cinnamon wing bars, the “bands” in Cory and Hellmayr’s name, are distinctive and contrast well with the dull body coloration. As such, adjusting Cory and Hellmayr’s name to Cinnamon-banded Flycatcher creates a name that is both distinctive and useful in the field; and it provides a tenuous link to older literature. Although it is obviously not the only tyrannid with cinnamon wing bars, within the Bran-colored complex, those wing bars do contrast strongly on this species, more so than in *rufescens* for example, and more so than in most gray-brown tyrannids. (See the next page for a couple of photos of *crypterythrus* from Macaulay.)

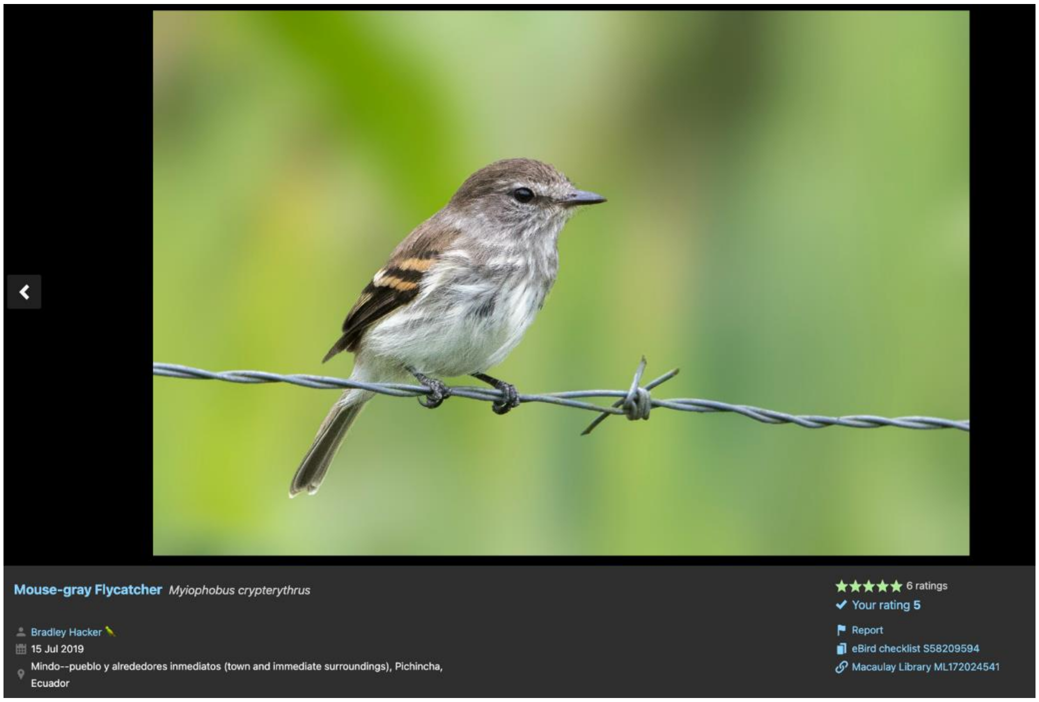
- For voting on this one, it’s a little awkward to maintain a Y/N system, but let’s try it, with YES meaning you favor sticking with Mouse-gray, and NO meaning that you like Cinnamon-banded better. We recommend the latter.

C. *M. rufescens*

The derivation of “Rufescent Flycatcher” for *M. rufescens* is also straightforward. Although “Rufescent Flycatcher” could apply equally well or better to many other tyrannids, this is the English name used by Cory and Hellmayr (1927), who treated it as a separate species, and so “Rufescent” provides continuity with earlier literature and with the species epithet.

- A YES vote on means you favor Rufescent, which we recommend. A NO vote is for something else (and will elaborate).

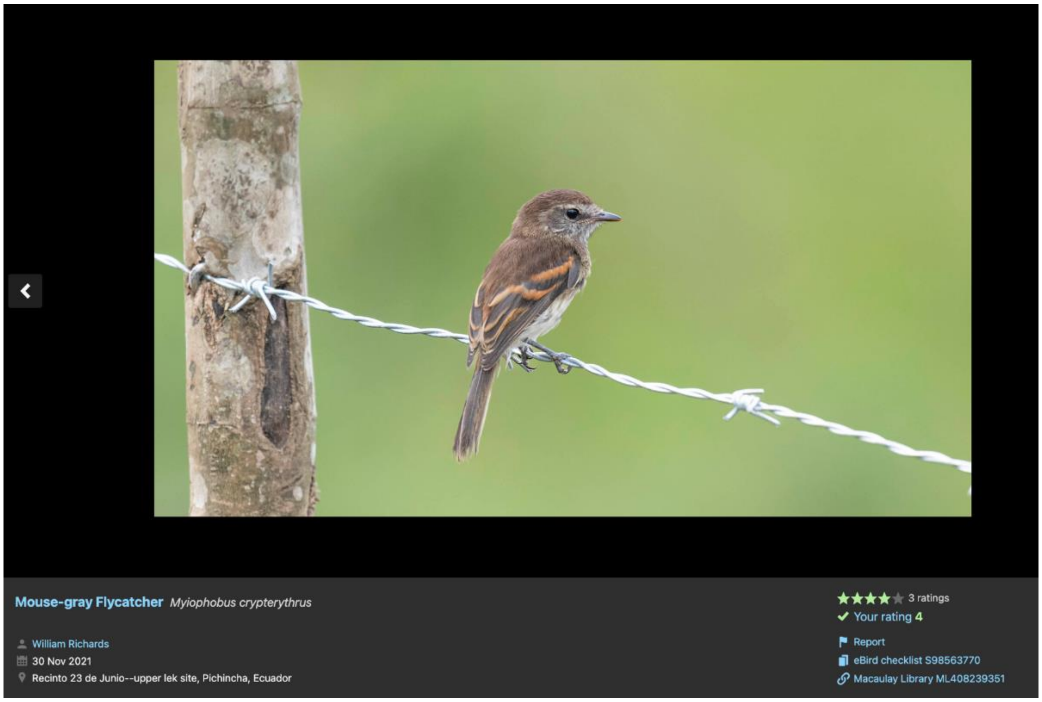
Alvaro Jaramillo and Van Remsen, September 2023



Mouse-gray Flycatcher *Myiophobus crypterythrus*

Bradley Hacker
15 Jul 2019
Mindo--pueblo y alrededores inmediatos (town and immediate surroundings), Pichincha, Ecuador

★★★★★ 6 ratings
✓ Your rating 5
Report
eBird checklist S58209594
Macaulay Library ML172024541



Mouse-gray Flycatcher *Myiophobus crypterythrus*

William Richards
30 Nov 2021
Recinto 23 de Junio--upper lek site, Pichincha, Ecuador

★★★★☆ 3 ratings
✓ Your rating 4
Report
eBird checklist S98563770
Macaulay Library ML408239351

SACC comments on Proposal 963 (species split):

Comments from Lane: "YES to recognizing 3 species (*M. fasciatus*, *M. crypterythrus*, *and* M. *rufescens*). Fernando Angulo tells me that *M. crypterythrus* and *M. rufescens* overlap broadly between La Libertad and Lambayeque depts in Peru, with the former occurring in the deciduous thorn scrub of the foothills and the latter fairly strictly in the river valleys that pass through (generally more coastal than the former). That there is no obvious interbreeding given this broad overlap, it seems clear that Zimmer misinterpreted the specimen that has stymied this taxonomic issue for so long. However, I will note that I was surprised to hear *M. crypterythrus* giving vocalizations very similar to typical vocalizations of *M. rufescens* (https://macaulaylibrary.org/asset/495506591), suggesting that such vocalizations are within the repertoire of the latter, but seem not to be used regularly. By contrast, I am unaware of the "standard trill" of most *Myiophobus* appearing in the repertoire of *M. rufescens*."

Comments from Stiles: "A- split *rufescens* from the rest- this at least seems clear so YES; B- recognize *crypterythrus* as separate- YES, another example of a split of a Pacific form of extreme SW Colombia and Ecuador; C- further splits? NO at this point, though if there are various sspp. of *fasciatus* in E South America, their relationships to the Mesoamerican *furfurosus* should be looked into."

Comments from Zimmer: "YES for treating *rufescens* and *crypterythrus* as separate species from C American and cis-Andean *fasciatus.* As stated in the proposal, the dawn songs, diurnal songs, and rattle calls of *crypterythrus* are similar enough to the homologous vocalizations of cis-Andean and C American *fasciatus* that it is easy to recognize each vocalization as a homolog, but are distinct enough to suggest separate species status, whereas the rattles, single-notes, and duet vocalizations of *rufescens* are qualitatively very different to my ears. Plumage characters, particularly of *rufescens*, are also quite different, and I am unimpressed by the alleged "nicely intermediate" specimen from Pacasmayo – it is in such poor condition that it is difficult to make any concrete assertions regarding intermediacy. The clincher is the information passed by Fernando Angulo to Dan regarding the broad overlap in range, coupled with habitat segregation, of *crypterythrus* and *rufescens* in La Libertad and Lambayeque depts., Peru, without evidence of interbreeding or intergradation."

Comments from Claramunt: "YES. It would have been nice to see some modern and more detailed analysis of geographic variation, but the plumage and song differences seem well demarcated. This complex is clearly another victim of "the big lumpings" of the mid-XX century."

Comments from Niels Krabbe (voting for Remsen): "YES. I clearly must vote yes to ranking *rufescens* as a species for the reasons given by Dan (with Fernando) and Kevin. However, the vocal differences between *crypterythrus* and the *fasciatus* group are much less convincing. As described in Brds Wrld, *crypterythrus* is much higher pitched and has longer notes, but those are only two differences. The wider frequency span (bandwidth) is a natural consequence of the higher pitch and the slower pace is correlated with the longer notes. However, to my ear the dawn song of the *fasciatus* group is also louder and of a different, almost oscine-like quality that I am not sure how to quantify, so I will go along with three species."

Comments from Robbins: "YES. Although like proposal 961 (*Tolmomyias viridiceps* split) there is no published evaluation of the *Myiophobus fasciatus* complex, I believe information provided in the proposal supports the recognition of three species. The primary song of *fasciatus* and *crypterythrus* appears quite similar, with the latter's higher in frequency. At first listen, I thought the vocalizations of *rufescens* were quite distinct from the other two, until I listened to Dan's recording of *crypterythrus* that sounds quite similar to *rufescens*. However, given what Dan relates from Fernando Angulo that *crypterythrus* and *rufescens* apparently overlap without interbreeding over a relatively broad area, I'm swayed to vote Yes for the recognition of three species. Clearly, an in-depth study is needed in the area that Angulo has identified, but until then, I believe it is better to recognize three species."

Comments from Bonaccorso: "YES. Plumage and song differences (at least those available) support the split. I am a little uncomfortable about the lack of good genetic sampling, but I guess the evidence available is sufficient."

SACC comments on Proposal 985 (English names):

Comments from David Donsker (voting for Bonaccorso):
"*M. fasciatus*: YES. Bran-colored Flycatcher
"*M. crypterythrus*: NO to Mouse-gray Flycatcher. YES for Cinnamon-banded Flycatcher. A superior name in many respects. I like the link to the Cory and Hellmayr name but with a more evocative revision. Although "Mouse-gray" is used by HBW/BLI, eBird/Clements and the iOC WBL, it don't think it's all that deeply established to insist on retaining it.
"*M. rufescens*: YES. Rufescent Flycatcher."

Comments from Rasmussen (voting for Robbins): "I vote for the first two BLI names. I like Alvaro's suggestion, although some of them (including one I photographed last year) don't show cinnamon on the wingbars; maybe it's fresh plumage only? But Mouse-gray seems misleading to me, and it hardly has any gray at all, ever, so just seems meaningless at best and likely to cause people to expect something that looks quite different. Mice come in many shades of gray, but this bird isn't gray!

Comments From Steve Hilty (voting for Areta):
"*M. fasciatus*: YES, keep Bran-colored Flycatcher for *Myiophobus fasciatus.*
"*M. crypterythrus*: NO. Use Mouse-gray for *M. crypterythrus;* I am familiar with bird in the field, and this pretty much sums up the color, and it is memorable.
"*M. rufescens*: YES. Rufescent Flycatcher is fine, and in agreement with sci. name—however, there is a Rufous Flycatcher (*Myiarchus*) in Peru, and someone will probably complain about these two names (Rufescent and Rufous) being too close. I don't think it's a problem. If we can live with Black and Blackish, and a half dozen or more other black "something-or-other" (antbirds) I don't see why there is a problem with these flycatchers."

Comments from Gary Rosenberg (voting for Claramunt):
"YES to retaining Bran-colored Flycatcher for *M. fasciatus*. This makes sense to retain a long-used name that everyone is familiar with. I am always in favor of retaining one name for a split if

possible - make it easier to learn the new ones - and refer it back to the more widespread form. I call it Bran-flavored Flycatcher in the field.

“NO to Mouse-gray. I much prefer Cinnamon-banded. Mouse-gray is a misleading name - and sort of is confusing with Mouse-colored Tyrannulet - that is also split and one of the new forms of that (or two) is (are) sympatric with *crypterythrus* - but the main reason is that the bird is not really “mouse-gray,” and the wing-bars are much more of a prominent feature - thus “cinnamon-banded” seems reasonable. I do worry a little about confusion with Orange-banded Flycatcher - which, of course, is in a different genus now.

“YES to Rufescent for *rufescens.* I am not worried about confusion with Rufous Flycatcher.”

Comments from Josh Beck (voting for Del-Rio): “Vote: YES/NO/YES. I actually don’t think Mouse-gray is theoretically that bad a name; as others point out the wing bands are not always cinnamon. However, Mouse-gray invites too much confusion with Mouse-colored, so if there is still room to adjust the name it would not be a bad idea, and Cinnamon-banded works well for this species.”

Comments from Stiles: “I have been looking over this one; the names for *fasciatus* and *rufescens* have pretty general approval, but insofar as votes go, there is a stalemate in buff-or rufous-banded and mouse-gray for *crypterythrus*. I note that Pam Rasmussen has seen examples of this taxon with bright buff wingbars and others with these white or nearly so, and i have seen this variation as well in several I have netted and measured in Nariño. So, it seems worth noting that a very common pattern in many tyrannids is for the juveniles to have brighter, more yellowish to buff to rusty wing markings that in adults are paler, more whitish - raising the possibility that the very rufous-barred individuals in the photos shown might simply be young juveniles? In randomly looking about for a way to break the stalemate between mouse-gray and rufous-barred, I chanced to look through the Smithe color guide and found a color swatch that comes pretty close to the color of the dorsum of *crypterythrus*: Drab! So perhaps with a modifier to adjust the match more precisely, a potential E-name could be "Brownish-drab Flycatcher"..(or grayish-drab if this seems better to some). Either way, it captures the overall nondescript, dingy color of this race - so perhaps worth running this possibility up the flagpole to see if people salute it or shoot it?”

Additional comments from Rasmussen: “Gary makes good points, but I don't think Brownish-drab is an improvement, as it could apply to so many flycatchers. Even though Cinnamon-banded only applies to some individuals, at least for those it is distinctive, which is more than can be said for Brownish-drab. It is also a much more pleasing name, so I'll continue to vote for Cinnamon-banded.”

Comments from Zimmer:

“A) *M. fasciatus*. “YES to retaining “Bran-colored Flycatcher” for the more narrowly defined, but still, much more widely distributed *M. fasciatus*. In my opinion, this is a classic case for retaining a well-established and memorable name for one of the daughters in a split, when that daughter’s distribution, familiarity to the ornithological/birding community and literature “footprint” is vastly greater than that of the other daughter.

“B) *M. crypterythrus*. “NO, I prefer Cinnamon-banded Flycatcher. If anything, *crypterythrus* is more “Mouse-brown” than “Mouse-gray”, and either way, is difficult to define, because mice come in all shades of brown and gray, as demonstrated by the fact that we already have a Mouse-colored Tyrannulet and a Mouse-colored Antshrike, and the two are VERY different in color!

“C) *M. rufescens*. “YES to Rufescent Flycatcher.”

Comments from Lane:
“A) YES to retaining "Bran-colored" for *M. fasciatus*.
“B) NO to "Mouse-gray" for *M. crypterythrus* (it's not gray!). Honestly, I'm not enamored of any of the proposed names here. "Cinnamon-banded" draws much attention to the wing-bars (or worse: suggests that there is a cinnamon band on the body plumage--I'd associate "band" with a body feature before I'd think "wing-band" unless "wing" was specifically mentioned) which in my experience aren't really that attention-grabbing, and are often washed out buff or off-white. I'd prefer something that suggested muted colors (paralleling "Bran-colored") or maybe the habitat. Something like "Mud-colored Flycatcher," "Clay-colored Flycatcher," or perhaps "Thicket Flycatcher" or "Thorn-scrub Flycatcher." Just throwing out a few random ideas here.
“C) YES to "Rufescent" for *M. rufescens*.

Additional comments from Josh Beck: “Despite initially voting for “Cinnamon-banded” for *crypterythrus*, I increasingly dislike the name. It’s not appropriate for many individuals, and I think a better name can and should be found, or it should be left as Mouse-gray. This case shouldn’t be as hard as the recent Becard names difficulties.

“I think Dan’s suggestion of Thicket Flycatcher has real merit. These are not terribly hard birds to ID. They are vocal and perch up frequently and are in open habitat, so instead of looking for a physical descriptor, perhaps we can find something good in habitat, range, or voice. Otherwise I would actually just as soon leave it with Mouse-gray.

“For coining a new name, Thicket Flycatcher honestly works pretty well. Ecuadorian, though an oft used name, works (i.e., the range is pretty similar to Ecuadorian Ground-Dove). Tumbes could work though there’s a newly minted Tumbes Tyrannulet, so if we don’t like Mouse-gray because of confusion, Tumbes is perhaps better avoided. Equatorial is a thought. Hopefully others will have thoughts or inspiration here.”

Comments from Barry Walker: “This is something close to home for me birds I have been watching and recording for 40 years. I feel:

“Maintaining Bran-colored for *fasciatus* is logical and descriptive - don’t fix it if it isn’t broken

“Maintaining Rufescent for *rufescens* is a no-brainer - we have been unofficially calling it that for years, its descriptive and I don't think it’s an issue with the English name being close to *Myiarchus semirufus.*

“M crypterythrus is a bit more tricky - lots of Flycatchers inhabit thickets and other suggestions just don’t make the grade for me , especially Cinnamon-banded Flycatcher as many do not have cinnamon bars but white or off white (especially in the Marañon Valley?) - it would be such a wrong name. Mice come in different shades, but some do come in the shade of *crypterythrus* thus for lack of a viable alternative I would be inclined to stick with status quo of Mouse-gray which we are familiar with.

Additional comments from Remsen: “Based on the comments from Steve Hilty, Barry Walker, and others, I think Mouse-gray is slightly better, if only being in current use, albeit briefly, so I change my vote to YES in favor of retaining Mouse-gray.”

Additional comments from Donsker: “Thanks for sending along Barry’s comments. Please change my vote to Mouse-gray for *M. crypterythrus*.”

Additional comments from Zimmer: “Given comments by Barry, Steve, and Gary regarding many examples of *M. crypterythrus* with whitish rather than cinnamon wing bars, I’m fine with sticking with “Mouse-gray Flycatcher”, even though I don’t find it especially evocative. So please change my vote on this one to YES.”

Comments from Stiles: “No problem with names for *fasciatus* and *rufescens*, and for *crypterythrus*, I definitely prefer Mouse-gray (my initial doubt here was whether the Mouse-X should be restricted to the original Mouse-X species, *murinus*, but that seems not to be a problem). I dislike “Band-winged” because all specimens have wing-bars, but the emphasis on those with bright buffy-to rufous bands for this name leads me to suspect that this would be basing the name on a juvenile plumage, which I rather consider unwise.”

Treat Mouse-colored Tyrannulet *Nesotriccus murinus* as more than one species

Note: This is a modification of SACC proposals 956 and 987. Options 1 and 2 of proposal 956 passed 7-3, Option 3 was rejected 4-6, and Option 4 was rejected unanimously (0-10). Proposal 987 passed unanimously (8-0). In this NACC proposal, Parts A-D below correspond to Options 1-4 of SACC Proposal 956 (Option 5 of the SACC proposal was to subsume *Phaeomyias* into *Nesotriccus*, which NACC has already done). Part E (English names) corresponds to SACC Proposal 987. The taxonomic proposals are presented first below, followed by a version of the English names proposal modified for NACC. Comments from SACC on both the taxonomic and English name proposals are placed at the end of the English names proposal.

Background:

The old SACC note for this species, then placed in *Phaeomyias*, read:

> Ridgely & Tudor (1994) noted that vocal differences suggest that *Phaeomyias murina* might consist of more than one species. Ridgely & Greenfield (2001) considered the subspecies *tumbezana* (with *inflava* and *maranonica*) of southwestern Ecuador and northwestern Peru to represent a separate species based on differences in vocalizations. Rheindt et al. (2008c) found genetic evidence consistent with two species, and Zucker et al. (2016) found additional evidence for multiple species within *P. murina*. SACC proposal badly needed.

New Information:

Zucker et al. (2016) provided key material to discuss species limits in *Nesotriccus murinus*. What they found is that the Cocos Flycatcher, *Nesotriccus ridgwayi,* is embedded within *N. murinus* and that the latter includes 4-5 distinct clades that might merit species status. Harvey et al. (2020) recovered a similar phylogenetic structure. Lanyon (1984) had already shown that *Phaeomyias* and *Nesotriccus* were closely related, and Rheindt et al. (2008) also provided phylogenetic information to begin to unfurl this riddle. This is a good example of why "express" splits should not be done even if obvious and why deep studies are needed: there are surprises hidden everywhere.

As I see it, we have the following options:

A) Split *N. tumbezanus* (with ssp. *inflavus*): *tumbezanus/inflavus* and *maranonicus* are narrowly parapatric, are deeply diverged and differ in vocalizations. Zucker et al. (2016: 300) wrote:

> Lowland *tumbezanus* and montane populations matching *maranonicus* in plumage, voice, and mitochondrial DNA occur within about 10 km of each other on the lower slopes of the western Andes, where they appear to segregate by habitat and elevation (Angulo et al., 2012; Schmitt et al., 2013; F. Angulo P., D. Lane, pers. comm.). Vocal, morphological, and genetic data divergence between *tumbezanus/inflavus* and

maranonicus (including montane Tumbes populations), combined with their nearly sympatric distributions, suggest the two merit recognition as separate species. Further work is needed to ascertain if interbreeding or introgression occurs in this region.

B) Split *N. maranonicus*: keeping the splits 1 and 2 under a single species *N. tumbezanus* would be another option, but this is not recommended given the vocal differences (see Schulenberg et al. 2007), the relatively deep genetic break and the near parapatry of these taxa.

C) Split *N. incomtus* (with ssp. *eremonomus*): note that the paper by Kroodsma et al. (1987) on the vocalizations of *ridgwayi*, coupled to its divergent morphology (see Sherry 1985, cited in Sherry 1986), and level of genetic divergence argue against the lump of *ridgwayi* with *incomtus/eremonomus*. I am not aware of any formal analysis comparing *incomtus/ eremonomus* vs. *murinus/wagae/ignobilis*, but the differences are striking based on my recordings and field experience with both in Argentina/Bolivia and Venezuela, backed up also by recordings by others (notably by P. Schwartz in Venezuela). The calls, diurnal song and dawn songs are clearly different, even when they share an overall "*Phaeomyias*" feel/structure to them.

D) Make further splits in *N. murinus/wagae/ignobilis*: This clade is possibly the most problematic because there is a relatively deep split that may indicate the existence of two species. The type of *N. murinus* is lost (type locality Brazil), and *wagae* is from E Peru (type locality Chirimoto), and *ignobilis* (type locality Villa Montes, Bolivia) was lumped with *murinus* by Fitzpatrick (2004). Therefore, without more work comparing type specimens and matching those to the clades in Zucker et al. (2016), it is not clear which name should be applicable to which population, and it looks like a decision should be made regarding a restricted type locality and possibly a neotype designation for *murinus* (which apparently has not been done). I also want to note that at least the southern populations traditionally ascribed to nominate *murinus* and *ignobilis* are highly migratory, and that it is likely that several of the more northern examples from the Peruvian Amazon (green spots) in the tree of Zucker et al. (2016) are southern migrants. The problem with "who is who" gets diluted (at the species level) if one decides not to split these two clades, which is probably the best course at present given the lack of detailed studies analyzing their vocalizations, the borderline genetic differences, and the mess with which names to apply.

Rheindt et al. (2008):

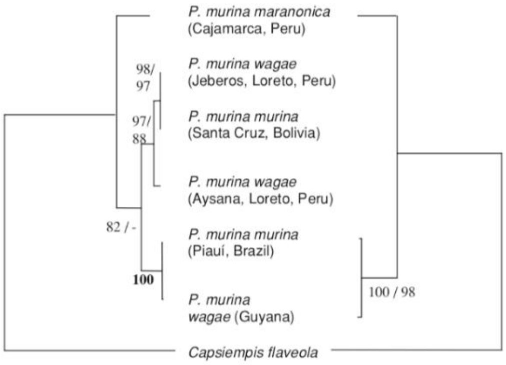
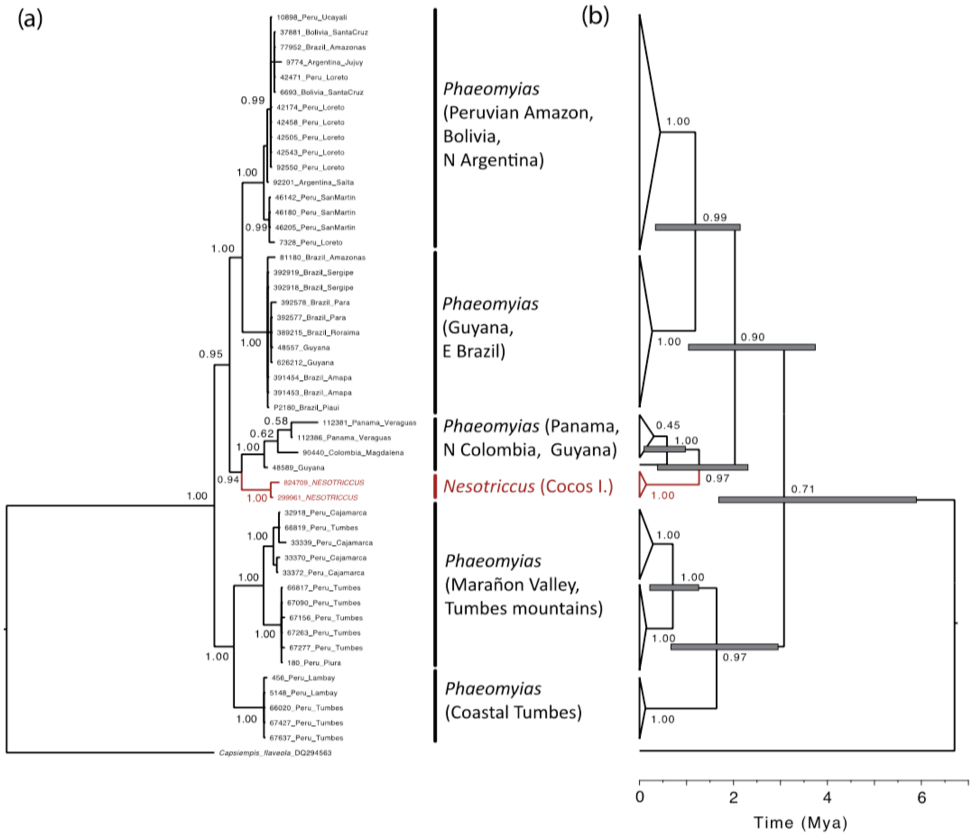
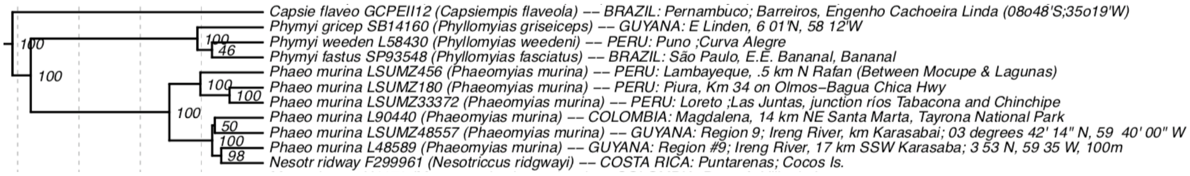


Fig. 2. Maximum parsimony (MP) phylograms of *Camptostoma obsoletum* and *Phaeomyias murina* (at equal scale), with bootstrap support figures (MP / ML); numbers in bold indicate identical bootstrap support for both modes of analysis. Left topologies are based on ND2 sequences, right topologies are based on concatenated sequences.

Zucker et al. (2016):



Harvey et al. (2020):



Recommendation:

I recommend YES to Parts A, B, and C (i.e., recognize *P. tumbezanus*, *P. maranonicus*, and *P. incomtus*) and NO to Part D (further splits within *N. murinus/wagae/ignobilis*) until further work clarifies what is going on there.

The Guyana samples present in the *murinus* clade or in the *incomtus* clade might be seen as problematic by some. However, the ones in the *murinus* clade could pertain to southern migrants (I am not sure what these specimens look like, and I cannot access the supplementary material to see whether the collection date makes sense for a migrant). Regardless of this, Guyana is not the type locality of any of these birds, and having two nearby samples in different clades can also be interpreted as evidence of two species (much as in *tumbezanus* and *maranonicus*). Also, keeping the *incomtus* and *murinus/wagae/nobilis* clades as one unit creates a somewhat odd paraphyletic species with two distinct vocal types (i.e., vocalizations of *incomtus* and the *murinus/wagae/nobilis* clade differ noticeably, even when no formal analyses exist). I therefore think that split C is quite straightforward, but what I think is not safe to do is to perform any more splits in the *murinus/wagae/nobilis* group; therefore, I recommend a NO on D.

Votes are needed for the following:

(A) Split *N. tumbezanus* (with ssp. *inflavus*) from *N. murinus*.
(B) Split *N. maranonicus* from *N. murinus*.
(C) Split *N. incomtus* (with ssp. *eremonomus*) from *N. murinus*.
(D) Make further splits within *N. murinus/wagae/ignobilis*.
(E) See next page for details.

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Submitted by: J. I. Areta

Date of Proposal: February 2023, modified by R. T. Chesser on 21 January 2025

Part E: Establish English names for *Nesotriccus murinus* complex

If the proposal to modify species limits in this group passes, then new English names are needed for the component species. Actually, one of them, *N. murinus*, the only one that occurs in the NACC area, does not need to change because of strong asymmetry in the distribution and familiarity – see SACC guidelines for English names, Section C2, which were based on the guidelines adopted by NACC.

Below is the SACC proposal followed by votes and comments. The voting results are online here. The proposal passed unanimously 8-0. As a result, SACC names are now:

Mouse-colored Tyrannulet (*N. murinus*)
Tumbesian Tyrannulet (*N. tumbezanus*)
Marañon Tyrannulet (*N. maranonicus*)

Naturally, I recommend NACC follow SACC on this one. Certainly, retaining Mouse-colored for the species in the NACC area should not be controversial.

[***Note:*** Although the proposal suggests Marañon Tyrannulet for *N. maranonicus*, NACC does not use diacritical marks in English names, so the NACC name for this extralimital species, to be used only in the Notes of the species account for *N. murinus*, would be Maranon Tyrannulet.]

Submitted by: Van Remsen

Date of Proposal: 7 January 2025

Proposal (987) to South American Classification Committee

Establish English names for *Nesotriccus murinus* complex

SACC proposal 956 passed to treat the Mouse-colored Tyrannulet (then *Phaeomyias murina*, now *Nesotriccus murinus*) as consisting of three species: (1) the widespread *murinus* group of subspecies, and two peripheral taxa (2) *tumbezanus* (with *inflava*) of the Pacific coast of NW South America, and (3) *maranonicus*, endemic to the Marañon region. We voted not to recognize a fourth species, *incomta* (with *eremonoma*) pending publication of additional data on vocalizations and a possible problem in nomenclature in terms of which names apply to which taxa; see Nacho's Discussion section in 956. But it is important for us to anticipate this possible future split in choosing English names.

I had previously asked for someone to write a proposal on names for the new and potentially new species. No takers. Then, I just asked for feedback on whether to go with short names or compound names, and the only response I got was from David Donsker. So, it is clear no one wanted to tackle this situation for reasons obvious below.

Simplified distributions of the 4 groups are as follows:

1. *incomtus* (with *eremonomus*): W. Panama to northern and eastern Colombia, NE Ecuador, N Venezuela, the Guianas, and Trinidad.
2. *murinus* (with *wagae*): everything south of *incomta* south to S Bolivia, Paraguay, N Argentina, and SE Brazil.
3. *tumbezanus* (with *inflavus*): SW Ecuador and NW Peru to Lambayeque.
4. *maranonicus*: Marañon.

These taxa have only weak plumage differences – as in many small tyrannids, it's the voice that really distinguished them other than range. So, plumage-based names would seem to be out. The species *tumbezanus* and *maranonicus*, therefore by default, should probably be modified by Tumbes/Tumbesian and Marañon, respectively. Not very inspiring, but if anyone has any better ideas, speak up. I haven't researched this extensively, but the English names in the literature that I can find are in Cory & Hellmayr (1927): "Mouse-colored Tyrannulet" for nominate *murinus*, "Waga's Tyrannulet" for *wagae*, "Northern Mouse-colored Tyrannulet" for *incomtus*, "Tumbez Tyrannulet" for *tumbezanus*, and "Chapman's Tyrannulet" for *inflavus*. The other taxa were not yet described. Also, Ridgely & Greenfield (2001, Birds of Ecuador) already treated *tumbezanus* as a separate species based on voice and called it Tumbesian Tyrannulet, and this was followed by the IOC lists, which also treated maranonicus as a separate species "Marañón Tyrannulet" as well as *incomtus* as "Northern Mouse-colored Tyrannulet" and designating *murinus* as "Southern Mouse-colored Tyrannulet". This classification, which was also followed by eBird/Clemens without waiting for SACC or NACC to endorse the splits. (And as it turns out we have rejected treating *incomtus* as a separate species for lack of published data, as explained in SACC 956.)

Tangentially, beyond our area, *Nesotriccus* has always been called "Cocos Flycatcher", but now that we know it is embedded in a group called "Tyrannulets", I would recommend that NACC consider a switch to "Cocos Tyrannulet." The key ingredient of the name is Cocos, not "Flycatcher", which applies to dozens of tyrannid genera. A change to "Tyrannulet" would signal that *Nesotriccus* is no longer a monotypic genus, and would make its last name consistent with other members of the expanded genera. I normally favor stability, but in this case I favor drawing attention to a change in classification by flagging it with a partially new name. But that's not a SACC issue.

The Ridgely-IOC-eBird names are ok, but I think it's worth at least considering adding "Mouse-colored" to the names of Tumbesian and Marañon to identify them as congeners and separate them from the other ca. 59 species in 12 genera with the "last name" of just plain "Tyrannulet." If we eventually split *incomtus*, as is already the case in two major world classifications, we already have a long compound name if we follow IOC etc. and go with "Northern Mouse-colored" and Southern Mouse-colored, why not also go with Tumbesian Mouse-colored and Marañon Mouse-colored to set off the group? But that's not a problem if and until we split up the *murinus* group. For now, just "Mouse-colored Tyrannulet" is just fine for N. murinus, and retaining that well-established parental name for the by-far most widely distributed and familiar species of the trio.

So, I recommend we go with:

Mouse-colored Tyrannulet (*N. murinus*)
Tumbesian Tyrannulet (*N. tumbezanus*)
Marañon Tyrannulet (*N. maranonicus*)

A YES vote endorses these names. A NO vote is for something else – and please provide alternatives.

Van Remsen, December 2023

SACC comments on Proposal 956 [note that some SACC comments pertain to subsuming *Phaeomyias* into *Nesotriccus*, which NACC has already done]:

Comments from Lane: "Well drat. This proposal basically has rendered a paper I am working on as largely moot, which is irritating ... not least of which because Tom asked me for information on this case, and I relayed to him but that I was writing about it currently. I agree that the species split is clearly necessary, as is the reason for synonymizing *Phaeomyias* under *Nesotriccus*; so, YES to parts 1-3 and 5, and NO to part 4 (recognizing splits within *murinus*/*wagae*/*ignobilis*). Zimmer made a few unusual mistakes when reviewing these taxa, which has resulted in compounded taxonomic and distributional mistakes through all subsequent works, and I hope that that topic will remain relevant enough to warrant me finishing and publishing my work."

Comments from Claramunt: "YES to treat *Phaeomyias* species under the genus *Nesotriccus*, unfortunate but unavoidable. Reluctant YES to 1, 2, and 3 because the information is not all out and analyzed in a peer reviewed paper. NO to part 4. Dan: we still want to see your paper."

Comments from Stiles: "A,B-both YES to splits of *tumbezanus* and *maranonicus* given genetic differences and probable parapatry, maintaining *ridgwayi* as separate; C-YES to separate *incomtus* and *eremonoma* on genetic distinctions; D-NO to further splits without further genetic and vocal data; and E-YES to *Nesotriccus* for all, its priority is undeniable."

Comments from Shaun Peters: "Normally I would not get involved in the SACC proposal process, but since I am preparing some comments to send to both Clements-eBird and IOC regarding their split of *Phaeomyias murinus* and some other taxonomic decisions (presumably based on a what is coming from WGAC), I thought I'd share my thoughts with you, which you will find in the attached pdf* (converted from original word file to reduce file size). Also attached are the maps from the pdf and Supplementary Table 1 from Harvey *et al* listing specimen localities.

"To summarize:

“The proposed split of *P. murinus* in to 4 species is essentially based on genetic data and, in particular, paraphyly. This mainly comes from Zucker *et al*. (2016), but they only used a single mitochondrial gene (ND2). Harvey *et al.* (2020), which used UCEs, has limited sampling (6 samples, no samples from *P. m. murinus*). The trees in these two papers are broadly similar with the *tumbezanus* and *murinus* groups forming separate clades and *Nesotriccus ridgwayi* as sister to the *incomtus* group, but not with true paraphyly, but what I call 'pseudo-paraphyly' - this being the case in Harvey *et al* and also in Zucker *et al* once the *tumbezanus* clade is separated off as a separate species. 'Pseudo-paraphyly' does not require a mandatory split under a BSC.

“Zucker *et al*. then proposed a split of *maranonicus* (which now includes Andean *tumbezanus*) from lowland *tumbezanus/inflavus*, mainly based on their deep genetic divergence, but also citing differences in morphology and vocalizations. There do appear to be differences between coastal *tumbezanus* and *maranonicus,* but there are few available recordings of Andean *tumbezanus* and *inflavus* (the latter being the most distinct taxon in morphology), and there may well be vocal differences between *inflavus* and lowland *tumbezanus*.

“Harvey *et al*. then split *incomtus* from *murinus* based on paraphyly but, as previously stated, this is actually a case of 'pseudo-paraphyly' so is not necessary under BSC. Nacho mentions differences in vocalizations, but the situation is a little more complicated than he stated. Listening to vocalizations over the whole range of the *murinus* group, eastern and southern birds (*murinus*) generally have rather burry dawn songs and calls with northern birds (*incomtus*) having clear dawn songs and calls, although some daytime songs from *incomtus* have a burry quality. Interestingly, recordings of *wagae* resemble *incomtus* in their pure tone, whilst those of *ignobilis* seem closer to *murinus* than *wagae*. Thus there may well be three (or four) vocal types within the *murinus* group. There is also the fact that *incomtus* and *murinus* are very similar in morphology and it is *wagae* that stands apart (see Zimmer, 1941, although based on Dan's comments on the proposal this may not be the case??).

“Thus, there may well be more than one species involved within the *murinus* group, but it requires more widespread genetic analysis (of both nuclear and mitochondrial genes) as well as a detailed comparison of vocalizations across the whole range of the *murinus* group.”

[* distributed to SACC members separately]

Comments from Shaun Peters (now voting for Pacheco): “Since a split of a broad *tumbezanus* (including *maranonicus*) is not on the table I would have to vote 'No' on Parts 1 and 2. Although lowland *tumbezanus* and *maranonicus* clearly differ vocally, I'm not certain about the vocal affinities of Andean *tumbezanus* and *inflavus* (more work is needed here). 'No' votes on parts 3 and 4 are more straightforward for me - more work (how the genetic, morphological and vocal data marry up) needs doing here, Thus, here are my votes

“Part 1 - NO
Part 2 - NO
Part 3 - NO
Part 4 - NO
Part 5 – YES”

Additional comments from Areta: “Shaun, thanks for the diligent analysis shared. I should have checked for the restricted type locality for *murinus*; however; even if there might be more splits in the *murinus-wagae-ignobilis*, data are far from satisfactory and nomenclatural problems should be cleared before proceeding on this front. For example, although *murinus* has been restricted to a type locality, apparently no neotype has been designated; thus, flimsy ground upon which to decide.

“Also, bear in mind that the support for the Guyana and Colombia samples in the Harvey et al tree is so low as to render this a complete uncertainty, and cannot be interpreted as introgression with any confidence. So, there is no "pseudo-paraphyly" here.

“After reading Shaun’s comments (and incorporating the caveat that the Harvey et al. tree cannot be used to discard or confirm paraphyly in the *murinus* group), the genetic and vocal data are consistent with the treatment that I advocate: split *incomtus*/*eremonoma*, split *tumbezanus*, split *maranonicus*, and leave *murinus* as one species until the proper studies needed to sort out their taxonomy are published (it is possible to split the NE and SW clades as separate species, but I didn´t want to go that far in the proposal, given the confusion surrounding the distribution of the different taxa, the lack of a proper vocal analysis, and type-specimen issues). As I mentioned in the proposal, the likely coexistence of taxa from two different clades in Guyana provides further support to split *incomtus*/*eremonoma* from the *murinus* group. Now, whether these breed there or not, I don´t know. Maybe Dan has researched this more in-depth for his paper.

“I think that there are plenty of questions here to be answered, but I think that the SACC proposal is consistent with the minimum number of necessary splits that are well-supported by the data."

Comments from Bonaccorso: “YES to part 5. The evidence from the broad sampling of Zucker et al. (2016) on ND2 and the restricted UCEs sample from Harvey et al. (2020) are consistent with *Nesotriccus* well nested within *Phaeomyias*. If *Nesotriccus* is an older name, the change of all *Phaeomyias* of *Nesotriccus* is adequate.

“NO to the splits (parts 1, 2, 3, 4). The splitting of these taxa would be based solely on Zucker et al. (2016) ND2 data and their presumable paraphyly. Such a split would need a more integrative approach with more genetic data, including potential contact zones and some diagnostic characters, either morphological or vocal.”

Comments from Robbins: “The combination of genetic and vocal data do support the minimum splits that Nacho has proposed, thus I vote for acceptance of 1,2,3, and of course, # 5, using *Nesotriccus* for all these taxa.

“With regard to whether birds breed in Guyana, samples that we collected during March in the Rupununi do breed there (see Appendix 1 in Robbins et al. (2004. Avifauna of the Guyana Southern Rupununi, with comparisons to other savannas of northern South America. Ornitologia Neotropical 15:173-200). For example, I recorded persistently singing birds (ML 145030, 145010) on the same day/site that birds were collected that had enlarged testes, e.g., 7 x 4 mm (KU 90771). With regard to the two LSU Guyana samples in Zucker et al. (2016), thanks to

Steve Cardiff, a photo is attached below of those specimens (collected by Santiago). Note how strikingly different those two specimens are, even though both are adult females with similar gonadal data, taken at the same locality within two days of each other (during the austral winter). So, I suspect one is a migrant and the other a resident. The paler bird is more consistent with material that we have collected throughout the year in Guyana.”

“To add a bit of perspective, three specimens (KU 90771, 90884-5) taken in the southern Rupununi during late March to mid-April are indistinguishable from a specimen with enlarged testes (6 x 3 mm) taken on 31 October (KU 96872) from Jujuy, Argentina! Furthermore, in Birds of the World, which has already split the *murinus* complex into multiple species, states the two subspecies of the Northern Mouse-colored Tyrannulet (*Nesotriccus incomtus*) differ in that Central American *N. i. eremonoma* is distinguished from nominate by having the wing coverts edged dull buff. The aforementioned March-April birds from Guyana have the wing coverts edged dull buff.

“So, I would submit that delineation of subspecies using plumage would seem highly problematic: voice and genetic data should define taxa in this complex.

“Clearly, more in-depth study is needed, but I think it is a major step forward in recognizing the proposed splits in this proposal.”

<u>Comments from Zimmer</u>: "This is definitely a messy taxonomic and nomenclatural case, and one for which we don't have enough data to completely untangle. However, I think there is enough evidence for us to move things forward on some fronts, so, here goes: **YES to splits 1, 2 & 3**, in spite of some unresolved issues regarding vocal variation within *tumbezanus* (coastal versus Andean) and how that relates to both *inflavus* and *maranonicus.* I think it is a step forward to make these splits to at least begin to address some of the vocal and genetic variation. But, like Nacho, I don't think we know enough about what's going on to justify any further splitting at this time, within the murinus/wagae/nobilis group, so **NO on 4.** Also, a clear **YES on 5**, since *Nesotriccus* is clearly embedded within *Phaeomyias*, and the priority of *Nesotriccus* is undisputed.

<u>Additional comments from Lane</u>: "Thanks to some queries from Niels Krabbe on voices of some Ecuadorian populations of this complex, I have reviewed recordings in both Xeno-canto and Macaulay Library and found that it seems that both have made a real hash of the application of taxonomic names to populations. Macaulay has already instituted the split of *N. incomtus* from *N. murinus*, but many of the recordings placed in the former sound more to me like *wagae*, which is placed in the latter, and which is the cis-Andean form I know best. As Nacho lays out, the type locality of *incomtus* is Cartagena, and there are not a lot of recordings from around there for me to feel like I really have a grasp of what that taxon sounds like (I can't find any real dawn song from around Cartagena, for example). My gut feeling is that the name *incomtus* is best applied to birds from northern Colombian lowlands east along the Venezuelan coast (possibly in the northernmost edge of the Llanos as well?), and, presumably, into Guyana (as per the Zucker et al. phylogeny), but I don't hear recordings from Guyana that sound like those from Colombia. To my ear, many of the dawn songs available in ML from Venezuela and

Guyana sound very much like the *waga* song I know from Peru (see here for a near topotypical example: https://macaulaylibrary.org/asset/234404). Indeed, nearly all the highland birds on XC from central Colombia and Ecuador seem to sound much more like *wagae*, but with higher-pitched voices. This suggests that the *incomtus* group is far more range-restricted than it seems to have been thought (at least based on the maps available on XC and eBird/Birds of the World... which may or may not be of any value in this discussion anyway), but more importantly, unless each listener is carefully combing the recordings for topotypical examples and working outward from there, they will be completely swamped by misinformation as to the voice characters of each group. I hear much variation within all these *murinus/incomtus* populations, including what appears to be several unrecognized taxa (the highland birds from Colombia and Ecuador, and then birds from NE Brazil seem fairly distinctive compared to other cis-Andean populations. That last tidbit calls into the light the proper application of the name *murinus*: is it from NE Brazil or SE? According to Peters, Pinto restricted the name to Bahia, so presumably that means this unique voice type from NE Brazil is true *murinus* (assuming Pinto did his homework!), leaving the more widespread form, including the migratory populations Nacho talks about, as *ignobilis* (which was unhelpfully synonymized by HBW, clearly a move that must be reversed!), which sounds more like *wagae*, but still seems distinctive to my ear.
"The long and short of it is: this complex is a mess, and I think this all needs to be ironed out before we go splitting the *murinus/incomtus* complex, even if it remains paraphyletic with respect to *N. ridgwayi*... In some ways, this study mirrors that of *Nyctiprogne* in having a phylogeny that needs some "ground-truthing" to make sense of the patterns it draws. So, with all that, I am changing my vote for Part 3 to NO. We need so much more information to make that call!"

Additional comments from Robbins: "Yes, indeed this is a mess that needs to be sorted out. Given what Dan has underscored, I'll change my 956.3 vote of splitting *incomtus* to a NO until things get clarified."

Comments from Niels Krabbe (voting for Remsen); "As pointed out by Dan, Mouse-colored Tyrannulet encompasses a larger number of populations of different vocal types than the literature would suggest, and for many of these there is still too little material available to properly define them.

"After listening to recordings, I must agree that the call and dawn song of *maranonicus* are strikingly different from those of *tumbezanus* occurring on the same slope, but it is unsatisfactory that this suggested split is based partly on Lane's and Angulo's personal comments. So as a reminder that SACC proposals should be based on published material, I cannot vote for this split at present. That *Nesotriccus* has priority for the entire complex is, as shown by both Zucker et al. (2016) and Harvey et al. (2020), indeed, unquestionable.

"So my vote goes:

"Options 1-4: NO
"Option 5: YES apply the generic name Nesotriccus for the entire complex."

Comments from Glenn Seeholzer (voting for Jaramillo): "In general I feel that the perfectly sampled, integrated taxonomic treatise should not be the standard for every proposal. Very few have the resources or time to indulge in such work meaning that obviously necessary and justified changes will be stalled for lack of a peer-reviewed paper stating what was obvious at the start. On the other hand, the split in Part 3 (already instituted by the Clements/eBird taxonomy) feels hasty given what Dan and Shaun have uncovered so this definitely feels like a case where more in-depth vocal analysis is necessary.

“YES to Parts 1 and 2 - This split would be based primarily on the assumption of close parapatry between the aligned clusters of vocal, plumage, and genetic traits represented by *tumbezanus*/*inflavus* and *maranonicus*. A quick review of recordings on Xeno-canto and ML shows that the distinctive *maranonicus* vocal group is clearly present in the upper elevations of the west slope in Piura (see Dan's great documentation here at 1900m) and published evidence that *tumbezanus* occurs to at least these elevations nearby (Angulo et al. 2011). While it would be nice to have this better documented, I think the burden of proof is on those who would say that these groups are not distinct enough, are not actually in contact, or are somehow hybridizing and should be lumped.

“NO to Parts 3 and 4 - This would create a paraphyletic *incomtus*/*murinus* with respect to *ridgwayi*, which I'm fine with because paraphyly is compatible with the BSC. *Ridgwayi* is clearly a diagnosable unit, but it is unclear what the taxonomic units are among the constituent *incomtus* + *murinus* taxa. While the geographic extremes of *incomtus*/*eremonoma* vs. *murinus*/*wagae*/*ignobilis* may be quite different at their extremes, as Nacho states in the proposal, Shaun and Dan seem to have uncovered more complex patterns of vocal variation with potential evidence of intermediates (*wagae*) and multiple distinct vocal groups in each putative species. For the same reasons that it seems premature to split *murinus*/*wagae*/*ignobilis* with so little documentation (Part 4), it seems premature and potentially incorrect to split *incomtus*/*eremonoma* and *murinus*/*wagae*/*ignobilis* (Part 3). I feel a detailed published analysis of vocal variation is warranted in this case to define the diagnostic vocal traits of the units and their geographic distributions before considering further splitting.

“YES to part 5 - *Nesotriccus* has priority over *Phaeomyias*, so ciao *Phaeomyias* and long runs of vowels.”

SACC comments on Proposal 987 (English names):

Comments from Jaramillo: “YES for these names:
Mouse-colored Tyrannulet (N. murinus)
Tumbesian Tyrannulet (N. tumbezanus)
Marañon Tyrannulet (N. maranonicus)”

Comments from Gary Rosenberg (voting for Remsen): “YES.

“In the name of stability, I think adopting “Tumbesian” for *tumbezanus* and Marañon for *maranonicus* makes sense, especially given this is how the IOC currently treats the two forms. Ridgely treated the two under “Tumbesian” - and given that they are now split, retaining Tumbesian for the one that is mainly found coastally in the Tumbes Region makes total sense. I vote yes for "Marañon" as well - especially given that the Latin name is "*maranonicus"* - so that makes sense - and that is what the IOC calls it - so a one for unification and stability! I do note, however, that a portion of the population are found outside of the Marañon proper - but I guess you can say that those locations are technically in the overall Marañon drainage.

"I do not understand why eBird chose their own name for *tumbezanus* creating "Tumbes" Tyrannulet - which is doubly confusing since they also changed "Tumbes Tyrant" to Tumbes Chat-Tyrant. Thankfully, the IOC has not adopted that change! I think it is a very bad idea for the eBird team/Clements to go rogue and do their own bird naming - and not following either the SACC or the IOC - all this does is add confusion and created further destabilization in bird names."

"I also vote YES to retain (for now) Mouse-colored Tyrannulet.

"I would be in favor of adding "Mouse-colored Tyrannulet" to both Tumbesian and Marañon to further distinguish this group of tyrannulets - which would certainly be helpful for learning purposes - allowing people to focus on this rather unique group of tyrannulets as a group - I am all in favor of making tyrannulet identification easier for birders and scientists alike."

Comments from Zimmer: "YES to the English names suggested inn Proposal 987 for the splits in the *Nesotriccus murinus* complex: Mouse-colored Tyrannulet (*N. murinus*); Tumbesian Tyrannulet (*N. tumbezanus*); and Marañon Tyrannulet (*N. maranonicus*). And, for what it's worth, I would agree with going to the long compound names (Northern, Southern, Tumbesian and Marañon Mouse-colored Tyrannulets) if and when *incomtus* is split."

Comments from Donsker (voting for Areta): "I vote YES for Proposal 987 establishing English names for the splits of the *N. murinus* complex.
"That is:
Mouse-colored Tyrannulet (*N. murinus*)
Tumbesian Tyrannulet (*N. tumbezanus*)
Marañon Tyrannulet (*N. maranonicus*)"

Comments from Steve Hilty (voting for Bonaccorso): "I am fine with the following, but personally I would choose the longer versions (see discussion below):

Mouse-colored Tyrannulet, *N. murinus*
Tumbesian Tyrannulet, *N. tumbezanus*
Marañon Tyrannulet, *N. maranonicus*

"As noted by a couple people, I think it would be helpful to incorporate the original name, Mouse-colored" into these new names as you also mentioned. This helps preserve some of the history of the taxonomy—and for the "lay person" that doesn't think about these names and all of these convoluted taxonomic twists and turns every day, but still tries to maintain control of a personal list—I think the longer names provide several advantages (a geographical clue; a taxonomic clue; and a window into historical decisions).

“Frankly, for those of us close to these names and the associated taxonomy, it is easy to forget that the primary users of these names are non-professional ornithologists, and the more information a name contains, the more useful it is to most people. The length of the name is, in most cases, irrelevant. We have been living with and using numerous 4- and 5-word English bird names for years without issue or objection.

“In cases where several similar species are involved, I find longer names preferable because they convey more information than abbreviated names, and especially if they incorporate geographical or behavioral attributes. As an aside, shortened names that rely heavily on descriptive colors—rufous-crowned, rufous-browed, rufous capped, rufous-fronted, and so on *ad nauseam* often quickly become confusing, especially in field use.

“Thus, these below would be my first choice:

Mouse-colored Tyrannulet, *N murinus*
Tumbesian Mouse-colored Tyrannulet, *N. tumbezanus*
Marañon Mouse-colored Tyrannulet, *N. maranonicus*

“And in the likelihood that the north-south split occurs (voice and some plumage differences are certainly apparent), why not use (as you suggest):

Northern Mouse-colored Tyrannulet
Southern mouse-colored Tyrannulet”

Comments from Josh Beck (voting for Claramunt): “YES on this proposal. One comment: Tumbesian is preferable to Tumbes as noted by Gary to avoid confusion. However, if Mouse-colored is kept in the names, Tumbes Mouse-colored Tyrannulet might be preferable to Tumbesian Mouse-colored Tyrannulet based upon syllable count. I’m mildly in favor of keeping Mouse-colored in the names due to the use (already by eBird/Cornell and perhaps likely in the future by others) of Northern and Southern Mouse-colored Tyrannulets when treating *incomtus* as separate from *murinus*.”

Comments from Lane: “YES to Mouse-colored Tyrannulet (*N. murinus*), Tumbesian Tyrannulet (*N. tumbezanus*), and Marañon Tyrannulet (*N. maranonicus*). Unless we consider added "Mouse-colored" to the name of Cocos Island Flycatcher, I don't think it's necessary to use it in the names of *N. tumbezanus* or N. maranonicus. In comparison to the core *N. murinus* group, both of these western forms are actually quite distinctive in appearance and (especially!) voice, so it doesn't phase me to not maintain the link between them and their "Mouse-colored" origin. If/when *N. murinus* is split up, we can decide then if it is imperative to maintain "Mouse-colored" in the daughter species' names... for now, I'm still waiting to see how that shakes out before I worry about names.”

Comments from Stiles: “On this one, I find the proposed names acceptable (Mouse-colored, Tumbesian (I prefer this to Tumbes as it makes it more clear that a particular zoogeographical region is involved) and Marañón (for much the same logic). If and when the *incomtus-murinus* split is published and accepted, I am persuaded by Steve's view that the E-names of N and S M-c T would be the most logical way to go - and that adding M-c to the names of Marañon and

Tumbesian would also be appropriate in relating the split to the previous, un-split situation of everything in *murinus*. Given the relatively wide distributions of both *murinus* and *incomtus*, I think that finding more appropriate distribution-based names for these would be difficult - and the overall phenotypic similarity of all 5 taxa (which do represent a distinct little clade) best expresses their relatedness.”

Transfer Lesser Whitethroat *Sylvia curruca* to the genus *Curruca*

Background:

The Lesser Whitethroat was long placed in the genus *Sylvia* together with most of the other "sylviid" warblers (e.g., Watson et al. 1986). However, genetic work found that the traditional *Sylvia* warblers formed two deeply divergent clades (Voelker and Light 2011, Cai et al. 2019), and most global taxonomic authorities have opted to recognize two genera for the clades, with the Lesser Whitethroat being placed in the genus *Curruca* Bechstein, 1802 (Dickinson and Christidis 2014, Clements et al. 2021, Gill et al. 2024, HBW and BirdLife International 2024).

New Information:

In molecular phylogenetic studies, members of Sylviidae form three main clades, with two clades showing relatively shallow divergence, and the third showing much deeper divergence (Voelker and Light 2011, Cai et al. 2019). The most deeply divergent clade is composed of 7 species, including the Blackcap (*Sylvia atricapilla*) and the Garden Warbler (*Sylvia borin*), whereas the remaining two clades comprise 25 species, including the Lesser Whitethroat, a vagrant to the NACC region (Fig. 1). Traditionally, many of these species were included in an

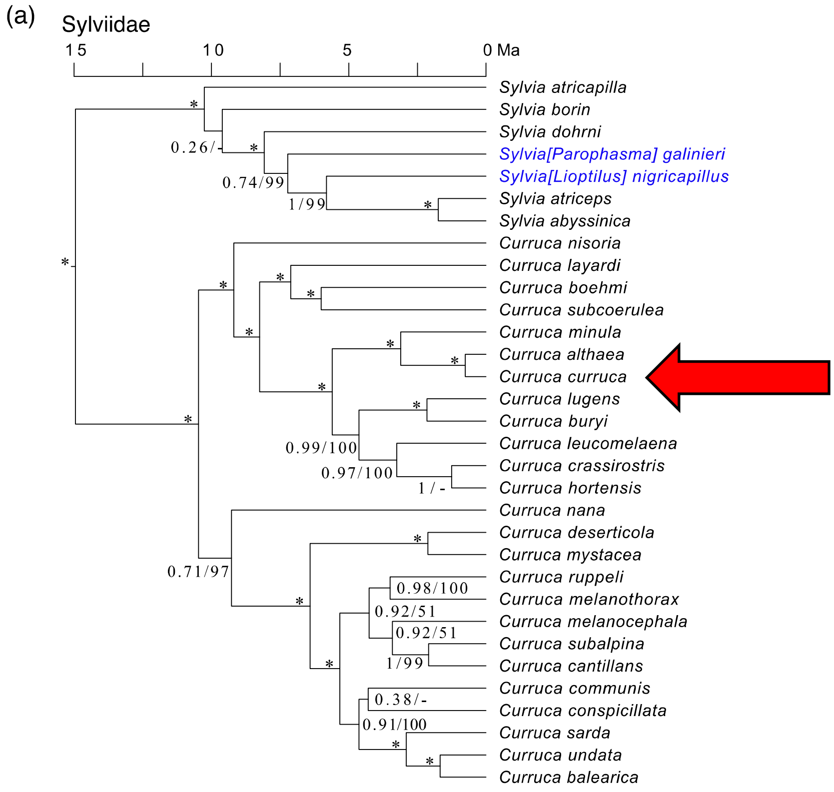

Figure 1. Phylogeny from Cai et al. (2019) showing the deep divergence between *Sylvia* and *Curruca*. The position of the Lesser Whitethroat is shown with a red arrow.

expanded *Sylvia* (e.g., Watson et al. 1986, Voelker and Light 2011, del Hoyo and Collar 2016). However, the deep divergence between the first clade and the remaining two, estimated to have occurred 15-20 million years ago (Voelker and Light 2011, Cai et al. 2019), led Dickinson and Christidis (2014) to split *Sylvia* into two genera. The most deeply divergent clade retained the name *Sylvia*, and the remaining species, including the Lesser Whitethroat, were transferred to the genus *Curruca*. Although slow to be adopted by other global taxonomic authorities, most now use this treatment for these species (e.g., Clements et al. 2021, Gill et al. 2024, HBW-BirdLife 2024)

Recommendation:

Because the Lesser Whitethroat is a bird that is only a vagrant to our region, I recommend we adopt the treatment used by most global taxonomic authorities and transfer the Lesser Whitethroat to the genus *Curruca*.

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Submitted by: Shawn Billerman

Date of Proposal: 14 January 2025

Transfer Bluethroat *Cyanecula svecica* to the genus *Luscinia*

Background:

The taxonomic position of the Bluethroat, along with many members of the Muscicapidae, has long been unsettled. It has been variously placed in the genera *Cyanecula* (e.g., del Hoyo and Collar 2016), *Erithacus* (Ripley 1964), *Cyanosylvia* (Wolters 1975), and *Luscinia* (Dickinson and Christidis 2014, Gill et al. 2024). The treatment within the AOS Checklist has been variable: Bluethroat was placed in *Cyanecula* in the first edition of the Checklist (AOU 1886), was transferred to *Luscinia* in the fifth edition (AOU 1957), then recently moved back to *Cyanecula* in the fifty-ninth supplement (Chesser et al. 2018). See the Recommendation for more information on our treatment of this species.

Historically, the genus *Luscinia* included a wide range of species, many of which have now been placed in separate genera, including the rubythroats in *Calliope*, the bluetails in *Tarsiger*, and the *Larvivora* robins, owing to substantial polyphyly of the traditional *Luscinia* with respect to the genera *Enicurus*, *Ficedula*, *Irania*, *Myophonus*, and others (Sangster et al. 2010, Zhao et al. 2023). As noted above, the Bluethroat has sometimes been placed in *Luscinia* but also in other genera such as *Cyanecula* (e.g., del Hoyo and Collar 2016).

New Information:

Molecular phylogenetic work has showed that the traditional grouping of *Luscinia* was polyphyletic. Sangster et al. (2010) recommended that the genus be broken up into several smaller genera, including *Calliope*, *Larvivora*, and *Tarsiger*. The Bluethroat, however, together with the White-bellied Redstart (formerly *Hodgsonius phaenicuroides*), was found to be sister to *Luscinia sensu stricto* (Sangster et al. 2010, Zhao et al. 2023; Figs. 1, 2), leading Sangster et al.

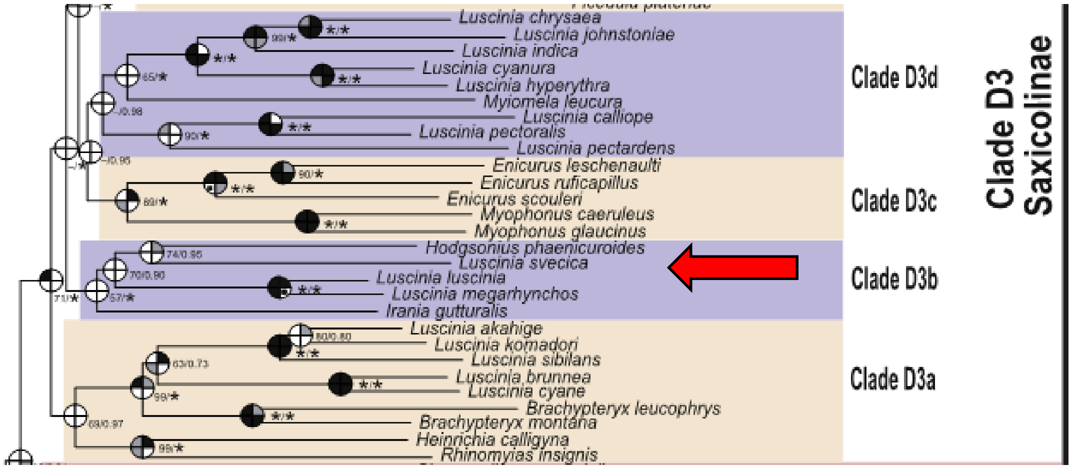

Figure 1. The Bluethroat is in Clade D3b, where it is sister to *Hodgsonius [Luscinia] phaenicuroides*, and together these two species are sister to the two nightingale species (*Luscinia sensu stricto*). Figure from Sangster et al. (2010).

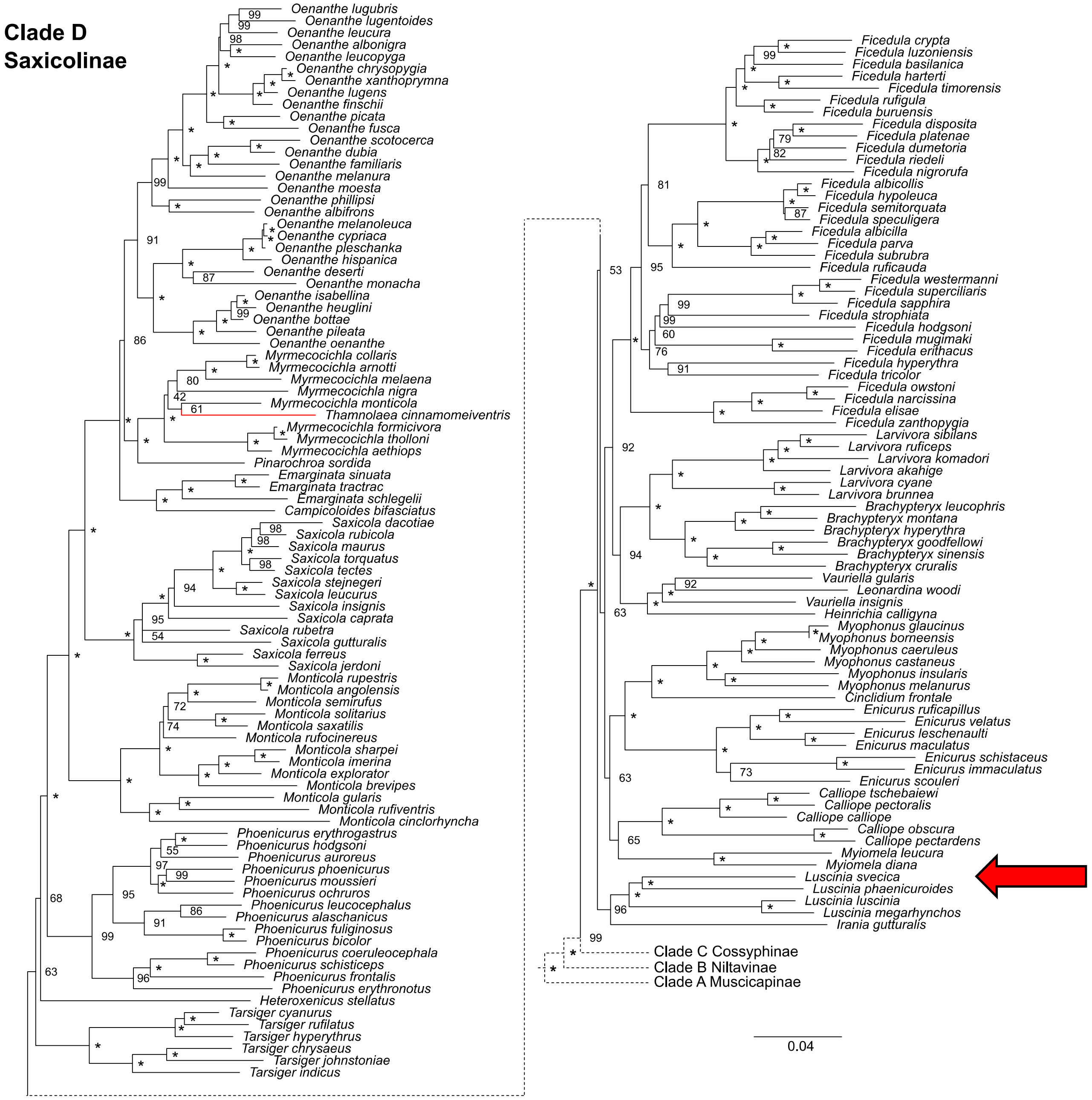

Figure 2. The Bluethroat (noted with a red arrow) is again sister to *Luscinia phaenicuroides*, with these two species in turn sister to the two nightingale species. The overall position of this well-supported clade is different than in Sangster et al. (2010), but the inclusion of the Bluethroat in *Luscinia* is still well-supported. Figure adapted from Zhao et al. (2023).

(2010) to recommend that the Bluethroat and White-bellied Redstart be placed in the genus *Luscinia*, together with the Thrush Nightingale (*Luscinia luscinia*) and the Common Nightingale (*Luscinia megarhynchos*). All major global taxonomic authorities have now adopted this recommendation, placing the Bluethroat in the genus *Luscinia* (Dickinson and Christidis 2014, Gill et al. 2024, HBW and BirdLife International 2024). Although the topologies of the family-level phylogenies of Sangster et al. (2010) and Zhao et al. (2023) differ, the clade of *Luscinia sensu stricto* (including the Bluethroat) was recovered in both studies with strong support.

Recommendation:

NACC moved the Bluethroat to the monotypic genus *Cyanecula* in 2018 (Chesser et al. 2018) following the treatment of del Hoyo and Collar (2016), who had opted to treat both Bluethroat and White-bellied Redstart (*Hodgsonius phaenicuroides* in their taxonomy) in monotypic genera on the basis of "unique morphological characters." These species in turn were most closely related to the nightingales of the genus *Luscinia*. The decision by NACC was also motivated in part by the mediocre support values uniting Bluethroat and White-bellied Redstart to the nightingales in the trees of Sangster et al. (2010). Although this approach is valid, global taxonomic authorities (e.g., Dickinson and Christidis 2014, Clements et al. 2019, Gill et al. 2024), including the updated HBW and BirdLife International (2024) checklist, have since chosen to adopt a more inclusive *Luscinia*, which includes both the Bluethroat and White-bellied Redstart. This reflects the consistently close relationship of these two species to the *Luscinia* nightingales (Sangster et al. 2010, Zhao et al. 2023), and the much stronger support values for these relationships recovered in Zhao et al. (2023).

Based on the phylogenetic studies of Sangster et al. (2010) and Zhao et al. (2023), as well as the treatment of Bluethroat in the major global taxonomic lists, I recommend that NACC transfer this species from *Cyanecula* to *Luscinia*. This would not require a change to the linear sequence in the Checklist.

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Submitted by: Shawn M. Billerman, Cornell Lab of Ornithology

Date of Proposal: 14 January 2025

Transfer Greater Necklaced Laughingthrush *Garrulax pectoralis* to the genus *Pterorhinus*

Background:

The Greater Necklaced Laughingthrush, a species introduced to Hawaii (island of Kauai), has long been placed in the genus *Garrulax* together with most other laughingthrushes (e.g., Deignan 1964, Dickinson and Christidis 2014). Many global taxonomic authorities, however, have now divided the laughingthrushes into 3 genera (*Pterorhinus* Swinhoe, 1868, and *Ianthocincla* Gould, 1835, in addition to *Garrulax*), largely based on deep genetic divergence within *Garrulax sensu lato* (Cibois et al. 2018, Cai et al. 2019, Clements et al. 2021, Gill et al. 2024).

New Information:

Molecular phylogenetic studies have mostly shown that although *Garrulax sensu lato* is mostly monophyletic, it is an old group with three well-supported clades similar in age to other well-established genera in Leiothrichidae (Cibois et al. 2018). The Greater Necklaced Laughingthrush belongs to the clade placed in *Pterorhinus*, which is sister to the clade placed in *Ianthocincla*; together, these two groups appear to be sister to the third clade, *Garrulax sensu stricto* (Cibois et al. 2018, Cai et al. 2019; Fig. 1). The genus *Pterorhinus* is defined mostly by genetic data, as the members of this clade are diverse and show few morphological similarities that could be used to define the group (Cibois et al. 2018).

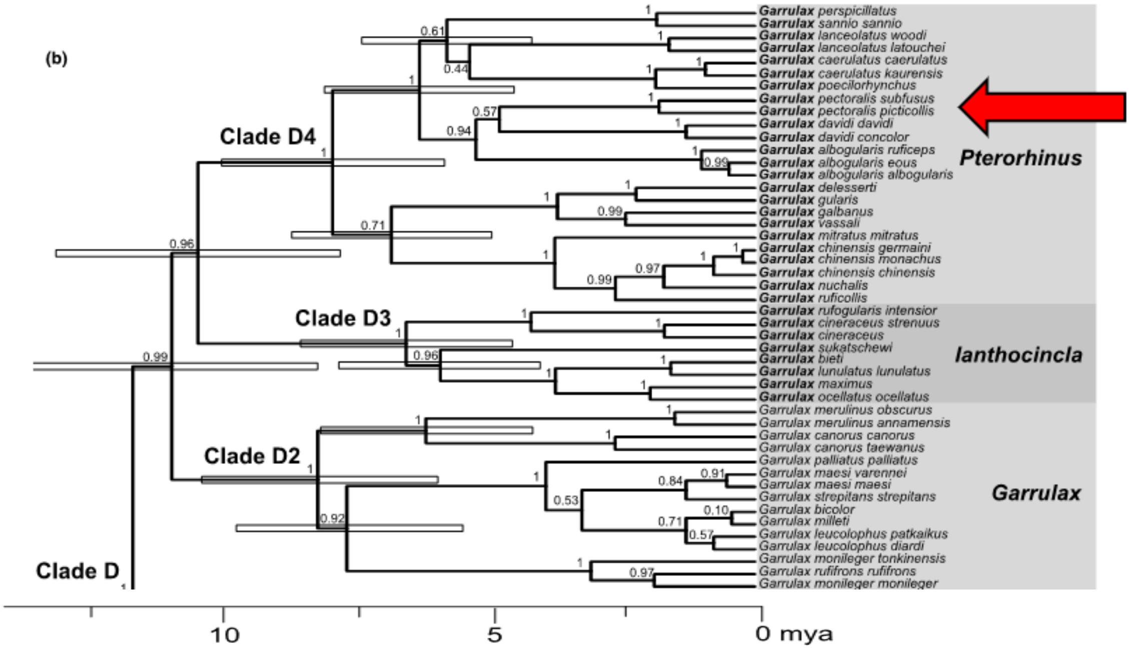

Figure 1. Phylogeny with proposed new genera for part of Leiothrichidae from Cibois et al. (2018). The position of the Greater Necklaced Laughingthrush is indicated with a red arrow, and falls within the clade of *Pterorhinus*.

Recommendation:

Although the breakup of *Garrulax* is not strictly necessary based on rules of monophyly, I recommend that we follow the major global taxonomic authorities in adopting *Pterorhinus* for the Greater Necklaced Laughingthrush, a species which is introduced into our region, and even then over a fairly limited extent.

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Submitted by: Shawn M. Billerman, Cornell Lab of Ornithology

Date of Proposal: 14 January 2025

Transfer Eurasian Jackdaw *Corvus monedula* to *Coloeus*

Background:

The Eurasian Jackdaw (*Corvus monedula*) was described in the genus *Corvus* (Linnaeus 1758), and most authorities including the AOS traditionally retained that classification. Others, such as the IOC World Bird List, transferred this species and the Daurian Jackdaw *C. dauricus* to the genus *Coloeus*. The Working Group on Avian Checklists (WGAC) addressed this discrepancy in 2023 and voted to place these two species in *Coloeus*. A minority view on WGAC preferred to recognize *Coloeus* but as a subgenus of *Corvus* rather than as a separate genus. This species occurs mostly outside of North America but has been a vagrant visitor to the eastern US and Canada since the 1980s (Smith 1985). Approval of this proposal will align us with the IOC, Clements, and Birdlife-HBW lists, which have adopted or will adopt the WGAC position.

New Information:

Multiple independent molecular studies have placed the two jackdaw species as basal to all other *Corvus*. Jønsson et al. (2012) presented both nuclear and mtDNA sequence data for all

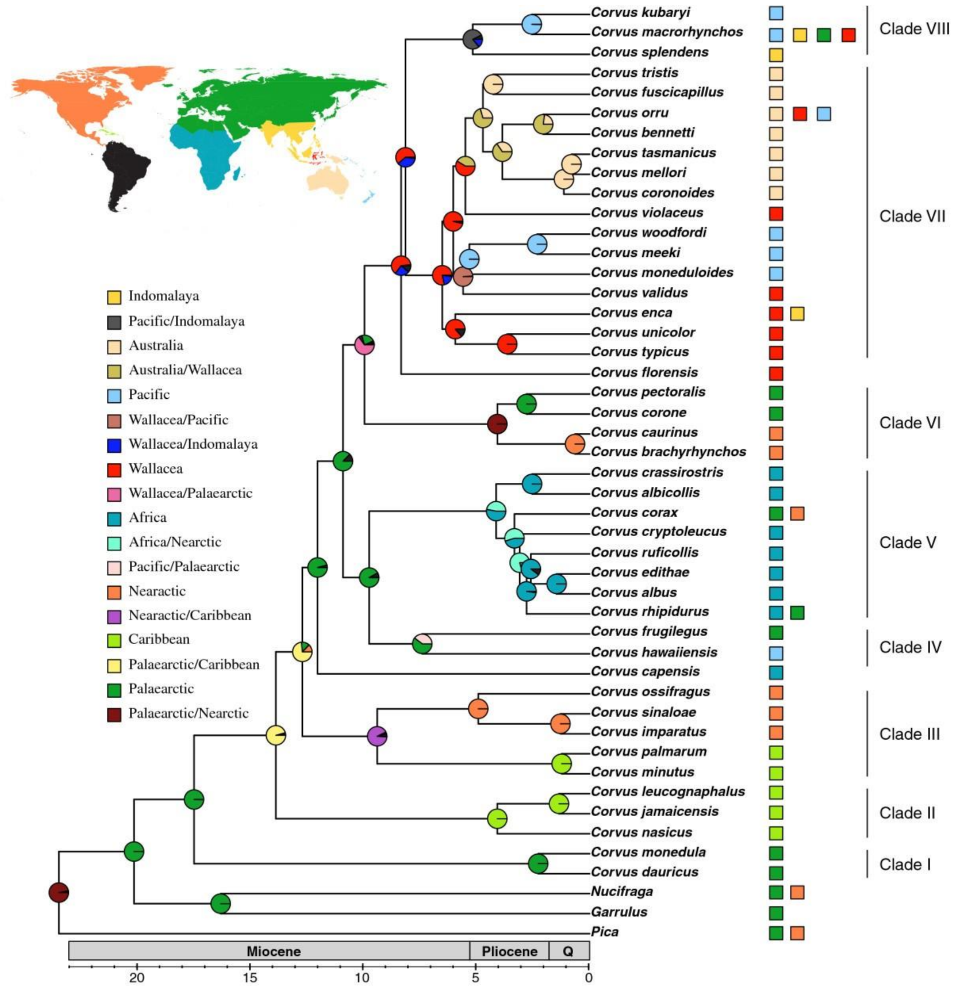

Figure 1. BAYES-LAGRANGE ancestral area analysis of *Corvus*, presented as a chronogram based on BEAST dating analysis of the combined nuclear and mitochondrial DNA sequences (Jønsson et al. 2012). The posterior probability for the node separating jackdaws from the rest of *Corvus* is 1.0 (see Jønsson et al. 2012, Figure 1).

extant species of *Corvus* and showed that *C. monedula* and *C. dauricus* are basal with strong support, suggesting a mid-Miocene split (Clade I, Figure 1).

The same results were found in two well-resolved supermatrix phylogenies of the Corvides: one generated using eight nuclear and four mitochondrial loci (Jønsson et al. 2016), and another using thousands of ultraconserved elements (UCEs) combined with 12 Sanger-sequenced gene regions (McCullough et al. 2022).

Likewise, genomic data for seven species of *Corvus* spanning the phylogeny of the genus showed that the two jackdaw species were basal and strongly divergent from the other sampled taxa, with an estimated divergence time of 13 million years between the jackdaw and crow lineages (Weissensteiner et al. 2020, Figures 2).

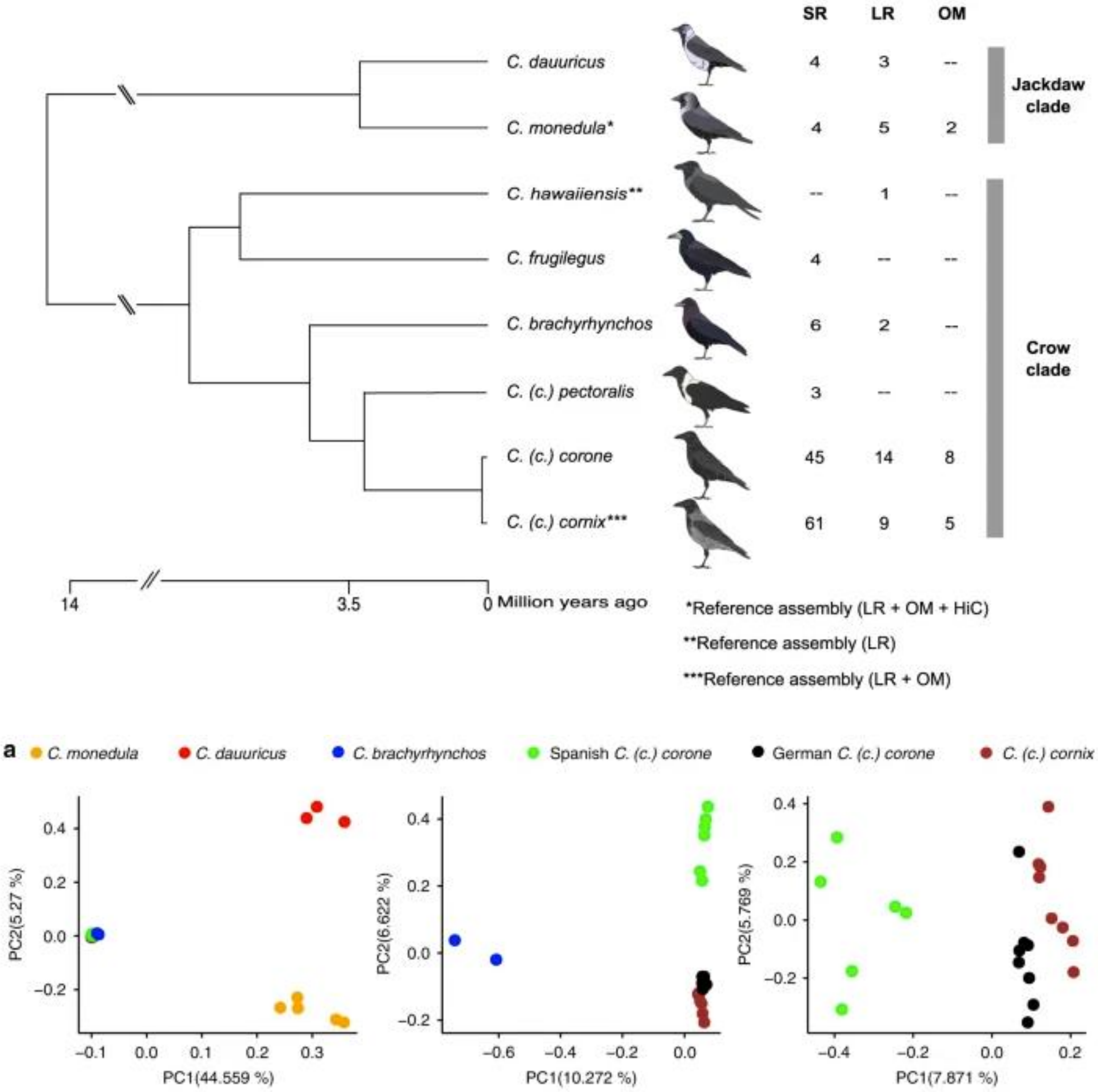

Figure 2. Top – Phylogeny of species sampled by Weissensteiner et al. (2020) showing the deep divergence of jackdaws; values in columns represent the number of individuals used for short-read sequencing (SR), long-read sequencing (LR), and optical mapping (OM). Bottom – Principal component analysis based on structural variation (SV) genotypes of species sampled by Weissensteiner et al. (2020); left plot shows results for all individuals analyzed together, with the crow clade tightly clustered and separate from the jackdaw clade along PC1.

In addition to their deep divergence from other *Corvus*, WGAC members argued that vocal differences support recognition of *Coloeus* as a separate genus. I am not aware of any published study on *Corvus* (including jackdaw) vocalizations, but I encourage NACC members to listen to recordings available on Xeno-canto and Macaulay Library.
https://xeno-canto.org/species/Coloeus-monedula
https://search.macaulaylibrary.org/catalog?taxonCode=eurjac&mediaType=audio

Recommendation:

As discussed by the WGAC, there are three possible treatments here:

A. Continue to place jackdaws in the genus *Corvus.*
B. Transfer jackdaws to *Coloeus* but treat *Coloeus* as a subgenus of *Corvus.*
C. Transfer jackdaws to *Coloeus* and recognize it as a full genus.

I recommend option C because of the deep divergence combined with a qualitative assessment of vocal differences, and to bring NACC in line with other global authorities given that the main distribution of these species is outside of North America.

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Submitted by: Carla Cicero

Date Submitted: 25 January 2025

Treat Black-throated Trogon *Trogon rufus* as more than one species

Note: This is a modified version of SACC proposal 921, which proposed treating *Trogon rufus* as five species. Ultimately, SACC voted to treat *T. rufus* as four species and to consider the newly described and poorly known *T. muriciensis* as a subspecies of *T. chrysochloros*.

Background:

SACC's pre-existing note on *T. rufus* read as follows:

> Dickens et al. (2021) found evidence that *T. rufus* should be treated as five separate species, including one newly described: *Trogon muriciensis* of the Atlantic Forest patches of northeastern Brazil; they recommended elevating the subspecies *tenellus*, *cupreicauda*, and *chrysochloros* to species rank. SACC proposal badly needed.

Trogon rufus (Black-throated Trogon) is a polytypic species with one of the largest distributions of any trogon. It occurs in three disjunct regions: (1) from Honduras to the Chocó of northwestern South America, (2) Amazonia; and (3) the Atlantic Forest from Alagoas through Brazil to eastern Paraguay and Misiones, Argentina. These populations have all been treated as conspecific from Cory (1919; albeit as *T. curucui* due to early name confusion), Pinto (1938), and Peters (1945) through the present. See the map (Fig. 1 of Dickens et al.) on the next page for the distribution of the subspecies of *T. rufus* and for sampling locations from Dickens et al. (2021), and the illustration (their Fig. 6) on the following page for plumage characters of the various forms of *T. rufus*.

New information:

Dickens et al. (2021) examined 906 specimens at 17 museums, including all taxa and all available type specimens, and gathered spectrophotometric data, patterning, bare parts coloration, and standard morphometric data on subsets of these specimens. They quantified vocal characters from 273 songs from throughout the distribution and including all named taxa. They also analyzed genetic samples (ND2 and cyt-b) from 29 specimens from throughout the distribution and all taxa. In other words, Dickens, Britton, Bravo, and Silveira conducted an amazing study in terms of sample size, geographic coverage, and critical data. Although the genetic sampling included only mtDNA, I don't think data from additional genes would have made a difference in terms of determining species limits. The analyses are really excellent, and I encourage everyone to check out the great graphics (my favorite is Fig. 3a on male uppertail covert hue, although the color illustrations by Eduardo Brettas of the taxa and their critical features is tough to beat). If only we had papers of this quality and depth, from sophisticated analyses to classical taxonomy. The only weakness, given the vocal differences, is the absence of playback trials, although to do that properly would require fieldwork in multiple regions in the Neotropics.

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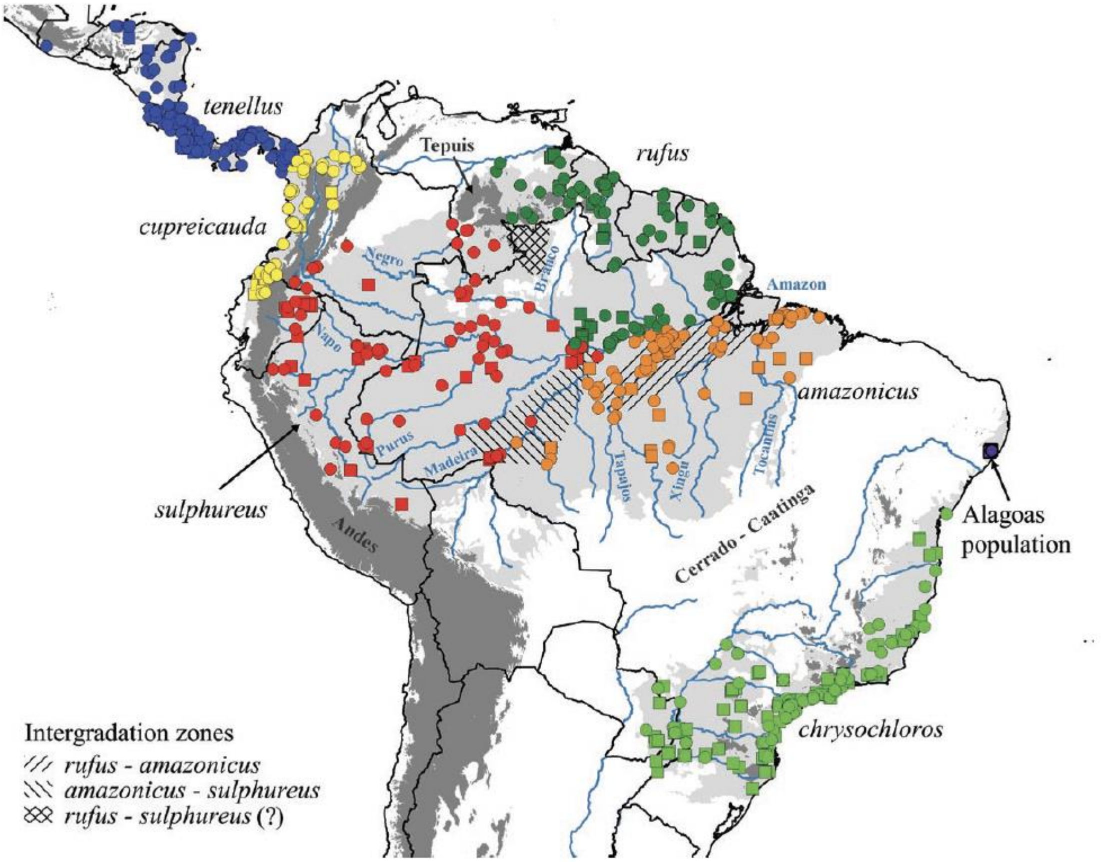

Figure 1. Localities of all examined museum skins (circles) and song recordings (squares) of species and subspecies in the *Trogon rufus* complex as well as the position of intergradation zones between Amazonian subspecies. Light grey = tropical and subtropical humid forests; dark grey = > 1000 m a.s.l., blue lines = major rivers.

A detailed synopsis of all these data would take up a lot of space here. Check out the details for yourselves, but here's what stands out to me: the voices of all their proposed species are qualitatively and quantitatively different (Fig. 4); in contrast, no such major differences are found among the Amazonian taxa that they recommend be treated as subspecies of *T. rufus* sensu stricto. For three of their proposed species-level taxa, 100% of the recordings were correctly classified to species using linear discriminant analysis for three of their proposed species-level taxa (*chrysochloros*, *cupreicauda*, *tenellus*).

As for the genetic data (ND2 1041 bp, cyt-b 1011 bp), the results show prefect congruence between geographic samples and relationships, and genetic distances within geographic clusters and within taxa are small (Fig. 5C). The cis-Andean taxa are all weakly differentiated, with time calibrations suggesting divergence times among species at ca. 3-4 million years. The big break is between cis-Andean and trans-Andean taxa, with a divergence time estimated at ca. 5 million years ago. The single sample of *cupreicauda* is fairly divergent from the 9 samples of *tenellus.*

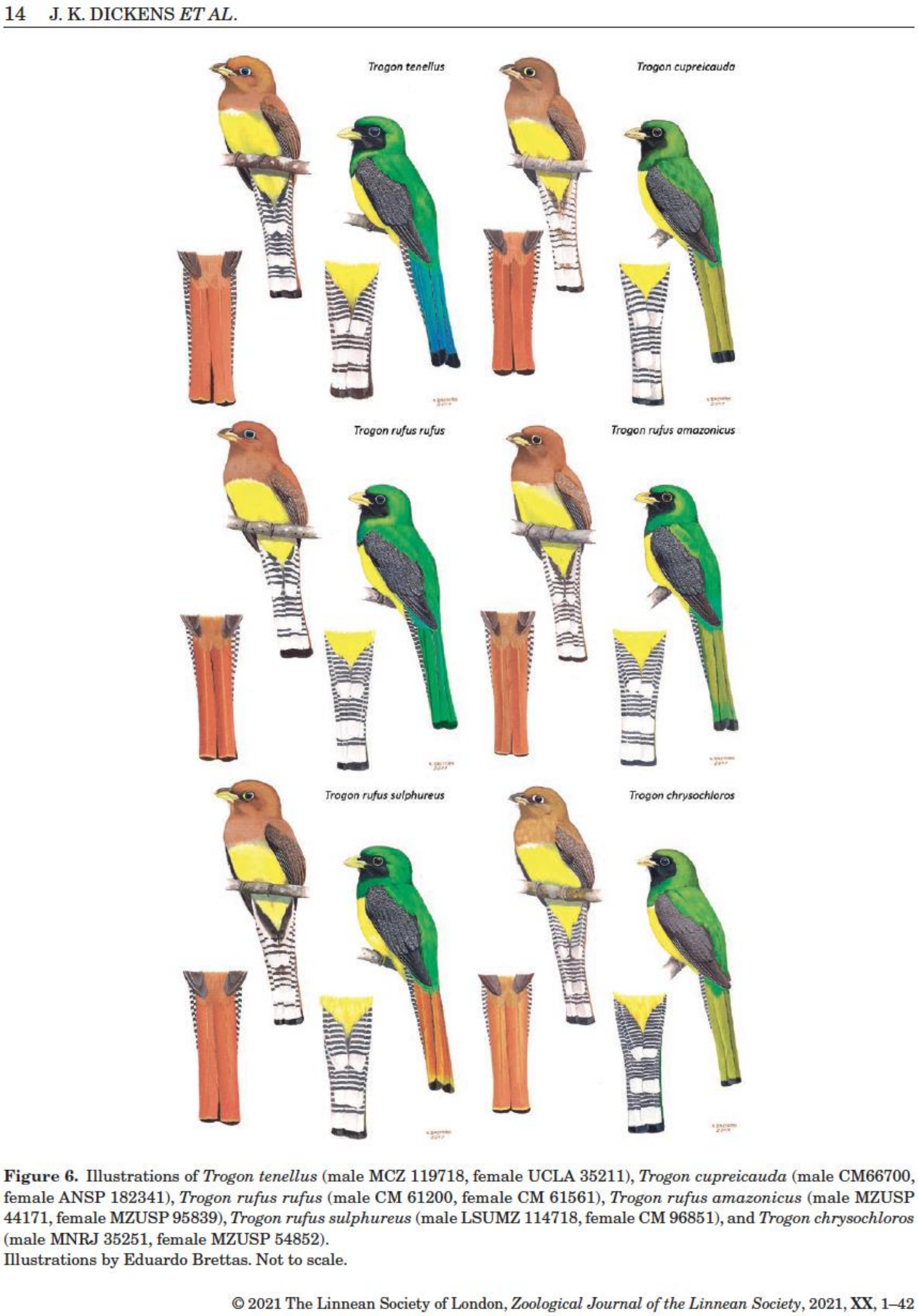
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Figure 6. Illustrations of *Trogon tenellus* (male MCZ 119718, female UCLA 35211), *Trogon cupreicauda* (male CM66700, female ANSP 182341), *Trogon rufus rufus* (male CM 61200, female CM 61561), *Trogon rufus amazonicus* (male MZUSP 44171, female MZUSP 95839), *Trogon rufus sulphureus* (male LSUMZ 114718, female CM 96851), and *Trogon chrysochloros* (male MNRJ 35251, female MZUSP 54852).
Illustrations by Eduardo Brettas. Not to scale.

(There are lots of little nuggets within this 42-page paper that I could itemize, but I will stop at just one because it has direct relevance to species limits. Atlantic forest *chrysochloros* is almost exclusively insectivorous, in contrast to the other omnivorous taxa, and it has a more heavily serrated bill for grasping large arthropods; they are also known to be regular followers of monkeys, army ants, and coatis, evidently more so than other trogons, so this all fits. Actually, I can't resist adding a second one because as the authors note, it is important to keep in mind when assessing plumage in trogons: they found evidence that there may be an environmental influence on iridescence, which changes with elevation in *chrysochloros.*)

A summary of their recommended species classification is as follows. See the paper for detailed diagnosis of each, including coloration, pattern, eyering color, and song features. These sections also contain detailed descriptions of each taxon, detailed synonymies, and useful notes on type specimens.

- *Trogon tenellus* Cabanis, 1862*:* Central America to extreme NW Colombia (dpto. Chocó)

- *Trogon cupreicauda* (Chapman, 1914)*:* N Colombia south on Pacific slope to NW Ecuador

- *Trogon rufus* Gmelin, 1788: including nominate *rufus* of Guianan Shield, *T. r. sulphureus* of western Amazonia, and *T. r. amazonicus* of eastern Amazonia

- *Trogon muriciensis* sp. nov.*:* Alagoas Forest region; known only from type locality at Estação Ecologica de Murici.

- *Trogon chrysochloros* Pelzeln, 1856*:* Atlantic Forest region

The quality of evidence for species rank varies among the 4 newly recognized or new species, so I think we should subdivide the proposal as follows below. Comparative analyses of the vocal data (Fig. 2 from Dickens et al.), as well as the phylogenetic tree (their Fig. 5C), are included on subsequent pages. I also suggest listening to the recordings of the various taxa on xeno-canto: https://www.xeno-canto.org/explore?query=trogon+rufus&pg=1

A. Treat *tenellus* as a separate species from *T. rufus*

Because *tenellus* is entirely trans-Andean, it is not parapatric with any subspecies of *T. rufus*, and thus the decision on taxon rank must rely on comparative methods. The vocal differences from other taxa are given as follows:

> Song: Diagnosed from neighbouring *T. cupreicauda* by fewer notes per phrase, longer note durations and generally higher note frequencies, particularly the introductory note high frequency. Note frequencies, particularly the introductory note high frequency, are higher than for *T. rufus* subspecies. Fewer notes per phrase, slower pace and longer durations of notes and pause following introductory note than *T. chrysochloros*.

In terms of phenotype, It has a breast band, unlike *T. r. sulphureus* or *T. r. amazonicus*, from which it differs in several other less conspicuous details (p. 26, and see the color plate (Fig. 6, p. 14, and above). The Diagnosis states that its pale (blue-gray to white) eyering distinguishes it from everything else, including parapatric *cupreicauda*, but *chrysochloros* also is listed as having the same color eyering.

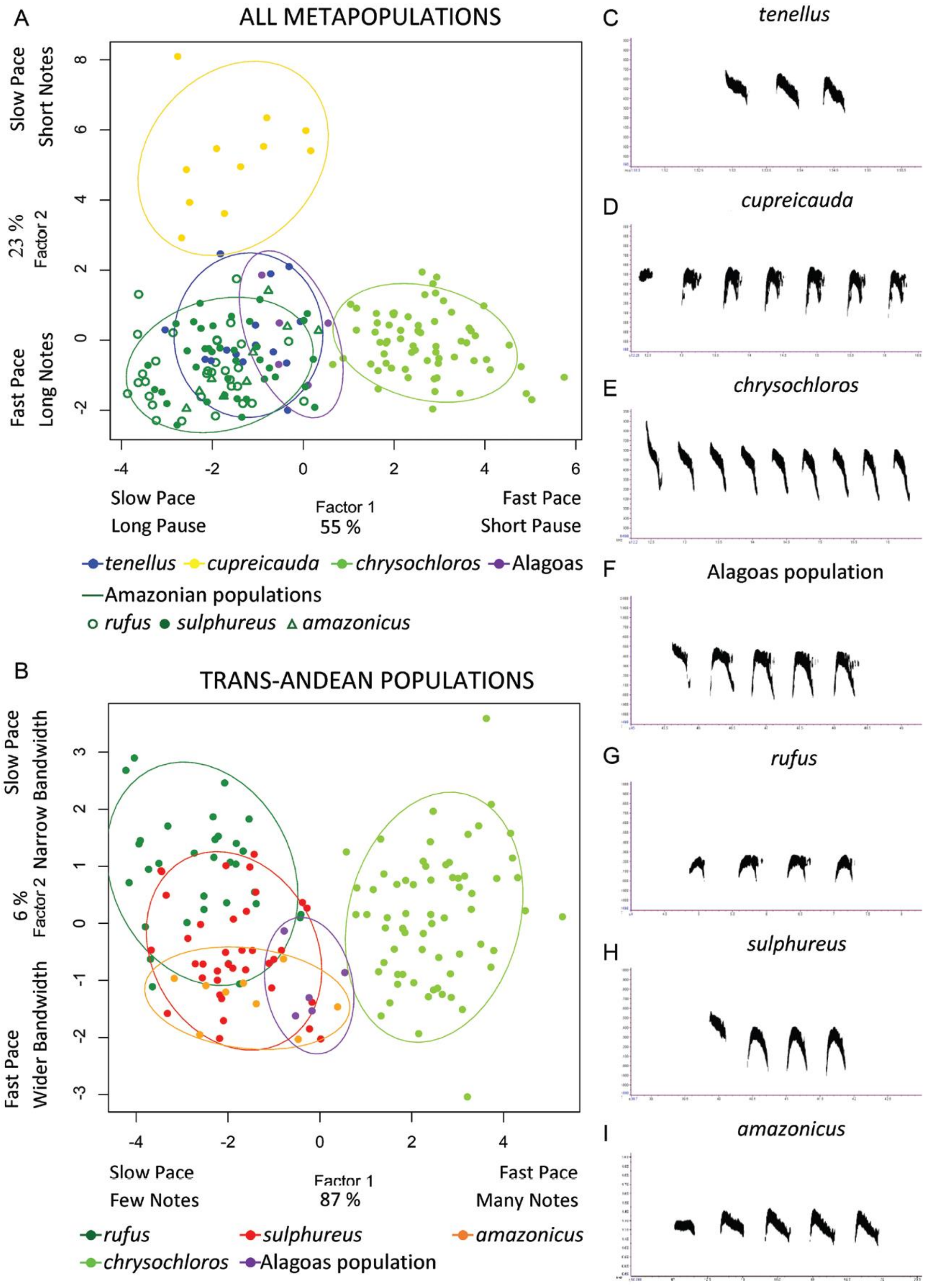

Figure 4. Linear discriminant factors one versus two of songs between (A) all populations in the *Trogon rufus* complex (different colours represent metapopulations and different marker shapes Amazonian populations) and (B) trans-Andean populations. Ellipses indicate 95% confidence intervals. C–I, show typical songs of: C, *Trogon tenellus* (ML57352, Costa Rica); D, *T. cupreicauda* (XC7020, W Ecuador); E, *T. chrysochloros* (XC85456, São Paulo, Brazil); F, Alagoas population (ML181311, Alagoas, Brazil); G, *T. r. rufus* (XC 119312, Amazonas, Brazil); H, *T. r. sulphureus* (ML30944, Peru); I, *T. r. amazonicus* (XC20747, Pará, Brazil).

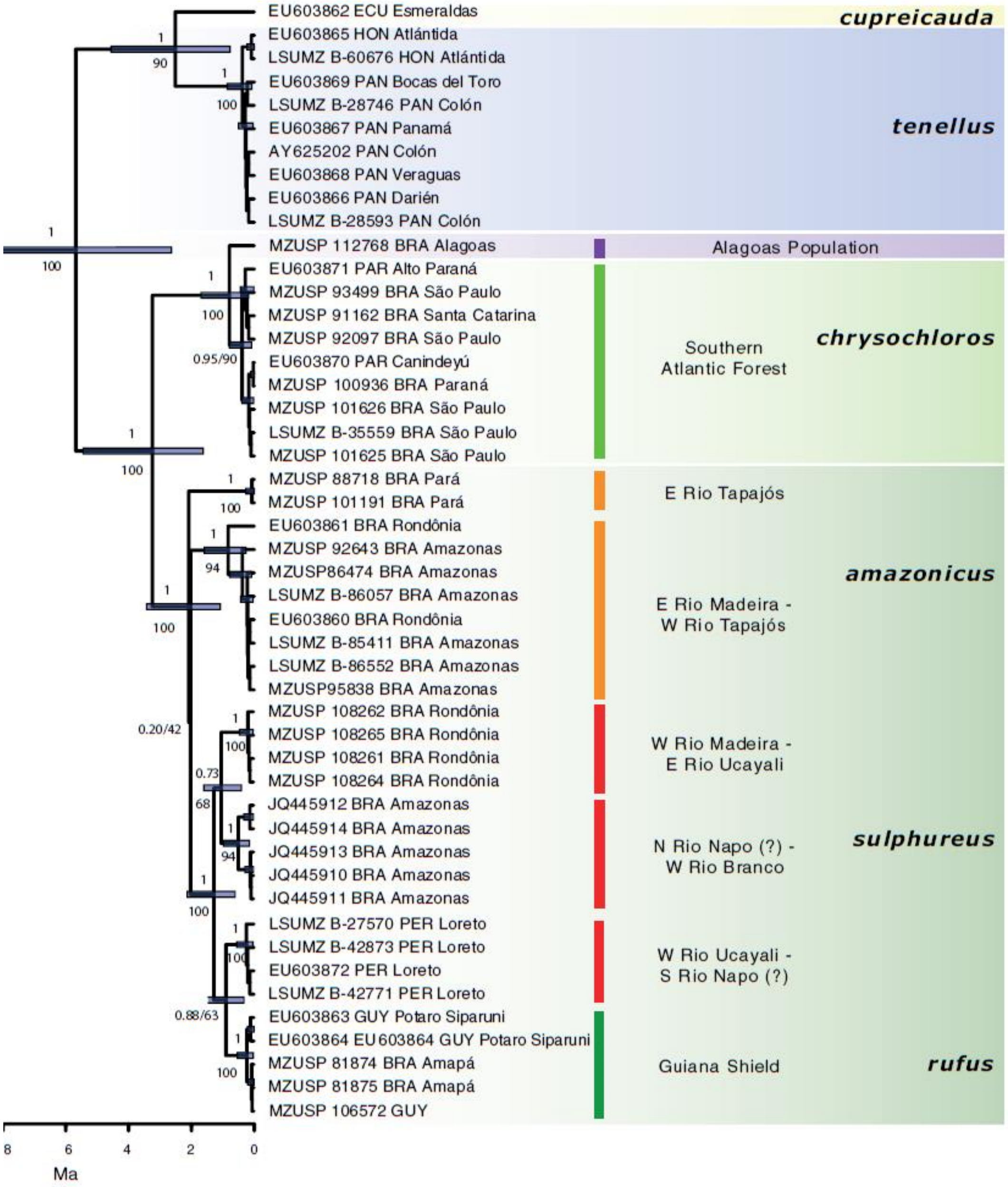

Figure 5C. 50% Maximum Clade Credibility time-calibrated gene-tree indicating phylogenetic relationships among clades in the *Trogon rufus* complex and outgroups derived from Bayesian inference based on ND2 sequences. A single unsupported and conflicting node between the Bayesian and maximum-likelihood topologies is collapsed (basal node of *amazonicus*). Numbers above nodes represent posterior probability values obtained for the Bayesian analysis. Numbers below nodes represent bootstrap support values for the maximum-likelihood gene tree. Node bars represent 95% confidence intervals for the highest posterior density (HPD) of divergence times.

B. Treat *cupreicauda* as a separate species from. *T. tenellus*

This is the other trans-Andean taxon, and it is sister to *tenellus* as one would predict on biogeographic grounds. If someone does not endorse *tenellus* as separate from the *rufus* group, then also endorsing *cupreicauda* as a separate species from either of those would be unlikely and would require special explanations. The vocal differences from other taxa are given as follows:

> Song: Compared to *T. tenellus*, the song has more notes per phrase, shorter note durations and generally lower note frequencies. It also has more notes per phrase, shorter note durations but a longer pause after introductory note, and generally higher note frequencies, especially for the introductory note, than in *T. rufus* subspecies. Compared to *T. chrysochloros*, the song has a slower pace, longer pause following the introductory note, generally longer note durations and generally lower note frequencies.

In their Factor Analysis (Fig. 4A), *cupreicauda* shows no overlap with the other taxa on the axis heavily weighted by pace (slow) and note length (short).

In terms of phenotype, the most striking feature to me is that the tail color is closer to distant *chrysochloros* than to any other taxon, and in fact, it differs the most in this feature from parapatric *tenellus*. The yellow eyering distinguishes it from all other taxa. See p. 28 for a listing and discussion of other color differences.

C. Recognize *T. muriciensis* as a species

As rightfully emphasized by Dickens et al., little comparative material was available:

> Diagnosis: We had little material available for the diagnosis of the new species *Trogon muriciensis*, particularly regarding external morphology, so caution must be taken until more information is collected. For comparison of plumage coloration and barred patterning, only the holotype was available. For morphometric traits, in addition to the holotype, we had measurements from the paratype and a ringed individual. For other discrete traits, we had photos from online depositories, in addition to those of the holotype (Supporting Information, Fig. S8) and ringed individual. For the song, we had slightly more material, with recordings from five separate individuals (including the holotype).

The vocal differences from other taxa are given as follows:

> Song: Compared to *T. chrysochloros*, the song of *T. muriciensis* has fewer notes per phrase, slower pace, longer note durations, longer pause following introductory note and generally lower note frequencies. It is similar to *T. r. rufus* but with generally more notes per phrase, higher introductory note frequencies and higher loudsong note low frequencies. Compared to *T. r. sulphureus*, it has wider bandwidth frequencies and generally more notes per phrase, whilst against *T. r. amazonicus*, it has faster pace, shorter note durations and a higher frequency introductory note. In relation to *T. tenellus*, it has a greater number of notes per phrase, shorter pause after the introductory note a generally lower introductory note high frequency, and generally lower peak and high loudsong note frequencies. It differs from *T. cupreicauda* by having fewer notes per phrase, longer note durations but a shorter pause after the

> introductory note. The bandwidth frequencies of the introductory and loudsong notes are generally wider than all other taxa, except *T. chrysochloros*.

In terms of phenotype (with their caveats concerning N), there is no single diagnostic character than I can see, but rather a combination of differences not shared with any other taxon. See p. 30 for an enumeration of the ways it differs from adjacent *chrysochloros* and members of the nominate group. Genetically (Fig. 5), the single sample is sister to the nine samples of *chrysochloros*; however, all nine are distant geographically, i.e. from São Paulo south (no samples from closer Bahia to RJ), so the closer relationship among the nine samples would be expected on the basis of isolation by distance alone.

D. Treat *chrysochloros* as a separate species from *T. rufus*

The vocal differences from other taxa are given as follows:

> Song: More notes per phrase, faster pace, shorter note durations and pause following introductory note, as well as higher note frequencies and wider introductory note bandwidth than *T. rufus* subspecies. The greater number of notes per phrase, faster pace and shorter durations are also diagnostic against *T. tenellus*. Compared to *T. cupreicauda*, the pace is faster, the pause duration shorter and frequencies usually higher.

In their Factor Analyses (Fig. 4A, B [beware that B is mis-labeled as Trans-Andean]), *chrysochloros* shows no overlap with the other taxa on the axes heavily weighted by pace, pauses between notes, and number of notes.

As for phenotype, this taxon has a relatively smaller bill that is more highly serrated than any other taxon. The density of barring on the wing panel and undertail coverts is diagnosably higher than for any other taxon. The Diagnosis says that it can be diagnosed from all other taxa (except evidently *muriciensis*) by its blue-gray to white eyering, but I think this is an error – see *tenellus*. See p. 25 and the plates for additional differences.

Discussion:

There is no doubt that all taxa are diagnosable at some phenotypic level. But at what rank? Dickens et al. noted introgression at the phenotypic level between *T. r. rufus* and *T. r. amazonicus*, *T. r. amazonicus* and *T. r. sulphureus*, and *T. r. rufus* and *T. r. sulphureus*; thus, they treated them as subspecies of the same species, which is the logical treatment. Contact zones are valuable test cases. Those three contact zones provide a defensible standard for seeing which phenotypic characters matter and which ones don't in terms of barriers to gene flow. In the vocal analyses, these three taxa mostly overlap in every feature analyzed, so song, as we have known empirically for 70+ years, matters. The characters that do not seem to matter in terms of barriers to gene flow, at least in this group of trogons, are tail color (which varies dramatically among the three), presence of subterminal tail band, presence of breast band (no surprise here, because there is intraspecific variation in this in other trogons), width of undertail barring, eye-ring color (ranges from blue to yellow even within *amazonicus*) and any mensural characters. In other words, almost every plumage and morphological character they measured is irrelevant as a barrier to gene flow, and so an important message of this paper is that all these characters may be irrelevant when considering species limits in trogons. (And I wish they had measured the implications of this for the highly flawed Tobias-BLI 7-point scoring scheme in terms of taxon ranking, but this would have added a tangent that would likely have increased the paper length by a page.)

The only other known contact zone is the one between *tenellus* and *cupreicauda* at the Panama-Colombia border, where there are no documented cases of introgression. Therefore, with the usual caveats, I think we can take the differences between *tenellus* and *cupreicauda* as potential isolating mechanisms, at least if they differ from those between taxa in the *T. rufus* group. These "if/then" comparative extrapolations come with obvious solutions, but in the absence of alternatives, they at least provide defensible rationale for extrapolation to allopatric taxa in terms of assessing potential barriers to gene flow, for better or worse. However, none of the phenotypic characters other than voice show any difference from those shown by Dickens et al. to not be potential isolating mechanisms in the *T. rufus* group.

That leaves vocalizations as the best proxy for estimating gene flow or lack of it.

I personally am not the person to assess what the vocal differences mean in a trogon framework – way too rusty. Are the reported differences comparable to species level differences in other trogon groups? I will leave that up to those of you who have extensive recent comparative experience with trogon voices. To me, when I listen to the recordings, all I hear are the features in common, which to me are considerable; but then again, many for-sure trogon species sound moderately similar. Is the absence of playback trials crippling given what I perceive as subtle differences? Ridgely & Greenfield (2001) (ergo also Mark Robbins) in the taxonomy volume of Birds of Ecuador were consistently alert to vocal differences between bird taxa west and east of the Andes in that country; however, they considered the differences between voices *cupreicauda* and *sulphureus* to be only "slight", which is slightly worrisome to me. So, I look forward to your comments.

A. Treat *tenellus* as a separate species from *T. rufus*. I hesitate to make a recommendation on this one other than noting my subjective feeling that I trust the authors of this paper on this one because of the depth to which they have gone in these analyses. Certainly, *tenellus* occupies a nearly unique multivariate space in the Factor Analysis of plumage characters in both sexes (Fig. 2), more distinctive than any other taxon other than *chrysochloros*. However, in terms of vocal characters, *tenellus* overlaps nearly completely with the *T. rufus* group despite differing in average ways from them.

B. Treat *cupreicauda* as a separate species from *T. tenellus*. Because there is no phenotypic evidence of gene flow between presumably parapatric *cupreicauda* and *tenellus,* I regard this alone as a sufficient criterion for species rank between those two. See the Discussion on p. 32 --- the authors were keenly aware of the significance of this. With *cupreicauda* vocalizations not overlapping with those of the other taxa in multivariate space, we have additional indirect evidence for species rank. I'm also impressed with the phenotypic differences between *cupreicauda* and *tenellus,* which are arguably greater than between any two adjacent taxa. The genetic distance between the two appears at least as large as that between any two adjacent taxa in the tree; however, there were no genetic samples from

Colombia, much less NW Colombia, closer to the contact zone than the sample of *cupreicauda* from Provincia Esmeraldas, and perhaps sampling within that ca. 700 km gap might produce a different result.

C. Recognize *T. muriciensis* as a species. With a tiny N and no truly diagnostic characters known, this one unfortunately represents the weakest case for species rank, as the authors noted.

D. Treat *chrysochloros* as a separate species from *T. rufus.* With its distinctive plumage characters and vocalizations, the evidence for this split is strong in my opinion.

Recommendations for SACC:

A. Treat *tenellus* as a separate species from *T. rufus*. I'm ambivalent on this one, which is awkward, because the evidence is nearly mandatory for treating its sister taxon *cupreicauda* as a separate species, which would make *Trogon rufus* a paraphyletic taxon if *tenellus* were included in *T. rufus* but not *cupreicauda*. On the other hand, at the population-species level I don't think monophyly, especially when only two labile mtDNA loci were sampled, is a valid requirement for species rank (as I have argued several times previously). With every passing month, it seems that new data reveal ancient hybridization among species that would make perilous the use of any single gene tree as representing the "true" history.

B. Treat *cupreicauda* as a separate species from *T. tenellus*. YES. I think these have to be treated as separate species given the parapatry with no sign of introgression.

C. Recognize *T. muriciensis* as a species. Ambivalent. The endangered status of this one should not, in my opinion, influence the taxonomic decision; otherwise, this undermines the credibility of the scientific process. Certainly this should be recognized as a separate subspecies, minimally, and I look forward to others' comments on this.

D. Treat *chrysochloros* as a separate species from *T. rufus*. YES on this one for reasons given above.

English names:

SACC voted in favor of three of the species splits detailed above (A, B, and D), but voted not to recognize *T. muriciensis* as a species. They then considered English names. After considerable discussion (see https://www.museum.lsu.edu/~Remsen/SACCprop921e-X.htm), SACC settled on the following English names:

Trogon tenellus - Graceful Black-throated Trogon
Trogon cupreicauda - Kerr's Black-throated Trogon
Trogon rufus - Amazonian Black-throated Trogon
Trogon chrysochloros (incl. *muriciensis*) - Atlantic Black-throated Trogon

Three of these (Graceful, Kerr's, and Amazonian) were the names recommended by Dickens et al. (2021), whereas Atlantic Black-throated Trogon was approved rather than the name that Dickens et al. recommended for *T. chrysochloros*, which was Southern Black-throated Trogon. The map makes it clear that Atlantic is an appropriate name for the Atlantic Forest species *T. chrysochloros* (including *muriciensis*).

The comments of Dickens et al. (2021) on the other three names were as follows;

> For common names, we propose retaining the compound name, 'black-throated trogon', to indicate the phylogenetic affinities of all the species involved. To differentiate between them, we propose using historical species names where available, namely the 'graceful black-throated trogon' for *T. tenellus* and 'southern black-throated trogon' for *T. chrysochloros*. Given the lack of historical common names for Amazonian taxa, we preferred naming each after the geographic region in which it is found, namely the 'Amazonian black-throated trogon' for *T. rufus*, 'Guianan black-throated trogon' for *T. r. rufus*, 'western black-throated trogon' for *T. r. sulphureus* and 'eastern black-throated trogon' for *T. r. amazonicus*. Given that 'Chocó trogon' is already the established common name for *T. comptus* Zimmer, 1948, we suggest naming *T. cupreicauda* 'Kerr's black-throated trogon' after the pioneering explorer in the region who collected the holotype.

NACC Recommendation and Effect on the Checklist:

Only one of the proposed species, *T. tenellus*, occurs in the NACC area. If we approve the SACC splits, this species will replace *T. rufus* on the Checklist under the name Graceful Black-throated Trogon. We recommend that NACC approve the four-species classification adopted by SACC, the English name for NACC species *T. tenellus*, and the English names, to be used in the Notes for *T. tenellus*, for the three species that occur exclusively in the SACC area.

A simple YES/NO for (a) the SACC splits, and (b) the SACC English names should be sufficient, with details of your rationale if voting NO.

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Submitted by: Van Remsen

Date of Proposal: September 2021, modified by Terry Chesser on 30 January 2025

SACC comments on taxonomic aspects of Proposal 921 (for the lengthy discussion of English names see https://www.museum.lsu.edu/~Remsen/SACCprop921e-X.htm):

Comments from Areta:
"A. YES. Plumage and genetics support this split. See comments on C regarding vocalizations.

"B. YES. Mostly based on the apparently distinctive vocalizations (despite methodological shortcomings), marked change in tail hue over a short distance near the zone of geographic proximity between them and less so based on the phylogenetic information (the single sample of *cupreicauda* is quite away from the southern limit of *tenellus*).

"C. NO to recognizing *muriciensis* as a separate species. The data is very limited and unsatisfactory to make this move. The shallow genetic divergence, lack of diagnostic plumage features, and similar vocalizations to *chrysochloros* indicate to me, at most, subspecific status. Regarding the vocalizations, I have trouble in seeing the most basic parameters of the spectrograms in Figure 4 (e.g., time and frequency values), but these seem to have been built using different scales, thereby presumably distorting the similarities and differences among vocalizations. It also seems to me that there are important vocal differences among sexes in *Trogons* (fleetingly disregarded by the authors) that were not taken into account when making the comparisons; this can easily be heard and seen in spectrograms when couples of birds duet or respond to each other. I have trouble in matching the presumably "typical" songs depicted in the spectrograms to results of the discriminant analyses: songs which look very different overlap widely, suggesting that measurements were not able to capture key features of the sounds or that the spectrograms are not so typical. Finally, several recordings of *chrysochloros* sound exactly like *muriciensis*.

"D. YES to splitting *chrysochloros* from *rufus*. Plumage, vocal and genetic data agree in this split."

Comments from Bonaccorso:
"A. YES, but it makes more sense to accept 921B.

"B. YES. *T. tenellus* and *T. cupreicauda* seem to have reached enough phenotypic differentiation (especially tail color; I love the tail-hue figure!) and vocal differences. However, I am not impressed by their genetic differences because the range of the species along Colombia was not covered in the phylogenetic analysis, thus, differences could result from isolation by distance. However, the possibility of parapatry and lack of intermediate individuals supports the potential lack of gene flow.

"C. "YES, but a bit hesitantly (I agree with Nacho that the data are sparse). Phenotypically, the Alagoas population is not so different from *chrysochloros*. However, vocally (and I am no expert), it seems to be very different. Still, the low sample size (N = 5) may not reveal the variation spectrum of vocalizations in the Alagoas population. Genetic differentiation is weak and based on one sample, whereas genetic sampling of *chrysochloros* does not cover localities closest to the Alagoas population. In short, I think the decision should be based on the acoustic data if some of you (experts on the subject) think the level of differentiation merits species status.

“I disagree with the position that no special considerations need to be made when the decision is about endangered species. I think there has to be asymmetry when considering species that may or may not be endangered. If, in the end, they are no good species, some money and effort may be lost, but at least the habitat of the species may get some level of protection for a while (which, needless to say, will benefit many other species). It is much worse to err in the other way. Not recognizing an endangered species and waiting for more data may have much worse consequences, especially in cases like this, where the range seems tiny. Also, from my experience with the Blue-throated Hillstar, the fact that we recognize new, endangered species does not guarantee immediate incorporation into IUCN lists (even after repeated requests to BirdLife International). Meanwhile, deforestation continues, and funds are not available for research, research-based conservation, or protection. So, our delay in recognizing these species may have fatal consequences under the current circumstances.”

“D. YES. In this case, vocal and genetic evidence seems to support species status. I don´t think phenotypic differentiation is so strong or useful in defining species status in this case. *T. sulphureus* seems very different from *rufus* and still, they hybridize.”

Comments from Pacheco: “A, B and D. YES. Plumage, genetics, and vocal repertoire provide a good endorsement for treating these taxa at the species rank.

“C. A vacillating YES, for the exact reasons explained by Elisa. Obtaining more data (vocalizations, genetics) and conservation measures for this population becomes dramatic races against the clock.”

Comments from Stiles: “YES to all except C.”

Comments from Jaramillo: “YES to A, B, C, D. I guess it would be good to give a set of opinions and viewpoints, but they have all been made by others so far. I do think I am giving benefit of the doubt to the researchers who put this together in painstaking detail, and have provided us a framework. In a sense, I do not feel qualified to counter their arguments on these opinions.”

Comments from Lane:
“A. YES.
“B. YES.
“C. No. The characters distinguishing its voice from *T. chrysochloros* seem weak, and weakened even more by the recording on X-c recorded in 2021 <https://xeno-canto.org/628115> that has a song with more notes. I just don't think this taxon will have significant defining characters once it is given more scrutiny.
“D. YES.”

Comments from Remsen: YES to all except C, for which the evidence for species rank is weak, as noted in the proposal and several comments."

Comments from Robbins: "Although the authors have gone into extraordinary detail from a plumage, soft part, and vocal standpoint, the genetic data are weak and in the case of proposal B, potentially suffer from the lack of sampling. I would prefer to have more genetic data before making a definitive recommendation; however, that might not be forthcoming for some time. The reason I put emphasis on that data set is that there are not consistent differences in morphology and vocalizations across this complex. It should go without saying that differences in plumage among most of these taxa are relatively small, with male *tenellus* and *rufus sulphureus* standing out in dorsal tail coloration. Moreover, much of those dorsal tail differences are represented within the Amazonian populations, i.e., *amazonicus*, nominate, and *sulphureus*. Thus, if one treats Amazonian populations as a single species then dorsal tail pattern becomes questionable as a species-defining character.

"A) NO, because of the above concerns with the genetic data coupled with overlap between *tenellus* and the Amazonian group of *rufus* in vocalizations. Also, see comments under B.

"B) NO, we need data where *tenellus*, at least theoretically, comes into contact with *cupreicauda* at the Panamanian/Colombian border. Note that there is only a single genetic sample of *cupreicauda*, and it is from Esmeraldas, far from the potential contact zone. That interface might shed light on the importance of dorsal tail pattern (given the differences between these two) and the importance of vocalizations. Depending on those data, I could see where *tenellus*/*cupreicauda* are treated as a species and it is considered a separate species from cis-Andean populations.

"C) NO, data are too limited to assess taxonomic status.

"D) NO, although given current data, I lean more towards recognizing this as a species, primarily because of differences in vocalizations as outlined in Fig. 4 in Dickens et al. and the disjunct distribution. However, to be consistent with what I have outlined above I vote No. Note that plumage and perhaps soft part colors are not unique to *chrysochloros,* and the limited genetic data indicate that it is close to Amazonian populations (no surprise)."

Comments from Zimmer:

"A. YES. Despite lack of diagnostic vocal differences, I have a hard time accepting that there are two conspecific taxa whose ranges are widely disjunct, and separated not only by the Andes, but by the presence of another taxon in the same group (*cupreicauda*), that is demonstrably different from both the trans-Andean taxon and the cis-Andean taxon, and that *cupreicauda* is parapatric with *tenellus* without evidence of intergradation between the two.

"B. YES. Although it is not listed as a subset of this Proposal, it would also follow that I would also vote "YES" on treating *cupreicauda* as a separate species from *rufus/amazonicus/sulphureus*. As Van noted (in the Proposal), phenotypic/plumage characters are all over the map in "Black-throated Trogon" (*sensu lato*), and even within some subspecies groups (the Amazonian/Guiana *rufus*-group), and really don't seem to work as isolating mechanisms. Given that, to quote Van "That leaves vocalizations as the best proxy for estimating gene flow or lack of it." Accordingly, to my ears, songs of *cupreicauda* are distinct from those of *tenellus* to the north and west, and those of the various Guianan/Amazonian cis-Andea forms to the east and south. The absence of phenotypic evidence of gene flow between these two parapatric populations is further supporting evidence, which, admittedly, is weakened somewhat by the lack of sampling from closer to the potential contact zone."

"C. NO, at least not for now. Sample sizes are just too small in my opinion, particularly given the absence of data from *chrysochloros* from Bahia to Rio de Janeiro, more proximate locales, which, when added to the mix, could well narrow any apparent gaps regarding potential differences in vocalizations, plumage, and genetics. I would note, however, that from the vocal samples I've listened to, *muriciensis* is closer (at least in song characters) to Amazonian populations than to fellow Atlantic Forest taxon *chrysochloros.* This would certainly fit an established pattern of several Atlantic Forest taxa from the Alagoas-Pernambuco center of endemism having their closest relatives distributed in SE Amazonian rather than the S Atlantic Forest. For example, think *Automolus lammi* (closer vocally and genetically to *A. paraensis* than to *A. leucophthalmus*), or *Thamnophilus aethiops distans*, or *Hemitriccus griseipectus naumburgae.* There's also the broader pattern of widespread Amazonian taxa having highly isolated but closely related populations in the northern part of Brazil's Atlantic Forest (south through Espírito Santo to N Rio de Janeiro for many of them) – think Cinereous Antshrike, Cinereous Mourner, Ringed Woodpecker, Bright-rumped Attila, Thrush-like Wren, White-winged Potoo, etc. Obviously, there are other species-pairs where the close affinities are between Pernambuco regional specialties and SE Atlantic Forest birds (*Myrmotherula snowi/unicolor* comes rapidly to mind), but the point is, that there are numerous examples of taxa from the forest fragments of Alagoas/Pernambuco being more closely related to Amazonian counterparts than to any taxa from farther south in the Atlantic Forest. So, it's not implausible to me that *muriciensis* could be distinct from *chrysochloros,* but I do think we need more data points to be confident that is the case."

"D. YES. This one is the most different vocally (perhaps, along with *cupreicauda*) from any of the others, and those vocal differences are congruent with phenotypic distinctions (even if it turns out these aren't important as isolating mechanisms), ecological differences, genetic differences, and established biogeographic patterns."

Change the English group name of species of *Amazona* from “Parrot” to “Amazon”

Note: This is a modified version of SACC Proposal 1026, which passed unanimously 10-0.

The three major global bird lists that provide annual updates, the IOC World Bird List, the HBW/BirdLife International Checklist, and the eBird/Clements Checklist, currently use the group name “Amazon” instead of “Parrot” for all members of the genus *Amazona*. Many other sources have used “Amazon” over a period of years, and usage in influential works seems to be increasing. These include Wolters (1975, *Die Vögelarten der Erde*), Collar et al. (1992, *Threatened Birds of the Americas*), Gwynne et al. (2010, *Birds of Brazil: the Pantanal and Cerrado of Central Brazil*), Athanas and Greenfield (2016, *Birds of Western Ecuador*), McMullan and Navarrete (2018, *Fieldbook of the Birds of Ecuador*), Freile and Restall (2018, *Birds of Ecuador*), Kirwan et al. (2019, *Birds of the West Indies*), Hilty (2021, *Birds of Colombia*), Pearman and Areta (2021, *Birds of Argentina*), Dyer and Howell (2023, *Birds of Costa Rica*), and Howell and Dyer (2023, *Birds of Belize*). NACC and SACC, however, have traditionally used the name “Parrot” for species of *Amazon*, and it seems likely that other sources that use “Amazon” do so explicitly to follow NACC/SACC. SACC recently voted to change the English group name of *Amazona* species to “Amazon”, so that three major global lists and SACC now use this group name, making NACC the obvious outlier.

“Amazon” is also widely used for members of *Amazona* in the pet trade and aviculture. Google results from July 2024 give a good illustration of the strength of the use of “Amazon” in popular references to *Amazona* parrots.

Search parameters	**No. hits**
Yellow-headed Amazon	10,700,000
Yellow-headed Parrot	836,000
Amazona oratrix	3,300

We see several other significant advantages to embracing the name “Amazon” for all members of *Amazona*:

1) Parrots are familiar and highly recognizable at a family level, so applying a name like “Amazon” will not confuse the relationships for ornithologists, birders, or the general public. When an *Amazona* is seen, it would be generally understood to be a parrot.

2) Within Psittacidae, the use of “parakeet” and “parrot” represent little more than morphotypes, with no strong tie to phylogeny. In general, parakeets are small, slender, and long-tailed, whereas parrots (except in Australia) are large, thick-set, and short-tailed (*Rhynchopsitta* and *Ognorhynchus* being notable New World exceptions in being long-tailed). The result is a somewhat dizzying array of similar names with very little to inform more specific relationships, physiology, or other traits. Trochilidae and Tyrannidae have similar issues, but thankfully use a much wider array of names that convey important aspects of appearance and phylogeny. Applying “Amazon” for one of the most diverse genera would be a major aid to understanding and although the NACC has explicitly not tried to make English names

taxonomically concordant (e.g., warbler, tanager, sparrow, etc., are used as morphotypes), they have also acknowledged that, when not too disruptive, such changes can be beneficial (e.g., the recent adjustment to use Blue-throated Mountain-gem to help clarify relationships).

3) The name "Amazon" is almost identical to the genus *Amazona*, making it easy to remember both the English name and the scientific name. This would help birders and ornithologists to remember which species are in *Amazona* as opposed to other genera. This is useful for field identification because species of *Amazona* have a distinctive, shallow-winged flap and vocal similarities. Since the genus is so distinctive, it is helpful to have an English name that effectively identifies these differences.

4) There are 60 recognized species in the New World with the name parrot (Table 1), spread across 12 genera. Six of those genera are monotypic and, other than *Amazona*, the most diverse are *Pionus* and *Pyrilia* with seven members each. *Amazona* accounts for more than half of the species with the name "Parrot":

Genus	**No. species**
Alipiopsitta	1
Deroptyus	1
Graydidascalus	1
Ognorhynchus	1
Pionopsitta	1
Triclaria	1
Pionites	2
Rhynchopsitta	2
Hapalopsittaca	4
Pionus	7
Pyrilia	7
Amazona	32

Table 1. Genera using the name "parrot" in the New World.

5) Usage of "Amazon" instead of "Parrot" for *Amazona* species is already very widespread and would not involve a learning curve or much disruption.

6) With so many species from diverse lineages known as "parrot" worldwide, there are several names that are confusing for birders and, accordingly, eBird sometimes sees people picking the entirely wrong taxon. By providing more clarity on *Amazona* vs. other parrots, these problems will be alleviated somewhat. Below are four of the more confusingly similar names for unrelated taxa (the first two species of *Amazona* occur in the NACC area):

a. White-crowned Parrot *Pionus senilis* vs. White-fronted Parrot *Amazona albifrons*
b. Red-capped Parrot *Purpureicephalus spurius* vs. Red-crowned Parrot *Amazona viridigenalis*
c. Red-winged Parrot *Aprosmictus erythropterus* vs. Orange-winged Parrot *Amazona amazonica*
d. Turquoise Parrot *Neophema pulchella* vs. Turquoise-fronted Parrot *Amazona aestiva*

Effect on the Checklist:

There are 32 species of *Amazona*, according to NACC and SACC taxonomy (followed by Clements et al.); other taxonomies have recognized additional species, with splits in Mealy Parrot *Amazona farinosa* and Red-lored Parrot *Amazona autumnalis* currently being the most widely embraced.

If accepted, each of the names below would change to xx Amazon (e.g., Festive Amazon, Vinaceous-breasted Amazon, etc.).

Scientific Name	**English Name**	**NACC**	**SACC**
Amazona festiva	Festive Parrot		x
Amazona vinacea	Vinaceous-breasted Parrot		x
Amazona tucumana	Tucuman Parrot		x
Amazona pretrei	Red-spectacled Parrot		x
Amazona viridigenalis	Red-crowned Parrot	x	
Amazona finschi	Lilac-crowned Parrot	x	
Amazona autumnalis	Red-lored Parrot	x	x
Amazona dufresniana	Blue-cheeked Parrot		x
Amazona rhodocorytha	Red-browed Parrot		x
Amazona arausiaca	Red-necked Parrot	x	
Amazona martinicana	Martinique Parrot	x	
Amazona versicolor	St. Lucia Parrot	x	
Amazona auropalliata	Yellow-naped Parrot	x	
Amazona oratrix	Yellow-headed Parrot	x	
Amazona ochrocephala	Yellow-crowned Parrot	x	x
Amazona barbadensis	Yellow-shouldered Parrot		x
Amazona aestiva	Turquoise-fronted Parrot		x
Amazona agilis	Black-billed Parrot	x	
Amazona albifrons	White-fronted Parrot	x	
Amazona xantholora	Yellow-lored Parrot	x	
Amazona collaria	Yellow-billed Parrot	x	
Amazona leucocephala	Cuban Parrot	x	
Amazona ventralis	Hispaniolan Parrot	x	
Amazona vittata	Puerto Rican Parrot	x	
Amazona farinosa	Mealy Parrot	x	x
Amazona kawalli	Kawall's Parrot		x
Amazona imperialis	Imperial Parrot	x	
Amazona violacea	Guadeloupe Parrot	x	
Amazona brasiliensis	Red-tailed Parrot	x	
Amazona guildingii	St. Vincent Parrot	x	
Amazona amazonica	Orange-winged Parrot		x
Amazona mercenarius	Scaly-naped Parrot	x	

Recommendation:

A simple YES (strongly recommended) or NO vote would suffice, with rationale provided if a NO vote.

Submitted by: Marshall Iliff

Date of Proposal: July 2024, modified by Terry Chesser on 31 January 2025

Votes and comments on SACC Proposal 1026:

<u>Comments from Remsen</u>: “YES. The world is “voting with its feet”, or fingers in this case, and I see no point in being an outlier and plenty of disadvantages. More importantly, having a 1-1 match between genus name and group name has lots of positives, as Marshall outlined in the proposal.”

<u>Comments from Zimmer</u>: “YES” for all of the reasons summarized my Marshall in the Proposal.”

<u>Comments solicited from Peter Kaestner</u>: “As a non-voting comment, I too would support Marshall’s proposal to use the moniker “Amazon” for the parrots in the genus *Amazona*.”

<u>Comments from Rasmussen (voting for Robbins)</u>: “I definitely think "Amazon" is the way to go here, for all the reasons stated in the proposal. The only reason I can see not to do so is that by no means are all found anywhere near the Amazon, but that carries little weight in my view especially given the genus name.”

<u>Comments from Josh Beck</u>: “YES. I think it makes sense and is good to align with other uses and good to highlight the grouping of these species / help people clue in on *Amazona* species by flight style and voice. I personally am not bothered about aligning with the aviculture trade, and while the general public likely won't know what an Amazon is, they also are not likely to know that the name exists, and the general name of parrot will continue to serve just as well for anything that looks like a parrot.”

<u>Comments from Don Roberson</u>: “I'm an enthusiastic "YES" for all the reasons outlined by Marshall. In fact, I'd thought that change had been years ago.

“I think it is wise to do what reasonably can be done to reduce the numerous English names that involve "warblers", "flycatchers", "parrots," and other words that span multiple unrelated bird families globally. There are 4-5 families of "parrots," six or so families of "flycatchers," and 10 or more families with English-named "warblers." In the vast majority of situations, stability in English names will (and should) prevent widespread, disruptive changes, but this proposal easily passes the test, even for Mexican species of Amazona that (presumably) would require NACC adoption.

“It will also serve as precedent that when changing an English name to better reflect the formal scientific name (e.g., adoption of Schiffornis and many others) it is not required that the English name (Amazon) exactly match the Code name (*Amazona*). Further, once adopted, it becomes stable, and not dependent on changing taxonomic revisions (e.g., Elaenia applies to both the genera *Elaenia* and *Myiopagis*). Although of almost no concern to SACC, might there come a day when the flycatcher genus *Empidonax* might be renamed, in English, to "Empid" instead of "Flycatcher," or if that is too slangy, "Gnat-Tyrants"? [The late, great Rich Stallcup liked to call them "gnat-kings", from the Greek root of *Empidonax*.] Such of change would finally remedy the global problem of having two Dusky Flycatchers and two Gray Flycatchers in the world. The current global checklist compromise of tacking on "African" to the Old World taxa is not an actual solution. But turning back to parrots that may become Amazons, will there come a time when some currently unanticipated split leave the SACC debating whether the English name should be Amazonian Amazon or, more simply, Amazon Amazon. Oh, joie de vire.”

Comments from Jaramillo: “YES. I was going to be a no vote on this largely as a knee jerk reaction to the fact that Amazon is the cage bird trade for them. I am not a fan of the cage bird trade, and dislike the names they use. Conure is just an ugly sounding word, but it is a knee jerk based on the fact that I am not pro wild bird trade in any way.

“But reading the proposal, I see there are benefits, and Marshall is certainly in tune with the issues of incorrect logging of certain parrot species on eBird, and that this may offer a simplified take, where there are fewer parrot species to deal with in South America, as half will then be Amazons. It makes sense. It does not detract in any meaningful way, and people seem to like the name. So I have moved to the yes side.”

Comments from Andrew Spencer (voting for Claramunt): “YES. I very much like having a direct relationship between the genus and a unique group name, especially in a family as diverse as parrots. I've been calling them Amazons for years. I also found that while guiding it was handy to have a term for *Amazona* parrots when teaching the people I was guiding about parrot ID, and how to narrow the options down quickly. Now if only they were easier to ID to species once narrowed down to genus.”

Comments from Lane: “YES, but reluctantly. I have really seen the use of “Amazon” and other parrot group names such as “Conure” as the cagebird trade’s labels for these parrots, and felt that it was good to separate the names used in the scientific and birding literature from that world. I guess that I was ignoring the fact that I was mostly seeing the AOS NACC/SACC names vs the usage in the rest of the world rather than a difference between bird trade vs science/birding, so I will have to reconsider my views on the names here. In addition, Marshall makes a good point about the adoption of Amazon to “loosen up” constraints on availability of names that might otherwise compete with other “Parrot” spp elsewhere.”

Comments from Stiles: “YES. I will go along with Amazon as an E-name for the *Amazona* species it makes a lot of sense in various ways (and is on the whole, OK in Spanish as well.

Comments from Donsker (voting for Bonaccorso): “SACC 1026. I strongly vote for using "Amazon" for members of the genus *Amazona* as discussed in Marshall's proposal.”

Treat Red Grouse *Lagopus scotica* as a separate species from Willow Ptarmigan *L. lagopus*

Background:

Red Grouse *Lagopus scotica* was described by Latham as *Tetrao scoticus*. It was treated as a species by Peters (1934) and Witherby et al. (1941) but, probably due to the description of an apparently intermediate subspecies (Salomonsen 1936), it later came to be generally regarded (e.g., by Vaurie 1965, Voous 1973, Wolters 1975) as a subspecies of Willow Ptarmigan *L. lagopus*. However, some more recent sources (e.g., Madge and McGowan 2002) have again treated *L. scotica* as a separate biological species.

New Information:

Both AOU (1998) and the current Clements/eBird list (Clements et al. 2024) treat *scotica* as a group under *L. lagopus*, and the IOC list (Gill et al. 2024) and HBW/BirdLife list (HBW and BirdLife International 2024) treat *scotica* as a subspecies of *L. lagopus*. However, WGAC recently voted, by a slim 4-3 margin, to recognize *L. scotica* as a separate species based on differences in plumage and genomics; this was based largely on Sangster et al. (2022), which serves as the basis for much of the following discussion. The plumage differences between *scotica* and the rest of *L. lagopus*, which were the original reasons for considering *scotica* specifically distinct, are pronounced: its plumage is all dark, lacking the white wing patches and white belly of the other subspecies, and it does not molt into the distinctive white winter plumage of the other subspecies. In association with the latter difference, *scotica* undergoes only two molts per year rather than three. The only exception to the general rule among the other subspecies of *L. lagopus* is *variegata* (Salomonsen 1936), which is found on small islands off the coast of Norway. This subspecies, although it has a mostly white winter plumage, is characterized by black or brown blotches in its winter plumage, and also keeps its summer plumage longer into the winter months than do other subspecies of *L. lagopus*. Thus, it could be considered intermediate between *scotica* and other subspecies of *lagopus* (e.g., by Vaurie 1965 and Cramp and Simmons 1980), although to the impartial observer it seems clearly to be closer to the other subspecies than to *scotica*, if intermediate at all.

Several studies of mitochondrial DNA (Gutiérrez et al. 2000, Lucchini et al. 2001, and Höglund et al. 2013) produced mixed results, but in most of these *scotica* was not distinctive. For example, in most trees in Gutiérrez et al. (2000), *scotica* was sister to their sample of *lagopus* from Finland, and these were sister to their sample of *lagopus* from Alaska, and Höglund et al. (2013) found *scotica* to be nested within their Scandinavian samples of *lagopus*. Studies of nuclear DNA, however, have typically found *scotica* to form a monophyletic group sister to the rest of *L. lagopus*. Quintela et al. (2010) sequenced 76 SNPs and found that their 62 samples of *L. lagopus* from Scandinavia were distinct, with very little admixture, from their 32 samples of *scotica*, as shown in their STRUCTURE plot (Fig. 1). Although Höglund et al. (2013), using a less extensive set of SNPs, could not distinguish their 5 samples of *scotica* from 75 samples of *L. lagopus* from Scandinavia, North America, and Russia, Kozma et al. (2019) sequenced genomes of 34 individuals of *scotica*, *L. lagopus*, and Rock Ptarmigan *L. muta* and found that *scotica* and the rest of *L. lagopus* are sister taxa, with *L. muta* sister to them (Figs. 2, 3).

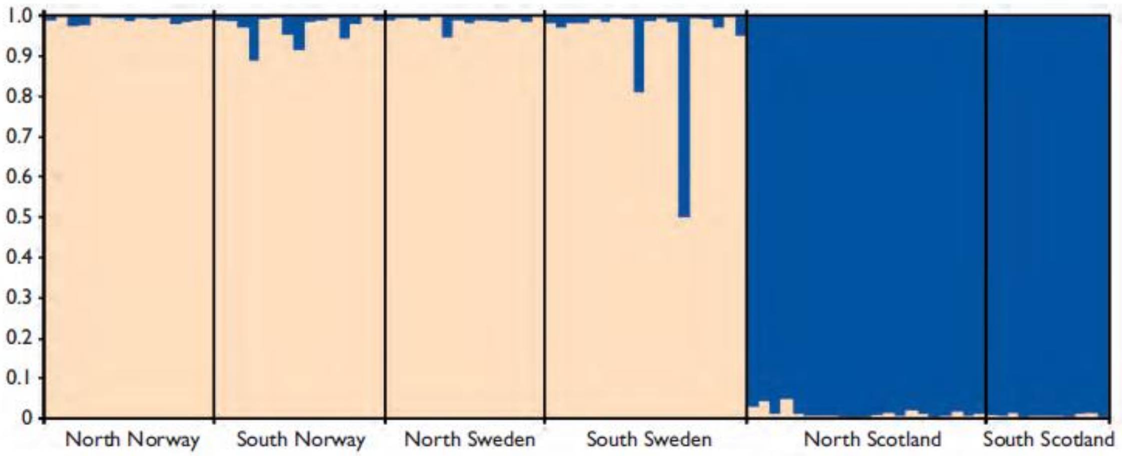

Figure 1. STRUCTURE plot based on 76 SNPs from 62 individuals of *L. lagopus* from Scandinavia (left side) and 32 individuals of *scotica* from Scotland (right side). From Quintela et al. (2010).

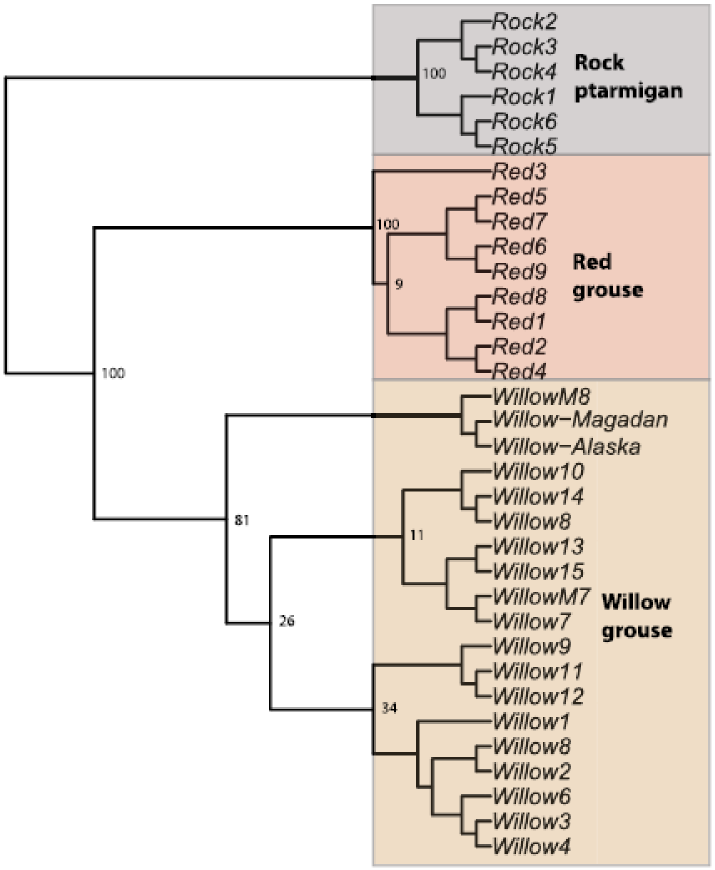

Figure 2. Maximum likelihood tree based on genomes of 34 individuals of *Lagopus muta*, *L. lagopus*, and *scotica*. Numbers at nodes represent bootstrap support values. From Kozma et al. (2019).

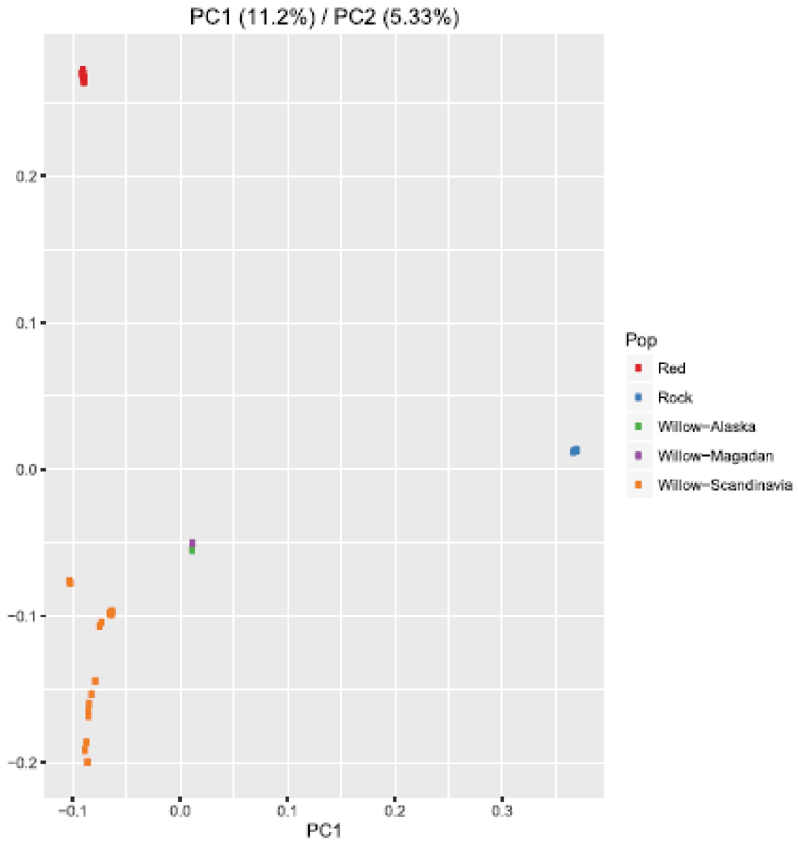

Figure 3. PCA of 34 genomes of *L. muta* (Rock Ptarmigan), *L. lagopus* (Willow Ptarmigan), and *scotica* (Red Grouse) from Kozma et al. (2019). Note that Red Grouse and Willow Ptarmigan are separated only on PC2, whereas both are separated from Rock Ptarmigan on both axes.

Despite claims of integrative taxonomy, Sangster et al. (2022) did not discuss vocalizations, a key character that readily distinguishes the three universally recognized species of ptarmigan: *L. lagopus*, *L. muta*, and White-tailed Ptarmigan *L. leucura*. In fact, Pam's quick review of vocal recordings concluded that if *scotica* differs vocally from *L. lagopus*, it must only be in minor ways.

Recommendation:

The case for species status under the BSC seems weak. The differences between *scotica* and the rest of *L. lagopus* in plumage and genetics are clear, but whether these are related to reproductive isolation in these geographically disjunct species is not clear and was not addressed in Sangster et al. (2022), raising the possibility that *scotica* is merely a well-marked and geographically isolated subspecies. Differences in vocalizations between *scotica* and the rest of *L. lagopus* would provide stronger support for species status under the BSC, but these appear to be lacking. Thus, we are required to determine species status based on differences in plumage and DNA, with few if any associated vocal differences. Perhaps not surprisingly, two members of WGAC who voted in favor of species status characterized this as a judgment call, and the deciding voter in the 4-3 decision wrote that he could toss a coin on the issue, but in the end cast a weak vote in favor of species status.

My general inclination is to accept global or Old World views on taxa that either do not occur in our area or occur in our area only as accidentals. Because *scotica* is not found in the NACC area, this would mean accepting it as a separate species from *L. lagopus*. However, we have not always gone along with the global view if we felt that the data did not support species status (e.g., the proposed separation of *Garrulax taewanus* from Hwamei *G. canorus*, which we rejected in 2019). The case against BSC status for *L. scotica* is not as strong as that against *G. taewanus*, which freely interbreeds with *G. canorus* on Taiwan; nevertheless, the data supporting species status for *scotica* are less than what we generally require. I see this issue from both perspectives and have no strong recommendation one way or the other, with perhaps a slight preference for going along with the global view despite the less than stellar evidence.

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Submitted by: Terry Chesser

Date of Proposal: 11 February 2025

Treat *Asio wilsonianus* as a separate species from Long-eared Owl *A. otus*

Background:

The Long-eared Owl *Asio otus* is a Holarctic species found widely in Eurasia and North America. Two groups and four subspecies are recognized by most authorities. The Old World group has two subspecies: widespread nominate *otus* Linnaeus, 1758, is found from Western Europe across the Palearctic to the eastern Russian Far East, and restricted range *canariensis* van Madaráz, 1901, is endemic to the Canary Islands, where it is found on islands such as Gran Canaria and Tenerife and recently the Fuerteventura islands. Subspecies *canariensis* is 10% smaller than European birds and has the upperparts, upperwing coverts, and underparts more heavily mottled and vermiculated than nominate *otus* (Cramp 1985, Robb 2015). Its calls are like those of European birds (Robb 2015). Farther west on the Azores, the Long-eared Owl is also resident but their plumage pattern is like that of European birds. Cramp (1985) detailed minor differences between European and East Asian birds, the latter being slightly larger and with a slightly paler ground color with slightly less heavy dark speckling and vermiculation, but the differences are too minor to recognize additional Eurasian subspecies.

Within North America, two subspecies are generally recognized, *wilsonianus* Lesson, 1830, and *tuftsi* Godfrey, 1948. Western North American populations (*tuftsi*) have been said to average slightly paler but most authorities now consider the differences minor, clinal, and perhaps questionable given the nomadic character of the species. Some sources (Rea 1983, Unitt 1984, Dickerman 2015, Gibson and Withrow 2015, Pyle 2022) do not recognize *tuftsi*, treating all North American birds as *wilsonianus.* Unitt (1984) pointed out that K. C. Parkes (pers. comm. to A. M. Rea) looked at specimens of Long-eared Owls and doubted that *tuftsi* was valid. Unitt (1984) also pointed out that a bird banded in northern San Diego County at Escondido on 22 April 1934 was recovered at Corbeil, Ontario, Canada on 9 October 1934 (Lincoln 1936); this transcontinental migration was at odds with the then described ranges and movements of the species. Dickerman (2015) quoted Rea's (1983) statement that males are considerably darker than females (n=30), but Dickerman's (2015) analysis of a larger series of specimens (about 180) indicated that the darker and lighter birds likely represented color morphs and not sexual dimorphism. His results contradicted Rea's (1983) contention that most males are pale whereas most females are dark.

New information:

Morphological differences between New World and Old World

Although differences within Eurasia and within North America are relatively slight, the morphological differences between the geographically well-separated New and Old World birds are pretty striking. These differences were well-detailed by Cramp (1985). Briefly, North American birds are darker with both heavy dark ventral streaks and crossbars. They also have a darker and more richly pigmented rufous facial disk with a distinct blackish border. Eurasian birds are paler overall with less distinct darker markings and a tawny to grayish facial disc with an indistinct dark border (see photo of Buldir Island bird below, Figure 5, page 182, in Gibson et al. 2018). Ventrally the vertical streaks greatly predominate over any crossbars. Another distinctive difference is that New World birds have golden-yellow eyes while the irides are reddish orange in Old World birds. Pyle (1997) mentioned that nominate *otus* has 1-3 fewer bars on the flight feathers and has a large pale base to the outer primaries. The Buldir Island photo with the wing slightly spread shows this pattern well and appears quite different from photos showing the dorsal aspect of *wilsonianus* in flight. A check of specimens would be worthwhile to quantify this potential difference in wing pattern as well as variation in the face pattern.

Figure 5. Long-eared Owl (*Asio otus*), 8–11 Jun 2016, Buldir Island, w Aleutian Islands, Alaska.

Photo by M. Mudge and K. Pietrzak

Upperwing of female *A. o. wilsonianus* collected in Nebraska. University of Puget Sound, Wing and Tail Image Collection (https://www.jstor.org/stable/community.36062206)

Upperwing and facial pattern of a bird in flight, France (Thomas Galewski, ML291877951)

Behavioral differences between New World and Old World birds

Long-eared Owls differ from other owls in that the species forms winter roosts of several individuals in close association. The size of these roosts differs between North America and at least some parts of Eurasia, where very large concentrations are known in towns and villages, e.g., in southeastern Europe. These differences may be due to factors such as overall population size, landscape, and habitat differences, and perhaps different predatory species in the avifauna. Whether they relate to species-level differences in behavior is unexplored and presented here to highlight possible behavioral differences.

Those that are familiar with Long-eared Owls in North America, or at least think they are, know that they occur primarily in rather remote areas. They are local and at least in winter occur in dense cover, often in small groups of several birds, although sometimes larger aggregations into the teens and, exceptionally, up to 40 in southern Idaho. The largest number Paul Lehman has ever found is 18. Much more frequently only single birds or a few are found at a roost. In Missouri, Mark Robbins reports that winter roosts of Long-eared Owls have declined as concealing cover has declined. Based on reports to Robbins by Amish birders with a long history of observation in central-western Missouri, it appears these declines may be related to increasing populations of deer, resulting in overgrazing that has reduced concealing vegetation.

Ackerman (2023) presented a very different picture in parts of Europe. In northern Serbia she detailed censuses of winter roosts in towns where astounding numbers have been found. Here they occur in pines, spruce, and firs which are planted in towns. From November to March, hundreds of birds roost in these trees that border the town squares in the center of towns. She detailed the winter counts organized by Serbian ornithologist Milan Ružić and his volunteers in more than 400 villages in northern Serbia from Belgrade to the Romanian border. In 60 days of field work they counted more than 24,000 Long-eared Owls in hundreds of roosts. His record in single tree was 145 and his record in a single day at a single site was 743 birds! Ružić's published surveys for 2007-2008 (Ružić et al. 2009) reported a total of 368 roosts and 19,335 Long-eared Owls. Of these roosts, 355 were in human settlements and the remainder on large state-run farms. The mean number of owls per roost in this region of northern Serbia was 52.54, and almost 45% of the roosts were what the authors describe as "middle sized roosts" holding 31-80 individuals. Counts in Romania and Hungary have numbered up to 200 and in Germany and the Netherlands the counts are around 25, according to Ackerman (2023), who also reported that David Lindo found roosts of up to 17 birds in West London, United Kingdom, about 30 years ago where now he rarely finds more than four birds.

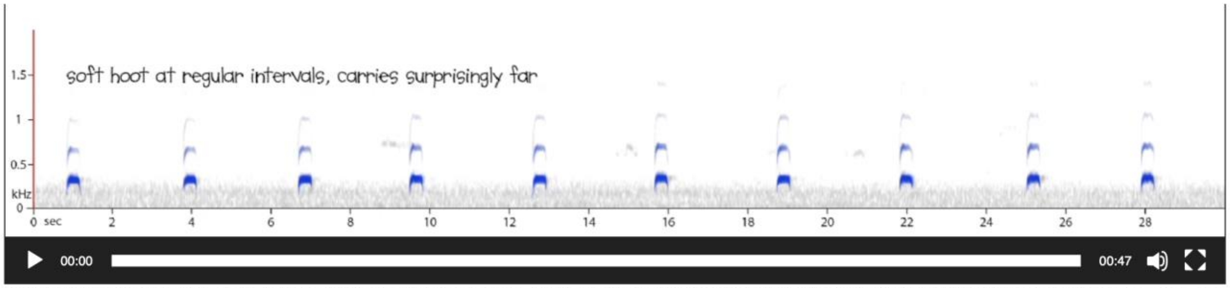
Long-eared Owls in the Old World thus form larger concentrations in winter and are more apt to occupy human-settled areas. Anecdotally, Bevier and Dunn noted a Long-eared Owl sitting prominently on a telephone line at night near the center of a northwestern Moroccan town in mid-December 1989. Such an exposed perch in an urban setting is something that neither of us had ever seen in North America.

Genetics

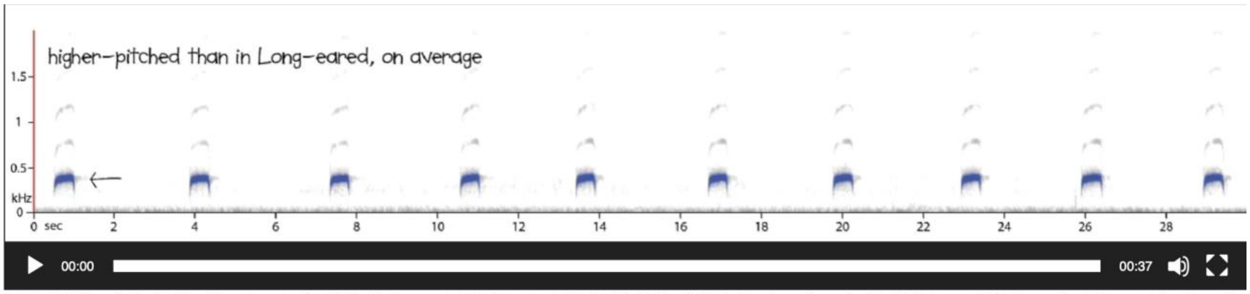
Take away, differences between Old World and New World taxa are minor and little studied. One report found that the mtDNA of *wilsonianus* differs from Scandinavian samples by 1.13% (see Table 3 in Johnson et al. 2010). They suggested that this represents around a half million years of isolation of North American birds from Palearctic birds. This, along with the vocalizations (see below), suggests a relatively close relationship between the two groups.

Vocalizations

Long-eared Owls give a wide variety of calls, but most of these are delivered around the nest and include begging calls of the young, said by those searching for them to be the best way to locate breeding sites of this secretive species. For this, see Savard et al (1995), which will be discussed later. We would add that this method is also an excellent way to locate breeding American Goshawks (*Accipiter atricapillus*), but during the day when one listens for begging juveniles. Long-eared Owls are largely silent in flight and very rarely call in winter, despite the plethora of Christmas Count reports. Territorial calling mainly consists of males during the spring giving a series of hoots from a perch, each hoot being delivered after a gap of some 2.5 to 3 seconds. Robb (2015) stated that *wilsonianus* hoots average higher-pitched than those of nominate *otus* and *canariensis*, which calls like *otus*, but stated that female Long-eared Owls occasionally hoot (a different quality note which is higher-pitched) so warn of claiming *wilsonianus* in Europe based on call. JLD listened to the one recording of a hooting male *wilsonianus* from Walla Walla County, Washington, published in Robb (2015; recording is ML49057). Robb (2015) did not detail other North American calls so there is no analysis of whether they differ, if at all, from those of nominate *otus*. Robb (2015) detailed many types of European calls, one of which was a somewhat harsh *Vvvw*. This call is given frequently during the breeding season and mostly by the female. We had difficulty discerning a difference in the male hoots between *otus/canariensis* and *wilsonianus.* Reviewing sounds in xeno-canto, there are many more recordings from Europe than from North America. The whistled begging calls sounded similar between North American and European birds.



CD2-57: Long-eared Owl *Asio otus otus* Pancas, Benavente, Portugal, 23:25, 28 February 2013. Hooting of a male in open Holm Oak *Quercus ilex* woodland, with large pools here and there following heavy rain. Background: Mallard *Anas platyrhynchos* and Tawny Owl *Strix aluco*. 130228.MR.232502.01



CD2-64: Wilson's Owl *Asio wilsonianus tuftsi* Walla Walla county, Washington, USA, 20:30, 20 April 1990. Hooting of a male. David S Herr and The Macaulay Library at the Cornell Lab of Ornithology.

Sonograms from Robb (2015). Top: nominate *otus* from Portugal; bottom: *wilsonianus* from Washington (by David Herr, ML49057)

Range

North American *wilsonianus* (with *tuftsi* best treated in our opinion as a synonym) is found across much of North America from British Columbia and southwestern Mackenzie east to northern Manitoba (Churchill), northern Ontario, southern Quebec, and the Canadian Maritime provinces, and south to northwestern and north-central Mexico (rather uncommon and perhaps declining) and across the northern portions of the southern states and then northeast to the mountains of western Virginia. Although widespread it seems to be always locally distributed and difficult to detect, particularly from much of the breeding range. In much of the more wooded breeding range, detections are perhaps easiest later in the summer (e.g., in southern Quebec) when the young are calling. In winter it disperses further south into Mexico (AOU 7th says recorded south to Oaxaca; Howell and Webb (1995) stated there is a specimen from Baja California Sur and another from Guerrero), and rarely to Honduras (see eBird https://ebird.org/checklist/S203631916). It is rare or casual south to Florida. There are seven records from Bermuda, with two found dead (specimen status unknown) and three photographed showing *wilsonianus* (Amos 1991, Dobson 2002, and eBird records). Long-eared Owl is accidental to Cuba (specimen at El Museo de Ciencias 'Felipe Poey' number 14.000358, collected by S. Roig at Cerro, La Habana Province 22 October 1932). It has been recorded north to southeastern Alaska where it is casual (mainly late fall) and as noted in AOU 7th is accidental in the Yukon (specimen 19 May 1977, Dempster Highway). It is accidental from

Labrador (8 December 1930, Red Bay, southeastern Labrador). North American *wilsonianus* is unrecorded in Greenland (no Long-eared Owl records) or anywhere across the Palearctic.

Palearctic nominate *otus* and *canariensis* (Canary Islands) breeds from Ireland, Great Britain, Fennoscandia, and across the Russian Federation to Amurland and Ussuriland, north to about the Gulf of Udskaya in the southwestern portion of the Sea of Okhotsk. Its southern limits are the Azores, Canary Islands (resident *canariensis*), northwestern Africa (Morocco east to northern Tunisia), northern Greece, Crimea, Caucasus, Asia Minor, Palestine and east to the Transcaucasia, northern Iran, southern Transcapia and probably the Paropamisus in northwestern Afghanistan (all from Vaurie 1965), northwestern and north-central China, and Japan, where Brazil and Yamamoto (1989) opine that there are perhaps no more than 100 breeding pairs. They breed south to about central Honshu and are more widespread in Japan in winter. The species has bred in northern and western Pakistan and Kashmir according to Grimmett et al. (1999). Rasmussen and Anderton (2005) stated that in South Asia it is a rare and irregular winter visitor and that breeding reports from western and northern Pakistan and western Kashmir need confirmation. It winters south to central Tunisia, Arabia, southern Iran, Tajikistan, and northwestern India (rare and irregular), eastern China, South Korea, and Taiwan. It is casual in migration or winter to the Nansei Shoto and the Izo islands, as well as to Sakhalin, Kuril Islands, northern Laos (King and Dickinson 1975), northern Myanmar, Bhutan, southern Tibet, the Faroes, Iceland, and Madeira. Cramp (1985) added that it is accidental to Bear Island (southernmost island in Svalbard) and Iraq.

Nominate *otus* has been recorded once and very likely twice from North America, both times from western Alaska. The first record was one aboard a ship at 63° 15' N and 173° 44' W, some 70 km (43 miles) southwest of St. Lawrence Island on 19 May 2006 in U.S. waters, where it "was almost certainly a migrant from Asia" and thus presumed to be nominate *otus* (Gibson et al. 2008). The single image taken by Captain E. Labunski was published in black-and-white (Gibson et al. 2008, p. 198). The image as published is a bit difficult to resolve with certainty, but we note these features that suggest nominate *otus*: the underparts appear to show more prominent streaks that lack prominent cross bars, and the facial disk appears pale with an indistinct dark border. JLD has asked Dan Gibson if he can locate the image in color. Perhaps the most useful mark, the reddish eyes, might be difficult to discern as the eyes look mostly, if not completely, closed. Then later, from 8-11 June 2016, a definitive example of nominate *otus* was present on Buldir Island in the western Aleutian Islands. A full frame color image of the bird atop a shed is published and one can clearly see wide open reddish eyes, a grayish facial disc, largely vertical streaks on the underparts and the primary pattern (Gibson et al. 2018, page 182 and reproduced above in this proposal). Overall this bird is paler than *wilsonianus.* The Alaska Checklist Committee formally added this subspecies to the Alaska list on the basis of this record (Gibson et al. 2018). It would also, of course, be the first completely solid record for North America.

The Long-eared Owl complex

In the past, some authorities (e.g., Cramp 1985) treated the two larger subspecies of Long-eared Owl from eastern Africa (*abyssinicus* from Ethiopia and isolated *graueri* from the mountains of central and eastern Africa) as conspecific with *A. otus*, but most authors now treat these as belonging to a separate polytypic species, Abyssinian Owl (*A. abyssinicus*).

Madagascar Long-eared Owl (*A. madagascariensis*) has long been treated as a species, and Cramp (1985) noted that it forms a superspecies with Long-eared Owl. The Macaulay Library at the Cornell Lab of Ornithology has five recordings of Madagascar Long-eared Owl. They sound completely different from any recording of Eurasian or North American Long-eared Owl we have heard and do not sound like the description in König and Weick (2008), who stated that the male's call is a lilting *ulooh,* uttered at intervals of several seconds. They admitted that the calls are poorly known. We could find no recordings of Abyssinian Long-eared Owl of either subspecies at the Macaulay Library or at Xeno-Canto. König and Weick (2008) wrote that the calls of *abyssinicus* are poorly known, but went on to state: "the song of the male is a drawn-out, disyllabic, deep who-woohm, rising in pitch and repeated at intervals of several seconds. The female has a similar, but slightly higher-pitched and softer songs." Morphologically, Abyssinian and Madagascar Long-eared Owls are quite distinct, the former with heavy dark cross-barring on the underparts as well as other differences. To our knowledge no authorities have yet split the Long-eared Owls of Eurasia, nominate *otus* group*,* and North America, *wilsonianus* group, as separate species.

Summary:

We view the split of the *otus* and *wilsonianus* groups into separate species as a difficult one to endorse with clear evidence. With night birds, logically one would first check the vocalizations, primarily the advertising calls delivered primarily early in the breeding season (spring). Although Robb (2015) said that the *wilsonianus* group delivers higher-pitched hoots, the difference is subtle and needs to be quantified. We have not tried to sort out all the other calls given mainly on the breeding grounds, particularly around the nest, to see if there are differences. The vast majority of vocalizations deposited at xeno-canto are of birds within the range of nominate *otus.* Like so many other aspects of North American *wilsonianus,* it seems much more poorly known and recorded.

The genetic difference between the two groups needs further analysis. Moreover, a 1.13 % difference in mtDNA (Johnsen et al. 2010) seems minor and borderline as the sole basis for a split. On the other hand, we feel that the morphological differences between the two groups are major. These include the more streaked, less cross-barred appearance of nominate *otus* along with its paler (grayer or tawny, not rufous) facial discs that are less distinctly outlined, paler overall coloration, and pattern on the outer primaries. Cramp (1985) provided a more thorough review of the differences. Perhaps the most "glaring" difference is the reddish to reddish-orange irides of the *otus* group versus the golden-yellow irides of *wilsonianus*. If one looks at that Buldir Island color image, it is hard to reconcile that individual as being the same species as "our" Long-eared Owl.

The significance of the behavioral difference is hard to know. It would certainly seem that at least in parts of eastern Europe, the species is far more abundant than anywhere in North America. There, the birds sit around in dozens, if not hundreds, in trees around crowded squares, a scene unthinkable in North America. Perhaps the relative abundance of Great Horned Owls (*Bubo virginianus*) and Red-tailed Hawks (*Buteo jamaicensis*) in North America versus Eurasian Eagle Owls (*Bubo bubo*) and Buzzards (*Buteo Buteo*) just lead to North American birds being more secretive. On the other hand, when one sees owls harassed, it is more often by corvids and they are abundant on both continents.

Going back to the issue of *wilsonianus* being more poorly known than nominate *otus*, researchers in Quebec have found that surveys should focus on listening for the begging calls of juvenile birds rather than detection of advertising calls by males or calls by females. Using this technique, three researchers working in southern Quebec in the Saguenay/Lac-Saint Jean area found 37 Long-eared Owl families in the summer of 1994 (Savard et al. 1995). This tripled the number of Long-eared Owl nesting sites found over the previous thirty years. These detections resulted from hearing the begging juveniles at night and then tracking them down. They stated:

> During those 13 night outings, from July 19 to August 6, 1994, we counted 37 long-eared owl families in 15 municipalities, for a grand total of 78 juveniles, all spotted from their calls. The long-eared owl was by far the most frequently encountered owl species in the area in July and August of that year (Table 1). However, we only saw two flying adults, thus confirming their reputation of discretion. After our survey of juvenile long-eared owls, the species appeared to be more familiar than any other species of nocturnal owl in the region! One only had to get off the beaten path, pay attention to juvenile calls, target the rearing period, and most of all, characterize adequately the species' habitat in the region.

They noted that the nesting areas were not in the middle of large forests but were near the edge of forested areas with openings nearby. These three Quebec researchers have perhaps discovered the Rosetta Stone from which others can now learn more about *wilsonianus* Long-eared Owls and bring our overall knowledge of the subspecies up to parity with Old World *otus.* However, it's been 30 years since the study so we wonder if their techniques for finding nesting groups have been broadly implemented.

Recommendation:

On balance we lean slightly to splitting the two groups into two distinct species, which is perhaps more consistent with other taxa within the Long-eared Owl complex. The two groups are separated by thousands of miles, and it would be highly improbable that individuals from either group would come into contact with each other. Long-eared Owls, unlike Short-eared Owls, need trees to nest and roost and that no doubt leads to the wide gap in the ranges. In both editions of their volumes on owls of the world, König et al. (1999) and König and Weick (2008) under "Remarks" provided the same exact comment: "Although one of the commonest owls in C. Europe and rather well studied, the taxonomy of this widely distributed species is not yet clear. In particular, geographic variation and the relationship of the Old and New World taxa require study." We agree, but outlining how to proceed with those studies seems difficult indeed, and we would question if New World *wilsonianus* is as well studied as nominate *otus*. If nothing else we feel that we are at least putting the issue out there, which might lead others to do more formal investigations, whatever those might be.

If the NACC votes not to split *wilsonianus* from *otus* another consideration is whether to put them into separate groups within the Check-list. The information within the proposal on Range would make it fairly easy to cobble this together, and it is already done in a more abbreviated fashion within the 7th edition of the AOU checklist. In addition, we now have an acceptable record of nominate *otus* from North America.

English names:

If NACC votes to split these groups, English names will be needed. We note in passing that Robb (2015) tentatively suggests the name of Wilson's Owl for *wilsonianus.* With the split of Northern Goshawks recently in mind, the English names of American Long-eared Owl and Eurasian Long-eared Owl would seem appropriate and would be symmetrical with Abyssinian Long-eared Owl and Madagascar Long-eared Owl within the complex. If NACC votes not to split, perhaps a consideration in a separate motion should be given to changing the English name to Northern Long-eared Owl, the name used, e.g., by Rasmussen and Anderton (2005).

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Submitted by: Louis R. Bevier and Jon L. Dunn

Date of Proposal: 12 February 2025

Treat Burmese Collared-Dove *Streptopelia xanthocycla* as a separate species from Eurasian Collared-Dove *S. decaocto*

Effect on NACC:

If this proposal is approved, *Streptopelia decaocto*, an introduced species in the NACC area, will be split into two species: *S. decaocto* and *S. xanthocycla*. Since *xanthocycla* is extralimital, the split would not add a species to the checklist, but it would require a change in the geographic distribution of *S. decaocto*.

Background:

The Eurasian Collared-Dove *Streptopelia decaocto* is a sandy gray medium-sized dove with a pinkish hue on its head and breast; the black collar on the back of its head and the squared tail are the most commonly used characteristics for its identification (Romagosa and Mlodinow 2022). It is believed that *S. decaocto* originally was from South Asia, from where it began its expansion to Central Asia and parts of Europe. By the 20th century, it could “jump” to Africa and was also introduced in North America (van Grouw 2022). In its original range (China, India, and Sri Lanka), it is common to find the species in arid or open semi-desert areas with scarce vegetation (Romagosa and Mlodinow 2022). In its non-native range, it can be found in suburban areas with little vegetation and agricultural land (Sibley 2014, Fagan and Komar 2016). The Eurasian Collared-Dove seems to avoid places where the mean minimal temperature reaches below 0°C (Fujisaki et al. 2010, Bermúdez et al. 2020), and it is considered to benefit from exotic plant species in suburban areas (Bermúdez et al. 2020).

The Burmese Collared-Dove, since its description, has been considered a subspecies of the Eurasian Collared-Dove, originally “Burmese Collared Turtle Dove” *Turtur decaocta xanthocyclus* (Newman 1906), currently *Streptopelia decaocto xanthocycla*. It is native to the central plains of Myanmar, but historical records indicate its presence in southeastern China (Smythies 1940, Yang and Shuihua 2024); it is considered allopatric to *S. decaocto sensu stricto* (Figure 1). As with *S. decaocto,* the Burmese Collared-Dove is common in suburban areas or farmland (Zöckler 2018).

Although *xanthocycla* and *decaocto* are similar in size, some authors mention that *xanthocycla* is larger, which is more noticeable in the tail. Females of both species may be slightly smaller than males (Roonwal 1940), less colorful on the nape and crown, and paler pink on the head and breast (Limparungpatthanakij et al. 2022). Remarkably, *xanthocycla* is slightly darker overall and has a distinctive yellow eyering, contrasting with the grayish-white eyering in *decaocto* (del Hoyo and Collar 2014). Newman (1906) was the first to treat *xanthocycla* as a different taxon than *S. decaocto*, based mainly on the differences in the eyering.

BirdLife International treats *S. xanthocycla* as a separate species from *S. decaocto* based on its unique eyering color, distribution, plumage, and song differences (BirdLife International 2024). The IOC and eBird/Clements checklists recognize the Burmese Collared-Dove as a separate species from the Eurasian Collared-Dove (Gill et al. 2021, Clements et al. 2022). The Howard and Moore checklist, however, still treats *xanthocycla* as a subspecies of *S. decaocto* (Trust for Avian Systematics 2021).

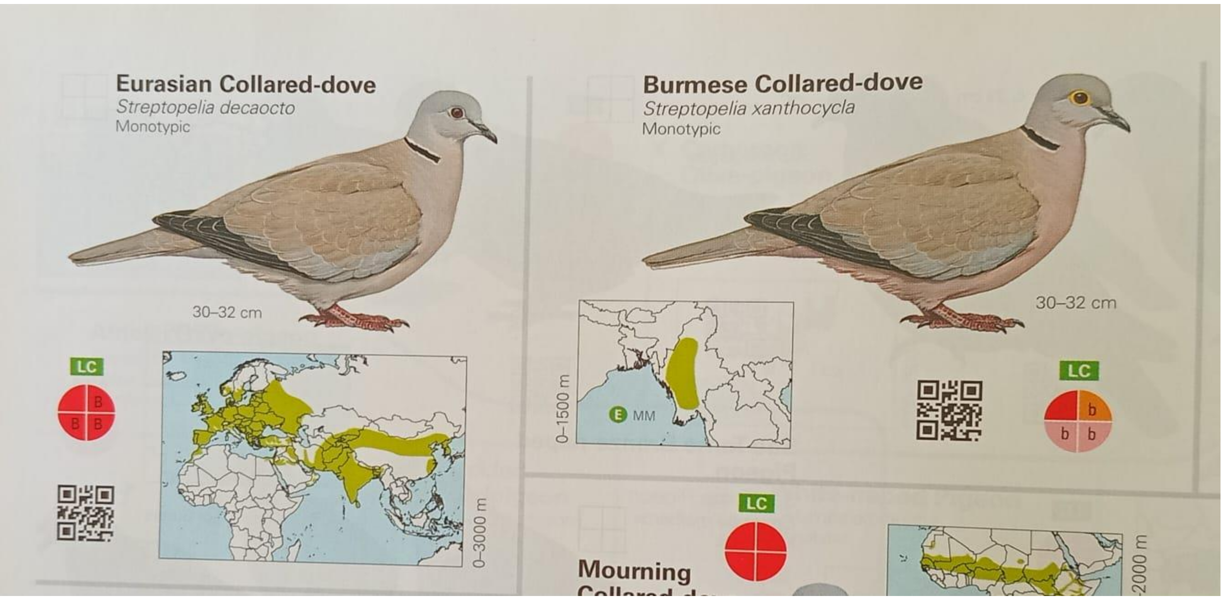

Figure 1. Geographic distribution of the Eurasian Collared-Dove and the Burmese Collared-Dove (del Hoyo 2020).

Behavior

The European Collared-Dove is usually solitary or in small groups, foraging in the ground or perched on wires, branches, or poles (Sibley 2014). It can nest in various tree species, roofs, poles, and powerlines; it has also been reported that it reuses the nests in the same reproductive season, which helps it to increase its population (Tinajero and Partida-Pérez 2015). Like most species of Columbidae, *S. decaocto* and *S. xanthocycla* consume mostly seeds and fruits (Winkler et al. 2015).

Phylogenetics

Johnson et al. (2001) built a molecular phylogeny based on mitochondrial (cyt *b*, COI, and ND2) and nuclear (FIB7) DNA, including 14 species of the genus *Streptopelia*. The authors analyzed the genetic data through parsimony and maximum likelihood. Both phylogenetic analyses recovered similar relationships for *S. decaocto*, which was the sister species to *S. roseogrisea* (African Collared-Dove; Figure 2 - maximum likelihood phylogeny). The sample of *S. decaocto* included in the analysis came from the Netherlands; unfortunately, the authors did not include *S. xanthocycla*.

Vocalizations

Ballintijn and ten Cate (1997) assessed sex differences in the vocalizations and syrinx of the Eurasian Collared-Dove. The authors studied nine males and six females kept in captivity. They found that males and females display similar vocal behavior, but that females tend to have

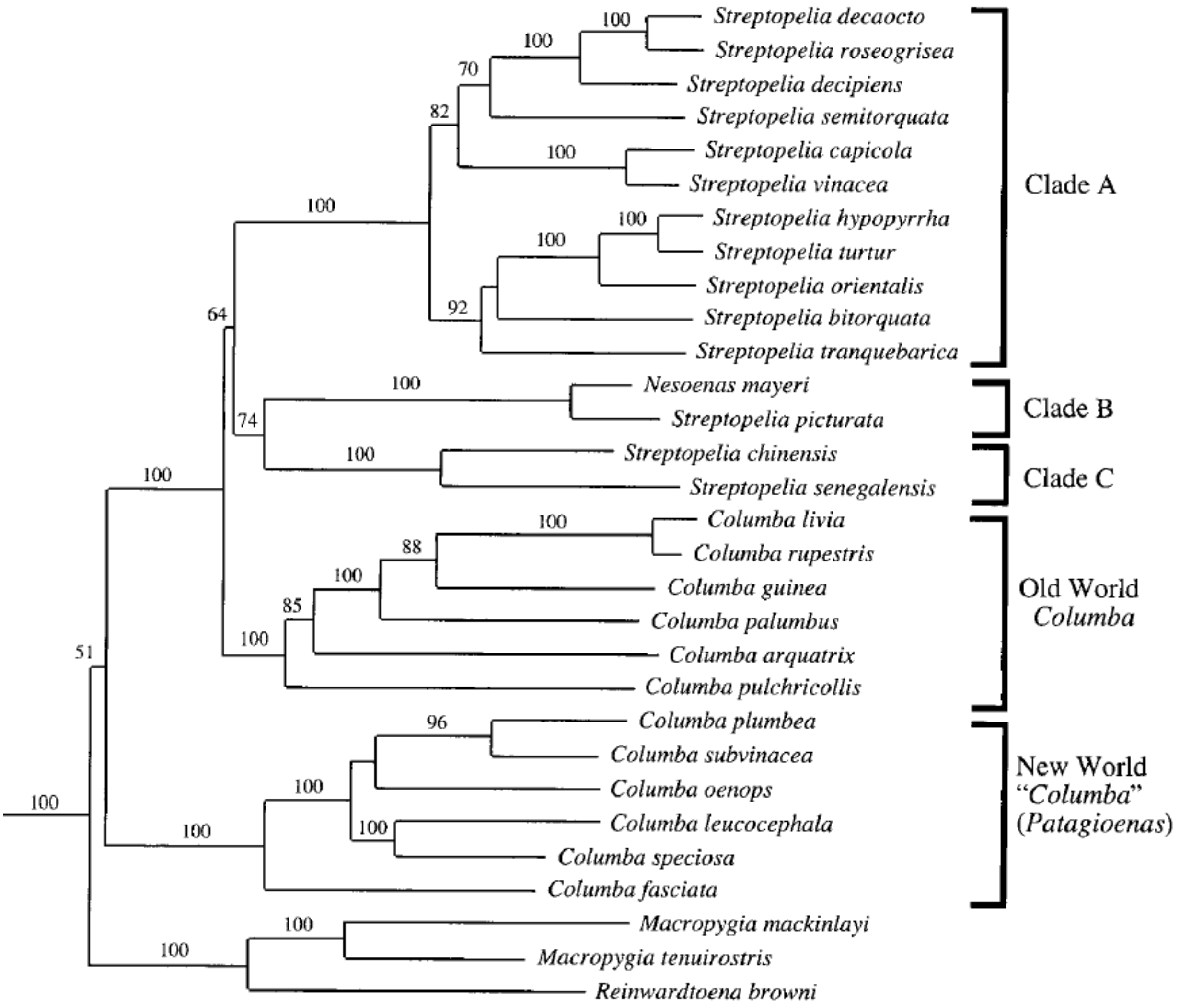

Figure 2. Relevant part from Figure 6 of Johnson et al. (2001). Maximum likelihood phylogeny using mitochondrial (cyt *b*, COI, and ND2) and nuclear (FIB7) genes. The numbers above branches indicate bootstrap support from 100 heuristic search replicates. Unlabelled nodes received <50% bootstrap support.

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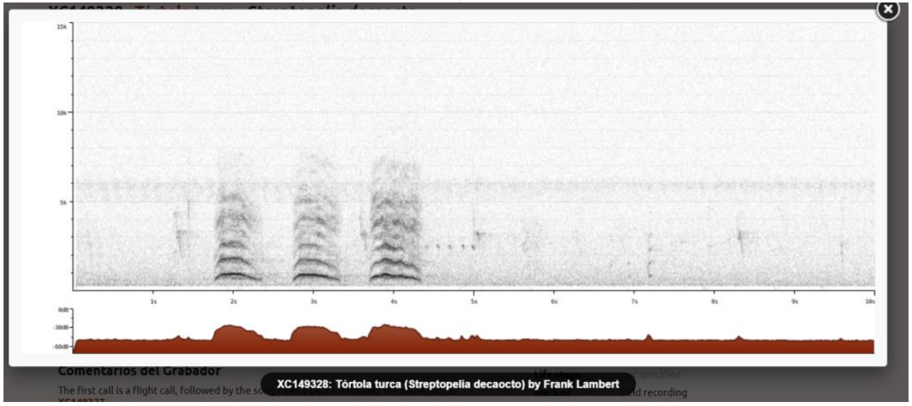
higher frequency and lower vocal output. The differences between male and female vocal behavior are thought to be the result of testosterone levels and syrinx morphology (Ballintijn and ten Cate 1997).

Xeno-canto has 1061 recordings of *S. decaocto*, including different types of calls, flight calls, songs, and wing beats. Macaulay Library has 2618 recordings of the Eurasian Collared-Dove. The song is described as a rhythmic, three-note "coo Coo cup"/ "whooa-wooooo-who" (Sibley 2014, Fagan and Komar 2016).

Recordings of *S. xanthocycla* are scarce and there has not been a formal analysis. The xeno-canto database has four recordings (most of them songs). Macaulay Library only has one recording that appears to be the same as “XC628153” (same date, author, and place). In contrast to the song of *S. decaocto*, the Burmese Collared-Dove song consists of two notes; there is an emphasis on the first note and a fall on the second note (Limparungpatthanakij et al. 2022).

S. decaocto - India. Flight call, song: XC149328 (Accessible at: www.xeno-canto.org/149328).

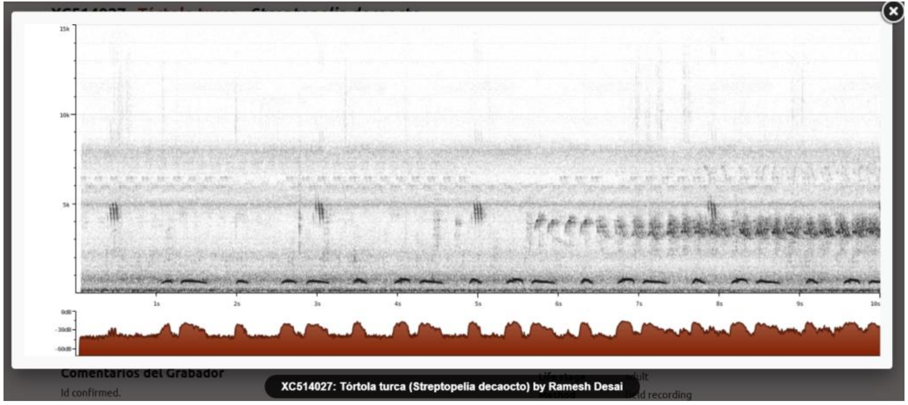


Comentarios del Grabador

The first call is a flight call, followed by the so

XC149328: Tórtola turca (Streptopelia decaocto) by Frank Lambert

S. decaocto - India. Song: XC514027 (Accessible at: www.xeno-canto.org/514027).

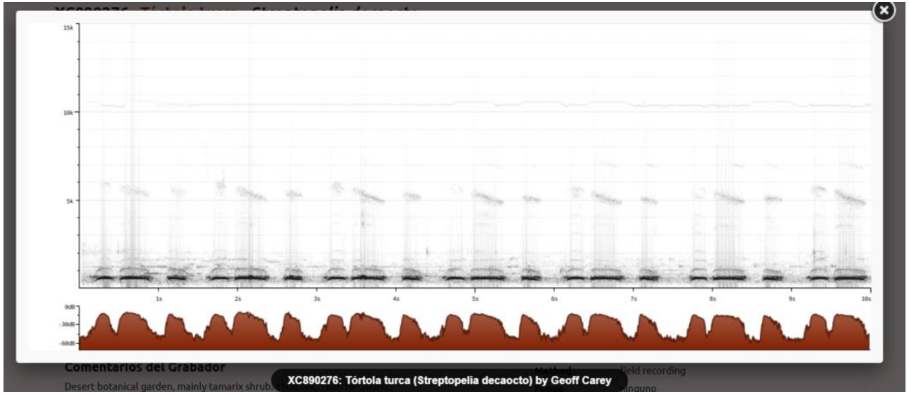


Comentarios del Grabador

Id confirmed.

XC514027: Tórtola turca (Streptopelia decaocto) by Ramesh Desai

S. decaocto - China. Song: XC890276 (Accessible at: www.xeno-canto.org/890276).

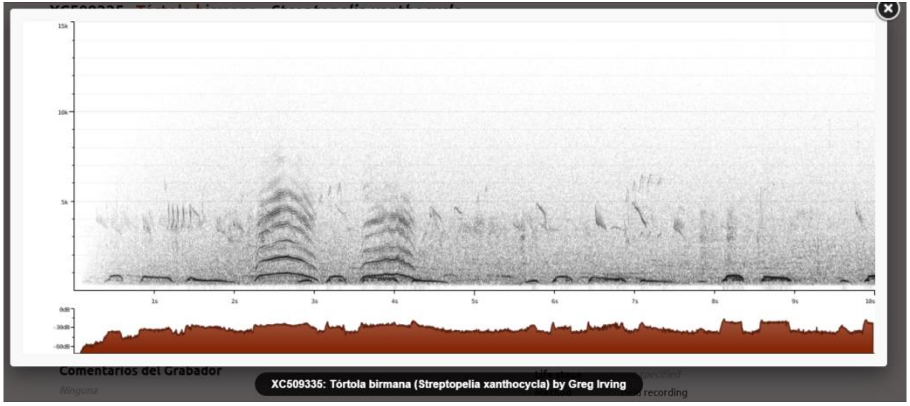


Comentarios del Grabador

Desert botanical garden, mainly tamarix shrub

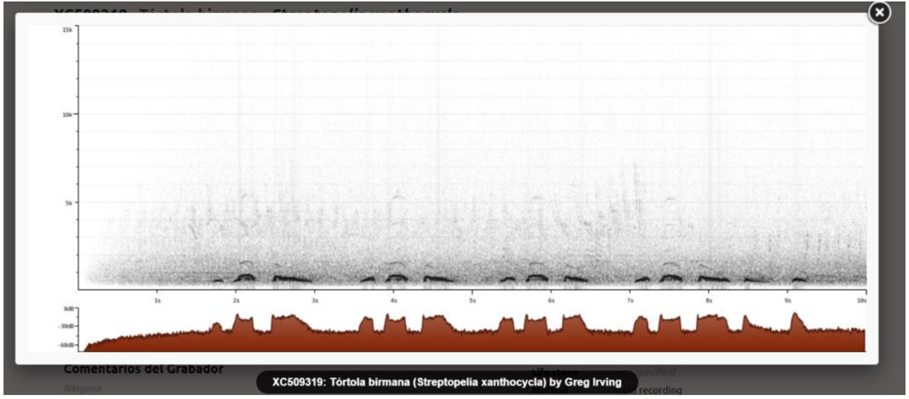
XC890276: Tórtola turca (Streptopelia decaocto) by Geoff Carey

S. xanthocycla - Myanmar. Call, song: XC509335 (Accessible: at www.xeno-canto.org/509335).



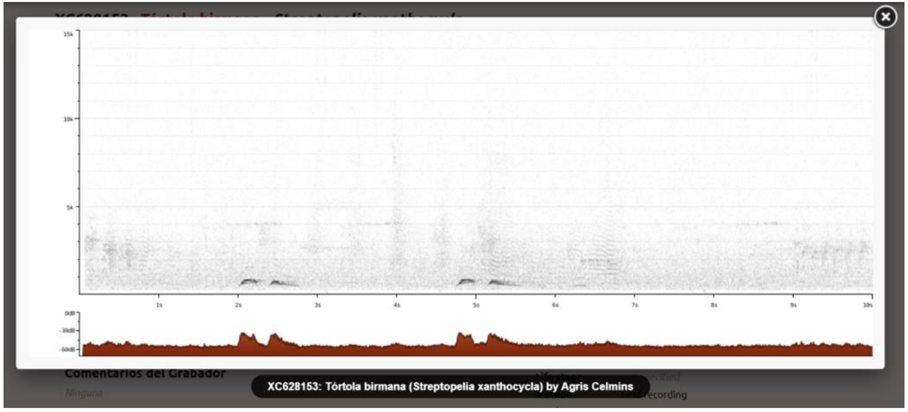
XC509335: Tórtola birmana (Streptopelia xanthocycla) by Greg Irving

S. xanthocycla - Myanmar. Song: XC509319 (Accessible at: www.xeno-canto.org/509319).



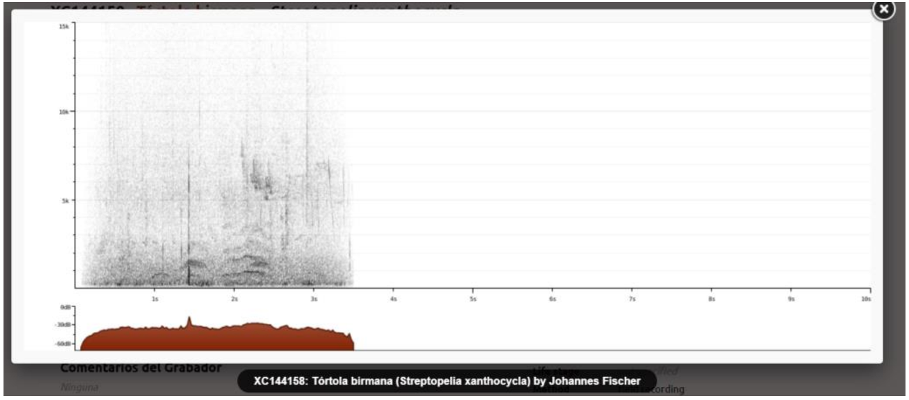
XC509319: Tórtola birmana (Streptopelia xanthocycla) by Greg Irving

S. xanthocycla - Myanmar. Song: XC628153 (Accessible at: www.xeno-canto.org/628153).



XC628153: Tórtola birmana (Streptopelia xanthocycla) by Agris Celmins

S. xanthocycla - Myanmar. Song, call: XC144158 (Accessible at: www.xeno-canto.org/144158).



New information:

An integrative study focused on the taxonomic status of the Burmese Collared-Dove analyzed genomic data including multiple samples from *decaocto* and *xanthocycla* (van Grouw et al. 2024). The authors also discussed plumage coloration, indicating that only minor plumage differences exist between *decaocto* and *xanthocycla*; although *xanthocycla* is considered darker than *decaocto*, *decaocto* from India and Sri Lanka are generally darker than European populations, looking more similar to *xanthocycla* (Figure 3). Morphological measurements from museum specimens showed that *xanthocycla* is larger than *decaocto* from India but overlaps in size with *decaocto* from China and Europe (Figure 4). On vocalizations, van Grouw et al. (2024) indicate that *decaocto* and *xanthocycla* are the only pigeon species that, as far as they are aware, call in flight. As already known, *decaocto* has a three-note song and *xanthocycla* has a two-note song. However, a rare three-note song is mentioned for *S. xanthocycla* (van Grouw et al. 2024).

Figure 3. From left to right Eurasian Collared Dove *Streptopelia decaocto*, Sri Lanka (NHMUK 1946.28.232), Burmese Collared Dove *S. xanthocycla*, Myanmar (NHMUK 1948.80.3337) and Eurasian Collared Dove, Serbia (NHMUK 1969.3.1); although Sri Lankan (and Indian) Eurasian Collared Doves are slightly smaller than Burmese Collared Dove, their dark upperparts are almost identical, whilst European (and Chinese) Eurasian Collared Doves are paler than Burmese Collared Dove but they are similar in size or even larger (see Table 1) and the white tail tip can be even larger (Jonathan Jackson, © Trustees of the Natural History Museum, London)

Figure 3. Taken from van Grouw et al. (2024).

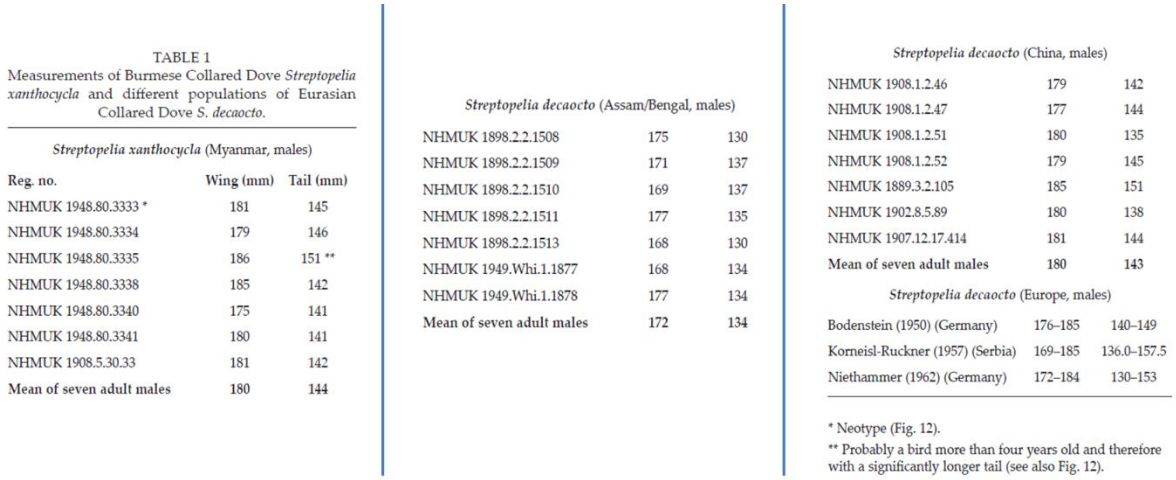


TABLE 1
Measurements of Burmese Collared Dove *Streptopelia xanthocycla* and different populations of Eurasian Collared Dove *S. decaocto*.

Reg. no.	Wing (mm)	Tail (mm)
Streptopelia xanthocycla (Myanmar, males)		
NHMUK 1948.80.3333 *	181	145
NHMUK 1948.80.3334	179	146
NHMUK 1948.80.3335	186	151 **
NHMUK 1948.80.3338	185	142
NHMUK 1948.80.3340	175	141
NHMUK 1948.80.3341	180	141
NHMUK 1908.5.30.33	181	142
Mean of seven adult males	**180**	**144**
Streptopelia decaocto (Assam/Bengal, males)		
NHMUK 1898.2.2.1508	175	130
NHMUK 1898.2.2.1509	171	137
NHMUK 1898.2.2.1510	169	137
NHMUK 1898.2.2.1511	177	135
NHMUK 1898.2.2.1513	168	130
NHMUK 1949.Whi.1.1877	168	134
NHMUK 1949.Whi.1.1878	177	134
Mean of seven adult males	**172**	**134**
Streptopelia decaocto (China, males)		
NHMUK 1908.1.2.46	179	142
NHMUK 1908.1.2.47	177	144
NHMUK 1908.1.2.51	180	135
NHMUK 1908.1.2.52	179	145
NHMUK 1889.3.2.105	185	151
NHMUK 1902.8.5.89	180	138
NHMUK 1907.12.17.414	181	144
Mean of seven adult males	**180**	**143**
Streptopelia decaocto (Europe, males)		
Bodenstein (1950) (Germany)	176–185	140–149
Korneisl-Ruckner (1957) (Serbia)	169–185	136.0–157.5
Niethammer (1962) (Germany)	172–184	130–153

* Neotype (Fig. 12).

** Probably a bird more than four years old and therefore with a significantly longer tail (see also Fig. 12).

Figure 4. Measurements of *S. xanthocycla* wing and tail, compared to different populations of *S. decaocto* (Table 1 of van Grouw et al. 2024).

Back to the genomic data, van Grouw et al. (2024) did maximum likelihood phylogenetic analyses based on whole-genome sequencing data from three historical samples of *xanthocycla*, all of them from central Myanmar, and seven publicly available genomes from *decaocto*. The historical samples were extracted from the toe pads of three individuals (collected between 1905 and 1936) and digested in a PCR-free laboratory. Reciprocal monophyly was recovered between *decaocto* and *xanthocycla*, both on an analysis based on the mitochondrial genome and one based on the nuclear genome (Figure 5). A long branch was recovered for the *xanthocycla* samples in the nuclear-genome phylogeny, which may be explained by differences in the DNA quality of the samples rather than by clear divergence as interpreted in the paper.

Finally, van Grouw et al. (2024) conducted an admixture analysis to assess possible gene flow between *decaocto*, *xanthocycla*, and the species *S. risoria* (*S. roseogrisea*, African Collared-Dove). The authors found higher gene flow between *decaocto* and *risoria* than between *decaocto* and *xanthocycla*. However, we think these results should be taken carefully, considering that *xanthocycla* was sampled from historic specimens while *decaocto* and *risoria* consisted of modern samples. The authors discussed that the natural barrier that separates *decaocto* and *xanthocycla* is the tropical broadleaf forest of northeast India and Bangladesh.

Recommendation:

Phenotypic (plumage coloration, morphological measurements, vocalizations) and phylogenetic data provide integrative evidence that supports *xanthocycla* as a separate species from *decaocto*. Most of the global avian checklists already accept the split; the WGAC voted to split *S. xanthocycla* from *S. decaocto* based on the differences in eyering color, plumage, size, and song. Comments from the WGAC mention that *decaocto* and *xanthocycla* are allopatric and that no hybrid zone is known, even considering the great vagility of *decaocto*.

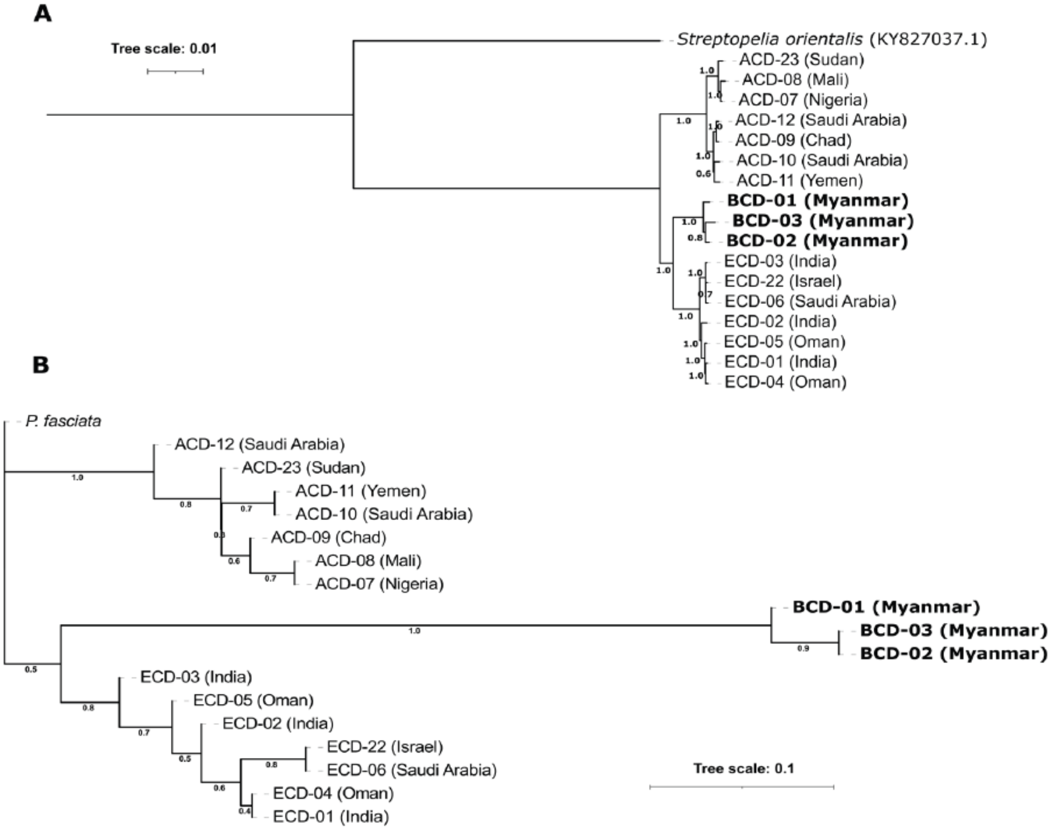


Figure 5. Figure 8 from van Grouw et al. (2024). Estimated phylogenetic trees based on (A) maximum likelihood of whole mitochondrial genome. (B) Species tree based on 1000 independent maximum likelihood trees generated from random nuclear regions. BCD represents the Burmese Collared-Dove, ACD represents the African Collared-Dove, and ECD represents the Eurasian Collared-Dove.

We recommend the split of *S. xanthocycla* from *S. decaocto*, in alignment with WGAC and most global checklists.

a. Please vote YES or NO to the split.

The current English name for *S. decaocto* is Eurasian Collared-Dove. The English name for extralimital species *S. xanthocycla* is Burmese Collared-Dove.

We recommend keeping the English name Eurasian Collared-Dove for *S. decaocto*, in keeping with global usage and with our rules regarding retention of the English name of the parental species in cases of strong asymmetry of distribution and familiarity of the daughter species.

b. Please vote YES or NO to keeping the English name Eurasian Collared-Dove for *S. decaocto*.

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