2023-A-1
Separate *Basileuterus culicivorus* (Golden-crowned Warbler) into as many as four species:

NO. The proposal outlines good reasons why it's prudent to wait until additional genetic/genomic and vocal data, with good sampling within and among populations and across contact zones, are published.

NO. As recommended in the proposal, phylogeographic studies and vocal analyses with extensive geographic sampling are necessary before any taxonomic changes.

NO on all the splits, as recommended in the proposal. Multiple species-level taxa are almost certainly included in *B. culicivorus*, but this is a complex group that requires a comprehensive analysis of all taxa, including especially a quantitative analysis of vocalizations and careful playback experiments, in addition to more modern genetic analyses to confirm relationships among taxa. Boesman's initial findings should provide a catalyst for more rigorous examination of voice.

Instructive is that the Birdlife/Tobias scheme treats *hypoleucus* as a separate species. I've collected specimens in the contact zone in Bolivia. Gene flow is extensive, apparently without barriers, and vocalizations seem to be identical. Yet this actually counts in favor of treatment as separate species in their scheme (for reasons that are baffling). This subspecies differs from the *auricapillus* group in having mostly whitish (vs. yellow) underparts. Apparently this awarded *hypoleucus* sufficient "points" to treat it as a separate species smack in the face of no differences in voice and minimal if any genetic difference. The logical inference in a comparative framework is that ventral coloration per se is not directly involved in reproductive isolation in this group; therefore, "points" involving ventral coloration, or perhaps yellow-white differences elsewhere in the plumage, should not be used in "scoring" species rank in this group, yet yellow-white differences are nevertheless counted in their scoring system in the genus.

NO. Although it seems that more than one species is involved here, the proposal gives good reason why we should wait until more thorough genetic and vocalization data is gathered and analyzed, especially in contact zones. If split, no taxon should retain the English name Golden-crowned Warbler.

NO. While I think the Tobias et al. (2010) criteria have some merit for first-pass evaluations of possible species limits, in that they often prove on followup investigations to warrant splits, I am leery of having those decisions drag the rest of our taxonomies along without solid additional research. The vocal analyses of Boesman (2016) do not represent that kind of research in my mind, but are rather an additional first-pass-type look: intriguing, and warranting further pursuit. We noted in the 53rd Supplement (2012) when lumping *culicivorous* and *hypoleucus* the "presence of mixed pairs and intermediates where their ranges overlap (Hellmayr 1935, Willis 1986, Robbins et al. 1999). That the HBW decision appeared to ignore this in splitting off *hypoleucus* anyway indicates to me that caution and additional data are warranted. The proposal and the SACC comments are excellent in adding still more evidence for the need for comprehensive studies of this group.

NO. Genetic data only includes Cyt b, and more comprehensive geographic sampling of vocalizations are needed.

NO. I agree with the proposal (and others on NACC and SACC) that this is a system worthy of additional future study, but I disagree with BirdLife that we should split these lineages based on what we have on hand. A comprehensive study of this group would be most welcome.

NO. This seems like a very confusing group, and as others have said, likely involves multiple species, however given the possible confusion over the paraphyly of the *culicivorus* group (though this is only based on mtDNA), I think it is especially important that a broader scale series of playback experiments are undertaken with careful consideration of the populations and subspecies included.

NO. The proposal is clear that more data is needed, especially when *B. culicivorus* is not monophyletic. A more complete phylogenetic study is needed.

NO to all. I had initially voted yes on splitting out the North American taxa: *flavescens*, *brasierii* (note the spelling correction in a footnote in Dickinson and Christidis 2014), *brasierii* not *brashierii* or *brasherii*), *culicivorus,* and *godmani* as from Boesman's findings, the songs are consistent, *and different*, from all South American subspecies. And, there is a good break in eastern Panama from the South American subspecies. I realize the mtDNA indicates the West Mexican subspecies (*flavescens*) differs, but if vocalizations are very similar that seems more important, at least without additional genetic studies. I realize too that the nominate subspecies was not vocally recorded, but hard to believe there would be a significant difference given the break between *brasierii* and *culicivorus* is in northern or central Veracruz. Since that would split out the nominate *culicivorus* group of subspecies, that would force the hand of SACC to determine what to do with the 10 South American subspecies, which, as noted, is a very muddled situation. Of all of those, *auricapilla* has priority. So, a comprehensive solution is best, even though that may take a considerable amount of time to sort out. Perhaps the initiative should come from SACC, as once that is done, I think the North American subspecies can likely be split out as a single species. Are the species limits within *Basileuterus* currently inconsistent? For instance, I note that AOS and others split off *tacarcunae* (Tacarcuna Warbler) and *melanotis* (Costa Rican Warbler) from *B. tristriatus.* I note under voice in Vallely and Dyer's Bird's of Central America (2018) that the voice of Tacarcuna Warbler is "poorly known but seems much like Costan Rican Warbler. While anecdotal, it does raise a warning flag. With the North American subspecies within *B. culicivorus* the differences seem more clear cut.

2023-A-2
Treat *Antrostomus cubanensis* (Greater Antillean Nightjar) as two species

YES. The vocal differences are quite pronounced, and voice is clearly important for species discrimination in nightbirds (as noted in the proposal). It would be nice to have genetic data but I think the vocal differences are sufficient to recognize this split.

YES. In nightjars, song differences are the overwhelming determinant of species limits. In my opinion, burden-of-proof should be on treatment as conspecific if voices differ. Although the N is not impressive, all available evidence points to a consistent difference in song and thus species rank for both taxa. Of note is that the two species of whip-poor-will have more similar songs than these two do, in my opinion.

YES. Song differences are evident and as indicated in the proposal, songs are of great importance in nocturnal birds.

YES. I was surprised that the songs are so different, yet the taxon has not yet been split.

YES. Vocalizations of nightjars have proven to be good indicators of species limits.

YES. Strong vocal differences between these two isolated forms. Given the importance of vocalizations in Caprimulgiformes, splitting these two seems appropriate.

YES. Vocalizations are quite different between the proposed taxa, we know that nightjars care about differences in vocalizations. Sample sizes are low, but results seem clear that these differ vocally, and therefore are likely reproductively isolated and separate species.

YES. While the sample sizes are rather small, as some have noted, the differences in vocalizations are striking, and has been shown for nightjars, extremely important indicators of species status.

YES. A good example where vocalizations can lead to a speciation event, between *ekmani* and *cubanensis*.

YES. As noted in the proposal, I think the matter (the published papers from several decades ago) was just overlooked. I'll reiterate that overall the differences between Hispaniola and Cuba are profound and in almost all cases different, apart from some shared avifauna found throughout the Greater Antilles, or nearly so. Cuba shares much closer affinities with the Bahamas which share a shallow bank of connection. Now, can we please revisit the species status for the Grand Cayman and Cuban Bullfinches?

2023-A-3
Treat *Geothlypis semiflava* (Olive-crowned Yellowthroat) as two or three species

NO. Reasons for holding off on recognizing multiple species in this group are well laid out in the proposal. Additional data are clearly needed.

NO, for all the reasons given in the recommendation in the proposal. This is another group begging for a comprehensive analysis. This proposal will act as a helpful summary of the situation and what is needed (and could be published as a mini-review short paper somewhere, perhaps *Ornitología Neotropical* or *Cotinga*).

NO. A thorough analysis of vocal, morphological and genetic traits is necessary. Further analysis should capture the geographic variation described for the three subspecies within *Geothlypis semiflava*. Understanding the processes that drive the divergence between the two Central American subspecies would be a very interesting project.

NO. The data are insufficient to split the species at this point.

NO. For reasons outlined in the proposal.

NO. Genetic data are needed from more genes and more individuals of each proposed species. Comprehensive analyses of vocalizations are also needed.

NO. This split is premature, but is yet another group that would be a fantastic group for a comprehensive study of genetic and phenotypic variation within and among populations.

NO. Reasons are outlined in the proposal. I think this is another group that could likely be split, but I would like to see further analysis of vocalizations and additional genetic data from more than one individual per taxa and more than just mtDNA to clarify relationships among *chiriquensis*, *bairdi*, and *semiflava*. In light of Freeman and

Montgomery's (2017) results, I'd also be curious about how responsive other *Geothlypis* are to each other's songs; I would suspect that all respond to some degree to the songs of different species, as mentioned in the proposal.

NO. A robust phylogenetic study of this group is needed, more genetic data and vocalizations are needed to support this split.

NO. The proposal is laid out well. The Central American authorities have little enthusiasm for a split here. Obviously, much more is needed, but for now, no change is warranted. As for variation in appearance and songs, one needs to look no further than *Geothlypis trichas* for geographical differences in primary songs, and especially in appearance.

2023-A-4
Treat *Setophaga graysoni* as a separate species from *S. pitiayumi* (Tropical Parula)

YES. Plumage and genetic data (nuclear and mtDNA) are enough to convince me that *graysoni* can be recognized as a separate species, especially considering no rationale was provided for lumping them in the first place. We will always want more information, but in this case I think we are ok making this split.

YES. Reasons are outlined in the proposal. This is a very tricky call, and I agree with others that additional analyses and data would be preferred to sort out the entire *Setophaga pitiayumi/americana* group, but I think given the available evidence, splitting *grasoni* is a reasonable first step.

YES. There are enough data (plumage and genetic) to separate *S. graysoni* from *S. pitiayumi* and the allopatric populations supported this separation, even if a complete study of parulas is carried out.

YES. An extremely well written and thorough proposal on the group. While we would of course like to have a comprehensive study of genetic and phenotypic data for the entire complex, I feel comfortable splitting *graysoni* based on what we have at hand. Socorro is an island with numerous endemics, and the phenotypic differences between the island and mainland forms seem pronounced—at least for plumage. The vocalizations seem variable and without a more quantitative analysis of song differences, I think it's a bit early to say definitively that song is acting as a premating barrier to gene flow, but given the biogeography of the system and the paraphyly in the limited molecular data that have been published so far, I vote to recognize *graysoni* as a separate species.

NO. I was on the fence about this, mainly because *graysoni* was lumped without a clear rationale and the genetic data are suggestive of a split. However, I agree that a more thorough study of genetic, vocal, and plumage variation across the complex is needed.

NO. Just too many problems, in my opinion, to change the current taxonomy. I'm not opposed to piecemeal taxonomy, but I think this is a case in which the entire complex needs a thorough view, vocally and genetically. I just can't see how we can change the current taxonomy, yet again, based on a set of fragmentary evidence that is somewhat ambiguous. For example, a more rigorous genetic study (and one that included *insularis* and an array of *pitiayumi* populations rather than just a single individual from Chacachacare Island off Trinidad[!]; see Evans et al., Table 1) would be needed to convince me that *graysoni* is actually the oldest lineage in the group, although it is plausible that the ancestor of *pitiayumi* + *americana* generated the colonization of Socorro.

NO. I could vote either way, but given the equivalence I think it is best to keep the status quo until better data are given. The plumage is the only real concrete evidence of a dramatic difference, but even this could be interpreted as indicating that only subspecies status is merited. The genetic evidence needs much more through geographical/taxonomic sampling to

NO. The genetic results between Socorro Island (*graysoni*) and Trinidad (*pitiayumi*) in such a complex group are interesting but really not informative about biological species limits, in my view. That's a lot of geographic space in which we know a lot of variation exists, so wider and deeper sampling are needed. Considering Baja specimens of *graysoni* (Lamb 1925) and the diversity in *pitiayumi*, much more work is needed in this group. The phenotypic distinctness of this taxon is noteworthy, and I could see an eventual split being supported. Lamb (1925:37) had some interesting observations: "The taking of these two birds, in the winter and summer of two successive years, would indicate that the species is of more or less regular occurrence in the Cape Region of Lower California. The capture of a specimen in July suggests the possibility of breeding at the point of record." I wonder if there might be occasional gene flow between this remote island population and mainland birds (like *insularis-pulchra*). Population-level study of *pitiayumi sensu lato* will be very interesting.

NO. Only three samples were included in the phylogenetic study by Evans et al. (2015), which is not representative of the variation in plumage and voice in *Setophaga pitiayumi* in its wide geographic range. Integrative taxonomic analyses are required to illuminate our understanding of the evolution of the Tropical Parula species complex and to recommend taxonomic changes.

NO. I am thinking with the former genus *Parula* that fewer species are needed, not more. For instance, the split of *Setophaga pitiayumi* from Northern Parula (*S. americana*) seems pretty iffy, especially as Moldenhauer (1992) [Moldenhouer, R.R. 1992. Two song populations of the Northern Parula. Auk 109:215-222] detailed that there was distinct geographic variation within the primary songs within the U.S. with birds east of roughly Mobile Bay to Ohio having accented endings (an abrupt end with a pop) as opposed to those west of Mobile Bay. Those from farther west sounded just like *S. pitiayumi nigrilora*. It is not surprising then that hybridization is apparently frequent in the southern Edwards Plateau region, and hybrids are not infrequently seen in the Rio Grande Valley, Texas, during the winter. Given that West Mexican (*pulchra*) birds sound different from *nigrilora* and that *insularis* interbreeds frequently with *pulchra* in western Mexico (more substantiation needed as to the extent?), a split of *graysoni,* or restoring species status seems unwarranted without a comprehensive study of all taxa, including the ten other subspecies of *S. pitiayumi* not even being discussed, especially the six in South America. I must admit that I've never heard any Parula type song that suggested an American Redstart to me, so more extensive vocal studies with recordings would be helpful. If split, I do agree with the choice for an English name, Socorro Parula.

2023-A-5:
Separate *Piranga flava* (Hepatic Tanager) into as many as five species

YES. Based on the very different call notes of *testacea* (montane Costa Rica and Panama, which I've recently seen), I vote to split this from the *flava* and *lutea* groups. I also vote to split the *hepatica* group from the rest based on its evident distinctness. Neither of these seem especially problematic to me to justify on the basis of present data, though I recognize a) this is a novel treatment and b) my yes votes are the only ones. The rest of the taxa are mainly South American and can be kept under the oldest name *flava* until the more complex relationships there are resolved. This would actually not be dissimilar to the split we enacted with gnatwrens recently.

NO. This proposal lays out the complexity of the problem nicely, and makes good recommendations for future work that is needed to resolve this. As noted in the proposal, greater taxon sampling for genetic data, quantitative analyses of both plumage and voice (also with good sampling), and a better understanding of the variation across potential contact zones are needed before a definitive determination can be made on species limits.

NO. Conflicted and a very reluctant no! As outlined in the proposal, all evidence suggests multiple species-level taxa within this group, perhaps at least 7. It is tempting to at least split the group three ways, as they are treated in several recent classifications, and just kick the can down the road for the other potential splits. Summer Tanager is roughly "about as different" from *hepatica* as the three main groups are from each other in terms of plumage. But if you look at the published evidence for even just a 3-way split, it's largely anecdotal, and is actually not as rigorous as Hellmayr's plumage-only case for conspecificity. This whole complex is screaming out for a formal study with better sampling in terms of taxa, genes, geography, and voice, and will make a great dissertation-level study. Even just a study of vocalizations only, including perhaps playback, might meet minimum standards for splitting off our *hepatica* group from the rest.

NO. While it seems likely that we will eventually have to split this group, the many uncertainties involved suggest waiting until more comprehensive work provides a more solid foundation – otherwise we risk floundering through a series of changes until we get it right.

NO. Keep all five lumped for now. This is a great proposal that flushes out the complexities of the taxon, and the complexities of data that we use to make these types of decisions. I agree with the proposal that the data, either individual data sets like genetics or vocalizations or collective data sets, are not persuasive enough to split off taxa, even though there are plenty of hints that more than one species is involved. I can live with the extreme ecological and morphological diversity in a single species for now, given that the taxon is monophyletic, there are no known instances of true sympatry or even parapatry, and that this question can be much better answered with more data.

NO. For now, I agree with the proposal that splits are premature. I agree with others that it does seem like multiple species are likely involved in this complex, and I am definitely not opposed to splitting things out in pieces, but it is unclear to me even where to make the first split, and really which subspecies go with what group. For that reason, I am voting to keep this species together as a single taxon until more research can untangle these seemingly messy relationships.

NO. An excellent thought–provoking proposal. No for now. My sentiments are the same as for much of the rest of the Committee. If it weren't for WGAC, wouldn't this matter be turned over to SACC for consideration first, as South America seems to be where most of the questions are? As for the NACC area, I have little doubt that two species are involved, a northern *hepatica* group from northern Nicaragua to the southwestern USA, and a ? group from northwest Costa Rica to ? I've heard the calls (contact calls) from the Cordillera Central of Costa Rica to the mountains at Cerro Azul just northeast of Panama City These calls have been described by Vallely and Dyer (2018) as a "rapid phrase of three to four sharp notes that rise in pitch *cu-chit-it*, or *chudidit*. This is lifted directly from Stiles and Skutch (1989) and their Costa Rica field guide. Ridgely and Gwynne (1989) describe it as "a fast *chup, che-teh,* o *chup-chitup.* It is completely unlike the single, sharp, loud, *chup* that I know well from northern Mexico and southwestern USA, California, and from Honduras. I played the calls of the northern *hepatica* group to a very accomplished, thoughtful, and experienced Costa Rican birder and tour leader (Mario Cordoba), and he had absolutely no idea what species they pertained to. This is more than just two related taxa with slightly differing calls, these are in my opinion, based on limited experience (but published), "night-and-day differences." Vallely and Dyer (2018) show a large gap between the range in Nicaragua, presumably all (recordings on Xeno Canto of all northern subspecies give a sharp and single *chup* call) of the northern group, and the Cordillera de Guanacaste of Costa Rica. The placement of *testacea* (Costa Rica and Panama) in Nicaragua is based on Ridgway's (1904) inclusion (p. 87, volume II) from Chontales, but there is a footnote that says: "I suspect that the bird from Chontales, Nicaragua, may be *P. t. figlina*, or at least an intermediate between the two forms." Chontales is a district on the northeast side of the huge Lago de Nicaragua. Ridgway doesn't give the specific location of the specimen, but the Cordillera Chontaleña runs through the district. If the location and identity of the specimen is correct (to species), it could have been a migrant from the north. In any event central and southern Nicaragua do not have Hepatic Tanagers. While *testacea* seems to be placed by all, or nearly all, with the "highland group," I'm not sure what that is based on, and in particular whether it is based on vocalizations. Subspecies *testacea* is found in the mountains of the Darien, but whether it barely spills over into Colombia, I'm not sure. Hilty and Brown (1986) in their Colombia field guide don't map it there, so there is a gap between eastern Panama and the Andes of Colombia. As for calls, Hilty and Brown (1986) cite Miller (1963) as saying the call is a sharp *chup* but add that the call in Panama is *cup-chitup* (citing R. Ridgely 1976) and (Hilty and Brown 1986) that the "voice of Colombian birds needs confirmation." Ridgely and Greenfield (2001) in their Ecuador field guide say the call of the "highland group" is a distinctive *cup-chitup,* sometimes shortened to just *chup.* The former sounds like Panama birds, the latter like northern birds. I can say from somewhat limited experience of a few days with *testacea* in Costa Rica and Panama, I never heard anything but multi-syllabled calls, much as previously described above. I wondered if that was from direct field experience in Ecuador, or perhaps since they were talking about the "highland *lutea* group" as a whole, the Panama experience reflected the calls described. The call from Schulenberg et al. (2007) in their Peru field guide is described as a rich *chuck* or *chup,* thus sounding like the northern *hepatica* group. On the other hand Fjeldsa and Krabbe (1990) in their Birds of the High Andes say the alarm call is a repeated *tjik* and the call is *yuhtidit*, sometimes with an extra *yuh* at the end, given in flight or from a perch. They point out that Hepatics wag their tail slowly when excited. I'm not sure I've ever noted that in the northern group, but will now look. Fjeldsa and Krabbe (1990) do seem to illustrate the range (*testacea*) as just barely spilling over into northwest Colombia. Schulenberg et al. (2007) also illustrate a blotchy red and yellow male and call it "1st alt.?" I believe that 1st alternate males of at least some subspecies in the northern *hepatica* group are just like females and the only time a blotchy bird is seen only briefly later in the summer when transitioning to definitive adult male plumage.

The plumage of the northern *hepatica* group, at least the northern subspecies, has the brightest parts being the crown and the chin and throat, regardless of sex or age (dull red in adult males/ bright yellow in females and immature males until a little over a year old (by fall will look the definitive adult male). The extensive and dull gray cheek really separates the bright crown from the throat and makes the latter areas really pop out. More southerly groups have more colored faces (except lores?) and those areas don't pop out.

To come to a close, if *testacea* is indeed with the "highland group" than it could take quite a while to sort out the taxonomy, but if it is a stand-alone then from a NACC perspective, one could separate the northern *hepatica* group as one species, and monotypic *testacea* as another species. Then let SACC determine how many more species there are in South America (2-5). Or, at a minimum, since the call notes are so different in *testacea* from the northern group*,* a potential start would be to split the northern *hepatica* group as its own species.

NO. The Hepatic Tanager has a wide geographic range, inhabits a variety of ecosystems, and displays morphological, vocal, and genetic variations. However, there is not enough evidence to support any split within the species. Further phylogeographical and vocal studies, including playback experiments, with broad geographic sampling, especially in contact zones and migratory areas in North and South America, are necessary.

NO. As we can see in the proposal, it is a very complex group. I consider that there is not enough evidence to separate them into five species, for now. The proposal clearly explains what studies are necessary to resolve species limits in this complex.

NO. There is certainly a lot going on with this group in terms of lack of monophyly with *lutea,* genetic differentiation in general, and the massive geographic range and habitat diversity. However, I think it would be premature to split now given that there is no clear consensus as to an incremental step forward toward a more accurate taxonomy that would be ‘certain’ in light of additional future evidence. Excited for whoever tackles this group.

2023-A-6:
Treat *Stilpnia cucullata* (Antillean Tanager) as two species

YES, but on the fence. Taxa exhibiting this type of variation in allopatry in the “old” genus *Tangara* have often been treated as two species (e.g., *T. argyofenges, heinei*, and *viridicollis* or *T. peruviana* and *preciosa*). I do not think any examples of these show the degree of morphological differentiation that is shown within *cucullata*. Vocal data would be nice, but such data wasn’t used to treat the above examples as heterospecific.

YES. I went back and forth with this proposal and am going to go ahead and vote yes. The genetic and plumage color differences between these two subspecies are similar in magnitude as that observed among their closest relatives that are defined as species. Furthermore, the bill depth difference in *S. c. vitriolina* to its relatives is surprisingly large. Thus, I would argue these two isolated taxa represent separate species.

YES. Weak yes, and on the fence. But, as others have pointed out, the extremely large differences in bill size, plus differences in plumage which are on par with differences between *Stilpnia* (e.g., *peruviana* and *preciosa*, which have similar plumage patterns but differences in color of some of the patches).

YES, but PEND would be even better. The genetic and plumage (roughly) differences are comparable to others in this genus that we define as species, particularly with *Stilpnia cayana* and *Stilpnia vitriolina.* As noted by Wiley (2021) in BOC Checklist (No. 27) there are records of one or both subspecies of *S. cucullata* between Grenada and St. Vincent (pair on Mustique Island on 14 Nov 2009, eight on Union Island on 5 Oct 2015 and one on Bequia on 23 June 2015). In Isler and Isler (1987) there is a description of a pretty distinctive song for one of these subspecies: “A series of clear whistled notes, increasing in volume, and ending abruptly; introduced by a squeaky sound that suggests the song of the Palm Tanager, *Thraupis palmarum*. This comes from Clark (1905) in his Birds of the southern Lesser Antilles. Proc. Boat. Soc. Nat. Hist. 32:203-312. I’m not sure what taxon he is describing. He describes the call as characteristic *chirp.* Oberle (2008) has songs and calls of one or both taxa on his Cantos de aves del Caribe/ Caribbean bird song. 3 CDs. Cornell Lab of Ornithology, Ithaca, NY. From my reading of Wiley, it seems that nominate *cucullata* is found pretty much everywhere on Grenada while *versicolor* on St. Vincent is pretty much found in higher elevation and in more pristine woodland, although Wiley (2021) indicates that there is much habitat degradation at lower elevations.

I’m left with the feeling that this feels “rushed” to meet the timetable of WGAC and a more thorough investigation would at least perhaps reveal if there are at least distinct vocal differences between taxa, and that information might be available, and might include both taxa. Even so, given a yes or no to a split, I favor the former. I haven't thoroughly investigated the shared relationships of St. Vincent and Grenada. We have noted that more southerly Grenada shares affinities with mainland South America. Treating the two as subspecies doesn't seem particularly obvious to me. I have no field experience with either taxon. If you continue to regard the two as just subspecies then why not go ahead and merge both Lesser Antillean taxa into *Stilpnia cayana.* I don't think this has been done, but if one of the reasons not to split *versicolor* and *cucullata* is a small genetic distance, the same rationale could be used for a further lump. Admittedly, not splitting is maintaining the status quo (for most) while a lump with mainland *cayana* is breaking new ground.

NO. Nice summary of the available information, but I am not convinced that these are best treated as separate species. More sampling and data are needed, with a larger number of individuals analyzed for genetics/genomics and a quantitative analysis of vocal differences (including playbacks).

NO. Bill size and shape are highly plastic, adaptive characters than show rapid evolution in allotaxa, especially in insular taxa, e.g. Geospizinae and drepanids, and in themselves are not sufficient evidence for treatment of taxa as separate species; however, bill size and shape are clearly one of the first things that can change during the speciation process and often signal divergence in diet. HBW-BLI scheme awards 2 points for bill size differences and the remaining 5 come from differences in plumage shades, not plumage pattern. Because bill size and plumage shades are characters that show geographic variation within monotypic species with clinal variation, they should not in my opinion be sufficient evidence, on their own, for species rank. Using a yardstick approach, I can't think of any two sympatric species of tanagers in the formerly broadly defined "Tangara group" that differ only in variation in color shades but with the same basic pattern. In other words, if such variation were a regular theme in differences among sympatric or parapatric species in the group, I would take them more seriously.

Addendum: Two members point out that *T. peruviana* and *T. preciosa* differ only in color shades yet are treated as separate species. On the surface that is true, but what is not mentioned is that in this case, that patch change also produces a conspicuous actual pattern difference. In *peruviana*, the chestnut head contrasts strongly with the blackish back, whereas in *preciosa*, the back is also chestnut, thus matching the crown and producing a different pattern, i.e. no crown-back contrast. Firme et al. (2007) found that the two are virtually parapatric with no signs of gene flow. As for *heinei*, *argyrofenges*, and *viridicollis*, the three differ in pattern as well as strongly differ in coloration. I'm not saying that lack of pattern differences are an isolating mechanism per so, only that we don't have any taxa in the Tangaras that differ only in color shades (that I can think of); from what I can tell, if you looked at black-and-white photos of nominate *cucullata* and *versicolor*, I doubt you could tell which was which.

As for wanting better genetic data, what would those data indicate with respect to a decision to call them species or subspecies? Even without any genetic data, we already knew they must differ to some degree genetically unless we think that the bill and color differences are environmentally induced. Is there some defensible threshold of degree of differences in neutral genetic variation on which to distinguish species vs. subspecies?

NO. There does not seem to be a compelling reason to make this split at this time.

NO. The two currently recognized subspecies show morphological differences in plumage and bill size, in addition to a 0.8% difference in mitochondrial DNA (similar to other species in the genus). However, morphological and genetic data from a larger sample size and vocal analysis are necessary before splitting the subspecies.

NO. I think we need to wait for full genetic data to be published for both subspecies and the entire range, in addition to morphological data on the specimens and a formal analysis of coloration.

NO. While I think these may ultimately be split, I'm not comfortable splitting them with the limited data we have on hand. One of our charges as a committee is to act 'conservatively' in making taxonomic changes. I think that more quantitative genetic and phenotypic data would be informative to better contextualize potential inter-island dispersal and how differentiation compares to other full-species splits within Thraupidae.

NO. At first it seems like a judgment call or maybe even a good case for a split, but in looking at actual photos, to me the plumage differences among males between islands are often not pronounced, to the point where I could not guess accurately which was which even in good photos. I also expected to be able to see a difference in bill size, but I can't, and several of the Grenada birds look really big-billed while several of the St. Vincent birds did not. In the absence of other compelling data, I reluctantly conclude this is not a strong case for a split.

Note: I believe there is an error in the Recommendations section of the proposal, in which *versicolor* was meant instead of *vitriolina* as written.

2023-A-7:
Treat *Ramphocelus flammigerus* (Flame-rumped Tanager) as two species

YES. This is definitely a tough one, and more work can clearly be done on this hybrid zone to further clarify things, but to me, despite the overall lack of genetic divergence, the patterns in phenotype are striking and indicative of selection, as the hybrid zone has remained remarkably stable in terms of cline center and cline width over a 100-year period. That stability to me, along with a relatively narrow width (~32 km) is definitely suggestive of selection maintaining these two groups as distinct.

NO. Reasons are given in the proposal. It sounds like more published analyses may be forthcoming (including the SNP data on the hybrid zone presented at AOS 2022 and plans to study other areas of contact), so I think it's best to hold off and perhaps revisit in the future. Also, no mention was made of vocalizations in the proposal - do these subspecies differ vocally, and how do vocalizations vary across the hybrid zone? The data suggest few pure parentals in the hybrid zone and some selection against hybrids, but is there any evidence of assortative mating based on plumage and/or voice where they contact? This is an interesting situation but further published evidence is warranted to support a split.

NO. I agree that apparent levels of gene flow preclude recognition of these taxa as separate biological species.

NO. I would have to know a bit more about the level of hybridization. The key phrase "…some selection against hybrids" is nebulous: was that selection in the hybrid zone? What was the strength of the selection? Ordinarily any selection against hybridization in the hybrid zone should warrant species status for the two taxa, but we will need to wait to see the resulting paper(s) spell this out more clearly.

NO. The two forms come into contact in multiple areas and there appears to be enough gene flow for this committee to consider them one biological species. They would clearly be separate species according to other concepts, or a more relaxed definition of a biological species. Unless there is more evidence about strong selection against hybrids in the contact zone, perhaps best to continue treating them as one species for now.

NO. I gather studies are ongoing, but intergradation seems frequent in the stable hybrid zone. There don't seem to be vocal differences. Studies are ongoing, so why split when most authorities do not?

NO. Published mitochondrial DNA data show no structure associated with geography or plumage coloration; furthermore, mtDNA suggests high levels of gene flow. The proposal mentioned that genomic research is ongoing and we should wait for that new information given that what is currently available does not provide enough evidence for a split.

NO. Although the color pattern is clearly different in the two subspecies, the results of the genetic data indicate that there is gene flow between the two subspecies, perhaps an incipient process of speciation is being observed, but at this time the limit between the two subspecies is not supported by the biological species concept.

NO. That the contact zone consists of a hybrid swarms with mostly intermediate birds indicates absence of assortative mating – these two populations treat each other as "same" when it comes to mate choice, so, in my opinion, why shouldn't we? The cline in variation between the two is best seen in the rump color data in Morales-Rozo et al.'s Fig. 6D (based on Sibley's 1956 samples). The hybrid swarm that comprised the contact zone is sufficient reason to treat them as conspecific.

Of course selection maintains the two as separate subspecies – otherwise the cline would extend all the way through the ranges of both populations, and no subspecies designations would be warranted. The view that if selection curtails gene flow into the core populations means that they should be considered separate species is essentially a PSC interpretation that we do not follow. If we did, then all parapatric taxa would be considered as either two separate PSC species or one PSC species with clinal variation --- in other words, there would be no such thing as parapatric subspecies under that interpretation. In a stable system, the degree of selection determines the geographic structure of phenotypic variation AEBE. If selection is clinal, then the geographic variation is clinal (and no subspecies warranted; if the selection is not clinal, then the phenotypic variation shows some structure, and if that that selection is abrupt enough to produce plateaus of phenotypic variation bridged by zones of intergradation, then the plateaus form diagnosable units that can be labeled as taxa (subspecies under BSC). In all three cases, selection is in operation --- it's just that geography of selection differs. What really counts is that there is no selection against interbreeding in the contact zone between the two taxa in this case, ergo a classic case of intergradation between two subspecies. Or at least that's the way NACC has been applying the BSC for at least 40 years.

NO. These appear to be acting as a single species under the biological species concept. I also saw the talk at AOS-BC and think these are suggestive of a single species. F1s can be very difficult to find in nature, but if I recall there is substantial backcrossing suggesting that there is asymmetric introgression and likely more gene flow than most hybrid zones between full species under the BSC. I'll also say that the presence of a cline in itself isn't evidence against species status under the BSC (see data on Black-capped Chickadee x Carolina Chickadee), but contextualizing those clines in geographic space and the nature of F1s and backcrossing is important in my opinion to make informed decisions on thresholds of reproductive isolation for delimiting species under the BSC. I would be happy to reconsider if more data were published, but given what is presented in this proposal, these are better treated as subspecies.

NO. I also was at the AOS talk and, while the data suggest some level of selection against hybrids, I don't think we should make a change at this point. If the resulting publication shows stronger evidence for specific status, then that could lead to a different outcome here.

I kind of hate to bring it up, but what happens between *icteronotus* and *passerini* where they apparently meet on the Atlantic coast of Panama? Since we treat *icteronotus* and *flammigerus* as conspecific, it seems inconsistent to not include *passerini* (+*costaricensis*) as well (*passerini* would have priority), unless they are parapatric. The latter seems unlikely, and as far as I can tell all have similar vocalizations.

2023-A-8:
Treat *Cacicus uropygialis* (Scarlet-rumped Cacique) as two or three species

YES. Three species (option D). Reasons are outlined in the proposal. Data are incomplete, as they often are, but the distributions of these three taxa strongly suggest divergent genomes between which genes no longer would or could flow freely. The voice of extralimital *uropygialis* strongly suggests species rank. The evidence for treating *pacificus* as a separate species from *microrhynchus* is weak (and the branching pattern in Powell et al. needs rechecking), but that near-parapatry without any phenotypic sign of gene flow tips the weight of the existing evidence towards a YES for me. What's needed, obviously, is a thorough study of the putative contact zone.

Addendum: Several voters suggest that the contact zones have not been studied sufficiently to detect gene flow. From the purely genetic standpoint, this is of course correct. But from the phenotypic standpoint, I disagree. Wetmore clearly studied phenotypic characters in the potential contact zone, based mainly on a fairly noticeable difference in bill shape. Otherwise, how would he have been able to determine that the two taxa came close to each other without any signs of intermediacy? As for the contact zone between *pacificus* and *uropygialis*, there isn't one according to all existing data. One is a lowland taxon, the other Andean, with a gap that has been sampled by collectors and birders without finding either for more than a century. In both cases, sure, it would be great to have modern transects, but all available evidence points to near-parapatry without any evidence for gene flow. Then, add the vocal differences qualitatively documented for *uropygialis* vs the lowland taxa. In my opinion, burden-of-proof falls on conspecific treatment. I'm sympathetic to the desire for more rigor, but I think we have sufficient evidence to make that rigor apply to a data-set that would treat them as conspecific.

YES. Three species (option D). Genetic differences are similar to other species of blackbirds. Together with vocalization differences, and apparent lack of contact/gene flow between geographically adjacent forms convince me that there are three species involved.

YES. Three species (option D). Another proposal I'm on the fence about, but given the near parapatry of *pacificus* and *uropygialis* with no apparent interbreeding, and the phylogeny that recovers *pacificus* and *uropygialis* as sister, outside of *microrhynchus*, this warrants a three-way split. However, some questions do remain for me, namely, how sure can we be that there is no interbreeding between *pacificus* and *uropygialis* without extensive sampling in the region, given how similar in plumage the two are?

YES. A weak yes for three species (option D). The proposal is compelling, but so are comments indicating that further study is needed. I see no range break in the distribution map in Vallely and Dyer (2018) between *microrhynchus* and *pacificus.*

YES. Two species (option B). Again I am the only one to choose this option but (like some others) I am leery of placing too much stock in the results in Powell that are the basis for considering *pacificus* and *microrhynchus* non-sister. Until that is verified, I am putting it to one side. I agree with Boesman that *pacificus* and *microrhynchus* are somewhat different vocally (and I am especially familiar with *microrhynchus* in Costa Rica), but I think that a proper vocal analysis is needed in this case because the differences between the two lowland taxa are not so very pronounced as they are between *uropygialis* and the other two. I also would like to see better evidence of parapatry between *pacificus* and *microrhynchus* (no disrespect to Wetmore, but…). But I don't have the same uncertainty about splitting *uropygialis* from the other two, based on size/structure, voice, and elevational zone.

NO. There may well be more than one species, but I don't think the evidence is there yet to support a split. The genetic data from Powell et al. (2014) are based on n=1 for subspecies *uropygialis* and *pacificus*, and n=2 for *microrhynchus*. The vocal data are not quantitative, and the reference to sonograms excludes *uropygialis*. The proposal noted that there is a lot of vocal variation within each (*pacificus* and *microrhynchus*), with "the absence of recordings from eastern Panama and Colombia from near the putative area of contact." The proposal also notes "remarkable variability in vocalizations," which begs for a more thorough, quantitative vocal analysis with good sampling. The biogeography and ecology are interesting, but much of what is discussed is speculative: "ecological conditions change fairly abruptly in that region, perhaps caused by differences in rainfall" (*yet no analysis involving ecological niche modeling has been done*)... "there is no sign of gene flow" (*yet gene flow has not been explicitly examined*)... "these two populations have diverged to the point that they have adapted to different ecological conditions, and neither has conquered the conditions in the minor elevational gap (if there really is one)" (*is there really a gap? requires further study*). Given all of this, I believe the best course of treatment is to maintain the status quo pending further genetic and vocal study with good sampling, including across putative contact zones, as well as ecological niche data.

NO. I vote for A, one polytypic species. Status quo. There is too much reading of tea leaves here for me—we still lack a solid published analysis of the group. Contrary to the reasoning in the proposal, there is a huge range of gene flow level possibilities between free interbreeding and no gene flow, and we have two actual or nearly parapatric zones in which gene flow has not been measured. We cannot infer a lack of gene flow between closely related taxa when it has not been closely examined (especially when they are "perhaps so similar that intergrades would not be detected"). We also have many examples of intraspecific taxa in which characters change abruptly in a steep cline despite substantial gene flow across much of the rest of the genome. (Note: In the proposal, the pasted-in tree snip is from Powell 2014: fig. 1 and is mtDNA. The topology matches that of the mtDNA+nuDNA tree of their fig. 4. The nuclear dataset consists of only four nuclear loci across the entire study, but no nuDNA data were acquired for these taxa (their table 1), so the signal here will be entirely mtDNA and thus very problematic for inferring species limits.)

NO. Quantitative vocal data, in addition to nuclear and mitochondrial genetic data from a larger sample size and wider geographic area, are needed before deciding on any splits.

NO. Although I believe that more than one, and likely three, species are involved, I think that this complex needs a more thorough analysis before we split any off. The analyses in the field and lab to date have not been looking at pertinent species-level questions. These taxa are morphologically very similar, and I am not sure if phenotypic evidence of gene flow could be detected in this complex without larger samples from areas of parapatry. Especially informative would be genetic samples from these areas. Caciques are so variable vocally (individual and intra-population), using vocal characters to judge reproductive isolation would require a much deeper analysis.

NO. I think we need more data and analysis before we split.

NO. The ecological differences and geographic distributions are certainly intriguing, but we don't have data to back up the claim that there is no gene flow between adjacent (or at the very least, geographically proximate) metapopulations. The vocal data are also qualitative and could use a larger sample size and more geographic context to convince me that these are separate full biological species. Future studies may well split them, but for now, I vote to keep as a single polytypic species.

2023-A-9:
Treat *Sporophila ophthalmica* as a separate species from *S. corvina* (Variable Seedeater)

NO. I agree with the proposal that it's best to retain these as a single species, given the available information - especially the evidence for extensive hybridization and gene flow.

NO. A hybrid swarm connecting two taxa indicates free gene flow. In my opinion, those are the only data needed. These birds treat each other as "same" when it comes to the all-critical behavior of mate choice … so why shouldn't we? That HBW/BLI use this hybrid swarm to award 2 points towards treating them as separate species, yet claim to be following the BSC, is stunning and reveals how anti-biological the Tobias et al. scoring scheme is with respect to the speciation process. Other "points" come from anecdotal, unquantified "possible" differences in habitat (1) and paler plumage shade in females (1), despite habitat differences and plumage shade being features that can vary clinally within a species, much less between subspecies. Take those points away, and the HBW/BLI score doesn't reach the threshold of 7 points.

NO. Continue to keep as one species. extensive gene flow is occurring and black *S. c. corvina* is embedded within pied subspecies in the phylogeny.

NO. As noted in the proposal and other committee members' votes, the hybrid swarm, as well as *S. c. corvina*'s position embedded within the pied subspecies all suggest that this is best treated as a single species.

NO. When you start reading about hybrid swarms it's hard to have much enthusiasm for looking for an alternative view.

NO. Extensive gene flow between the subspecies in Costa Rica and Panama leads to a hybrid swarm. Furthermore, the pied subspecies group *Sporophila corvina ophthalmica* (which includes three subspecies: *hoffmanni*, *hicksii*, *ophthalmica*) would result as a paraphyletic group if it did not include the black subspecies *S. c. corvina*.

NO. Agree with the proposal. Evidence is that gene flow is extensive and species-level barriers have completely broken down where the two taxa come together.

NO. I agree with the proposal. We need more evidence for the split.

NO. Gene flow seems extensive where the putative taxa overlap. Best treated as a single species.

NO. Hybrid swarm = no split.

ABSTAIN.

2023-A-10:
Treat *Molothrus armenti* as a separate species from *M. aeneus* (Bronzed Cowbird)

YES. Vocalization differences, combined with lack of evidence for lumping them to begin with.

YES, but weak. For starters, no written rationale was given for the lump, so why does it deserve to be treated as having precedent? The various characters are outlined as far as known. The smallest cowbird, even in comparison to the small western subspecies of Brown-headed (*obscurus*), the "Dwarf Cowbird." I would have preferred more comparative detail on vocalizations. It was largely unknown when Jaramillo and Burke (1999) published their New World Blackbirds book. I am also interested in behavioral aspects. Those that have seen the helicopter display of the Bronzed Cowbird. Jaramillo and Burke (1999) call it the "hover display" and it can last up to 21 seconds. I have seen it with the northwestern subspecies *loyei* but presume the other two subspecies give the "hover display" too.

While we are on Bronzed Cowbirds, I've wondered about the east-west division with the two groups. In the nominate subspecies females are colored much like males, just slightly duller, while in *loyei* and *assimilis*, the females are gray. I suppose the two groups come into contact in the Isthmus of Tehuantepec, but otherwise seem to be largely separated. At the northern end of the range (West Texas), the species is scarce, but apparently both subspecies occur there.

YES. This is also a SACC issue, and I think we should make our decision in view of what they decide. Nonetheless, I think there never was a rationale for lumping them in the first place, and the two taxa are markedly different. It may be difficult to determine if these differences would be reproductively isolating, as *Molothrus* does not have any other similar situations, but for me the default is to split them, and if a rationale is given to lump, then we can go ahead and vote on that. As far as the English name, Bronze-brown Cowbird is okay, unless someone has a better idea.

YES. Unfortunately no further recordings have come online, but hopefully that will be remedied soon. But for now, I don't see why this doesn't fall into the "no justification for the lump" category that could get it out of purgatory.

NO. I originally voted yes, but after reading the comments of others and discussing this with Dan Lane, who now has experience with *armenti*, I change my vote to no. This is one for which detailed studies of voice and behavior are needed before a change can be made.

NO. I am not convinced that these should be treated as separate species. The morphological differences are consistent with subspecies-level treatment, genetic data that includes *armenti* are lacking, and the vocal data are qualitative. Further study is needed.

NO, but not strongly so. This seems to be a judgment call based only on diagnosability rather than on more solid comparative species limits criteria. It is different, but are those differences large enough to represent biological species limits? That seems unclear to me.

NO, but weak. I am curious to hear what others have to say about this one. The vocalizations that I went through do sound different to me, but I'm not always a good judge of what qualifies as different in terms of voice, especially in this Bronzed Cowbird group that I do not have a lot of experience with.

NO, but weak**.** I was inclined to vote yes after reading the proposal and Gary Stiles' comment on the SACC site, mainly based on the fact that there are phenotypic differences and that *aeneus* and *armenti* were lumped with a lack of evidence. However, listening to recordings on xeno-canto I found noticeable variation within *aeneus*, both in songs and calls (although I'm not an expert in voice); in addition, the Birds of the World account mentions that there are dialects defined and widespread, although dialects are not known for the entire range of the species (Ellison and Lowther 2020). Therefore, I believe that quantitative vocal data needs to be evaluated to understand the variation within *aeneus* versus *armenti*.

NO. Although there is a morphological difference, there are no genetic data or serious song analysis to support the separation into two species.

NO. We should certainly coordinate with SACC on this one and I would be inclined to change my vote if there is a strong sentiment to recognize *armenti* as a separate species. This does raise an interesting philosophical question of what is the 'status quo' if there was never any rationale for the prior lumping. I think some genetic data would be very useful in delimiting species here.

2023-A-11:
Treat *Icterus fuertesi* as a separate species from *I. spurius* (Orchard Oriole)

NO. Reasons are given in the proposal for rejecting this split.

NO. For once, we have lots of great data on the taxa thanks to the Omland lab. However, those data, in my opinion, all indicate that these are not two separate species. The thorough analyses of vocalizations show only minor differences, and whether those differences make a difference to the birds is unknown. Differences in color shading are not, in themselves, sufficient criteria for species rank; in contrast, such differences are a common pattern in taxa treated as subspecies. Hoffmann et al. showed the color difference is just a difference in relative concentration of phaeomelanins. As for genetic differences: of course they show genetic differences given their distributions – the only surprise would be if those genetic data showed no differences. However, I have one lingering concern: the relatively small gap between the two despite (apparently) seemingly suitable habitat in the gap. This suggests to me that these two genomes may be incompatible if neither can occupy the gap between the two; in other words, if these two taxa were really conspecific, why can neither occupy the gap?

By the way, comparison of this situation with that in Hooded Oriole, currently treated as polytypic, is a good one.

NO. These seem to be good subspecies not yet diverged to biological species level.

NO. Do not split *I. fuertesi* from *I. spurius*. Both vocal and genetic differences are minor, similar to that seen within other species of orioles.

NO. For the reasons outlined in the proposal, the lack of genetic differentiation and lack of vocal differences seem to support keeping them as a single species.

NO. Interesting, but nothing particularly compelling to split. One can wonder that perhaps splitting the two groups of Hooded Orioles into separate species is equally as justified. There are pretty distinct color differences in those two groups as well as slightly more black in males around the face in the eastern group. Vocalizations in the two groups of Hooded Orioles do not differ as far as I know, nor do they with the two subspecies of Orchard Oriole. Yes, but why the range gap? Is it real? The two groups of Hooded Orioles (*cucullatus* and *nelsoni*) are well separated by range in the U.S., but perhaps overlap in north-central Mexico.

NO. Evidence from genetic, vocal, and plumage coloration suggests that *spurius* and *fuertesi* represent two forms of the same biological species.

NO. Although the color differences are very noticeable and consistent, I would expect the taxa to have more vocal differences if they were reproductively isolated. Playback experiments with stuffed models would be interesting.

NO. I agree with the proposal to reject the separation.

NO. I agree that the plumage and song differences are slight and suggestive of a single species, especially in light of the shallow genetic differentiation.

NO. If there were something besides plumage (which is very intriguing, because it isn't just your usual darker vs. lighter, but has been shown to be explicable on the basis of relative proportions of phaeomelanins vs carotenoids), or if they were truly parapatric, I would be more likely to vote for a split. Nevertheless, I am impressed by the quantity and quality of recent papers on this subject that all seem to point to the same answer (except, it should be noted, for at least some differentiation in calls), and it's unfortunate that such studies aren't yet available for most species-limits questions.

2023-A-12:
Treat *Chlorothraupis frenata* as a separate species from *C. carmioli* (Carmiol's Tanager)

YES (weakly). This is primarily a SACC issue and we should be as consistent as possible, plus the revised proposal with the addition of UCE data (although unpublished) lends stronger support to a split in view of the apparent paraphyly of *carmioli*. I'm still bothered by the very limited genetic sampling and the lack of a quantitative comparison of vocalizations with good sampling across the range of the different subspecies. Playback experiments also would be useful given the strongly allopatric distribution of *frenata* relative to the other subspecies. The plumage differences are interesting but subspecies-level. Despite these caveats, I'll go along with the split for agreement with the SACC. We clearly need a separate proposal on the English names.

YES, but weakly, mainly based on the fact that I don't have much experience with the Central American *carmioli* group and none with *frenata.* I'm a little unsure about declaring that these are only subspecies differences. You could easily say that about *H. olivacea* too. I do trust that those that have analyzed the situation feel that the vocalizations are distinct. I'm also left feeling that this is a SACC problem. Perhaps they have already dealt with it, or are doing so now? A few other comments. Isler and Isler (1987) say that in South America, subspecies *frenata* "appears to live primarily at mid levels inside forest, to travel in much smaller groups than in Central America, and to be encountered more often with mixed species flocks" (T.A. Parker data). I haven't seen this supported from other South American references. All of the vocal data and the differences between the two groups are summarized by Isler and Isler (1987), some 35 years ago, and I haven't seen them effectively challenged. Plate 96 (figures 21) in Ridgely and Greenfield (2001) illustrate the species (Olive Tanager *Chlorothraupis frenata*) as sexually dimorphic. The text says that the female is similar to the male but has yellowish lores with a clearer yellow and unstreaked throat. The text also says "sometimes considered conspecific with Carmiol's Tanager (*C. carmioli*) of s. Central America." In reviewing photos, what inspired Griscom to give the scientific name of *magnirostris* to the western Panama subspecies? It (the bill) is certainly not dramatically larger in that one specimen.

As for English names, "olive" should be avoided! It hardly sets it apart from some other species of *Habia.* Moreover, I see Ridgeley and Greenfield (2001) use Olive Tanager for the split *frenata*, retaining Carmiol's Tanager for the other three subspecies in Central America. Isler and Isler (1987) call the species with four subspecies, including *frenata,* Olive Tanager.

Add on (13 July) - for English names issue: After reading other comments, I have no issue with calling them by their genus name (*Habia* or *Chlorothraupis*), as it sets them apart as non tanagers. Nobody has ever objected to spindalis. Some feel we are stuck with the species in *Piranga* forever, but why? I talk to birders frequently about this issue, birders of all levels of experience, some representing the very best ones in North America. Nearly all think they are in what is presently constituted as the Tanager family, *Thraupidae.* Of all of the English names we have changed and adopted new names, all well accepted, *Piranga*, would be the easiest to pronounce. One statement I have often made with groups is first to tell them that the five species recorded in the U.S. that are called tanagers aren't tanagers. And, for the five species that are actually tanagers as the family is presently constituted, none are called tanagers. Ask one to name the five, no one has been able to do so until many hints are given. We had multiple polls with eBird users with the split meadowlark species for an English name, why not consider changing those species in *Piranga* as having that name for the last part of the English name? I realize that this is heresy for many, but I'm floating the idea, in part because of the "forever" statement. In a non-scientific sampling, no one I have talked to has objected to the change to *Piranga*. In fact they said it would be helpful and educational.

YES (weak). There are plumage coloration and vocal differences between *carmioli* and *frenata*. The recent UCE phylogeny includes a representation of the three species in the genus *Chlorothraupis*: *C. carmioli* (one individual from the *carmioli* group [*lutescens* from Panamá] and one individual from the *frenata* group [Perú]), *C. olivacea* (one individual from Panamá), and *C. stolzmanni* (one individual from Ecuador). The phylogeny shows that *C. carmioli* is paraphyletic, with *frenata* being the sister of *C. olivacea*, which together (*frenata* + *C. olivacea*) represent the sister clade of *carmioli*. Considering the phylogenetic relationships and vocal differences, along with the plumage differences between *frenata* and *C. olivacea*, I vote to treat *C. frenata* as a separate species from *C. carmioli*. However, a phylogenetic study with complete geographic sampling is recommended to better understand the relationships in the genus *Chlorothraupis*.

YES. The genetic analyses show that *frenata*, like *olivacea* and *stolzmanni*, should be treated as a separate species from *carmioli*. The songs in the genus are all somewhat similar. To my ear *frenata* is more like the songs of *C. olivacea* (e.g., https://media.ebird.org/catalog?taxonCode=lestan; https://media.ebird.org/catalog?taxonCode=lestan) and *C. stolzmanni* (https://media.ebird.org/catalog?taxonCode=ocbtan1), sharing the slow-paced cardinal-like whistled notes.These songs are amazing, and so "un-tanager" like. I guess I would favor Yellowish Tanager as an English name, but that is terribly boring.

It would sure be nice NOT to call these "tanagers", and we wouldn't be so restricted to choosing a better name!

YES. Although one can always demand more data, I think the vocal differences outlined in the proposal are sufficient grounds for treating them as separate species. As noted back in 1989 by Ridgely & Tudor, the highly disjunct range is suspicious, and even then, anecdotal information on voice suggested a potential split. We have accumulated enough recordings now that confirm those initial findings and places the burden-of-proof in my opinion on treating them as conspecific. I think the only valid basis for rejecting the proposal might be absence of playback trials.

There is another reason for conservatively treating them as separate species. As far as I can tell, the phylogenies published so far have not sampled both the *carmioli* group and the *frenata* groups. In both published analyses, *C. carmioli* is represented by the same sample of *frenata* from N. Peru, as if that sufficiently characterizes the genome of a species with disjunct populations separated by a thousand kilometers and on opposite sides of major biogeographic boundaries. This is yet another example of failing to appreciate the importance of broad taxon-sampling in phylogenetic analysis. How do we even know that the *carmioli* group is monophyletic? In fact, that two other *Chlorothraupis* occupy the intervening region in similar habitats makes me wonder if there isn't a leap-frog pattern going on here, with either *stolzmanni* or *olivacea* or both being more closely related to one of the *carmioli* groups than *C.* (*c.*) *frenata* and *C. c. carmioli* are to each other. Seems like a long-shot, but it is sufficient for me to object to a conspecific treatment without genetic data confirming the monophyly of our current *C. carmioli.* I'd also like to hear from Kevin B. how much confidence he has in that topology from 2007.

As for the plumage similarities, remember such similarities are roughly comparable to those between some species in genera closely related to *Chlorothraupis*: some *Habia* species, some *Piranga* species (e.g. *hepatica* and *rubra*, and definitely between the eventually to-be-delimited species within *hepatica*), and, most notably, the other two *Chlorothraupis* species, which are treated as separate species already. Check out Plate 32 in HBW Vol. 16 to see what I mean; *frenata* and *carmioli* are more similar to each other than either is to *olivacea* or *stolzmanni*, but not by much, especially if you allow for a little Gloger's Rule darkening of the latter two in those more humid regions.Take away the eyering of *olivacea* and presto, suddenly it no longer stands out.

As for English names, this requires a separate proposal in my opinion. "Olive Tanager" is clearly DOA because of *C. olivacea* and inconsistency into which taxonomic concept "Olive Tanager" applies. Let's bury that one forever. I also think we should consider changing the last name from Tanager to Chlorothraupis. Here we have an opportunity to de-Tanager another non-thraupid genus, with Spindalis and Chlorospingus providing precedents with which the world seems comfortable. As an English name, Chlorothraupis is no more intractable than Chlorospingus (or Hemispingus), although unfortunately it retains the "thraupis" part that still connotes a tanager. Although their close relatives in the Cardinalidae, *Habia* and *Piranga*, will likely retain "Tanager" in their name forever, at least using Chlorothraupis will help remind us of the family-level change. None of the four species occurs in an English-first country. and none is a particular familiar widespread bird, so this minimizes the impact of the instability caused by such a change.

17 March update: I am pleased to see that the genetic data confirmed what I suspected. They should make this case a no-brainer.

YES. I had originally voted no, but upon reading the revised version of this proposal, I agree with the author of the proposal that these are best treated as separate species. Both phenotypic and genetic differentiation between the *frenata* and *carmioli* group seem consistent with species-level differences in related lineages, and the biogeographic patterns with congeners in the middle of these two putative species implies to me they are reproductively isolated.

YES. It's unsurprising that two birds would convergently evolve (or retain the primitive condition of) drab olive plumage, even if not sister species (but we don't know that, just guessing from the distributions). But their songs are similar enough that they do seem likely to be closely related. In combination, the several subtle morphological differences, the biogeography (with intervening congeners), and the seemingly consistent differences at least in pace of song sway this to a split for me.

YES. My decision is based on the otherwise unusual distribution, differences in song, genetics, behavior, and the tree topology. Plumage differences are admittedly subtle, and probably wouldn't convince me on their own. However, they show differences similar to those differentiating related species (*Habia*, *Piranga*). The proposal is correct that the terminal *C. carmioli carmioli* is mislabeled in Ben Scott's thesis, but we researched how that happened and are confident the sample is *frenata*. We intentionally set out to sample as many subspecies as possible in the group. The proposal shows the Astral tree based on a coalescent analysis of different genes. In other words, an approach summarizing different gene trees. I will also add that a concatenated maximum likelihood tree (combining all genes together) shows the same topology with 100% support for all nodes shown in the proposal. So, we have high confidence in the tree topology. More individuals per taxa of course would be interesting, but at this point the preponderance of the evidence no longer supports treating *frenata* as part of *carmioli*. This also puts our vote in agreement with SACC. Lastly, Isler and Isler (1987) cite Parker data for behavioral differences between *frenata* and the other subspecies. *frenata* "appears to live primarily at midlevels inside forest, to travel in much smaller groups than in Central America, and to be encountered more often with mixed-species flocks".

YES. My original vote was a weak yes, but the seeing that *frenata* and *carmioli* are paraphyletic, with *olivacea* sister to *frenata*, my vote is a strong yes now. In addition to the genetic data, the hugely disjunct distribution with congenerics in between, plumage differences (weak, but then, all plumage differences in this genus are pretty minimal), and the consistent vocal differences all suggest that these are specifically distinct. As for English names, I would prefer to find an alternative name for *carmioli*, in line with NACC policy to assign new names to daughter species that have similar range sizes. For *frenata*, I like the name used by Clements, Yellow-lored, as it highlights the main plumage difference between the taxa.

YES, but weakly. The UCE tree showing that *carmioli* as presently recognized is paraphyletic is really interesting (the branch lengths are not so interesting; this is a tendency for UCE trees to have, even within species). Our cladistic concepts of monophyly versus non-monophyly have to be set aside for the biological species concept because we have numerous examples of good biological species that clearly don't fit a strict cladistic framework (Haffer had a good figure on this 30 years ago). For example, as we've recently shown in the Green-winged Teal, that biological species has a paraphyletic history with the Yellow-billed Teal and we keep *Anas crecca* together because they exhibit subspecies-level divergence only, which they conveniently show in contact through gene flow. One can envision a similar case here in which one population undergoes stronger divergent selection (*olivacea* in the Scott 2022 snipped figure) and speciated under the BSC while other populations remain at subspecies-level divergence (e.g., the similar phenotypes of *magnirostris* and *frenata* in the specimen photos). Unlike the teal, we don't have the test of gene flow in contact. But we also only have one specimen per lineage, not a great basis for determining species limits, and what seem to me to be subspecies-level phenotypic divergences. But after reading others' comments I am changing a weak no to a weak yes.

NO. There is little differentiation in coloration between the two groups and I think genetic data are needed. A formal study of songs is also needed.

2023-A-13:

Treat *Melopyrrha taylori* as a separate species from *M. nigra* (Cuban Bullfinch)

YES. I voted for this split in 2016 and again would argue a split is warranted based on plumage, vocalization, and morphological differences. Furthermore, bill size and shape as a diagnostic species trait fits with patterns seen among this species' relatives (other Caribbean finches and the Darwin's finches). The radiation of Caribbean finches/tanagers (bullfinches, grassquits, bananaquit, orangequit) is analogous to what has gone on with the Darwin's finches, but it's unrecognized/underappreciated for two reasons: 1) lack of recognition of their close relationship and 2) characterizing some of the divergent forms as subspecies. A yes vote would help rectify the second point.

YES, though weak. A went back and forth on this decision a fair bit, but am currently coming down on the side of supporting the split. I think the morphological differences, together with the vocal differences, are supportive of species-level differences, though as always with allopatric island taxa, a challenge to actually test in a meaningful way.

YES, for the reasons outlined in the proposal, but note Terry's dissenting view. I continue to view our species treatment in this genus and related *Loxigilla* as inconsistent. With the Barbados Bullfinch, a split I supported despite both sexes being looking alike, there is no vocal analysis, and mitochondrial DNA was extremely close between the two. With these two bullfinches, they remain genetically unstudied to my knowledge (especially *taylori* but they are vocally distinct by those that can separate out the high notes). Regardless of how these are treated, Hellmayr's (1938) treatment should not receive a pass or be considered precedent. I thought the modern biological species concept considered vocalizations, i.e., an evaluation of the entire picture? The one sentence footnote by Hellmayr hardly indicates that the matter received anything more than a passing glance, and in ambiguous situations like this one, the word "clearly" in his one sentence footnote is particularly grating.

However, if there is nothing particularly new since we voted on this last and after Garrido et al. (2014) why are we wasting time again? Can't we just send WGAC the comments from the last time around? It is not just this one, but I'm thinking of issues like Yellow-rumped Warbler, motions that we have voted on multiple occasions. While not pertinent here, is SACC considering all of these motions too? Sometimes these issues seem more germane to SACC than NACC, but obviously not in this case.

YES, barely. I think I voted No in 2016, but other comments are very important here: slight changes in bill morphology in related species have led to the incredible radiation of galapagos finches. On oceanic islands like the Caymans, at comparatively long distances from source areas, gene flow would be very low with the nominate subspecies. Together with differences in female plumage and song, I think that the morphological differences may be reproductively isolating.

YES. I also voted for this split in 2016. Hopefully, we have progressed about song differences in oscines being irrelevant. The differences in several aspects of the song alone are sufficient reason to place burden-of-proof on treatment as conspecific. No, the data are not as voluminous or rigorous as we might want, but they are sufficient, in my opinion, to return to earlier classifications. How in the world can Hellmayr's eclectic, vague, antiquated evaluation of differences be the only evidence we have for treating them as conspecific? That's embarrassing.

As for needing genetic data before making a decision, how would genetic data inform such a decision on species limits in allotaxa? If two taxa differ phenotypically, then the default option would be to assume that there are underlying genetic differences. How different do two populations have to be genetically to be treated as separate species? That question itself is pretty silly, because one genetic difference in a key character that produced an isolating mechanism would be sufficient, whereas thousands of differences at neutral loci mean nothing. Using the degree of overall genetic variation as a metric assumes that accumulation of those changes that are barriers to gene flow accumulate at roughly the same rate as neutral changes. We don't yet have the data to test this assumption. We don't even know whether the genes we can sample represent a random sample of all genes.

YES. Difficult case with substantial phenotypic differences among the putative allopatric taxa. I went back and forth on this multiple times and initially voted no, but find the other committee members' arguments to recognize *taylori* convincing upon revisiting this proposal. Genetic data would be nice to characterize how much divergence exists between these forms, but the phenotypic differences in bill morphology, female plumage, and vocalizations are certainly striking.

YES. Both taxa have enough differences in morphology, songs, and distribution (allopatric) to promote speciation between the islands. It would be desirable to have the genetic data but generally, when there is a differentiation in the vocalizations it has genetic differences, and these genetic differences should not be very deep. In the case of the islands, the isolation between them has played an essential role in the speciation process.

YES. I've long thought this should be split and voted for it the first time around. I've seen *taylori* on Grand Cayman (though not the nominate, not having been to Cuba). It's got a truly impressive beak, and the female plumage is distinctly different, unlike the female nominate. I like the comparison with St. Kitts Bullfinch and also with Darwin's finches as justification.

NO. I voted against the split in 2016, and I don't see compelling evidence to sway me toward supporting a split this time. The vocal data are most intriguing but there is substantial overlap in the characters that were quantified. Furthermore, it seems from the sonograms (and listening to the recordings) that the beginning of the song is similar between the two taxa whereas the middle to end part of the song is distinct. Studies have shown that the beginning of a song is important for species recognition while variation in terminal elements is important for individual recognition; thus, would birds from Cuba and Grand Cayman respond differently to songs of the two taxa? The proposal also mentions dialects on Cuba, so I think we need a better understanding of the vocal variation within versus between populations. Finally, the proposal states that "whether the vocal differences, if consistent, would serve as a reproductive isolating mechanism is not known." Playback experiments that test the responses of birds to songs of different populations would help shed light on that question.

NO. These still seem to be subspecies-level differences to me. I'll note that Hellmayr's (1938) lumping of them roughly coincided with adoption of the modern biological species concept. So the rationale to me looks like part of this conceptual change. Later: Upon revisiting this issue in April 2023, I retain my vote of no. These are subspecies-level differences under the BSC, and we know that we are oversplitting allopatric avian taxa in general (Hudson & Price 2014). The politics of differing taxonomic lists are not a relevant issue to me. When we are trying to force the products of a continuous divergence process into discrete taxonomic bins, not only are individual committees going to make some wrong calls (particularly in the gray areas), differences among committees will ensure that both correct and incorrect treatments will occur. I don't find that to be a weakness. Disagreements among authoritative works are a hallmark of a science in which much remains unknown.

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. This is a borderline case and a thoughtful decision. There are phenotypic (bill depth and plumage coloration) and vocal differences between two taxa, *nigra* and *taylori*. Furthermore, there is a lack of evidence behind the lump of the two taxa. However, the available evidence is insufficient to delimit biological species. Quantitative vocal data and playback experiments are necessary to evaluate the split.

2023-A-14

Revise the taxonomy of *Psittacara holochlorus* (Green Parakeet): (a) split *P. rubritorquis* (Red-throated Parakeet) from *P. holochlorus*; (b) lump *P. strenuus* (Pacific Parakeet) with *P. holochlorus*; (c) reconsider the split of *P. brevipes* (Socorro Parakeet)

NO to (a) and (c), **YES** to (b). Excellent and detailed proposal. (a) Do not split *rubritorquis* – evidence of hybridization; in addition, the difference in red throat feathers is not straightforward. (b) Lump *strenuus*. I'm on the fence about this one. There is at least some evidence for hybridization and UCE phylogeny shows close relationship between *strennuus* and *rubritorquis*. (c) Do not lump *brevipes* at this time. In the UCE trees, support uniting *brevipes* with *rubritorquis* and *strenuus* to the exclusion of *holochlorus* is not strong. Many of the UCE trees in the supplemental material contrast with the timetree and show *brevipes* well outside of this clade. Moreover, *brevipes* is distinct in many ways, as indicated in this proposal and in 2019-B-6. Update: After reading the revised proposal, my original votes remain, which would treat *rubrirostris*, *holochlorus*, and *strenuus* as one taxa and *brevipes* as another .

NO. I agree with the proposal that further study is needed on this group before making any of these changes - in particular, genomic data with more sampling focused on this group, quantitative vocal analyses of all relevant taxa, and an assessment of gene flow in putative areas of contact.

NO. (a) *holochlorus* and *rubritorquis* are sister taxa as confirmed with UCEs in the new Smith et al. (2022) study. The phenotypic differences between them seem slight (overlapping in morphometrics, similar vocalizations, similar habitat). Although the red-throated individuals are distinct and there appears to be some sorting by microhabitat / flocking behaviors as alluded to by Thurber et al. (1987) on coffee plantations, there seem to be intermediate phenotypes as well. Given the data we have on hand, I think it is best to consider these as conspecific subspecies. (b) It seems premature to lump *strenuus* with *holochlorus* without any genetic data. As far as I can tell, *strenuus* has not yet been included in any phylogenetic studies. From a phenotypic perspective, although the sample sizes are small, *strenuus* does seem to cluster in morphometric space away from *rubritorquis* and *holochlorus*, which also seems consistent with earlier observations by Forshaw (1973) that *strenuus* differs morphologically. For now, I would continue to treat *strenuus* as separate from *holochlorus*. (c) I agree that additional information would be best, especially given that there is a group that is actively working on this complex and we may shortly get more information. UPDATE: After reading the updated proposal including the UCE tree from Smith et al. (2022), I still think the best option is a no vote on each item.

NO. (a) There is not enough evidence of the split, we need a specific study. (b) Again, there is not enough evidence of the lump.

NO. (a) A phylogeographic study with thorough geographic sampling, including putative hybrid/intermediate zones (especially in western Guatemala, where individuals may have light gray eyering like *strenuus* and red throat and breast feathers like *rubritorquis*), is required to assess species limits in this complex. Quantitative vocal and coloration analyses would also be helpful. (b) I agree with the proposal, further study is needed before making a decision. Quantifying gene flow between *strenuus* – *holochlorus* and *strenuus* – *rubritorquis* is essential. (c) Additional genetic, vocal, and morphological study of this *Psittacara* (*brevipes* – *rubritorquis* – *holochlorus* – *strenuus*) complex, including multiple samples by taxa and thorough geographic sampling, is required to revise the taxonomy.

NO. Reasons are stated in the proposal.

NO. (a) I went into this proposal thinking I would favor splitting *rubritorquis*, but after reading the proposal, especially the discussion of the variation in the extent of the red throat patch, from just a few red feathers, as in *holochlorus*, to extensive red throat, has me thinking that *rubritorquis* is better treated as a subspecies, and may actually just represent a cline, especially as red was described as being more extensive in the southern portion of its range. Further, the lack of apparent vocal differences, and extremely close sister relationship with *holochlorus* has me inclined to wait until there is more solid evidence for a split. (b) Despite some occasional mixed pairs being found in some photos, the apparent sympatry with limited interbreeding noted, as well as the morphological, vocal, and ecological differences seem to support *strenuus* as a separate species. Addendum: (a) I am definitely tempted to split out this taxon after seeing the UCE tree, which shows it as embedded within this clade of taxa that we mostly recognize as species, but given that the tree is based on one individual per taxa, and the uncertainty noted above, I am not ready to split this taxon yet. (b) While the paraphyly of *strenuus* with respect to the other taxa gives me some pause, and suggests some change in taxonomy is needed, I think the ecological and morphological differences of *strenuus* are strong enough to suggest that this is a good species, and that further work is needed to clarify the population-level relationships of these groups (c) While the paraphyly is troubling, I think the weight of other evidence is strong, and the paraphyly is based on a single specimen of each taxa, so I would like to see a full population-level study to better understand patterns of this genus. Further, that an island taxon is paraphyletic with respect to mainland taxa is not in itself unusual, and in this case, does not necessarily warrant change.

NO. Both situations need more data before changing the status quo. The indications of hybridization need quantification. Genomic work would be highly informative. In examining the revised proposal, I vote no. This new work is a nice addition, but I would urge extreme caution in considering branch lengths here for species delimitation. They are strongly affected by effective population size and isolation or gene flow. LATER: The addition of the Komar 2021 components of the proposal does not change my mind. I am still no on a-c.

NO. An excellent proposal and convincing for maintaining the status quo in each case. It appears that our taxonomic treatment in this complex is complex for the reasons articulated.

NO. (a) These *Psittacara* parakeets show complex patterns of plumage variation, vocalizations, and genetics. A thorough analysis of all three (plumage variation, vocalizations, and genetics) is necessary to figure out where to draw species boundaries.The UCE data don't really add too much, as the results can be used to lump the entire complex, because of shortish branch lengths and monophyly, or to split all four taxa, because of paraphyly. I would not want to base the taxonomy on an analysis that used only one sample per taxon. (b) Before changing the species boundaries in this group , we need to have a better idea about gene flow, plumage, soft part, and size variation, and reproductive isolation. (c) Although the UCE data make our current taxonomy of the group paraphyletic, I think it is premature to upend our vote from 2019, especially given that the UCE data only used one sample per taxon, and that Smith et al. may be doing a more detailed analysis in the future.

NO. Well-researched proposal in a complex, messy group; in my opinion, the proposal could be turned easily into a mini-review publication of the taxonomic status of these forms. After reading all this, I conclude that the *holochlorus-rubritorquis-strenuus* situation is one of the messiest and most perplexing problems in species limits in Middle America. (a) All signs point towards an absence of barriers to free gene flow between *holochlorus* and *rubritorquis.* As noted in the proposal, a lot of additional data would be required to change the status quo classification, e.g., documentation of consistent differences in flight calls. Differences in degree of red feathering on green parrots has been known for more than a century to be problematic in assessment of taxon rank, as noted in the Griscom reference in the proposal. Sympatry in parrots must be studied with respect to mated pairs in breeding season; mixing at roosts is not sympatry. What is needed to change the status quo is a field study in the putative contact zones or, minimally, quantitative comparisons of flight calls. (b) The qualitative evidence so far suggests *strenuus* might have a better chance of being a BSC species than *rubritorquis*. Regardless, evidence presented is insufficient to make a change from the current classification. (c) Insufficient evidence for change from current classification. Supporting our current treatment of *brevipes* as a species Steve Howell (pers. comm.) assures me that the vocalizations of *brevipes* are so distinctive that one would have to be deaf not to appreciate them.

3/6/23 update: No changes in all my NO votes. That one taxon nests primarily in cliffs and the other in trees may simply reflect on what sort of cavities are available. My impression is that secondary-cavity nesters are highly flexible in what sort of cavity they will use … or at least I consider burden-of-proof to be for the case that such a difference is associated with species-level differences, especially in parrots.

2023-A-15
Treat *Eupsittula astec* as a separate species from *E. nana* (Olive-throated Parakeet)

YES. While they do look pretty similar, the differences are significant enough that even Peters and almost everyone else, including Parkes, kept them as separate species. Jamaica shares practically nothing with mainland Mexico and Central America otherwise. In examining photos, it is pretty easy to distinguish Jamaican birds from Mesoamerican birds by the larger, whiter bill of *nana*, and I think they differ from each other by a degree commensurate with some other present and former *Aratinga*. The degree of mtDNA differentiation can be interpreted either for or against species status, but in my opinion the burden of proof should be on continuing to treat them as conspecific, in the absence of published evidence for lumping beyond that they are generally similar.

YES. Reasons outlined in the proposal. This is definitely a gray area and certainly represents a judgment call, but I think the morphometric differences, combined with the relatively strong genetic differences (though only investigated in mtDNA), as well as the seemingly unjustified lump to begin with, I favor recognizing *astec* and *nana* as separate species.

YES. Biogeographically this makes sense, given that most bird taxa in Jamaica are considered endemic (or at least Caribbean endemic) species. I don’t think there are other bird species that share this distribution of *nana sensu lato* (mainland Mexico and Jamaica, without occurring on other Caribbean islands). The plumage differences are minor, but the greater amount of blue in the wings of *nana* can be seen in photos and the bill does look larger for *nana* in photos. The genetic differences, though not great, are about what would be expected between an island endemic and mainland species that have ceased gene flow.

YES. There was little reasoning presented to lump them in the first place, and there are plumage, genetic, and structural (bill size/shape) differences that suggest separate species. The photos are also compelling, as is the biogeographic argument regarding Mexico and Jamaica. In cases like this, when the original decision was based on little/no evidence, I don't think the null hypothesis should just be the status quo.

NO. This is a borderline case where there was no clear rationale for lumping these two taxa, and no modern comprehensive study to support them as separate species. My inclination for now is to retain the current treatment pending additional study.

NO. Although the motivation for lumping them back in the 70s remains unclear, the current evidence is not sufficient to split them in my opinion. As noted in the proposal, completely allopatric and especially island taxa are difficult to assess with respect to reproductive isolation, but this is a pretty shallow mtDNA divergence. We don't yet know what the nuDNA shows, but my guess is it will be consistent with mtDNA. Differences in plumage are slight, and although the vocalizations do seem to be slightly different to my ear and while eyeballing the spectrograms, a quantitative analysis would be much more preferable.

NO. There is still a possibility that *astec* or *nana* could be more closely related to another species, so I want to be conservative about this.

NO. This is a borderline case. Although *nana* and *astec* could be two separate species, current evidence does not support a split. Genetic analysis including representatives from the different taxa within the genus *Eupsittula* and quantitative vocal analysis between *nana* and *astec* are highly recommended.

NO. I can't support this split, which presently seems mostly based on the Tobias et al. (2010) criteria (which were developed for passerines) and an absence of good recent analyses examining species limits in this group in a comparative framework. This situation looks like it's in the gray area between subspecies and species and requires more careful study.

NO. I originally voted yes but after reading other detailed comments, I'm more on the fence. Moreover, I am uneasy about having the status quo changed based on sharply diverging opinions within NACC. OK, there was a published rationale for the lump, although it still strikes me as a borderline judgment call. The sharing of avifauna between the West Indies and mainland Middle America is limited. The back and forth discussion raised valid points.I would still like to see an analysis on why the situation between the Cuban and Hispaniolan Parakeets is that different from this case, although I haven't taken the time to look carefully at it. There is little sharing between Hispaniola and Cuba, unlike Cuba and the Bahamas where there is a shallow bank connecting them. Really, I would rather vote "pend" and perhaps revisit later in the month when I have more time. I have a hard time wondering how come I can't see the streaks in the photos that are evident in the specimens. Preparing spectrograms from the calls shouldn't be too hard, although awaiting more complete genetic studies may be many years away. Ideally it should be after more complete genetic studies are done. I suppose no is the best vote for now. Yes, the illustrations are highly misleading, really just botched. What's with all that yellow?! Here (and elsewhere) I'm anxious to see WGAC comments when they are ready. I have gone through other West Indian references and don't see much of a clamor to re-split these two unlike in some other cases of earlier lumping.

For what it's worth, I looked at Birds of Jamaica, a Photographic Field Guide by Audrey Downer and Robert Sutton (published in 1990 by Cambridge University Press). They have a flight shot of *A. nana.* I can discern streaks in the throat and elsewhere on the underparts, including the underwing coverts. I don't consider this difference minor. As indicated, I'm on the fence.

NO. In my opinion, the evidence presented affirms their continued treatment as subspecies and provides absolutely no evidence for species rank. Here is my dissection of the evidence:

Voice: No one has ever claimed that they differ vocally. This alone in my opinion is sufficient evidence itself against species rank. Taxa treated as species in New World parrots typically show diagnostic differences in flight calls – see my comments under A-17.

Morphology: The Tobias et al. magic threshold of 7 points is achieved only by virtue of treating the differences in bill and body size as worth 7 points; otherwise, it falls short. We can argue whether bill and body size differences should be used in assessing species rank; all I will say is that both features show substantial within-species variation and that they are two of the most labile characters in birds. Just as one example, male and female Western Sandpipers show bigger bill shape differences than do these two parrot taxa, and perhaps also even in body size. In the case of these two parakeets, the Magic 7 threshold would also fall short without the subtle differences in wing color being awarded 3 points. And just as a general reminder, the Magic 7 threshold was built on a model based on comparisons of 58 pairs of taxa that have an extraordinary phylogenetic skew, e.g., almost all were temperate passerines or 11 pairs of antbirds; taxon pairs from only three non-passerine orders were used (3 of 58 comparisons), none of which are Psittaciformes; see Remsen 2016 (JFO) for full details under "Phylogeny-free comparisons". Whether such a scheme can be applied to parrots is open to question. Whether it can be applied as the only "data" to rank these two parakeet taxa as separate species requires special pleading. Further, let's take a direct look at the two taxa in question. If one were to use the illustrations in del Hoyo and Collar (2014), where *astec* is shown as basically yellow below, one would be impressed with the differences in ventral coloration and even shape. But here's what they really look like, taken from photos in eBird (and feel free to browse more photos to be sure I haven't been biased in my selection):

This is *astec*, taken by Jesse Stuebner in Nicaragua:
https://search.macaulaylibrary.org/catalog?assetId=33015151

And this is *nana*, taken by W.S. Barbour in Jamaica:
https://search.macaulaylibrary.org/catalog?assetId=203099661

Are you impressed with the differences? Could you tell which one was which without the captions? Maybe my eyes are going bad, but I really don't see much in the way of difference between them. Even if someone had asked me which one was yellowest before, I'm not sure I could be certain. One thing is for sure: the illustration in del Hoyo and Collar (2014) is wildly inaccurate and misleading:

Some voters mentioned that there were inadequate reasons for the lump by Forshaw and subsequent authors. I disagree – I haven't read the Marien and Koopman (1951) paper in great detail (citation missing from the proposal – here is a pdf: https://digitallibrary.amnh.org/bitstream/handle/2246/5353/N1712.pdf?sequence=1), but I agree with their assessment that the differences in plumage between *astec* and *nana* are a notch lower than other taxa treated as species in the *Eupsittula*-*Aratinga* parakeets. That they included comparisons slightly beyond *Eupsittula* as currently defined is not a problem in my opinion – they are still members of the same tribe (Arini). In contrast to many Peters-era lumps, we have explicit, published rationale, albeit far short of what would be considered sufficient evidence in 2022. Here is a quote:

> "The Jamaica form is larger than *A. astec*, somewhat darker, and tends to lack the yellow feathering on the cere usually present on the mainland form. However, many specimens of *A. nana* do show traces of such yellow feathering."

Genetic data: In my opinion, they provide no evidence one way or another, or if anything, support subspecies rank. If *nana* has diverged from the mainland group to the point of showing some minor plumage differences, then it is no surprise that they are also genetically distinguishable. Was someone expecting that they would not differ genetically? Note that the % sequence divergence (ca. 1.8%) is more typical of subspecies than species-level differences. I personally would never use genetic distance as a metric for species rank in allotaxa, but by some standards, this degree of differentiation falls with the subspecies category, not species. That they are "*reciprocally monophyletic*" requires an added qualifier "*with respect to a sample of 9 and 7 individuals at # mitochondrial loci*". Reciprocal monophyly based on small N should be interpreted with caution and not as an absolute statement. Even so, using "reciprocal monophyly" to support species rank based on a few mtDNA loci is highly questionable in my opinion, and with 650 km of ocean separating Jamaican from mainland populations acting as a barrier to gene flow, reciprocal monophyly at rapidly evolving neutral loci is expected. The low degree of genetic divergence suggests to me that *nana* must be a relatively recent colonist (or possibly even an introduction by early civilizations?). Marien and Koopman came to the same conclusion of recent colonization based on the minor plumage differences:

> "That this event might have taken place relatively recently is indicated primarily by the fact that *A. nana* is so little different from *A. astec*."

In summary, I see no evidence for species rank but plenty of evidence consistent with continued ranking of the two as subspecies of the same species based on lack of documented vocal differences, minor phenotypic differences, and low degree of genetic divergence.

As an aside, the presence of *astec* only on Jamaica is almost certainly due to the presence on other islands of the Greater Antilles of other *Aratinga* (sensu lato) parakeets: *euops* on Cuba, *chloroptera* on Hispaniola. and *canicularis* on Puerto Rico. Evidently, there is only "room" for one parakeet of this size on each island. The somewhat anomalous distribution pattern of *A. nana* might be consistent with the well-known ability of parrots to colonize oceanic islands, even remote ones, so they may not conform to biogeographic patterns shown by most birds.

2023-A-16
Treat *Amazona guatemalae* as a separate species from *A. farinosa* (Mealy Parrot)

NO. This proposal summarizes the issues nicely, but I think it is premature to split these taxa given the lack of clear vocal differences and the need for further study in the putative contact zone.

NO. Reasons are given in the proposal.

NO. There is not enough evidence, even when the mitochondrial DNA has a substantial divergence (3.5-5.4%)

NO, not without a study justifying the split. Perusal of eBird photos from Panama shows that the characters of some birds in e.g. the Canal Zone and Darien do not fit nicely into the two types, suggesting intergradation over a rather broad area.

NO. Although mitochondrial DNA shows divergence (3.5–5.4%), *guatemalae* and *farinosa* are vocally overlapped. To assess the species status of *guatemalae* and *farinosa*, we need to understand the phylogenetic relationships of the taxa, as well as putative contact zones and gene flow. Also, I would like to see a quantitative vocal analysis where we can assess how different *guatemalae* and *farinosa* are, compared to other closely related species.

NO. This is a borderline case for me, and I could be convinced to vote yes, but for the time being, based on the very slight plumage and morphometric differences (some of which seem variable), as well as a lack of vocal differences between northern and southern groups, I think these are best treated as conspecific. The very strong mtDNA divergence is intriguing, and I would be especially curious for denser sampling from the area of possible contact.

NO. The molecular evidence is driven by mtDNA signal and is therefore not informative about species limits. An uncharacterized contact zone for taxa that are phenotypically so similar also suggests retaining current taxonomy.

NO. We would be remiss to split without a detailed study from the area of sympatry or parapatry in central Panama and that has not been done. Additional genetic studies would be helpful too. The coloration differences seem minor to me.

NO. Nice rigorous proposal. I agree with the recommendation of the proposal authors.

With such potential close contact, it would be helpful to have a better idea of what is happening where these taxa approach one another. It appears that we do not even know if they are allopatric. Mealy Parrots are incredibly loud and vocal and I would expect that reproductively isolated populations would have some vocal differences.

NO. However, to me this is more borderline than presented in the proposal and reflected in other committee member's comments. I'm intrigued by the relatively deep divergence in mtDNA between the two forms. Although many committee members dismiss mtDNA outright, I think that's a mistake. It can provide at least some information to evaluate. If there was widespread gene flow, you wouldn't expect that much structure in the tree and so many discrete differences. Also, the genetic sampling seems at least reasonably close to where the two forms come into contact. The proposal describes the plumage differences as "slight"; I wouldn't have characterized them in that way at first. However, it's obvious to me now after reading the proposal, and looking at photos, that many of these plumage differences aren't diagnostic. Given that, and the lack of vocal differences, I'm fine with treating these as conspecific at least for now.

NO. An easy one. As outlined in the proposal, there are no data that support treatment of *guatemalae* as a separate species. (The proposal is excellent, by the way, and the authors should consider publishing this as a note in BBOC or Ornitología Neotropical or another journal).

2023-A-17
Treat *Amazona tresmariae* as a separate species from *A. oratrix* (Yellow-headed Parrot)

YES. These two taxa have marked differences in morphology, genetics, and vocalizations.

YES. Initially I was dubious but have become convinced by the congruence of morphology, voice, and mtDNA.

YES. This is another borderline case for me, but the interesting pattern of paraphyly in this species complex for *tresmariae* is very interesting, and possibly suggests further revision of the Yellow-headed Parrot complex is warranted. My main concern isn't as much that these phylogenies are based on mtDNA, it's that they are all based on the same samples and same mtDNA markers, so the fact that they all agree is entirely expected. In addition to the inclusion of nuDNA markers, I would also like to see additional individuals of all of these taxa added to the phylogeny.

YES. Nice proposal and I like having varying recommendations. A geographically complex taxon (*sensu lato*), with rather surprising genetic results, even if they are "only" mtDNA. The degree of plumage difference is not as much as between *auropalliata* and *oratrix*, but still substantial (equal to difference between *oratrix* and *ochrocephala*).

YES. Differences in genetics, plumage, size. Multiple individuals of different subspecies have been sampled for mtDNA and show that *A. oratrix* is paraphyletic, with *A. o. tresmariae* sister to a clade with *A. auropalliata*, *A. o. oratrix* and *A. o. belizensis*. In addition, the branch leading to *tresmariae* is long. Although monophly and long branches are expected in small, island populations, that doesn't negate the fact that a longer branch indicates more evolution and monophyly indicates divergence. The UCE data are nice to have, but that data set is missing relevant taxa and doesn't sample multiple individuals. Also, the branch connecting *A. auropalliata* to the clade containing *A. oratrix* and *A. tresmariae* has low support so the UCE topology is still potentially consistent with the paraphyly seen in the mtDNA tree.

NO. Although there is some evidence supporting recognition of *tresmariae* as a separate species, I would prefer to see additional corroboration with better sampling of this group for genomic data as well as a quantitative study of the supposed vocal differences that includes sampling of all relevant taxa.

NO. I had originally voted yes on this proposal, due in large part to the paraphyly revealed by mtDNA analyses by Chaves et al. (2014) and Eberhard and Bermingham (2004). New data from the Smith et al. (2022) paper do not recover a paraphyletic *oratrix.* Rather, *oratrix* and *tresmariae* are sister to each other based on UCEs. My assessment of vocal differences was subjective. There still are differences in size and plumage, but given that these are each other's closest relatives based on the current data at hand, I think they are better treated as conspecific.

NO. Published information recognizes *oratrix* and *tresmariae* as sister taxa but genetic, vocal, and morphological evidence supports them as two subspecies and not as separate species.

NO. mtDNA is not sufficient for determining species limits. Insular populations being larger than mainland relatives is well known, as is genetic distinctiveness. Alone, they are not diagnostic of species limits. A decent subsampling of the nuclear genome would be useful, as would examination of these differences in a broader comparative framework. Diagnosability alone is not an effective criterion for determining biological species limits. After examining the revised proposal, I still vote no, recognizing that branch lengths in UCE trees are strongly affected by effective population size and isolation (or lack thereof) and thus have deeply uncertain value in species delimitation.

NO. I do not know these species at all, but the Tres Marias Islands seem to be an island group with significant endemism. Whether to split, or not, is, perhaps, a matter of taste. Or put another way, I'm not sure how much more research would clarify the situation. I started looking at other members of the complex, notably with Yellow-crowned (*A. ochrocephala*) and Yellow-naped (*A. auropalliata*) and see that they are lumped by Valley and Dyer (2018) along with (at least) the subspecies *belizensis* of *A. oratrix.* I commend others for listening to recordings; I have not.

NO. Well-researched proposal that I think should be published as a note somewhere. However, unless we want to start assigning species rank to separate mtDNA clusters, there is no evidence that *tresmariae* should be treated as a separate species. Here are my main points:

1. The only reason we are considering this is that the IOC followed the Navarro-Sigüenza-Peterson PSC list on Mexican birds on this one. This taxon doesn't even meet the BLI-HBW criteria for species rank based on plumage and morphology, which is very easy to do for parrots because of their complex plumage. Neither the original description nor any subsequent authority has treated it as a separate species despite species tending to be more narrowly defined until Peters. The differences in morphology between *tresmariae* and the rest of the complex have never been considered as evidence for anything more than subspecies rank, from Nelson's description in 1900 to the recent BLI-HBW assessment. Therefore, a conservative approach is warranted for any change.

2. Genetics. As noted by others, the genetic data are all mtDNA. So, we're dealing with mtDNA gene trees, which I thought we were long past considering as sufficient evidence for species rank. Assessments of monophyly and paraphyly are inappropriate, in my opinion, when based on gene trees, especially mtDNA and especially with low N. That the insular, isolated *tresmariae* forms a separate group from the parapatric mainland taxa is predicted by that isolation – the only surprise would have been a result to the contrary.

3. Vocalizations. Do they really differ? Certainly, we have no data to support this, only assertions. I listened to almost all the Macaulay recordings. I cannot see or hear any differences in pitch between, for example ML228923 (*tresmariae*) and ML35283611 (*oratrix*) – in fact, they could be the same individual bird. Variation among recordings is large by any standards, and quantitative analysis by homologous vocalizations would seem to me to be necessary before having any confidence in the alleged differences.

I really think it is a mistake for us to rank *tresmariae* as a species based on the data published so far, to the point that we will have opened NACC to intense and deserved criticism. Fiddling around with species limits in this group of parrots without a solid backbone of data is unwise. The message I get from the mtDNA trees is that species limits in the *ochrocephala* complex, including taxa long ranked as species such as *aestiva* and *barbadensis*, are in drastic need of a thorough revision based on additional genetic sampling and an actual study of vocalizations. Flight calls in parrots are a strong indicator of species limits; this is one of the main themes of the compilation of recordings of parrot voices and the accompanying booklet by some of the world's experts on vocalizations: Whitney, B. M., T. A. Parker, III, G. F. Budney, C. A. Munn, & J. W. Bradbury. 2002. Voices of New World parrots. 3-CD audio guide and booklet. Cornell Laboratory of Ornithology, Ithaca, New York. For one example within *Amazona*, the recently described species *A. kawalli* Grantsau and Camargo, 1989, differs strongly in voice from its sister species *A. farinosa*. Although originally detected from differences in plumage and structure in study skins, once its distinctiveness was recognized, consistent vocal differences were noted … to the point that you can distinguish them a long way away (Bret Whitney, pers .comm.).

More broadly, I question species limits in this entire complex — it is badly in need of a detailed view using modern genetic data and quantitative study of vocalizations. In parrots, voice matters! Difficult to study because of the variability, but flight calls are much less variable and typically diagnostic for species limits. Plumage pigmentation, in contrast, shows much individual variation, at least in the yellow-orange-red range, and for some species is known to be influenced by diet.

Addendum based on Smith et al.: The only sample of *oratrix* included in their analysis was from Veracruz on the Caribbean side of Mexico, hence *A. o. magna*; thus, even if one were to use comparative branch lengths, the critical taxon, nominate *oratrix* from the Pacific slope of Mexico (the likely source population for *tresmariae*) is missing.

So, I'm trying to phrase in my mind how a Supplement statement supporting would read if this proposal actually passes, and here's what I come up with:

> "The subspecies *tresmariae* is treated as a separate species from *Amazona oratrix* based on plumage (despite the differences not meeting even Tobias et al. criteria), vocalizations (despite lack of published, quantitative analysis, and some individuals in published recordings not being distinguishable), and genetic data (actually, only comparative branch lengths in a tree that is missing the taxon for which branch length would matter the most: nominate *oratrix* from western Mexico)."

In other words, I see no solid evidence under a BSC framework for ranking allotaxa as species for *tresmariae* to be ranked as a species. That framework consists of a yardstick comparative approach in which the population in question has diverged sufficiently that we predict that there would no longer be free gene flow if there was direct contact. In terms of morphological divergence, *tresmariae* falls short … or at least would require a formal comparative analysis to be interpreted otherwise. In terms of vocal divergence, *tresmariae* again falls far short, at least in terms of published analyses; differences in parrot flight calls, including within *Amazona*, are known to be associated with species limits in almost all cases. In terms of genetic divergence at neutral loci, whether this can be used in a BSC framework is debatable in my opinion because of the extreme variance in genetic distance among species pairs of birds known to be reproductively isolated. If 1.8% sequence divergence were applied across the board in birds, many thousands of tropical populations would be elevated to species rank even though in many cases there are no known phenotypic differences.

As a reminder, NACC does follow the BSC, not the ESC, PSC, GLSC, or anything else, and so even if some members favor one of those alternatives, they are asked to evaluate proposals under a BSC framework.

NO. Reasons are stated in the proposal recommendation.