[2023-C-1: Treat *Lepidothrix velutina* as a separate species from Blue-crowned Manakin *L. coronata*; (a) recognize *L. velutiina* as a separate species, (b) adopt Velvety Manakin as the English name for *L. velutina*; and (c) adopt Blue-capped Manakin as the English name for *L. coronata*]

[2023-C-2: Transfer Thicket Antpitta *Hylopezus dives* to *Myrmothera*]

[2023-C-3: Treat American Three-toed Woodpecker *Picoides dorsalis* as a subspecies group of *P. tridactylus*]

[2023-C-4: Treat *Colaptes aeruginosus* as a separate species from Golden-olive Woodpecker *C. rubiginosus*]

[2023-B-5: Treat *Melanerpes santacruzi* as a separate species from Golden-fronted Woodpecker *M. aurifrons*]

[2023-C6a: Treat *Sclerurus obscurior* as a separate species from Tawny-throated Leaftosser *S. mexicanus*]

[2023-C-6b: Treat *S. pullus* as a separate species from Tawny-throated Leaftosser *S. mexicanus*]

[2023-C-7: Revise the taxonomy of *Amaurospiza* seedeaters]

[2023-C-8: Treat *Pipilo socorroensis* as a separate species from Spotted Towhee *P. maculatus*]

[2023-C-9: Revise generic limits among *Rhodothraupis*, *Periporphyrus*, and *Caryothraustes*]

[2023-C-10: Treat *Pachyramphus uropygialis* as a separate species from Gray-collared Becard *P. major*]

[2023-C-11: Treat *Chlorospingus hypophaeus* as a separate species from Yellow-throated Chlorospingus *C. flavigularis*]

[2023-C-12: Treat *Melozone occipitalis* as a separate species from White-eared Ground-Sparrow *M. leucotis*]

[2023-C-13: Treat *Granatellus francescae* as a separate species from Red-breasted Chat *G. venustus*]

2023-C-1:
Treat *Lepidothrix velutina* as a separate species from Blue-crowned Manakin *L. coronata*; (a) recognize *L. velutiina* as a separate species; (b) Adopt Velvety Manakin for *L. velutina*; and (c) adopt Blue-capped Manakin for *L. coronata*

YES. (a) The differences in vocalizations alone are sufficient evidence for me, regardless of degree of genetic differentiation. (b) Not only does Velvety have a long track record, but it also is an apt description that is also memorable. The word “velutinus” means “velvety” in Latin, so that’s nice. (c) Good idea. This maintains the connection with “Blue-crowned” and has the bonus advantage of already being used historically. Retaining Blue-crowned for either daughter species would be contrary to our guidelines because this is a classic parent-daughter split with both daughters having large ranges; therefore, retaining “Blue-crowned” for one of the daughters would lead to perpetual confusion. This is a case in which “stability” (retaining Blue-crowned) is disadvantageous because the species classification itself has been destabilized --- time to learn new names to go along with a new taxonomic concept.

YES. Great to see such a thorough study and proposal in which all the evidence points to the same inescapable conclusion. I agree that the plumage differences would be difficult for us to discern in nature but perhaps not for the birds themselves, though they wouldn’t normally encounter the other taxa in any case.

Do we not need to vote on the English name Velvety Manakin? I put in an option 1b and vote yes, especially as it’s a direct translation of the specific epithet, and it has already been comfortably adopted by SACC.

YES. (a) The combination of genetic and vocal data along with plumage differences support this split. In addition, this has already been adopted unanimously by the SACC and I don’t see any reason not to go along with that treatment for sake of consistency. (b) Velvety Manakin seems like a good name.

A. YES. (a) The vocal differences are striking and undeniable. Everything else (genetics, plumage) just adds more evidence that they should be treated as different species.

B. YES. Seems like a good appropriate name, in circulation already, and SACC agrees.

C. YES. Agree with whatever SACC decides, as this species is completely within their area.

YES on (a) (b) and (c). Excellent study and proposal. We should follow SACC as this is largely in their jurisdiction.

YES. (a) The data and the analysis is clear, *L vetulina* is a different species from *L. coronata*. (b) I agree with the English name proposed. Velvety Mankin for west-of-Andes populations. (c) I agree with the English name Blue-capped Manakin for east-of-Andes populations.

YES. This seems like a very straightforward split, given the very strong differences in vocalizations and the deep divergence involved. I vote to adopt the English names Velvety Manakin for *Lepidothrix velutina*, and Blue-capped Manakin for *L. coronata* east of the Andes. A yes vote also brings NACC into alignment with SACC.

YES on (a), (b), and (c). The genomic and phenotypic (especially voice) show strong concordance and in this case indicate species limits.

YES. (a) The strikingly different vocalizations and the strong genetic divergence support the treatment of *Lepidothrix velutina* as a separate species from *L. coronata*. (b) The English name Velvety Manakin for *L. velutina* is a great choice.

YES to all parts, including adopting the English name of Velvety Manakin which I agree is a great English name. All lines of evidence argue for this split.

2023-C-2:
Transfer Thicket Antpitta *Hylopezus dives* to *Myrmothera*

YES. See SACC comments.

YES. I think we should follow SACC on this rather than create a more heterogeneous genus, but the phylogeny suggests that the single-genus status of *Grallaria* should also be reevaluated (obviously beyond the scope, though).

YES. Support for the position of *dives* is not that strong but it clearly belongs to the clade that includes *Myrmothera* plus "*Hylopezus berlepschi*" and "*H. fulviventris*" apart from other *Hylopezus*. In addition, this has already been adopted unanimously by the SACC and I don't see any reason not to go along with that treatment for sake of consistency.

YES. Moving *H.* dives *to Myrmothera* seems like the best course of action to avoid a large cumbersome and heterogeneous genus if *Hylopezus* and *Myrmothera* were lumped. This action also agrees with the SACC decision, which involved many more species.

YES. I agree that moving these taxa into *Myrmothera* is more prudent than a treatment with multiple, smaller genera. Follow SACC.

YES. I agree with the proposal; the reconstruction resolved the position of *H. dives* into *Myrmothera* and with this movement it will produce stability of the taxonomy in the group. I agree with the SACC

YES. This seems like the best approach for this taxon, and voting yes will also bring us into alignment with SACC.

YES. This is a good taxonomic solution for this portion of the genus *Hylopezus* as currently configured.

YES. Multilocus phylogeny supports the transfer of *Hylopezus dives* to *Myrmothera*.

YES. The best option at this time. Follow SACC.

2023-C-3:

Treat American Three-toed Woodpecker *Picoides dorsalis* as a subspecies group of *P. tridactylus*

NO. I agree with the proposal that it's premature to re-lump these taxa. As noted, the mtDNA data are intriguing and I believe that genomic data are being generated so I'd prefer to wait until such data are published. Furthermore, a quantitative analysis of vocal differences is clearly needed - as noted in the proposal, "Unfortunately, our recordings are insufficient for comparison of these geographical groups. This species, the only Holarctic woodpecker, would be an ideal subject for the study of geographical variation ... (Winkler & Short 1978:16)." Sounds like a fun project!

NO. I browsed recordings of these and their vocal array seems pretty similar, with possible differences including the drumming of *dorsalis* diminishing near the end instead of ending abruptly, and the pik notes sounding higher-pitched, but there is enough variation that a thorough study is needed to demonstrate whether the differences are real and playback experiments may also be helpful. Since we know that genomic data are on the way it would clearly be better to wait, especially as the mtDNA distances are not insignificant.

NO. I agree that we should wait for a more rigorous analysis of genetic differentiation. They are very allopatric, so a direct test of BSC is impossible, but given that there are additional data potentially on the way, I agree that we should wait to assess those data before we change the status quo.

NO. I agree with the proposal. A complete study of the groups involved (*dorsalis* and *tridactylus*) is needed, involving both morphological and molecular data.

NO. I agree with the recommendation in the proposal; a taxonomic change is not advised without any new evidence supporting it, even when it is to reverse the taxonomic treatment to a previous one.

NO. I agree with the recommendation of the proposal that re-lumping these taxa based on no additional data is not warranted, and goes against many of our other decisions where we have declined to split taxa on largely similar grounds. I agree that phenotypically the North American taxa are not especially distinct from European taxa, the same cannot be said of all Old World taxa, notably subspecies *funebris* and *albidior*, so I think a full phylogeographic study with nuclear DNA is warranted with sampling across North America and Eurasia to fully resolve this group.

NO. Reasons are given in the proposal.

NO. I worry we probably made a mistake in 2003 by splitting these two. The Committee at the time was relatively inexperienced in assessing the importance or lack of it in genetic differences between allotaxa. I can't remember how I voted, but I think it was a NO to the split because thorough documentation of vocal differences was lacking. In 2013, Dickinson & Remsen (H&M classification) followed the split just because the policy was to follow NACC explicitly, not necessarily because those species limits were endorsed.

So, why am I voting NO on this one, thus supporting the status quo? Because with the benefit of being able to listen to online recordings, I am fairly certain that I hear distinct differences between the calls and maybe the drumming of Western Palearctic and North American birds. My impression is that the European birds have a lower-pitched, richer call than Nearctic birds, and this is likely what led to the statement quoted from Winkler & Short: "*North American birds (New York) are different in all their measurable characters*." I also think I hear the same drumming differences that have been pointed out. Of course there is no formal analysis we can point to other than the Table in Winker & Short, which as those authors noted had a low N for Nearctic recordings. Also, the useful Xeno-canto recordings are mostly European, with minimal material from almost all of the Eastern Palearctic. So, in voting NO, I'm betting that a more formal analysis, or better, playback trials, will show that the vocal differences are at the level associated with known species differences in parapatric/sympatric woodpeckers.

As for the genetic differences noted in the proposal, I hope that today's NACC would consider these insufficient evidence on their own for species ranks. The genetic sampling has come strictly from neutral loci, which are a fair indicator of time-since-separation but a much, much poorer indicator of species limits. We now have examples of taxa treated as species with barely any genetic differences between them (e.g., capuchino seedeaters) as well as taxa that are indistinguishable phenotypically that differ in genetic distance by as much as 8% sequence divergence (e.g., *Bleda syndactylus*). With substantial gaps in suitable habitat between Nearctic and Holarctic populations, one would predict plenty of genetic differentiation at neutral loci. I continue to be puzzled by the call by some NACC members for more and better genetic data from allopatric populations. What would those data tell you about taxonomy? There is no magic threshold of genetic distance that indicates species rank. Even comparative genetic distances among related species are insufficient on their own for assigning taxon rank.

As for the plumage differences, weak or strong, we already know empirically that plumage differences in woodpeckers are essentially useless for predicting gene flow in contact. *Colaptes* flickers are the extreme example. The two major subspecies groups differ in virtually every plumage character to the point that del Hoyo and Collar treated them as separate species despite abundant data that gene flow is unconstrained between them: there is an extensive hybrid zone with nothing but intermediate individuals. (Inexplicably, this actually counts as 1 point *in favor* of species rank in the Tobias et al. scoring.). In contrast, Downy and Hairy are so similar that they are difficult to tell apart by plumage unless you see the underside of the outer rectrices. Red-breasted and Red-naped Sapsuckers can be distinguished by plumage from as far away as you can see them, yet gene flow is fairly high. Nuttall's and Ladder-backed are very similar in plumage yet hybridize only rarely. And so on. Thus, use of plumage differences as an indicator of potential gene flow is downright unreliable and therefore should play no role in our decision. Note, however, that the flicker voices are indistinguishable as far as is known, and sapsucker voices may also be indistinguishable, whereas in the for-sure species, vocal differences are detectable as far away as one can hear them. I will admit, however, that the plumage differences between Nearctic and Palearctic Three-toeds are indeed at the lower end of the spectrum.

In summary, using plumage or genetic differences in assigning taxon rank in woodpeckers is problematic.

As for editorial constraints being the reason for lack of explicit rationale in the Peters series, that is true, but there was nothing stopping Peters and other authors in those volumes from making clear that rationale in separate papers, which they and others in fact often did in notes in the *Auk* or their museums' own publication series. I would agree that that generation of taxonomists had knowledge of geographic variation of plumage in morphology that is unmatched by most ornithologists of today; however, ornithologists of that generation mostly predated the explosion of empirical research on the importance of voice in reproductive isolation. Furthermore, the person who had arguably the greatest knowledge of geographic variation of North American taxa, Robert Ridgway, had his decisions, for which at least oblique rationale had been provided, routinely overturned by subsequent authors such as James Peters. Modern research often shows that Ridgway's were "better". If Peters and others were going to overturn the foundational decisions of Ridgway, then they should have been obliged, in my opinion, to justify those changes in print somewhere. Same goes for the foundational work for Neotropical species by Hellmayr and Cory, for which rationale was often provided in footnotes. Therefore, I see no reason to be wedded to Peters-genre decisions just because those volumes' editorial policy was not to allow inclusion even of skeletal rationale. Finally, Peters himself must have been the one who set that editorial policy for those typically skinny volumes, so there is no valid reason for giving him a "pass" on this issue and treating those unjustified reversals of Ridgway-Hellmayr-Cory classification as sacrosanct.

In this particular case, Ridgway treated them as separate species, and Peters overturned this without comment, so in my opinion this is another point in favor of keeping them as separate species, as indicated by Banks et al. (2003)

NO. The similarities or differences in vocalizations need to be analyzed much more in depth to change the species status of these two allotaxa. As mentioned above, plumage differences don't carry much weight in closely related taxa of woodpeckers, and genetic differences in allotaxa like these tell us nothing about reproductive isolation.

NO. The amount of divergence in mtDNA is pretty substantial for intraspecific variation. Even given all the caveats of mtDNA, the argument to lump these into one species based on phenotypic data alone is not strong enough. I think it's prudent to wait for sufficient nuclear data (genomic analyses of multiple individuals). Vocal analyses would also be helpful.

NO. Here is a case where I wish we could have seen the previous proposal and the comments. I believed I voted for the split. I believe that some of the data we looked at then (2003) involved Black-backed Woodpecker (*P. arcticus*). From my faded memory I seem to remember that certain aspects of Black-backed were more similar to Old World Three-toed Woodpeckers than to New World birds. From my field experience with woodpeckers, I think voice is very significant. From those species I know best (North America and Asia), they are most vocal near dusk and at dawn when calls can be pretty much continuous. This is usually around, or not far, from roost cavities. I would think a recognizable difference in calls would cause a problem in pair-bonding. And as pointed out, drumming needs to be included in the analysis too. I do hear a difference in the drumming between American Three-toed and Black-backed Woodpecker and their calls differ too. No one suggests lumping these two as they are sympatric over much of their range. I agree with another comment about the flickers sounding the same and the Yellow-bellied Sapsucker complex. The longer I look at the latter, I've come to believe that, in particular, the recognition of Red-naped and Red-breasted Sapsucker as separate species is untenable. Here on the east side of the Sierra in migration and winter, I see so many hybrids. It's hard to believe that there are any isolating mechanisms taking place. With flickers, I nearly voted to split *Colaptes auratus mexicanoides* as a separate species as their calls, the breeding season "song" sounded quite different to North American flickers. On the other hand, a split of the "Yellow-shafted Flicker (Cuban *chrysocaulosus*) seems unwarranted as their "song" sounds identical to North American birds and I would guess the same is true for similar *gundlachi* from Grand Cayman. Of course, Gilded sings the same as N.A. birds to my ear. Off-hand can anyone come up with other cases where calls noticeably differ between subspecies of a single polytypic species other than the species we are discussing now? Yes, and the matter of "Holarctic Woodpecker" too. When all is said and done, I'm guessing we will still split New and Old World groups as separate species.

2023-C-4:

Treat *Colaptes aeruginosus* as a separate species from Golden-olive Woodpecker *C. rubiginosus*

YES. While the situation is obviously very complex, as the proposal makes clear, the clear differences I hear and see in all vocalizations between *aeruginosus* and *rubiginosus yucatanensis* leave no doubt in my mind that *aeruginosus* is a separate species, whatever happens with the rest of *rubiginosus*, as already recognized by IOC (which should probably have cited Howell and Webb instead of Moore, though), HBW/BLI, and Peters. This level of vocal differentiation is highly atypical of within-species variation in woodpeckers, in my opinion.

YES. I can't argue with those wanting more data, but my sentiments are with the other "yes" vote. Moreover, it's been nearly 30 years since Howell and Webb (1995). Surely central Veracruz isn't that remote, is it?

NO. This proposal does a good job of summarizing a complex situation and confusion over prior published data. Clearly this complex requires a thorough study of geographic variation in vocalizations and plumage, better sampling for genetic data, and assessment of putative contact zones and levels of intergradation between subspecies. I agree, this would be a great grad student project!

NO. I agree with the proposal. The data are incomplete and it is very difficult with current data to argue that *C. aeruginosus* is a separate species from *C. rubiginosus*. Complete genetic data for *aeruginosus*, at least, and more complete sampling of these groups are needed. It would also be desirable to have morphological and color data to have a better understanding of the group.

NO. A comprehensive phylogeographic study, with sampling across the geographic range including possible contact zones between subspecies, and quantitative vocal analysis are necessary to assess species limits in *Colaptes rubiginosus*.

NO. What an intriguing, uncertain situation. I agree that before we make changes more data are needed.

NO. As outlined in the proposal, multiple species are almost certainly involved, but with the data at hand, this situation is too complex to make potentially premature or incorrectly defined splits without more data. I hope someone takes on this project. Hopefully, the problems outlined in the proposal will provide a catalyst for such a project. Vocal differences or lack of them are strongly associated with species limits in woodpeckers, so It’s clear in my opinion that more than one species is involved; however, making a mistake in where to draw the lines the absence of stronger data are more objectionable to me than maintaining these taxa incorrectly as a single species.

NO. Given that the rationales for splitting *aeruginosus* by other parties were: a) an unidentified genetic sample that turned out not to be *aeruginosus* ; and b) vocalizations based on very small samples, I feel that we should hold off for a tighter analysis. The few available long call vocalizations of *aeruginosus* are dramatically different from the other taxa, but it would be better to have more samples of *aeruginosus* and to have samples of homologous vocalizations of *yucatanensis* closer to the contact zone.

NO. More data are needed. Apparently there are no genetic data, although previous authors assumed a sample was *aeruginosus*. The vocal data are intriguing; however, by themselves it’s not enough in my opinion. But further research in this group will likely show multiple species; the study by Dufort (2016) shows *rubiginosus* is polyphyletic.

NO. As others have noted, more data are needed.

2023-C-5:

Treat *Melanerpes santacruzi* as a separate species from Golden-fronted Woodpecker *M. aurifrons*

NO. This is a complex issue, but the proposal lays out the available data nicely and explains why additional study (with more sampling and a better understanding of the contact zone) is needed before splitting these taxa. Hopefully that would also include a quantitative analysis of geographic variation in calls.

NO. I think the problem may be even broader than this, involving *hoffmanni* and *rubricapillus* (which hybridize in Costa Rica). On a quick look, they actually sound rather different from each other (*hoffmanni* higher-pitched, *rubricapillus* more toneless and rattled) than the various forms of *santacruzi* seem to from *aurifrons* and yet it doesn’t stop them interbreeding. And the genomic results don’t show an easy route to splitting either of *santacruzi* or more narrowly within *santacruzi*. Perhaps a future genomic study involving more taxa will clarify matters, but it’s long been known that hybridization is extensive so we shouldn’t expect easy answers here.

NO. The group is very complicated both morphologically and in color patterns. And furthermore, the sampling in the Yanes-Quevedo study is not the most appropriate. More complete sampling is desirable to recognize *santacruzi* as a separate species from *aurifrons*. This sampling should include samples from different locations of *aurifrons*, *santacruzi* and *carolinus*.

NO. Although the proposal presents a good amount of previous research, species limits in these woodpeckers seem to be more complex than the available data allows disentangling. Understanding the contact zone between *santacruzi* and *aurifrons* will require larger sampling in the geographic range of *aurifrons*. And as mentioned in a previous comment, understanding species limits in the taxon currently known as *Melanerpes aurifrons* might require integrative studies with the inclusion of other closely related species.

NO. Another woodpecker proposal I am on the fence about, but, ultimately, I think the gaps in sampling localities from Llanes-Quevedo et al. (2022) are enough to give me pause. It is a little troubling that there appears to be so much introgression; however, given that almost all of their *aurifrons* samples come from near the contact zone with *grateloupensis*, perhaps this is not surprising. I'd be really interested to see sampling of *aurifrons* farther from the contact zone, and to have an actual transect to better understand what patterns of gene flow might be. I wouldn't be surprised if *aurifrons* was sampled from the core of its range, a K = 3 would be strongly supported, rather than the K = 2 which initially only separates *carolinus* from the other two groups. Utlimately, however, I think this is probably a good split, but based on the current evidence, support is not very strong.

NO. Reasons are outlined in the proposal. Side note: Regardless of whether there is assortative mating in the contact zone between *aurifrons* and *santacruzi*, the levels of admixture suggest that whatever reproductive isolating mechanisms might exist, they are notably incomplete. As Llanes-Quevedo et al. (2022) show, there is still considerable gene flow between the two.

NO. The proposal does a great job of outlining the pros and cons of the proposed split as well as the inadequacies of parts of the data. For all the reasons in the proposal, I say wait until these questions are resolved, and those questions provide an excellent guideline for what additional data are needed. I should also add, once again, that a strict monophyly criterion at the species level is not necessary in my opinion, as pointed out by none other than Willi Hennig.

NO. I agree with the proposal that we should wait for more extensive analyses, in particular if these taxa are mating assortatively near the contact zones. The genetic results are messy and not indicative of reproductive isolation.

NO. Lots of data and analyses are available for this complex, more so than many other similar geographically variable taxa. Nevertheless, despite the amount of data available, *M. santacruzi* still does not stand out as a separate species, as defined by the Biological Species Concept. As illustrated in Fig. 4 of Llanes-Quevedo et al. (2022), there is quite a bit of admixture/introgression. Would probably be a species using other definitions, but does not fit criteria of BSC.

NO. Reasons are outlined in the proposal.

NO. I found this proposal hard to follow, especially to the recommendation part. Yes, OK, maybe a split of *santacruzi* from *aurifrons* but then what to do with the rest of the taxa in the complex and their relationships? Distinct differences in calls suggest species level differences to me.

2023-C-6a:
Treat *Sclerurus obscurior* as a separate species from Tawny-throated Leaftosser *S. mexicanus*

YES. This split seems obviously required based on paraphyly with *rufigularis* (which is broadly sympatric with SA forms in Amazonia), the distinct vocalizations of Mesoamerican birds, and levels of genetic divergence.

YES. This split is supported by two independent molecular datasets that show paraphyly, complemented by clear vocal differences.

YES. I agree with the proposal; the molecular data are clear to separate *obscurior* from *S. mexicanus*.

YES. Phylogenetic and vocal data support the treatment of *Sclerurus obscurior* as a separate species from *S. mexicanus*.

YES. Reasons are outlined in the proposal. The genetic data are very strong for showing *mexicanus* as currently defined is not monophyletic. Their vocal distinctness is further support for recognizing North American taxa as separate from the South American taxa.

YES. Genetic and vocal data make this one a slam-dunk. Note the very different call of *mexicanus*. That the South American *obscurior* group is actually more closely related to *S. rufiventris* makes sense from a plumage standpoint as well.

YES. Two sets of genetic data as well as vocal data argue for splitting these two (apparently allopatric) taxa. However, the tree from d'Horta et al. 2013 shown in Fig. 2 of the proposal doesn't necessarily indicate lack of monophyly, it just shows no support for monophyly; Nevertheless, a deep, three way split between the two taxa in question and *rufigularis* supports a Yes vote. The Harvey et al. (2020) tree does show lack of monophyly.

YES. Overall, the data support species limits between the two taxa.

YES. The split seems warranted given the vocalization and genetic data at hand.

YES. I think a broad split of *mexicanus* is the best first initial course and that a further split of *pullus* awaits more data, particularly further elucidation of what is going on in eastern Panama. I note in Dickinson and Christidis (2014) that *pullus* is restricted to the highlands of Costa Rica and western Panama (to Veraguas, Cocle), *andinus* extends west to the Darien and San Blas, and *obscurior* to Cerros Pirre in the Tarien, Tacarcuna and Mali). My uncertainty then is eastern Panama and whether there is one or two species there. I look for clarification, if possible.

NO. The proposal does not include any information on phenotype, nor does it state how we know there is parapatry in the Darien. The samples for the genetic analyses are quite far from this area. In addition, there is much more variation in calls than is presented in the proposal. There are examples of fast delivery of notes in *mexicanus* (e.g., XC234698). The song figure is based on one sample of *mexicanus* (s.s.). Although I don't doubt that these are probably good species, looking through the proposal, the papers in the Lit CIted, listening to calls and songs on xeno-canto, and looking at Remsen (2003), I don't think that this case has been sufficiently made. If there is parapatry, what do the specimens tell us from that area? Have their voices been recorded in that area? Santiago Claramunt had similar qualms in his comments for the SACC. I think my vote could be changed with some better background material.

2023-C-6b:
Treat *S. pullus* as a separate species from Tawny-throated Leaftosser *S. mexicanus*

YES. Given the vocal differences between *mexicanus* and *pullus*, this split seems warranted.

YES. I can understand Committee members' hesitation on this one because of the small N on songs and calls, but if the sonograms and PCAs are even close to being representative, then *pullus* has one of the most distinctive songs of any taxa in this entire group. For example, *Sclerurus rufigularis*, which is sympatric with *Sclerurus mexicanus* s.l. and so has to be treated as a separate species, is more similar in songs and calls to members of the *mexicanus* group than it is to *pullus*. That slow song with an exaggerated inflection really stands out in the limited universe of vocal variation in the group. There is no known contact zone between *pullus* and *mexicanus*, so we're going to have to use such comparisons as a way to assign taxon rank.

YES. Deep genetic split, reciprocal monophyly, and the presented vocal differences are sufficient enough for me to designate as different Biological Species.

NO. This may ultimately be justified but the vocal differences from *mexicanus s.s.* don't seem so very different that splitting is required, and the genetic divergence, though relatively deep, doesn't mandate the split. Further study may well confirm that splitting is justified, as in so many cases of taxa with similarly discontinuous ranges (Mexico and northern CA vs. Costa Rica and Panama).

NO. This split is less clear to me. The genetic divergence is relatively deep, but the vocal differences are not as strong and it would be nice to conduct playbacks to see how *mexicanus* and *pullus* respond to each other. Also, is there a putative contact zone? A split may well be justified, but it seems like further study is warranted.

NO. The data are not very clear to support this separation, a more complete study is needed in this group.

NO. Available data support the treatment of *S. m. mexicanus* and *S. m. pullus* as subspecies. More research is needed to better understand the species limits within *Sclerurus mexicanus*.

NO. I reiterate my comment above: The proposal does not include any information on phenotype, nor does it state how we know there is parapatry in the Darien. The samples for the genetic analyses are quite far from this area. In addition, there is much more variation in calls than is presented in the proposal. There are examples of fast delivery of notes in *mexicanus* (e.g., XC234698). The song figure is based on one sample of *mexicanus* (s.s.). Although I don't doubt that these are probably good species, looking through the proposal, the papers in the Lit CIted,listening to calls and songs on xeno-canto, and looking at Remsen (2003), I don't think that this case has been sufficiently made. If there is parapatry, what do the specimens tell us from that area? Have their voices been recorded in that area? Santiago Claramunt had similar qualms in his comments for the SACC. I think my vote could be changed with some better background material.

NO. Distinctiveness and reciprocal monophyly are not species limits criteria; both are expected of allopatric subspecies. For now continuing to recognize them as subspecies seems warranted.

NO. It looks like only one *mexicanus* song was analyzed and included in the PCA plot in Fig. 5, so we don't have a good sense of the variation within *mexicanus* vs *pullus*. While the split is reciprocally monophyletic and decently deep, it seems on par with divergence observed among the South American populations, so I would treat these as subspecies for the time being.

NO. I need clarification on what is going on in eastern Panama. If Dickinson and Christidis (2014) are correct that *pullus* is restricted to the highlands of Costa Rica and western Panama, then Talamanca Leaftosser would be a good English name. The morphological differences between *mexicanus* and *pullus* illustrated by Vallely and Dyer (2018) seem pretty minor to me. Additional vocal studies would be very useful. The large range gap (much of Nicaragua) between these two taxa could well suggest species level differences. But again, what goes on in central and eastern Panama?

2023-C-7:
Revise the taxonomy of *Amaurospiza* seedeaters

YES to the proposal's recommendations, i.e. YES to "(c)" and NO to a-b-d. But only because this seems to be the best overall taxonomy given the collection of weak data we have and the lack of stability in treatment of these taxa in various classifications. The proposal does a terrific job of assembling every bit of published information on the group, but even combining all this information, I see big problems. The voices may be separable in DFA space, but are those differences really significant? DFA on the geographic dialects of a related species, *Passerina cyanea*, would likely be able to discriminate them as well. I lean more towards Boesman's qualitative impression that the songs are very similar, remarkably so in my opinion given the great distances involved from one end of this group to the other and the gaps in distribution. Playback trials would seem to be a necessary step. The genetic data are of interest, of course, but are weak in terms of sampling of genes (mostly ND2), taxa, N, and geographic sampling with populations. That's not the authors' fault – these are mostly hard-to-find, rare-to-uncommon birds – the studies did the best they could with the material available. The minor differences in bill size, body size, wing formulas, and coloration are all matched by intraspecific variation in these characters in broadly distributed polytypic species of other passerines and in themselves seem only to confirm that the taxa are phenotypically diagnosable and thus worthy of at least subspecies rank under the BSC. Nonetheless, some of these differences are roughly comparable to phenotypic differences in related taxa we rank as species, specifically *Cyanoloxia cyanoides*/*C. rothschildii*, for which we have better data.

YES to the four-way split (but not to the split of *relicta*), which seems practically mandatory, despite the relatively similar morphology and songs of all. Thanks Nacho for this excellent proposal and background research! The deep genetic divergences show these have been isolated for very long periods, and if it seems surprising that they differ so little in phenotype, that may reflect their being well-adapted to the dark bamboo thickets in which they occur. I finally saw *concolor* in Costa Rica last year (eating bamboo leaves!) and can attest to how well they blend in.

As for details, it is obvious that *aequatorialis* cannot be maintained as a subspecies of *concolor* (unless all forms are lumped, which would be at odds with divergence times). And, while current data do not strongly support species status for *relicta*, the phenotypic differences that earlier authors remarked on and that led HBW/BLI to split it (and it alone!) suggest that further study might bolster the case for its species status.

As for English names, I vote for continued use of Blue Seedeater for *concolor*+*relicta*, as this species has a much more extensive range than *aequatorialis*. I vote for Ecuadorian Seedeater for the latter, as this circumscribes nearly all of its range. The other names, Carrizal and Blackish-blue, seem not to need input.

YES to (c), NO to (a)(b)(d). This is a complex situation that is well-summarized in the proposal. The split of *aequatorialis* is justified based on the combination of genetic data and quantitative vocal analysis. Regarding English name, I'm fine with Ecuadorian Seedeater if that's the name agreed upon by the SACC since it's a South American taxon.

YES to (c), NO to (a)(b)(d). (a) An intriguing possibility, but the evidence presented thus far does not lead me to think that *relicta* would be reproductively isolated from *concolor*. Interesting that there are no other bamboo specialists in the range of *relicta*, indicating that its habitat use may differ markedly from *concolor*. (b) The phylogeny does not support this arrangement. (c) The genetics show that *aequatorialis* is more closely related to these two SA taxa, and not to *concolor* of Middle America. The plumage differences and possible vocal differences support this arrangement. If *carrizalensis* and *moesta* continued to be recognized (these two are wholly within the SACC area, so it is their call), then *aequatorialis* should be recognized as a species. (d) Although a five-way lump makes some sense given the all five taxa are allopatric and they are minimally differentiated genetically and phenotypically, such a lump would obscure what most consider to be species level differences, and the monophyly of each taxon.

YES to (c) and NO to the other options. The *aequatorialis* split is justified in my eyes in large part by the phylogeny recovered and the deep paraphyly that would exist if we were to continue to treat *aequatorialis* and *concolor* as a single species. This treatment represents the least disturbance to the status quo while resolving the new data and analyses at hand. It's a tricky situation with allospecies, and some of these decisions will inherently be subjective. I'll also warn that the LDA plot is not agnostic to prior classification and is therefore not the same as a PCA. These should be interpreted with caution with respect to phenotypic clustering / diagnostic differences among putative species.

YES to (c) and NO to (a), (b), and (d). I agree with the recommendations of the proposal. Current phylogenetic information, based on mitochondrial and nuclear data, supports (and requires) treating *aequatorialis* as a separate species from *A. concolor*. For English names, I vote for *A. concolor* to continue as Blue Seedeater; I agree with Ecuadorian Seedeater for *A. aequatorialis*, although this is a SACC decision.

YES to (c) and NO to (a), (b), and (d). (a) I agree with Areta et al. 2023, about the recommendation of a more rigorous studies on the taxonomic status of *relicta*. (b) There is no support for this lump. (c) The evidence is clear, *A. aequatorialis* is a separate species from *A. concolor*. (d) There is enough evidence to not lump the five taxa.

YES to (c) and NO to (a), (b), and (d). This is a very complex issue, made all the more difficult by the morphological similarity of the taxa involved. However, I agree with the authors of the proposal that option (c), which would split *aequatorialis* from *concolor*, but leave the other taxa as they currently are in the NACC/SACC list, is the best solution. While slight, the vocal analysis by Areta et al. (2023) seems to support recognition of these taxa as distinct species, and the very deep genetic divergence between northern *concolor* + *relicta* and the South American taxa seems far too deep for simply recognition as subspecies of a larger species. I think the morphometric, plumage, vocal, and genetic differences among the 5 taxa in question are consistent with recognition of 4 species.

YES to (c) and NO to (a), (b), and (d). I agree with the proposal authors that given present evidence this is the best treatment. *A. aequatorialis* does appear to be a good species, but the other treatments do not seem to be well supported.

YES to (c) and NO to (a), (b), and (d). I Agree with the conclusions of this well-crafted proposal, and in particular to split *aequatorialis*. From a NACC perspective, the main issue is whether to split *relicta* and that split is not well-supported. Howell and Webb (1995) state that the voice of *A. relicta* is "much like Blue Seedeater [*concolor*] but song slightly higher and faster…." For both taxa, the songs are described as "variable." I would regard the differences as pretty minor and not well-supported by archived recordings, and nothing for call notes. I have traveled a fair amount in Mexico and Middle America and have never seen either taxon and given that they are bamboo specialists, I would imagine they do a considerable amount of wandering around within their range in efforts to find flowering bamboo. The Isthmus of Tehuantepec is not always a species separator (e.g., Hepatic Tanager and for many other species) and the paler color for *relicta* seems to fit the general trend of paleness for East to West taxa within a species for Middle America.

2023-C–8:
Treat *Pipilo socorroensis* as a separate species from Spotted Towhee *P. maculatus*

NO. As indicated in the proposal, there is no evidence for treating *socorroensis* as a separate species. Substantial geographic variation in body size occurs in many widely distributed species (think Hairy Woodpecker). If the song and primary call of P. *socorroensis* were known to differ strongly (as in *maculatus* vs. *erythrophthalmus*), then we would have indirect evidence for treating *socorroensis* as a species.

Was that really all the BLI had to say about species limits? If so, then the Tobias scheme was ignored (which would have generated 1 point on body size and perhaps 1 point on plumage), but the taxon was ranked as a species anyway.

NO, for now. I'm impressed by the size difference, which (as detailed by Howell and Webb and HBW/BLI) is quite profound, and contrary to the common pattern of greater size on islands; other differences of *socorroensis* from widespread forms include browner males and reduced spotting on wing and tail. (And HBW/BLI did indeed score *socorroensis* as reaching their 7-point species-triggering status.) However, with the olive-backed forms (which Davis 1972 treated as a hybrid swarm) being considered conspecific with *maculatus*, and with no information on vocalizations of *socorroensis* (surprisingly, as there are many photos), it seems there is a tremendous amount of variability and lot of work to be done on species limits in *maculatus*. But if sound recordings become available for *socorroensis* and are shown to be distinctive, that would likely tip the balance toward species status, in my opinion.

NO. As noted in the proposal, there is not much information other than smaller size and some slight plumage differences that would justify species status. Genetic and vocal data of *socorroensis*, compared to other subspecies of *P. maculatus*, are needed.

NO. I agree with the proposal that it is premature to consider splitting off *socorroensis* without new information. Considering the great range of plumage in *P. maculatus*, and its propensity to hybridize with other *Pipilo*, I think we would need some better analyses of what characters are important for species recognition in the *Pipilo* (e.g., vocalization, plumage, mensural characters), and to what degree *socorroensis* shows species-specific characters.

NO. I agree that this split seems premature and some additional quantitative analyses of variation of *socorroensis* in the context of the entire complex are needed before this split is justified.

NO. The available evidence on the smaller body size of *socorroensis* and its slight differences in plumage are insufficient to warrant treating *socorroensis* as a separate species from *maculatus*. Quantitative vocal and genetic studies are needed to reassess the split.

NO. I think we need a formal study of the group, including morphological, genetic and vocal analysis.

NO. I agree with the proposal that at the moment, there is not enough justification for splitting *socorroensis*. However, I would not be surprised if with additional research, and acquisition of vocal and genetic data, it may prove distinct.

NO. I am going to preface my comments on this with some general comments that I made earlier via email to fellow NACC members but which, given the high number of taxonomic splits being proposed these days, warrant being put into the more permanent comments records.

In focusing tightly on a particular proposal, I often lose track of larger issues. But I think we need to keep in mind that there are some overarching issues that might inform our collective assessments more frequently. The first we know well but seem to rarely bring up in our proposals: while we often mention that original descriptions or historic treatments of what are now considered subspecies were described or recognized as full species, in the majority of these cases this was under a different species concept than the biological species concept we use today. With that fundamental philosophical shift, I'm not sure we are sufficiently discounting those historic perspectives, which would have been closer to what today we consider phylogenetic species. (And, yes, I, too, disagree with many of the Peters et al. lumping decisions without revealing their justification. But their lack of transparency was an editorial decision on the works themselves. I'm not generally willing to reverse those decisions using what amounts to the same process – i.e., deciding differently based on our own judgment of practically the same evidence, with effectively nothing new on the table). (Addendum: this is a generic comment, not as relevant to this particular proposal as to others.)

Another issue, which I know we aren't paying sufficient attention to, is that Hudson & Price (2014) presented rather compelling evidence that allospecies of birds are taxonomically oversplit (explicitly under the framework of the BSC). Moreover, "We show that in birds, divergence in song and plumage in allopatry corresponds poorly with whether species mate assortatively in hybrid zones and argue that this is because many other factors besides trait divergence affect propensity to hybridize..." (Hudson & Price 2014:821). In other words, the properly lauded yardstick method has a weakness here that we're collectively not incorporating into our thought processes or actions. Given that so many of our proposals are for increased splitting among allopatric taxa, and that evidence indicates these are already generally oversplit, I am tending to vote conservatively so as not to not contribute too much to continuing the problem. So if I seem unduly conservative, these larger issues are at least partly the reason.

This case of the towhees is probably a good example of the second general issue commented on above.

Hudson, E. J., and T. D. Price (2014). Pervasive reinforcement and the role of sexual selection in biological speciation. The Journal of Heredity 105:821–833.

NO. Yes, vocal data are needed, but it is worth noting, or repeating, that there is significant variation in vocalizations in *Pipilo maculatus* with both songs and call notes. Females of most subspecies have paler (duller) heads; with *arcticus,* the differences approach the differences in *P. erythrophthalmus.* In short, the vocalizations would have to be pretty strikingly different for me to come around to supporting *socorroensis* as a separate species from the other highly varied subspecies within *P. maculatus.*

2023-C-9:
Revise generic limits among *Rhodothraupis*, *Periporphyrus*, and *Caryothraustes:* (a) Adopt the following linear sequence: *celaeno, erythromelas* (extralimital), *poliogaster, canadensis;* (b) Transfer *Rhodothraupis celaeno* to *Periporphyrus;* (c) Transfer *Rhodothraupis celaeno* and *Periporphyrus erythromelas* to *Caryothraustes*

YES to (a) and (b), NO to (c). It's hard to square the minimal divergence between *Periporhyrus* and *Rhodothraupis* in Bocalini et al. with the much more substantial levels in Klicka et al. (2014) and Barker et al. (2015). Nevertheless, they are clearly sisters that are not so deeply diverged, so I vote YES to lump both into *Periporphyrus*. In fact it seems somewhat surprising that they have been maintained in separate genera so long, given their striking and distinctive similarities. (c) I vote NO to the proposed lump of *Periporphyrus* (+*Rhodothraupis*) into *Caryothraustes*, as this creates a notably more heterogeneous grouping that is considerably more deeply diverged.

YES to (a) and (b), NO to (c). I agree with the proposal recommendations. The shallow divergence between *Rhodothraupis* and *Periporphyrus* in the UCE tree, along with their similar plumage patterns, justify merging these taxa into the same genus. At this point, merging *Caryothraustes* and *Periporphyrus* doesn't seem necessary as noted in the proposal. The revised sequence seems fine to me.

YES to (a) and (b), NO to (c). (a) Follows the phylogeny and our sequencing rules. (b) I am not sure what to make of the differing hypotheses of the age of the split, but the similarities in plumage and habitat argue for them to be considered congeneric. Even using the much older hypothesized split from Barker et al, it could be argued that they should be considered congeneric, as the age of that split is within the range shown within other Cardinalid genera. (c) I agree with the proposal's recommendation that the genetic distance and differences in habitats and plumage argue that they should not be considered congeneric.

YES to (a) and (b), NO to (c). Given the shallow divergence in the UCE tree, it makes sense to me to merge these into a single genus. While there are differences in bill measurements and body size between *Rhodothraupis* and *Periporphyrus*, they still seem quite similar in their overall appearance and make sense as a single genus to me. (c) This is a fairly subjective decision in terms of whether or not to lump these two into a single genus or not. I prefer not to disrupt the status quo for genera that are already monophyletic unless it's really warranted, so I am down to follow the proposal's recommendations. The lack of "red-yellow" dimorphism present in *Caryothraustes* is distinct both from the new *Periporphyrus* that would result from this proposal, and the related *Piranga*, which makes the *Caryothraustes* at least somewhat unique.

YES to (a) and (b), NO to (c). The change in the linear sequence is necessary to consider geographic distribution. Phylogenetic data and the level of divergence, along with plumage coloration, support the transfer of *Rhodothraupis celaeno* to *Periporphyrus*.

(c). Available evidence supports keeping *Caryothraustes* and *Periporphyrus* as separate genera.

YES to (a) and (b), NO to (c). I vote yes on (a) and (b) based on short branch lengths. (c) NO, with little conviction. Subjective decision with comparative branch lengths borderline. No reason to disrupt status quo without better data.

YES to (a) and (b), NO to (c). (a) I agree with the sequence alteration. (b) I agree with the proposal, both topologies (Baker et al. 2015, Bocalini et al. 2021) are concordant, supporting the inclusion of *Rhodothraupis* to *Periporphyrus*, they have several similarities. (c) I do not think there is enough information to support the inclusion of *Caryothraustes* and *Periporphyrus* in a single genus.

YES to (a) and (b), NO to (c). Changing the linear sequence is required for the reasons outlined in the proposal. I am more on the fence about parts b and c, and am tempted by including all in *Caryothraustes*, but I think I am more comfortable with transferring *Rhodothraupis celaeno* to *Periporphyrus*, and keeping *Periporphyrus* separate from *Caryothraustes*. I am intrigued by the fact that *Caryothraustes* look very much like female plumaged *Periporphyrus*, clearly linking these two clades, however I think from a behavioral and ecological perspective, treating these as two separate genera may be the better approach (noting vocal and habitat differences).

YES to (a) and (b), NO to (c). The first two are well-supported, while the last is not at this time.

YES to (a) and (b), NO to (c). While merging *Rhodothraupis* into *Periporphyrus* is supported, the two species in reconstructed *Periporphyrus* are separated by a large distance (northeast Mexico to eastern Venezuela). I've only seen a few Crimson-collared Grosbeaks (all in South Texas), but I watched one for a few hours at Aransas NWR and it spent nearly all of its time on the ground consuming emergent green vegetation. The sexual dimorphism in both species in *Periporphyrus* is striking and as noted, not with the two species of *Caryothraustes*. I wanted to note that HY/SY year males of Crimson-collared Grosbeak look very much like females. I see that Hilty (2003) says that immatures of Red-and-black Grosbeak are similar to the female in the 2nd edition of the Venezuela guide.

What went on with Linnaeus in 1766 with giving the specific epithet of *canadensis* to Yellow-green Grosbeak? The closest it gets to Canada is eastern Panama.

2023-C-10:
Treat *Pachyramphus uropygialis* as a separate species from Gray-collared Becard *P. major*

NO. Although the females typically look very different, there clearly is some intergradation that needs study. Without known vocal differences, and with slim genetic divergence, this seems a very weak case for a split.

NO. I agree with the proposal that more study is needed, especially more thorough sampling of all taxa for genetic analysis as well as quantitative vocal comparisons.

NO. Reasons are stated in the proposal. They seem to be fairly closely related sister taxa with no known vocal differences. Under the BSC, these seem better treated as subspecies than full species.

NO. More detailed studies are needed to understand the variation (genetics, vocal, plumage coloration) within *Pachyramphus major*. According to the proposal, it seems that females, between *uropygialis* and the other four subspecies, are more differentiated than the males; I find it intriguing and definitely something worth further investigation (since in closely related taxa it is common for males to be more differentiated than females). The possibility of a contact zone between *major* and *uropygialis* in Oaxaca should also be explored.

NO. Reasons are stated in the proposal, especially the lack of documented vocal differences.

NO. Although the differences in female plumage are striking, other becards considered a single species (especially *P. polychopterus*) show a high degree of variation in female plumage among subspecies. Differences between males of *uropygialis* and the *major* group are less striking than the females and do not stand out compared to species variation among subspecies within males of other becard species. Genetic differences within these allopatric groups are rather small (for considering them species) and not informative. I think an analysis of vocal differences would be necessary to seriously consider *uropygialis* a separate species.

NO. We need more conclusive data to support the genetic data, such as a study of vocalizations and color patterns.

NO. I agree with the proposal that the differences described do not constitute distinction as separate species, and that treatment as subspecies seems appropriate. The difference among the females is very interesting, but the extremely low levels of genetic divergence, plus the apparent lack of differences in vocalizations all support treatment as a single species.

NO. All evidence to date indicates this is a perfectly good biological subspecies.

NO. I find the pretty striking difference in morphology of the females intriguing. Without vocal differences it's impossible to justify a split. Contact zones, if any, should be studied. As a note, the one record for the U.S. (Chiricahua Mountains, southeast Arizona) was a female and was obviously *uropygialis* on plumage (good photos). .

2023-C-11:
Treat *Chlorospingus hypophaeus* as a separate species from Yellow-throated Chlorospingus *C. flavigularis*

NO. This is a close one and I will be very surprised if further study does not show *hypophaeus* to be better treated as a distinct species. In photos on ML, it looks distinctly different from South American *flavigularis*, even though the latter vary considerably in iris color; *hypophaeus* looks smaller and more delicate, with pale areas in front of the eyes that bring out the very dark iris (vs dark or at least darker in *flavigularis*), as well as the other minor plumage differences pointed out in the proposal. Also, the three recordings of its call (from two different recordists) are not matched by online recordings of other taxa.

If this split should pass, I don't think any of the suggested E names are suitable—it doesn't look orange-throated in photos; Drab-breasted sounds like an insult, and it isn't really dark-breasted either.

NO. As noted in the proposal, more study is clearly needed that includes both genetic and vocal data involving all taxa.

NO. We need more data to make a good decision to change the consensus status quo of treating these as a single species. Little to no genetic data exist, nor do vocalizations. An intriguing system worthy of further study, but differences in iris color and slight plumage differences do not equate to separate species under the BSC.

NO. Iris and plumage colors are intraspecifically variable in the genus *Chlorospingus* (e.g., *C. flavopectus* is highly variable in both iris and plumage color); therefore these traits should not be recommended for defining species limits in *Chlorospingus*. We need studies on the integrative taxonomy of *C. flavigularis* to reassess the split of *hypophaeus*.

NO. As noted in the proposal, there are lots of indications that this might be a separate species but no data other than the plumage and iris differences, which are of unknown significance other than such differences are present among taxa currently treated as subspecies elsewhere in the genus. Species limits in the entire genus are badly in need of modern analyses that include vocalizations. Currently, there are insufficient data for altering the status quo, in my opinion.

NO. I agree with the recommendation in the proposal that it is premature to upset the status quo and call *hypophaeus* a separate species from *flavigularis*. I also agree that a genetic study that included all relevant taxa (including *parvirostis*) would be enlightening regarding species limits.

NO. There is not enough information for this split. It needs a formal study that includes genetic, morphometric, color patterns, and vocalizations.

NO. I agree with the proposal, that currently, there is just not enough information available to make an informed decision. All three taxa differ in iris color, so the emphasis put on that character by some authors does seem odd, and with no formal vocal analysis or any genetic data from *hypophaeus*, I think it is best to wait on a decision here.

NO. There is inadequate evidence to treat these as separate species.

NO. Intriguing. I'm interested in SACC's view of this. I see footnotes in Dickinson and Christidis (2014) that both *marginatus* and *hypophaeus* "may merit separate species status".

2023-C-12:

Treat *Melozone occipitalis* as a separate species from White-eared Ground-Sparrow *M. leucotis*

YES. Although this could go either way, I think the balance of evidence—plumage, genetic divergence, and vocalizations of these allopatric taxa—indicates species status for *occipitalis*. The plumage is usually quite distinct but it seems to be mostly a much greater coverage of melanin in *leucotis s.s.* than in *occipitalis*, and some *leucotis* can show a division between the black throat and breast spot. The following two adjacent photos of the different forms look more similar to each other than most. Still, given the congruence of the different lines of evidence I'm voting to split.

Melozone leucotis (Guatemala): ML220222951
Melozone leucotis (Costa Rica): ML84767081

YES (weakly). A 2 MY split is not "highly divergent" in my opinion, but is still deeper than some of the other splits within the genus, specifically the *aberti / fuscus / crissalis* complex. I'll also note that the sampling for the genetic component of this suite of analyses seems rather sparse. However, we do have differences in vocalizations, plumage, and morphology that provide support for two species under an integrative delimitation framework. It does seem that vocalizations are important for this group, and the extensive quantitative analyses do seem to support the taxonomic split.

YES. New information (i.e., plumage, morphology, vocalizations, and phylogenetics) and comparison of species limits in the sister clade (*M. cabanisi* / *M. biarcuata*) support treating *M. occipitalis* as a separate species from *M. leucotis*.

YES, weakly. With the study of vocalizations (a very important character for the group) and the morphometric analysis, plus genetics, I consider supporting the proposal, but the problem with the proposal is the number of the samples in the genetic analysis.

YES. Reasons are stated in the proposal.

YES. Additional analyses with higher sample sizes are desirable. However, the proposal includes multiple types of data that all lean in the same way. In my opinion, multiple lines of evidence are more suggestive of species vs. subspecies. The genetic taxa show more divergence in the proposed taxa than *aberti/crissalis/fusca*. I agree with the proposed English names.

NO. I originally voted yes based on the multiple lines of evidence put forth in the proposal, but other committee member comments regarding the limited vocal sampling (especially of *occipitalis*) as well as the lack of playback experiments have swayed me to change my vote to no.

NO. In my opinion, the plumage and genetic differences provide no evidence for species rank (vs. subspecies). For allopatric sedentary tropical birds, an estimated divergence time of 2 MYA is much more consistent with subspecies rank. (I think this committee consistently makes the mistake of evaluating genetic distances under a temperate latitude framework rather than a tropical one; even non-migratory temperate latitude birds are likely to have much greater dispersal abilities than ecologically comparable tropical ones, which often show dramatically larger degrees of divergence at neutral loci across minor geographic barriers and with minimal and sometimes no known phenotypic differences on either side of the barrier).

I also disagree with the interpretation of everyone else who has commented so far that the various lines of evidence point towards species rank. All of the above lines of evidence are also consistent with subspecies rank, ergo not relevant to a decision on taxon rank. I think we make this same conceptual mistake in many of our analyses, when we state that all lines of evidence favor [one or the other] treatment, when some or many of those lines ALSO favor the alternative treatment. It sounds impressive and somewhat self-soothing when we state that a decision is based on multiple types of data, but conceptually flawed unless all those lines all support one of the two alternatives.

So, in my opinion, the only data relevant to taxon rank in this case are the vocal analyses. As noted in the proposal, there are only 3 recordings of the male solo song of the critical taxon *occipitalis*, the one proposed for species rank. That right there cripples any interpretation of analysis of song differences, especially when they are similar to begin with. Then, there are 6 recordings of the duet of *occipitalis*, but as noted in the proposal, those duets are not diagnosable from those of the other taxa. Finally, the calls look as if they differ consistently, but N=5 for *occipitalis*. The figures offer just 1 example of each, so there is no way to "see" possible individual variation other than from the quantitative analyses; further, it looks to me, visually, that *nigrior* is at least as distinctive as *occipitalis*. I am sure I could present single examples of calls and songs from within Eastern Towhee that would match the differences I see here. Yes, the vocal data are suggestive, but are they solid enough to change a species-level classification that has been followed since 1938? I don't think so.

Even if the differences remain intact with larger N, do they matter to the birds? When the voices are fairly similar to begin with, I think playback trials are critical for deciding on whether these vocalizations are at the levels of difference associated with barriers to gene flow in this family.

Good points are made concerning our previous evaluation on species rank in the other *Melozone* complex, but now I want to go back and see how similar the data-sets are.

NO. Tentatively for now. First, I found it quite difficult to assess the degree of morphological differences. Photos of *occipitalis* in the proposal (of specimens) and online (of living birds) are not plentiful enough or high enough quality to get an idea of how different they really are, and how much variation there is within each taxon. As we know, intraspecific variation in this family can be huge. Second, analyses of vocalizations are also hamstrung by small sample sizes (particularly of *occipitalis*), which makes interpretation of variation within these taxa difficult. The few samples I hear sure sound different from *leucotis*, but songs differ from duets and both seem quite variable (at least within *leucotis*). As others have mentioned the genetic distance is not an instant earmark for species status, although it is much greater than in the *fuscus/crissalis/aberti* clade.

NO. The vocalization data, with only 3 *occipitalis* and noted individual variation in this complex, are not compelling for species-level treatment, nor are the other phenotypic differences. The depth of the split is suggestive (though most signal is likely mtDNA, with only 5 nuclear loci), but this is also not diagnostic. I am left wondering why they are not just a distinct subspecific group. I went back to my notes on our 2017 *Melozone* split and find that this *occipitalis-leucotis* case does not yet meet what I considered then to be my bar of acceptability.

NO. I originally voted yes but after reading other comments, carefully going over the motion, and researching the issue, I think a split is premature. I think it is fairly likely that two species are involved, but *nigrior* (north-central Nicaragua) as noted, is poorly sampled and is likely the least known of the four populations. It (*nigrior*) may be 285 km to the nearest population of *occipitalis*, but it is about 400 km to the nearest population of nominate *leucotis*, and on a biogeographical basis (separated by the lowland gap of central and southern and western Costa Rica, I'm not sure if it makes bio-geographical sense. In addition, I see from Gallardo (2014, Guide to the Birds of Honduras, published by Mountain Gem Tours) that there is a relatively newly discovered population in Honduras, a population that has not been considered in the two motions we have considered in recent years for the split of this species. This population is detailed by Anderson *et al.* (1998), thus is not in the 7th edition of the Check-list. Gallardo (2014) says that the population has not been assigned to subspecies, "however, its coloration is closest to that of *nigrior.*" It is found in the Department of central Olancho, on the Caribbean slope. It is found from 600-1200 meters (Gallardo 2014). He does describe the song, but I don't know if it is actually from the birds in Honduras or birds elsewhere to the south. Note Howell and Webb (1995) when describing the *leucotis* group describe birds from Costa Rica, not *nigrior* from Nicaragua. The other populations seem to be largely (entirely?) on the Pacific slope. I don't believe there has been any genetic sampling of birds from Honduras, or what spectograms show on the vocalizations. In short, given the lack of knowledge with Honduran birds and that little is known about *nigrior*, I think it is wise that we await more detailed studies of the above populations. If it was just nominate *leucotis,* plus *occipitalis,* I would likely support the split. Such is not the case.

NO. I vote no based on what seem to me to be minor plumage and vocal differences that are not offset by the molecular data. The plumage differences indicate that these are separate taxa but fail to indicate to me that they are of species rank, especially considering that *nigrior* is to some extent intermediate between *occipitalis* and *leucotis*. As to the vocal analyses, sample sizes for *occipitalis* are very small (5 for calls, 3 for songs, and 6 for duets) yet even with this poor sampling the calls and songs overlap on the PCAs in Fig. 3 (or are wildly variable in the case of the chip calls of *occipitalis*) as well as in the character data in Table 4 (if ranges were provided this would be very obvious). This leaves the molecular data, which show that *occipitalis* differs from *leucotis/nigrior* in both mitochondrial and nuclear DNA (with the same tree topologies) and that the level of divergence is similar to that in some other pairs of *Melozone* species and exceeds that found among the three species of sw US towhees (*fusca*, *crissalis*, and *aberti*). However, the fact that *occipitalis* and *leucotis* are more genetically divergent doesn't tell us much regarding biological species status in the absence of at least somewhat compelling phenotypic data, especially voice, which are lacking within *leucotis*, whereas the towhees are well known to differ in vocalizations. Moreover, tropical species have been shown to display much greater intra- and interspecific genetic divergences compared to birds of the temperate zone (e.g., Weir papers and many others), so that comparing genetic divergence in primarily temperate zone birds like the towhee trio (*fusca* also extends a bit south into the tropical zone) with sedentary tropical birds like *M. leucotis*, despite their being in the same genus, strikes me as an apples to oranges comparison. I also think that very different selection pressures operate on species that have been in contact over evolutionary time, as the towhees likely have, and widely separated small allopatric populations like those of *leucotis*, such that those in contact likely develop reproductive isolating mechanisms much more quickly. I consider it entirely possible that *leucotis* and *occipitalis* are separate species, but I don't see any compelling evidence of this based on current data. More and better vocal data (including playback?) would go a long way towards convincing me.

2023-C-13:

Treat *Granatellus francescae* as a separate species from Red-breasted Chat *G. venustus*

NO. At least not on present data. There are several photos of male *francescae* on ML and this one (https://macaulaylibrary.org/asset/345832631) especially does not look very distinct, especially given the variation shown within continental *venustus*. However, there are a few recordings of call notes of *francescae* (e.g. https://macaulaylibrary.org/asset/228747 and https://macaulaylibrary.org/asset/228905) that are very very different from the many of *venustus* on xc and ML. If these are really homologous to the common spik call of *venustus*, then they can hardly be conspecific. But this isn't clear without a proper analysis, so hopefully more recordings of *francescae* will become available and an analysis carried out, which might shift the balance.

NO. I agree with the proposal that more data (genetics and recordings with a quantitative analysis) are needed to justify a split.

NO. Following the rationale that is nicely presented in the proposal, more quantitative analyses are needed to change status quo and split *francescae* from *venustus*.

NO. The available evidence does not support the split of *Granatellus venustus* into two separate species (*G. venustus* and *G. francescae*).

NO. I agree with proposal's recommendation. Given that only one of 23 bird taxa on the Tres Marias Islands is considered specifically distinct from mainland taxa, and that *francescae* does not differ in any way that we know would be reproductively isolating, I think that we should maintain the status quo until more information n vocalizations and genetics are analyzed.

NO. We need a formal study that includes genetic, vocalizations and color patterns data.

NO. The evidence to split this taxon seems weak, and additional analyses of vocal differences and genetic differences (if any) are needed before I would be ready to split *francescae* as specifically distinct. I agree with the proposal that the vocal differences in song do not seem very strong, and sound quite similar to my ear.

NO. To reiterate my comment under *Pipilo*: In focusing tightly on a particular proposal, I often lose track of larger issues. But I think we need to keep in mind that there are some overarching issues that might inform our collective assessments more frequently. The first we know well but seem to rarely bring up in our proposals: while we often mention that original descriptions or historic treatments of what are now considered subspecies were described or recognized as full species, in the majority of these cases this was under a different species concept than the biological species concept we use today. With that fundamental philosophical shift, I'm not sure we are sufficiently discounting those historic perspectives, which would have been closer to what today we consider phylogenetic species. (And, yes, I, too, disagree with many of the Peters et al. lumping decisions without revealing their justification. But their lack of transparency was an editorial decision on the works themselves. I'm not generally willing to reverse those decisions using what amounts to the same process – i.e., deciding differently based on our own judgment of practically the same evidence, with effectively nothing new on the table). (Addendum: this is a generic comment, not as relevant to this particular proposal as to others.)

NO. For all the reasons in the proposal and in others' comments.

NO. Reasons are outlined in the proposal and in members' comments. Of the 23, the one that catches my eye as different is the *graysoni* Streak-backed Oriole. A striking color photo appears on the cover on Western Birds (Volume 48, No. 1, 2017). A review of Howell and Webb (1995) offers little to suggest a split is warranted and they are usually on potential splits, but perhaps they didn't visit the Tres Marias (I have heard they are hard to access). If the call notes are really different that's important, so I echo the comments above on this. More research is needed especially on vocalizations.