2025-B-1
Treat *Camptostoma thyellophilum* as a separate species from Northern Beardless-Tyrannulet *C. imberbe*

YES. Although the vocal differences may not be of the same level as for some others in the complex, I think they are consistently different enough that they would likely be important in mate selection. I notice there are very few eBird reports at all in what would be the contact zone between *thyellophilum* and *imberbe*, mostly highly disturbed but which may be a natural break as these are not forest obligates. (Or perhaps just an underbirded region.) And, of course there are several Yucatan endemics for which the differing habitat types on the Peninsula vs. mainland Mexico do seem to serve as some kind of barrier.

In the event that it passes, I agree with keeping Northern Beardless-Tyrannulet and adopting Yucatan Beardless-Tyrannulet for *thyellophilum*, although I don't see this addressed in the proposal or the paper on which it is based. I wonder whether "Yucatan Beardless-Tyrannulet" has any history of use; a quick Google search didn't help me to find out. But it's hard to imagine a more appropriate and helpful name for yet another small plain tyrannulet.

YES (weakly). The two putative species differ in vocalizations, both in their day songs and dawn songs. I appreciate that there is overlap of individual vocalization characters between the two (Fig. 3, 5). However, just because those individual characters overlap doesn't mean individuals of the different proposed species have combinations of characters that all overlap. I would also argue that many of the dawn song characters don't overlap that much. The sampling gap doesn't bother me that much as there are similar gaps in sampling (without vocal variation) in the distribution of *Camptostoma i. imberbe*. I don't think the genetic data help with this question since we only have one sample of each. Unlike the song variation, we don't know how much genetic variation

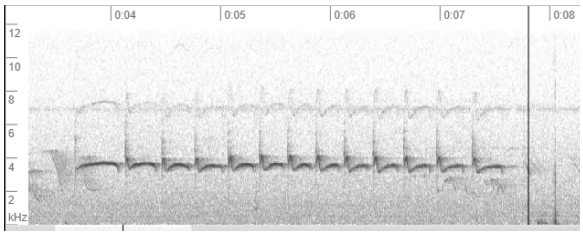
there is within species and how that varies across the distribution. Northern Beardless-Tyrannulet for *imberbe* and Yucatan Beardless-Tyrannulet for *thyellophilum* are fine with me.

YES. The vocal differences between *imberbe* and *thyellophilum* could indicate separate groups at the species level. As mentioned in the proposal, it is unfortunate that there is a sampling gap between the two groups and clear geographic limits cannot be established, nor known if there exists a contact zone. I asked a group of birdwatchers from Guatemala about recordings of the intermediate area; I'm waiting for a response.

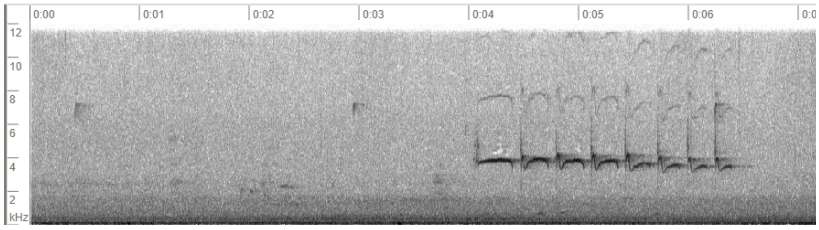
There is a well-marked precipitation gradient in the Yucatan Peninsula, drier in the north and wetter in the south, so I wondered whether it could have some kind of effect in the song, from *thyellophilum* to *imberbe*. I listened to recordings and looked at the spectrograms from the Yucatan Peninsula (including Cozumel Island), which should be from *thyellophilum*, and recordings from Chiapas, southern Guatemala, El Salvador, and northern Honduras, which should be from *imberbe*. The songs and calls I listened to seem to fall within the stereotypical vocalizations to one group or the other without any apparent gradual change.

I think the differences between groups warrant separate species status, although the geographic limits between both groups need to be examined and established. English names: Northern Beardless-Tyrannulet for *imberbe* and Yucatan Beardless-Tyrannulet for *thyellophilum*.

Camptostoma thyellophillum
Belize (song) - https://macaulaylibrary.org/asset/470379921

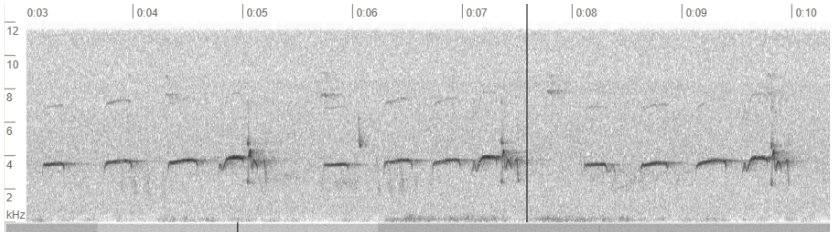


Yucatan (song) - https://macaulaylibrary.org/asset/630558288

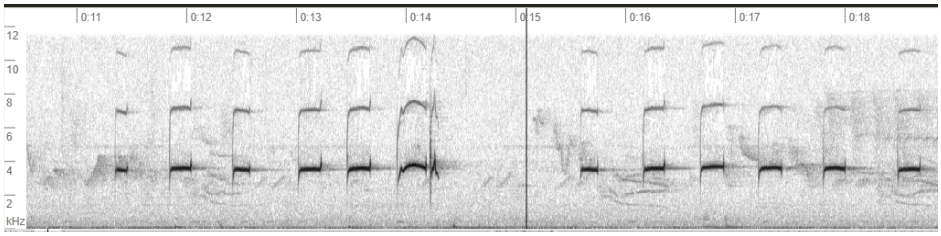


Campeche (call) - https://macaulaylibrary.org/asset/144891

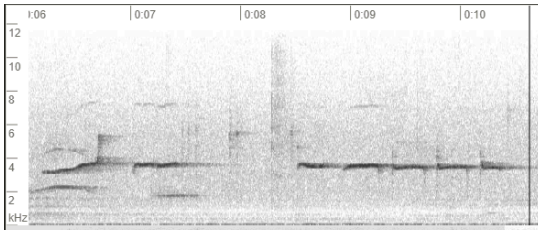
Quintana Roo, Cozumel Island (song) - https://macaulaylibrary.org/asset/624181442



Quintana Roo, continental recording - https://macaulaylibrary.org/asset/103315

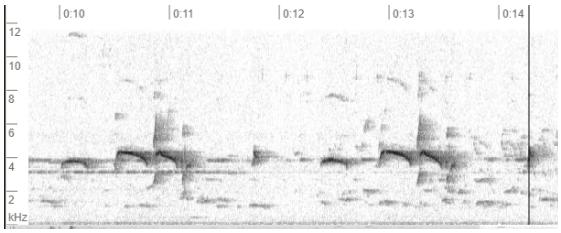


Northern Guatemala, Petén (call and song) - https://macaulaylibrary.org/asset/615715225

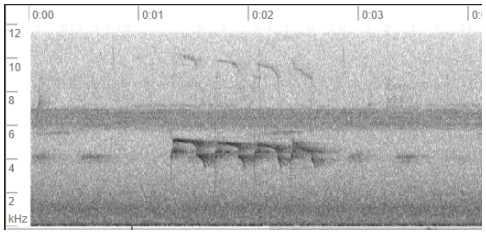


Camptostoma imberbe

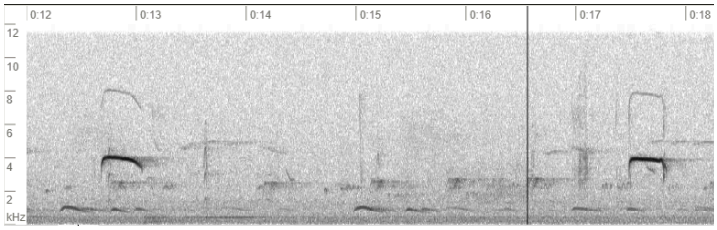
Chiapas - https://macaulaylibrary.org/asset/256385581



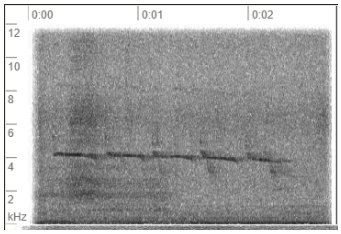
Southern Guatemala, Escuintla (song) - https://macaulaylibrary.org/asset/520350571



El Salvador - https://macaulaylibrary.org/asset/615060931



Northern Honduras, Cortés - https://macaulaylibrary.org/asset/614938076



NO. First off, I commend the authors for an excellent publication on vocal variation in this group. I hope we see many more papers of this caliber on songs in Neotropical taxa. I absolutely agree that there are more than two species in *Camptostoma*, but I'm not fully convinced that *thyellophilum* is one of them, at least not yet. I recommend that the other committee members read the paper that this proposal is based on. There are two figures (Figs. 3 and 5) with box-and-whisker plots of vocal variation within each taxon. To me, these figures are important to place the degree of vocal divergence in context of other taxa in the genus. There are average differences between *imberbe* and *thyellophilum*, but these are average differences only and the two taxa do overlap in multiple metrics. However, these average differences mask what seem to me to be fairly clear differences in listening to recordings online, although I admit I never did pay special attention to these when listening to them in the field despite having seen and heard both taxa many times. In particular, the more rapid and shorter dawn song lacking the harsh introductory notes of *thyellophilum* are noticeably different. My main issue is that there is a very extensive but unquantified contact zone with no obvious biogeographic barrier, and unfortunately there don't appear to be any song recordings from this region. If the differences in the songs, especially the dawn songs, hold up in closer geographic proximity, then I would be inclined to split *thyellophilum*, despite the apparent lack of plumage differences. Going back to my earlier point about placing the level of song divergence in the context of other taxa in the group, each of the five taxa in South America have much greater (to my ear) differences in song, with some even lacking the clear whistled notes that are so characteristic of *imberbe* (see, for example, the more chattering songs of *caucae* and *obsoletum*). Of course, just because *caucae* is very vocally divergent, doesn't mean that *thyellophilum* isn't also a good species. On that note, I do think that *flaviventre* is specifically distinct from *obsoletum* based solely on song differences, but I suppose we're waiting on SACC to vote on that first.

If this proposal does pass, then I vote to keep Northern Beardless-Tyrannulet for the widespread and more northerly *imberbe* and adopt Yucatan Beardless-Tyrannulet for *thyellophilum*.

NO. Although the sonograms show distinct vocalizations, there is considerable overlap in a number of characters (Figures 3 and 5 in the publication). Furthermore, I am bothered by the following paragraph (Lima and Vaz 2024:11): “A 350-km sampling gap between the closest recordings of *imberbe* and *thyellophilum* (Figure 1) currently limits our ability to characterize their contact zone. Photographic records on eBird indicate that *Camptostoma* does occur in that region where recordings are lacking. Since *imberbe* and *thyellophilum* are morphologically identical (Fitzpatrick 2004), only additional vocal or genetic sampling will determine the nature of their contact.” The only genetic data thus far include one sample of each taxon, and their divergence of ~2 Mya is interesting but those two samples are not near each other (one is from Campeche, Mexico; the other is from Grenada, Nicaragua - a distance of ~ 1000 km). More genetic data would be useful, but especially critical is vocal sampling across the gap to better understand the nature of vocal variation and whether it holds up where the two taxa may contact. Playback experiments may also help to understand how the taxa respond to heterospecific vocalizations.

If the committee votes to split, then I am fine with other member suggestions of Northern Beardless-Tyrannulet for *imberbe* and Yucatan Beardless-Tyrannulet for *thyellophilum.*

NO. I am very on the fence about this one, but I share the concerns of some of the other committee members on this, particularly the large sampling gap between *imberbe* and *thyellophilum*, which could provide very valuable insight into genetic divergence and whether vocal differences are maintained in parapatry. While the vocalizations do appear to be different, I am also concerned about whether these differences would be important in species recognition, as they certainly aren’t as different from each other as between subspecies of *obsoletum* (not necessarily important, as there are also likely additional splits to be made there as well). Also slightly concerning (though not a dealbreaker) is the fact that *imberbe* and *thyellophilum* are apparently morphologically “identical;” this could easily be a case of cryptic speciation in a group that already shows very little morphological diversity, but until some more clarity is provided around the zone of parapatry, I think for now I’d prefer to maintain these two taxa in the same species.

NO. These data are strongly suggestive of multiple species, but I would like to see more sampling of the contact zone and analyses of genetic data to be convinced they are reproductively isolated lineages. I acknowledge there is a strong historical precedent for inferring biological species status from vocalization differences in suboscines, but I would rather have a population-level study of genetic differences alongside these before we conclusively revise its taxonomy. A 2 million year divergence could easily be represented by intraspecific variation for a group that is this widespread, hopefully someone will follow up on the excellent Lima and Vaz (2024) study with some spatial population genetic analysis.

NO. I do not recommend separating *thyellophilum* from *imberbe*, I think we need molecular data from the contact zone.

NO. Not yet. I have listened to Howell opine about many taxonomic issues, and this one was never brought up and is not mentioned in Howell and Webb (1995). In his recent field guide on Belize with Dale Dyer (2023) there is no mention of the issue, but he does

say about the species: “Fairly common in north, uncommon to scarce and local in south.” He has a circle of absence on the map in southwest Belize, which spills over into Guatemala. This motion introduces a subject that will now be further researched and vetted. I look forward to hearing more, especially if there is a zone of sympatry. I’m not worried about the lack of plumage differences, based on our present understanding.

2025-B-2
Treat Bran-colored Flycatcher *Myiophobus fasciatus* as three species

YES. I vote for a three-way split for all the reasons listed in the proposal and comments. The songs and plumage are all quite distinct and at least in the one area of potential overlap *crypterythrus* and *rufescens* seem to segregate by habitat. Keeping Bran-colored Flycatcher for *fasciatus* makes logical sense based on range size and continuity of use, and it's the only species in our area so I'll go along with SACC for their names for the other two species.

YES. I vote to recognize the three species, splitting *crypterythrus* and *rufescens* from *fasciatus*. Vocalizations are distinctive among the three species, as well as plumage coloration. I agree with retaining Bran-colored Flycatcher as the English name for *M. fasciatus*.

YES. I support the three-way split, adopted in 2022 by eBird/Clements, and agree with retaining the familiar Bran-colored Flycatcher for *M. fasciatus sensu stricto*. Of course, who knows what is really meant by the color “Bran”; oat bran and rice bran are almost whitish, while wheat bran seems to vary quite a bit in darkness. But still it’s a familiar and distinctive name so better than coining something new that would likely be no more appropriate.

YES. Reasons are given in the proposal, and this puts NACC in line with SACC. I agree with retaining Bran-colored Flycatcher for *M. fasciatus*.

YES. Reasons are given in the proposal. I also agree with retaining the name “Bran-colored Flycatcher” for *M. fasciatus*.

YES (reluctantly). Given that this is largely a SACC complex, I am inclined to follow their unanimous decision. That said, I wish there were more quantitative analyses (phenotypic, genetic, geographic) in this assessment. Vocal differences are certainly important, and it seems like there was some basic misunderstanding of the vocal differences and geographic distributions of this group by Zimmer that this amends. The described vocal consistency across the broad geographic distribution of the *fasciatus* group is certainly interesting, but I’d rather see it analyzed quantitatively (like the Lima and Vaz (2024) study referenced in this set for example).

YES. I vote for recognizing the three species of *Myiophobus faciatus* (*M. fasciatus* Bran-colored Flycatcher, *M. crypterythrus* Mouse-gray Flycatcher, and *M. rufescens* Rufescent Flycatcher). I support the decision of SACC.

YES. Reasons are stated in the proposal, especially vocalizations. Bran-colored Flycatcher makes sense given usage and naming conventions when splitting. This puts us in line with SACC and eBird/Clements.

YES. Reasons are presented in the proposal. And yes to retaining Bran-colored Flycatcher.

2025-B-3
Treat Mouse-colored Tyrannulet *Nesotriccus murinus* as more than one species

YES. I agree with following SACC and splitting *Nesotriccus murinus* as more than one species. The English name for *N. murinus*, the species in the NACC region, should be kept as Mouse-colored Tyrannulet.

YES. I vote for elevating *tumbezanus* and *maranonicus* to species rank and using the names that SACC selected for those two extralimital species. I am against any further splits in the group at this time, given the nomenclatural issues surrounding *incomptus* and which species the name would apply to. The available vocal (and maybe genetic) evidence does indicate that there are multiple additional species in the group, but that nomenclature needs to be sorted out first. Therefore, I'll vote to retain Mouse-colored Tyrannulet for *murinus* for the time being.

YES. The SACC and AviList treatment as outlined in https://www.birds.cornell.edu/clementschecklist/updates-and-corrections-october-2024/ is the safest route for now, although further study will likely result in the recognition of more species. And yes to the retention of Mouse-colored Tyrannulet (though mice do come in a wide array of colors). Familiarity and wide usage rules here.

YES. I agree with following the SACC in this case, and for retaining Mouse-colored Tyrannulet for the species in the NACC region.

YES. I agree with the SACC decision to split *tumbezanus* and *maranonicus* from *Nesotriccus murinus*, while retaining *incomptus* for the time being.

YES. I am fine following SACC in this case.

YES. I agree to following the SACC for this split and also to retaining usage of Mouse-colored Tyrannulet, per established guidelines in the case of strong asymmetry in distribution and familiarity in usage.

YES. I agree to following the SACC.

YES. I agree to following SACC and to retaining Mouse-colored Tyrannulet for the taxon occurring in our region.

2025-B-4
Transfer Lesser Whitethroat *Sylvia curruca* to the genus *Curruca*

YES. This is a vagrant species in the NACC region, and aligning with global taxonomies is recommended.

YES. I'm happy to follow other taxonomic authorities on this one. *Curruca*, as defined here, is somewhat morphologically cohesive, and it is a very old split from *Sylvia sensu stricto*.

YES. *Sylvia* clearly should be split and this is the consensus name for this clade.

YES. It makes sense to transfer the Lesser Whitethroat to *Curruca* in light of the genetic data and in keeping with global taxonomic authorities.

YES. Reasons are outlined in the proposal.

YES. I agree with the proposal.

YES. Personally, I probably wouldn't have split the genus, but I agree to follow global authorities for naming of this species that is vagrant to our area.

YES. I agree with the proposal.

YES. I agree to following Old World treatments. In North America there is still only one record (Gambell, 8-9 September 2002) for North America but Lehman (2019) in his Birds of Gambel and St. Lawrence Island monograph says there are a dozen or more records for Japan and Korea, so more Alaska records would seem to me to be likely. The photos are adequate to identify the species as presently constituted. Svensson (2023) in the third edition of his Birds of Europe outlines the subspecific differences and notes vocal (song) differences, but with some overlap. I note that Svensson then retained the species in *Sylvia.*

2025-B-5
Transfer Bluethroat *Cyanecula svecica* to the genus *Luscinia*

YES. New molecular data strongly support the close relationship of the Bluethroat to species in the genus *Luscinia* sensu stricto. The change also aligns the NACC with global taxonomies for a species mainly inhabiting the Old World.

YES. I will follow other taxonomic authorities on this one, given that it's largely outside our area. I do worry, though, that there might be future changes or reversals in this group, given that the paper we're basing this on was based on just four loci and many of the deeper node support values aren't great. Based on the available data, though,

transferring *svecica* to *Luscinia* does seem the best course of action. The alternative would be to maintain a monotypic genus, which I would be fine with given the morphological heterogeneity in the group.

YES. This seems a reasonable approach and aligns with global taxonomies. However, it could easily be argued that reversion to *Cyanecula* is as well supported, and perhaps future phylogenies will show this is the case.

YES. This changes aligns with the molecular data and with global taxonomic authorities.

YES. Reasons are outlined in the proposal, and this aligns with global taxonomic authorities. If this was just based on the phylogeny of Sangster et al. (2010), I would be a bit more hesitant, but Zhao et al. (2023) includes more loci and shows greater support for the relationship; in Zhao et al. (2023), 26 loci (including 11 nuclear loci) were included in their data matrix for Bluethroat.

YES. This change is logical given the new molecular phylogenetic information on the group and brings us in line with taxonomic authorities for this species that is largely from the Eastern Hemisphere.

YES. I vote in favor of the change, just to put us in line with the current opinion of global authorities. Hopefully, support for this clade holds up with future data and analyses.

YES. I agree with the proposal, following the taxonomic authorities.

YES. Back to 'the way we were.' I note that Svensson (2023) has this species in the genus *Luscinia.* He's always had Bluethroat in *Luscinia.* I'm uneasy making taxonomic changes on Old World issues, perhaps especially at the genus level, unless there is pretty universal agreement by Old World authorities.

2025-B-6
Transfer Greater Necklaced Laughingthrush *Garrulax pectoralis* to the genus *Pterorhinus*

YES. I agree with the recommendation in the proposal; in this case, it is better to align with global taxonomies.

YES. Another case in which I think we should follow other taxonomic authorities, but reluctantly. If this one were largely in our area, I would likely vote against it. The clade ages are shallower than in some of the other generic splits we're voting on, and as is mentioned in the proposal there is little that ties each clade together in terms of morphology. Why not treat these as a broader, species-rich genus, especially when none of them appear to be morphologically cohesive?

YES. I've long been convinced that there are multiple genera within what was formerly *Garrulax*, and this treatment seems as good as any. (Note that the extraordinarily similar

Lesser Necklaced Laughingthrush is not even closely related! I've had trouble even telling them apart in the field…)

YES. This change makes sense to align with global taxonomic authorities.

YES. I agree that this change is not strictly necessary, and I personally would prefer to keep a single, expanded *Garrulax* rather than splitting it up into several genera that have no real morphological distinctions. However, given this is a species introduced into our area and other global taxonomic authorities have all decided to split *Garrulax*, I will vote to split as well.

YES. This makes sense in light of recent global taxonomic changes, but I agree with others that I might think differently if this were not a vagrant in NACC jurisdiction.

YES. Personally I don't think splitting the genus was necessary, but I agree to follow global authorities for the naming of this species that is introduced to our area.

YES. I agree with the proposal, following the taxonomic authorities.

YES. I don't know enough about the issue to come up with a cogent argument to vote against the motion, but I tend to agree about preferring an expanded *Garrulax* genus. The print was so small I had trouble seeing which species were in each genus, but I think I saw that Lesser Necklaced Laughing-Thrush (*Garrulax monileger*) under this arrangement is no longer in *Garralux.* Greater Necklaced and Lesser Necklaced laughing-thrushes so often travel around together where I see them in the northern part of the peninsula in Thailand. They look rather alike. It just doesn't seem intuitive to me that they should be in separate genera.

2025-B-7
Transfer Eurasian Jackdaw *Corvus monedula* to *Coloeus*

YES (option C-genus). I agree with transferring jackdaws to *Coloeus* based on their deep divergence and different vocalizations, and with aligning NACC with global taxonomies for mainly Old World species.

YES (option B-subgenus; option C-genus is acceptable too). I do worry that we're over-splitting genera. Yes, there are some morphological differences versus the rest of *Corvus*, but it's definitely borderline. I find the deep split versus the rest of *Corvus* to be the most convincing. The morphological and vocal differences seem to me to be within the range of variation of other crows, some of which have pale on the head, white eyes, small bills, etc. The Caribbean crow radiation (and some of the Micronesian ones I've listened to recordings of) have shorter nasal calls that at least somewhat recall jackdaws. I admittedly don't have a ton of field experience with jackdaws, but the times I've seen them I wasn't struck by them being terribly different from a small crow.

YES (option C-genus). Mainly due to the ancient divergence. And yes, there is quite a

lot of vocal divergence among *Corvus sensu stricto* (see, e.g., Piping Crow), but to me the vocal differences between jackdaws and the others are at a different level.

YES (option C-genus). The two species are deeply divergent from other *Corvus*, and this treatment puts NACC in line with global taxonomic authorities.

YES (option B-subgenus). I feel strongly that this split is unnecessary, and is just another example of successively splitting basal lineages from larger, diverse genera. I know this is a species that is not really part of our region, and global taxonomic authorities have mostly agreed to place these species in the genus *Coloeus*, I feel strongly enough to vote against that option.

YES (option C-genus, reluctantly). I'm not a fan of naming new genera unless there is a change in topology that requires a change to maintain monophyly. Furthermore, the two species don't seem that phenotypically different to me than the other members of *Corvus*. There is a relatively deep divergence between *Corvus* and the proposed *Coloeus* that might support the split. However, the main reason I'm voting for option C is that these taxa are outside our area and the other global authorities have already made this change.

YES (Option B-subgenus). There's no need to change a monophyletic genus that it's already doing its job of indicating these species constitute a clade. I'm fine with a subgenus if we want to recognize that, but I think splitting genera that are already monophyletic introduces unnecessary taxonomic instability.

YES (option C-genus). I agree with the proposal to transfer jackdaws to *Coloeus* and recognize it as a full genus.

NO (option A). Perhaps I'm thinking of the motion on Bluethroat and the transfer back to *Luscinia.* I'm uneasy about taxonomic changes at the genus level with Old World species, especially those in Europe. How universal is the genus change being embraced there. I note Svensson (2023) still has it in *Corvus* in his 3rd edition of his European field guide. I'm not opposed to the change, but I don't see the rush. If there is universal acceptance, sure, I'd go with it. I do see that choughs are in their own genus (*Pyrrhocorax*) so I can work through seeing Jackdaws in their own genus. As for calls, I am annually exposed to Cuban Crows (*Corvus nasicus*) and their remarkable parrot-like (*Amazona*) calls.

Looking at jackdaws they do remind me of little crows in many regards. Putting them in a sub genus is interesting, but it is a taxonomic category where the parameters have never been established. I think I reviewed instances where under our notes we recognize a subgenus. I found it on only one instance, perhaps twice. And here we adopt it for a vagrant from the Palearctic as the 2nd or 3rd instance? I can't go with option C for that reason.

2025-B-8

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

YES. Vocalizations, plumage coloration, and genetic data support *Trogon tenellus* as a separate species from *T. rufus* and *T. cupreicauda*. However, the contact zone between *tenellus* and *cupreicauda* and gene flow still need to be assessed. Graceful Black-throated Trogon is a nice name for *T. tenellus*, matching its specific epithet; my second choice would be Northern Black-throated Trogon, the English name currently in use by ebird/Clements.

YES. I agree to splitting *tenellus* from *cupreicauda*. The fewer and slower notes are quite distinctive, as is the blue versus yellow eye ring. When listening to *tenellus* in the field, I mistake it most often with the three noted song of *Poliocrania exsul*, rather than with other trogons. In this regard it differs from other taxa in the complex. The lack of major differences in undertail pattern is quite interesting, mostly as a point of comparison with other trogon species complexes.

I am strongly in favor of retaining “Black-throated” in the name for historical continuity and to show relationships. Plus, most people are just going to call it “Black-throated Trogon” in the field anyway, with the acknowledgment that they are in the range of one or other of the various species in the complex. In fact, I feel that this should have been done following the splitting of the White-tailed and Violaceous Trogons. I constantly forget which daughter species went with which parent species in each region. “Was Green-backed Trogon part of White-tailed or Violaeous?” I’m not a fan of long compound names, but in the case of trogons I think it is very helpful. That leaves the “species” name part of the English name. I’m not sure how the voting should be structured, given how the proposal was written, so I’ll just toss out my thoughts on the name. I highly encourage the rest of the committee to look through the many SACC comments on this one. I find the geographic names to be useful but boring. The two options here are Central American (a mouthful given the already compound name) or Northern. If we go with a geographic name, Northern is my preference, but again I find it boring. Field observers are going to know they are in the range of just one of the “Black-throated” trogons anyways, so why not go for something more evocative and memorable? Some SACC members suggested Graceful, which has historical usage going back to Ridgway, and loosely parallels the species epithet (*tenellus* = “delicate”). Although *tenellus* is not any more delicate/graceful than any other member of the complex, it is a graceful-looking bird and is smaller than most other trogon species that it occurs with. This is also the name that SACC settled on, although a few members explicitly stated that they meant their vote to be advisory to NACC, so I do think we should be explicit about our preference here. If there is strong opposition to Graceful, I could settle for Northern, despite my issues with it (Northern is also what eBird/Clements went with).

As an aside, it seems very possible that *cupreicauda* occurs in Panama, perhaps along the immediate Pacific coast of Darién. There are records quite close by in coastal Chocó, Colombia.

YES. I support the split of *tenellus.* I voted for Graceful Black-throated Trogon for SACC, so will do so again here. (I too have often had trouble in Costa Rica distinguishing the song of *tenellus* from that of Chestnut-backed Antbird.)

YES. I agree with following the SACC in recognizing *tenellus* as a separate species and in adopting Graceful Black-throated Trogon (although I would also be ok with Northern Black-throated Trogon).

YES. Reasons are outlined in the proposal. Despite the fact that *tenellus* and *rufus* show some similarities, the fact that *tenellus* and *cupreicauda* are clearly well-defined biological species argues strongly to also split *tenellus* from *rufus*. I do not have a particularly strong feeling towards the English name for this species, but generally agree with the arguments of other committee members, and find “Graceful Black-throated Trogon” to be a good name. While “Northern Black-throated Trogon” is also acceptable and appropriate, if there are alternatives to the relatively boring and uninspired “directional” names, I much prefer those.

YES. I am good to follow SACC’s lead here. Quantitative vocal differences were quite distinct and supported by genetic data and plumage differences as well.

YES. I agree to follow SACC for this split. I prefer the name Graceful Black-throated Trogon, but also ok with Black-throated Trogon as a second choice.

YES. There is multiple evidence to treat *Trogon rufus* as more than one species:
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

YES. It will be interesting to see about areas of contact with *cupreicauda* near the Colombian border, if any. As for the English name, Northern Black-throated Trogon may be boring, but it is helpful in stating that it is the northern taxon. I looked up the meaning of *tenellus* and got “delicate.” It is the diminutive form of the word *tener,* which means “tender” or “delicate.” I see that there are some fish in the genus *Tenellus.* This is from AI. I can see a gymnast as being graceful, but I would never think of them as being delicate. The two words just don’t mean the same to me. So, I support Northern Black-throated Trogon.

2025-B-9
Change the English group name of species of *Amazona* from “Parrot” to “Amazon”

YES. I support this for reasons given in the proposal, but mainly to avoid confusion with other parrots.

YES. On balance I think the change is more beneficial than not, for the reasons given in the proposal. It’s much less confusing to use a group name for the genus rather than continue to have confusion between similarly named but distantly related taxa. One

might also argue for changing *Graydidascalus* and *Alipiopsitta* to Amazon as well, since they do appear similar and are closely related, but one has to draw the line somewhere and keeping the same name for all the members of a fairly distinctive genus makes sense, and not for those to which that name has never been used (if indeed true for those two monotypic genera). A somewhat parallel case is that of the *Cyanoramphus* parakeets, a distinctive group very widely called kakarikis in New Zealand to which they are mostly (but not entirely) confined; changing the group name would simplify the species names, for example, New Zealanders use Orange-fronted for *C. malherbi* but all the global checklists use Malherbe's Parakeet because of *Eupsittula canicularis*. But I digress.

YES. Reasons are given in the proposal, and this aligns NACC with SACC and other global taxonomic authorities.

YES. Reasons are outlined in the proposal, and this aligns with SACC and global taxonomic authorities.

YES. Reasons are outlined in the proposal and this aligns us with SACC.
YES. I agree with the recommendations of the proposal and following SACC's unanimous approval.

YES. I agree with the proposal.

YES. The comment against this proposal raises good arguments, although here I first think of the genus and not the geographical area of the Amazon Basin. There seems to be pretty broad acceptance for this English name change. I find the arguments in favor to outnumber the arguments against.

Here's a somewhat unrelated issue but a question. A number of years ago we changed the English name of Blue-throated Hummingbird to Blue-throated Mountain-gem to reflect the shared genus of other species in *Lampornis*. Some years later, I recall reviewing a paper that indicated that White-bellied Mountain-gem (*L. hemileucus*) was not closely related to the other members in the genus. If it were moved to another genus, maybe its own, would we change the English name and take "gem" out? While we consider the issue overall, I often think of my pet issue of changing our North American birds called "tanagers" to "pirangas." It would be educational. The overwhelming number of North American birders still consider them to be in Thraupidae.

NO. My issues are as follows:

1) I fully agree that names like "parrot", "warbler," and "flycatcher" are simply morphotypes. That is a strong case for retaining "Parrot" for *Amazona*, all of the species of which perfectly embody that morphotype of the large, short-tailed, square-tailed Psittacidae (and related families), although I acknowledge that this breaks down with the Australian species.
2) Yes, the parallel with the genus name is nice, but by that logic, we should be changing a bunch more things to match the genus name more closely. The Mountain-Gem example used in the proposal was one where every other species in the genus was already called Mountain-Gem, and it was just *clemenciae* that was

changed from hummingbird to match the rest of the genus. Here, we're proposing to change all species within one genus, which perfectly matches the morphotype of "parrot" to another name, apparently mostly because it's used in the aviculture trade and therefore has more hits on google. That seems illogical and unnecessary to me.

3) The two sister genera to *Amazona* (*Graydidasculus* and *Alipiopsitta*) have very similar morphotypes, and having one group be called Amazons and the others Parrots implies that they are more different than they are.
4) Calling these birds "Amazons" implies (whether intentionally or not) that they are mostly found in the Amazon Basin, which is not true. Most of the species diversity is in Middle America and the Caribbean, and before the arrival of humans there was an even greater diversity in the Caribbean. Should we then call them "Caribs"? (yes, I know that's taken).

2025-B-10

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

YES. As mentioned in the proposal, there is not a strong case for recognizing *scotica* as a separate species under BSC. However, I am inclined to accept global treatment for species that do not occur in the NACC region. English names: Red Grouse and Willow Ptarmigan.

YES. It's outside our area, and the evidence seems moderately strong for species status. The differences in molt and seasonal coloration are striking, but my major concern is regarding vocalizations and displays. Given that this is a highly dimorphic grouse with prominent sexual displays, the display vocalizations of the two groups are, to my ear, nearly identical. Furthermore, what little I can find on Birds of the World and a brief literature search indicates that the courtship displays are similar. In fact, the first sentence on the topic in Birds of the World states that "Detailed accounts of courtship and mating behavior published only for Red Grouse in Scotland, but behavior in North America similar". The only part I can stretch into a difference is that when Willow Ptarmigan display, they raise the tail and droop the wings, which are white during display season in at least the Alaskan populations I've watched doing this. Given that *scotica* is solidly dark on the wings, this would make for a different-looking display. I don't know, however, whether this difference holds up in Willow Ptarmigan populations that are geographically adjacent to *scotica*. From the few photos I can find online, the populations in southwestern Norway (subspecies *variegata*) do appear to have solidly white wings and bellies in at least the early summer, so perhaps these differences are real.

The whole genome data are quite convincing, despite the lack of vocal differences. Based on biogeography, I would have expected *scotica* to be a weakly differentiated offshoot of the circum-Arctic *lagopus*, but it does not appear to be the case. Rather, it is nearly as different genetically as is *muta* in the PCA plot (note that the phylogeny is a cladogram).

As for names, both Willow Ptarmigan and Red Grouse have long historical usage and should be retained for *lagopus* and *scotica*, respectively, despite one being called a Ptarmigan and the other a Grouse.

YES. My vote is based on the congruence between plumage, molts, and the latest DNA analyses that do not support the notion that *variegata* is truly intermediate. Granted, vocal and/or display differences could clinch the case, and it is a bit disconcerting that they are so similar vocally even though the other ptarmigans are not. (However, even in Franklin's Grouse, which has a display difference from Spruce that at first seems to bolster species status, this breaks down with intermediate populations both in display and plumage.) For me, the Red Grouse just makes it over the line to species status based on its striking plumage and molt differences and the phylogenetics. It's not typical of subspecies-level divergence, in my opinion.

YES. The combination of plumage and genomic differences, along with the WGAC vote in favor (albeit weakly) of a split, lean me toward a yes vote.

YES. This is another very tough, borderline case, but I will go with the global consensus here and vote to split *scotica* from *lagopus*. However, I do have strong reservations, especially surrounding the apparent lack of vocal differences, which are very striking among the other ptarmigan species. If *scotica* does get split, then I vote to use the name Red Grouse since it has such long standing historical usage (and given it does not occur in our area, whatever name is agreed upon by global authorities is fine with me).

YES. Morphology and genetics provide some evidence. Borderline case for a species that is outside our area, so I prefer to err on the side of agreeing with local decisions in this case.

YES. I accept the global treatment given that the species does not occur in the NACC region. English names: Red Grouse and Willow Ptarmigan.

NO. The genetic divergence is shallow and some neutral divergence is expected of an island form when compared to continental populations. The same relationships recovered in the Kozma et al. (2019) whole-genome ML tree are not recovered with mtDNA. Diagnosability based on whole-genome data does not equal species limits under a BSC to me—many geographically isolated populations meet this criterion as well without being treated as separate species. I would have liked to have seen more demographic analyses with the genomic DNA, such as an estimate of the timing of the split and testing demographic models that compare historical vs ongoing gene flow vs no gene flow since the split. The main difference is the lack of polyphenism in the *scotica* group, which retains its reddish-brown plumage year round. However, the *variegata* group also has some differences in its winter plumage and the timing of its plumage, suggesting that these may be adaptations to local phenology across the lineage's distribution—which would make sense given how important camouflage is to this group's survival. I'm not convinced these are species under the BSC, but suppose it's really up to the taxonomic authorities of that region to decide on that.

NO. It is an island isolate, so I would expect genetic divergence. Given the lack of vocal or display differences, it should be treated as a subspecies. As noted by another committee member, the other two ptarmigan species really sound different. I note Svensson (2023) does not split it in the 3rd edition of his European Field Guide. He gives it a subcategory and treats it as a subspecies saying ”shape and habits as Willow Ptarmigan” and voice “identical to Willow Ptarmigan.” There are hundreds of thousands of British birders and serious students of birds. Given the desire to have their own endemic species, you would think that any sign of a difference from Willow Ptarmigan would have seized upon it. I don’t even see much in the way of comments saying it might be a separate species. The WGAC vote passed (barely) but there didn’t seem to be much enthusiasm and under our voting system the motion for a split would have failed. Everyone will still call *scotica* “Red Grouse,” regardless of whether it is recognized as a subspecies or a full species. Oh, the other British endemic “species” is Scottish Crossbill, a type that is not recognized by Svensson (2023) in the third edition of his field guide. It will be interesting to see if there is much European acceptance of Red Grouse as a full species. This is another case where I would have preferred to see how accepted the WGAG split was in Europe.

2025-B-11
Treat *Asio wilsonianus* as a separate species from Long-eared Owl *A. otus*

POSTPONED

2025-B-12
Treat Burmese Collared-Dove *Streptopelia xanthocycla* as a separate species from Eurasian Collared-Dove *S. decaocto*

YES (a) and (b). Reasons are mentioned in the proposal. Keep Eurasian Collared-Dove as the English name for *Streptopelia decaocto*.

YES (a) and (b). Reasons are in the proposal. The plumage, vocal, and phylogenetic differences are certainly borderline, but given that all three are consistently different and the taxa appear to be allopatric, tips the scales to species for me.

YES (a) and (b). In addition to the eyering, which is undeniably distinctive, the vocalizations of *xanthocycla* seem more distinctive to me than just the number of notes, with more inflection on the first or (if 3-noted) second note and a somewhat different rhythm in *xanthocyla*, even when three-noted (in which case the first note is very short). Of course this is based on a very small sample of *xanthocycla* but a huge one of the very familiar *decaocto* (of course, I didn’t check the majority of them!). And the DNA analyses seem to support their being moderately divergent, although as the proposal authors point out there is the possibility that this is influenced by an artifact. Even if so, the other differences seem to better reflect species status given the huge distances of its natural

range over which *decaocto* is essentially the same, and then the sharp break with the allopatric, distinctive *xanthocycla*.

YES (a) and (b). This is not a super strong case in my opinion, but the combination of differences presented in the proposal along with alignment with most global checklists for a species that is introduced in the NACC region leans me toward accepting the split. I also vote to keep Eurasian Collared-Dove for *S. decaocto*.

YES (a) and (b). Once again, this is a fairly borderline case. I do not have very strong feelings one way or the other on this split, but will agree to follow global taxonomic authorities here. I agree to keep “Eurasian Collared-Dove” for *decaocto*.

YES (a) and (b). Nice proposal. The plumage differences are pronounced and the vocalizations perhaps even more so with the differences in harmonic structure. This is well outside of our region, so I am generally inclined to follow global authorities there.

YES (a) and (b). Lots of evidence for this split: genetics, vocalizations, plumage color, etc. I agree to retaining Eurasian Collared-Dove for the English name, following rules governing splits (asymmetrical distribution, current usage).

YES (a) and (b). There are several lines of data that support the separation (genetics, vocalizations, plumage color).

YES. Reasons are outlined in the proposal. I think this is a good split and is in line with other like-plumaged doves in the genus *Streptopelia.*