2023-B-1: Transfer White-bellied Mountain-gem *Lampornis hemileucus* to the monotypic genus *Prodosia*

2023-B-2: Transfer subspecies *minor* (and extralimital subspecies *cinerascens*) from *Myiodynastes chrysocephalus* to *M. hemichrysus*, thereby removing *M. chrysocephalus* from the Checklist

2023-B-3a: Modify the classification of the Rallidae: transfer *Micropygia schomburgkii* to *Rufirallus*

2023-B-3b: Modify the classification of the Rallidae: transfer *Neocrex erythrops* and *N. colombianus* to *Mustelirallus*

2023-B-3c: Modify the classification of the Rallidae: transfer *Cyanolimnas cerverai* to *Mustelirallus* or *Neocrex*

2023-B-3d: Modify the classification of the Rallidae: slightly alter the linear sequence

2023-B-4: Treat *Poliocrania maculifer* as a separate species from Chestnut-backed Antbird *P. exsul*

2023-B-5a: Split *Xiphorhynchus aequatorialis* from Spotted Woodcreeper *X. erythropygius*: Elevate *aequatorialis* (with *punctigula* and *insolitus*) to species rank (BirdLife treatment)

2023-B-5b: Split *Xiphorhynchus aequatorialis* from Spotted Woodcreeper *X. erythropygius*: Elevate both *punctigula* (with *insolitus*) and *aequatorialis* to species rank

2023-B-6a: Revise the taxonomy of *Himantopus mexicanus* (Black-necked Stilt): treat it as a subspecies of *H. himantopus* (Black-winged Stilt)

2023-B-6b: Revise the taxonomy of *Himantopus mexicanus* (Black-necked Stilt): treat current subspecies (i) *melanurus* and (ii) *knudseni* as separate species

2023-B-7a: Treat *Chlorophonia sclateri*, *C. flavifrons*, or both as separate species from Antillean Euphonia *C. musica*: treat *C. sclateri* as a separate species from *C. musica*, retaining *flavifrons* as a subspecies of *C. musica* (two-species treatment, version 1)

2023-B-7b: Treat *Chlorophonia sclateri*, *C. flavifrons*, or both as separate species from Antillean Euphonia *C. musica*: treat *C. flavifrons* as a separate species from *C. musica*, retaining *sclateri* as a subspecies of *C. musica* (two-species treatment, version 2)

2023-B-7c: Treat *Chlorophonia sclateri*, *C. flavifrons*, or both as separate species from Antillean Euphonia *C. musica*: treat both *C. sclateri* and *C. flavifrons* as separate species (three-species treatment)

2023-B-8: Treat *Corvus minutus* as a separate species from Palm Crow *C. palmarum*

2023-B-9: Treat *Cyanocorax luxuosus* as a separate species from Green Jay *C. yncas*

2023-B-10: Transfer Tiny Hawk *Accipiter superciliosus* to the newly described genus

2023-B-1:

Transfer White-bellied Mountain-gem *Lampornis hemileucus* to the monotypic genus *Prodosia*

This proposal was rejected due to the name *Prodosia* Simon, 1919, being preoccupied by *Prosodia* Dyer, 1914, a genus of moth.

2023-B-2:

Transfer subspecies *minor* (and extralimital subspecies *cinerascens*) from *Myiodynastes chrysocephalus* to *M. hemichrysus*, thereby removing *M. chrysocephalus* from the Checklist

YES. Similarities in the vocalizations of *minor, cinerascens, and hemichrysus* (especially in the dawn song) compared to *chrysocephalus* support this change in treatment.

YES. Vocal analysis supports the transfer of *minor* and *cinerascens* from *M. chrysocephalus* to *M. hemichrysus*, even when vocalizations do not correspond to plumage-based grouping. However, as stated in the proposal, vocalizations are a primary indicator of species boundaries and affinities in suboscine birds.

YES. The morphological differences justifying the previous taxonomic arrangement seem pretty minor, and there are others that support this revised grouping, as outlined by HBW/BLI. The vocal differences between *hemichrysus* and *chrysocephalus* and similarities within the expanded *hemichrysus*, on the other hand, strongly indicate which taxa belong together in these suboscines.

YES. I agree with the proposal: vocalizations are innate and primary indicators of species limits and affinities in suboscine birds, and in this case they demonstrate that the traditional allocation of subspecies into the complex was in error. The vocal analysis is good enough to remove *M. chrysocephalus* from the Checklist and transfer subspecies minor from *M. chrysocephalus* to *M. hemichrysus*.

YES. Of course we would prefer a peer-reviewed paper on those vocalizations, but I think the differences shown by Boesman, and consistent with those mentioned by Schulenberg et al., shift the burden of proof to justifying maintaining the current treatment. Voice is everything in cases of species limits in parapatric and sympatric tyrannids, and so I think we are on solid ground to revise our classification based on the latest information on voice.

YES. In this case (suboscine tyrannids) vocalizations override plumage in consideration of where to draw species boundaries. A genetic analysis would be nice corroboration, but one could argue that vocalizations would still override the genetic results if those showed that *minor* and *cinerascens* were closer to *chrysocephalus* than to *hemichrysus*.

YES. This is a weak vote, however, and ideally I would like to see genetic evidence linking *minor* and *cinerascens* with *hemichrysus*, but the vocal data does seem very strong in this case, and regardless they do not belong with *chrysocephalus*.

YES. The vocal data are convincing, showing that prior plumage-based evidence was wrong.

YES. Based on the vocal evidence. This will mean one fewer endemics for the Talamanca.

YES. Based on the vocal evidence, which is a generally a good indicator of evolutionary affinities (perhaps more than the minor plumage differences) in suboscines like these.

2023-B-3a:

Modify the classification of the Rallidae: transfer *Micropygia schomburgkii* to *Rufirallus*

YES. I agree with the option of transferring *Micropygia* to *Rufirallus.*

YES. Changes in the genus are necessary in the clade to which *Micropygia schomburgkii* belongs. Transferring *Micropygia schomburgkii* to the genus *Rufirallus* goes in the right direction to carry out the necessary changes.

YES. However, what about the collateral effects of this? How many extralimital, unsampled species will be caught up in this? This seems like such a heterogeneous grouping that we may not know exactly which other taxa also belong in *Rufirallus* for a long time. I don't see any obvious morphological characteristics that would give clues (although photos of *Micropygia* do recall a small, spotted *Rufirallus viridis*). And vocally they don't seem diagnosable either. But anyway, *viridis* appears to be the type species for *Rufirallus* and I agree it is better than retaining or creating multiple monotypic genera.

YES. I agree with the proposal, the results supported the modification of the classification of Rallidae.

YES. Reluctantly and only to maintain monophyly of the genera in use. What is really needed is a comprehensive phylogeny of the Laterallini if we are to start altering generic limits, as well as an overview of vocalizations. Our new *Rufirallus* could either make perfect sense from vocalizations and under-appreciated plumage similarities, or it could be a heterogeneous mess. This will depend on a time-calibrated phylogeny, and an assessment of divergence times with respect to phenotype. But for now, this proposal is consistent with available published data and is better than maintaining the status quo.

YES. I prefer an expanded Rufirallus rather than three or four monotypic genera. The latter classifications convey little to no phylogenetic information.

YES. I agree with the proposal that those three species should be placed in *Rufirallus* rather than the implementation of 3 monotypic genera.

YES. Hear, hear! on eliminating monotypic and tiny genera when possible. So yes to: *Micropygia* to *Rufirallus*; *Neocrex* to *Mustelirallus*; and the proposed sequence change.

YES. Reasons are outlined in the proposal and noting the qualified “yes” votes above, but seems the best option available for now.

YES. I also eschew monotypic genera when possible, this seems the most sensical solution to me.

YES. Makes sense based on the topology and prevents the need for more monotypic genera. I agree a more comprehensive phylogeny is needed, but that may take a while. In the meantime, we have a responsibility to provide a classification that is reflective of known evolutionary relationships.

2023-B-3b:

Modify the classification of the Rallidae: transfer *Neocrex erythrops* and *N. colombianus* to *Mustelirallus*

YES. 1 without comment.

YES. The data support transferring *Neocrex erythrops* to *Mustelirallus* (too bad *N. colombianus* wasn’t sampled), and this is consistent with the SACC treatment.

YES. Transferring *Neocrex erythrops* and *N. colombianus* to *Mustelirallus* is a reasonable change following what the phylogeny suggests as well as the SACC treatment.

YES. This seems mandated by the phylogeny and unlikely to be problematic since *colombianus* has sometimes been considered conspecific with *erythrops* and there don’t seem to be other taxa that might also belong to the clade.

YES. This one actually makes more sense, phenotypically. Again, I look forward to a time-calibrated phylogeny and a synopsis of vocalizations in the new *Mustelirallus* group, especially with respect to the deep divergence between *albicollis* and the others.

YES. I agree with the inclusion of *Neocrex* with *Mustelirallus*, as it will also bring NACC into alignment with SACC.

YES. See comment above.

YES. Following the rationale presented in the proposal.

YES. Provides a classification consistent with phylogeny.

NO. After reading the comments of the SACC, I see that two ornithologists who know these species somewhat well feel that *Neocrex* should not be merged in *Mustelirallus.* They differ dramatically in vocalizations, and also somewhat in migration behavior and in morphology (plumage, soft parts).

2023-B-3c:

Modify the classification of the Rallidae: transfer *Cyanolimnas cerverai* to *Mustelirallus* or *Neocrex*

YES. 1 without comment.

YES. The genetic data by themselves are suggestive but insufficient. However, the sharing of phenotypic characters (red legs and red bill base) tip the scale for me in favor of this treatment.

YES. Somewhat reluctantly. This is only a small piece of DNA, and PP support values are ok, but not great. However, ML values for the position are 100. Brown et al. (2022) also make an argument for lumping into a single genus based on the comparatively old age of other rail genera.

YES. The genetic data are scarce, but given that I voted to lump *Neocrex* into *Mustelirallus*, it makes sense based on the data (genetic and phenotypic) to merge *Cyanolimnas* into *Mustelirallus* as well.

NO. Neither in *Neocrex n*or *Mustelirallus*. I guess I was the lone vote <u>not</u> to place *Neocrex* in *Mustelirallus*, so I definitely do not want to place *Cyanolimnas* there. I think that lacking one species (*N. colombiana*) of only a three-species clade, and using a part of one mtDNA gene is not solid enough evidence to make this change, especially with no vocal or behavioral data. In addition, Cuba is rife with monotypic genera, and I would like to see more data before scuttling one of these.

NO. The available evidence is insufficient to transfer *Cyanolimnas cerverai* to the genus *Mustelirallus*. Only mitochondrial data is available for *Cyanolimnas*, nuclear data must be analyzed before making any decisions. Also, red legs are present in *Mustelirallus* and its closely related genus *Pardirallus*.

2023-B-3d:

Modify the classification of the Rallidae: slightly alter the linear sequence

YES. 3 without comment.

YES. Reasons are given in the proposal.

YES. Required changes to the linear sequence.

YES. Minor sequence adjustment.

YES. I agree with the proposal.

YES. Required book-keeping based on the most recent genetic data

YES. Even if *Neocrex* is not moved into *Mustelirallus*, that clade (*Cyanolimnas, Mustelirallus, Neocrex*) still has more species (4) than *Pardirallus* (3).

YES. For the reasons stated in the proposal.

YES. Required to make a linear classification consistent with phylogeny.

2023-B-4:

Treat *Poliocrania maculifer* as a separate species from Chestnut-backed Antbird *P. exsul*

NO. I agree with the proposal that it is premature to split these taxa pending further integrative study (plumage, genomics, voice) with good geographic sampling that includes the putative contact zone.

NO. The available evidence so far is not sufficient to treat *Poliocrania maculifer* as a separate species from *P. exsul*. To consider separating *maculifer* from *exsul* requires a phylogeographic study with exhaustive sampling in the entire distribution area, including the contact zones, as well as vocal analysis.

NO. A split would be premature with the apparent intergradation, including individuals with hints of wing spots occurring into Costa Rica; similar vocalizations apparently differing only in frequency of 2- vs. 3-note songs; and no useful genetic information.

NO. I agree with the proposal, no recent data to support this separation.

NO. Resoundingly, for all the reasons given in the proposal. I basically stopped reading the proposal in detail when I came to BLI's own analysis: "BirdLife International split the *maculifer* group from the *exsul* group based on the following rationale: *P. maculifer* "[h]itherto considered conspecific with *P. exsul*, but (**although voices appear identical**) …. " [boldfacing by me]. This is a great example of the fundamental problem with the phenetic, point-based Tobias et al system. All characters are not alike. Thanks to the work of Mort and Phyllis Isler, Bret Whitney, and many others, vocal differences are known to be the key predictor of free gene flow or lack of it in parapatric and sympatric antbirds. Plumage differences are of minimal consequence as barriers to gene flow to these antbirds, so why should it make a difference to us if our criteria for species rank are rooted in cessation of free gene flow?

NO. I agree with the arguments in the proposal, that between the presence of intermediates between the taxa, and the very similar vocalizations, these do not seem like valid species, and should remain subspecies under a single broader *Poliocrania exsul*. While the contact zone does seem like it could be quite narrow, without a more detailed study of dynamics at the contact zone, the similarity of the vocalizations tips the scale on not splitting these taxa.

NO. Based on the present evidence it is baffling why there would be a movement for a split, especially given the widely acknowledged identical, or very similar, calls. There are no counter arguments that would overrule the very similar vocalizations. As always, more studies are needed.

NO. Vocalizations are so key for species recognition in the Thamnophilids that I would be very surprised if two reproductively isolated taxa occurring in parapatry did not show differences in song. The existence of a hybrid zone also indicates some amount of recent or current gene flow; for some reason hybrid zones bolster the score using Tobias criteria. A more thorough investigation of this zone would help elucidate the degree of reproductive isolation.

NO. Present data are insufficient.

2023-B-5a and 2023-B-5b:

Split *Xiphorhynchus aequatorialis* from Spotted Woodcreeper *X. erythropygius*: (a) Elevate *aequatorialis* (with *punctigula* and *insolitus*) to species rank (BirdLife treatment); (b) Elevate both *punctigula* (with *insolitus*) and *aequatorialis* to species rank

NO. Given the discrepancy between plumage and vocal data, I think it is prudent to wait for further analyses as recommended in the proposal. In particular, further analysis of vocal differences paired with genetic differences would be very helpful in resolving the taxonomic status of this complex. However, I agree with the proposal that multiple species are likely involved.

NO. As recommended in the proposal, quantitative analysis of vocal, morphological, and genetic traits, with thorough geographic representation, is necessary to assess species limits in the group.

NO. Although multiple species may be involved, I agree with the proposal that we should hold off on changing the current taxonomic treatment pending further study.

NO. Reasons are given in the proposal (which is outstanding). As noted in the proposal and by others' comments, this is a complex situation that requires a rigorous, formal, published study to change species limits, and it seems clear that changes are needed, even from the largely anecdotal information presented so far.

NO. Great proposal. Their in-depth analysis of what's known suggests that there are three vocal groups, which would explain why those of us who looked at this in much less depth earlier couldn't reconcile vocalizations with a neat and tidy two-species split. As the proposal authors indicate, this is ripe for a thorough study and, although it probably wouldn't be wrong to split *erythropygius* + *parvus* from the others and await further developments on the potential *punctigula* vs. *aequatorialis* split, it's still too unclear to do so with much confidence. So I vote no split for now, fully expecting (and hoping) that we can revisit it relatively soon on the basis of a thorough analysis.

NO. I agree with the proposal, "This complex is an excellent candidate for future work. Quantitative analysis of song, plumage, and genetic variation (the latter of which is lacking) would go a long way towards resolving species limits in the group."

NO. I agree with the well-presented proposal that although more than one species is almost certainly involved, we don't have enough data on vocalizations and genetics to draw where these splits are. We really can't do it piecemeal for the same reasons. The species, as currently defined, seems pretty rare across its range in Middle America.

NO. Current data do not seem compelling for a definitive split at this time. I agree that there might be more than one species involved, but the complex requires quite a bit more study before we could be confident in determining species limits.

NO. Reasons are stated in the proposal.

NO. The evidence does not support recognition of full biological species at this time. (And there should be some genomic data available soon that bear on this question.)

NO. I find no compelling evidence for change as the proposal outlines. I looked through Vallely and Dyer's (2018) Central American book which covers the subspecies *parvus*, *punctigula*, and *insolitus* and see that there is no mention of differences within the subspecies. They illustrate a bird from northern Central America, presumably *parvus*, and a darker bird from Costa Rica and Panama, presumably *punctigula,* or possibly *insolitus.* Nominate *erythropygius* is restricted to Mexico.

2023-B-6a and 2024-B-6b:

Revise the taxonomy of *Himantopus mexicanus* (Black-necked Stilt): (a) Treat it as a subspecies of *H. himantopus* (Black-winged Stilt); (b) Treat current subspecies (i) *melanurus* and (ii) *knudseni* as separate species

YES on (a), NO on (b). Although I am voting differently than everyone else so far on part a, I am actually in general agreement with all the comments in terms of lack of solid data. But rather than stick with our current status quo, why not just revert to previous classifications that considered them all conspecific? Yes, the data are weak, but all indications are that whenever two forms that differ in plumage are in contact, hybridization is frequent if not rampant, i.e., plumage differences seem irrelevant to gene flow. Other than plumage, all these birds are very similar vocally, ecologically, and genetically – what is the evidence that supports our current classification? Nothing, really, other than recent historical momentum. I think that by returning to a classification that treats all as subspecies (setting aside the *leucocephalus*- *novaezelandiae* situation, which is evidently more complex). *Himantopus* is clearly one of the world's great dispersers and colonizers, with conquest of hyper-remote Hawaii being the crown jewel, and their similarities may be maintained by recurrent pulses of gene flow even among allopatric populations. So: (a) YES, (bi) NO, (bii) NO. (The proposal does a good job of summarizing current information and providing some new data – I think it should be published as a summary piece somewhere as a stimulus to further research.)

I summarize my thoughts as follows. Our current classification is based on nothing more than considering the plumage differences sufficient for species rank, and even that is not consistent in view of treatment of *melanurus* as a subspecies of *mexicanus*. What we do know from recent data is that all indications are that plumage makes no difference to gene flow. Therefore, sticking to our current classification is something like copy-cat error perpetuation. If we were proposing a novel classification, then my vote would be NO, but alternative old and current classifications, which WGAC consider fair game for adoption, better fit what data exist. Further, the new genetic data in the proposal suggest that our classification is paraphyletic if *melanurus* is treated as a subspecies of *H. mexicanus*; I recognize that there are no support values presented for that topology, but those results should be cause for concern.

NO. This is definitely a very tricky situation, and I agree that more information is needed to make decisions about this complex. Despite the occurrence of intergrades between *melanurus* and *mexicanus*, as well as between *himantopus* and *leucocephalus*, the presence of intergrades does not necessarily imply a lack of reproductive isolation. Further work on the contact zones of both of these taxa are necessary to help understand the taxonomy of this group. In addition, further analyses of vocal differences are needed to pair with differences in plumage. I agree with the proposal that we are not ready to make a decision one way or the other on this complex without additional research, and a change one way or the other would represent a major upheaval of our taxonomy.

NO. This proposal does a nice job of summarizing a complex situation, and I agree with the recommendation that further study is needed to better understand taxonomic limits.

NO. Additional research with an emphasis on contact zones is required to advance our understanding of the species limits in the genus *Himantopus*. The available evidence is insufficient to suggest taxonomic changes.

NO. Any change would be based on insufficient information and would be too disruptive to a large number of user communities. And lumping the Black Stilt is not something I think we should do without serious involvement of New Zealanders, given the massive long-term effort to rescue it from extinction. I also think there may be vocal differences between some of the groups that need to be quantified, and properly conducted playback experiments would be helpful in making sense of this.

NO. Not enough evidence and no new evidence for these changes. We need more information and formal study and new characters.

NO. As usual, we need substantial reason to change the status quo, and the evidence in the proposal does not meet that standard. If anything, I would say that the evidence skews toward lumping all as one species, but better data on gene flow where ranges meet (and associated plumage differences) could change my mind.

NO. For reasons outlined in the proposal. New, compelling information is needed to understand species limits in this complex. Until we have some, we're likely to just continue shuffling taxonomy based on differing opinions.

NO. As summarized by others above, the available information is insufficient to make a taxonomic change at this time.

NO. I vote no on both parts, although I do feel that (a) is a possibility. Fascinating. It would be interesting to further analyze vocalizations with all taxa. I know the calls well of *mexicanus,* the classic "Marsh Poodle," and believe that *Himantops himantopus* is similar, but in the non-breeding season I suspect that all *Himantopus* are more silent and I'm around Black-winged Stilts then. It is not intuitively obvious to me why all stilts should be so similar. *H. mexicanus* is a rare migrant on Southern California's Channel Islands and I believe is still unrecorded from Alaska; the only records there (western Alaska, Pribilofs and Aleutians) are for Black-winged Stilts. I don't believe I've ever seen a stilt over the ocean, hence hard to see *mexicanus* and nominate *himantopus*. Yet, they made it to Hawaii as represented by *knudseni* which shows differences in plumage (especially in males) and structural features (longer bill and legs). Hayman *et al.* (1986) describe no differences between nominate *himantopus* and *mexicanus*, nor differences between New World subspecies, but about Australasian *leucocephalus* they say the calls "are softer and more nasal, rather like a toy trumpet." For Black Stilt they say the yapping calls are "like those of Pied Stilt [*leucocephalus*], but slightly louder and higher-pitched." Hayman *et al.* (1986) stated: "Interbreeds commonly with Pied Stilt; indeed, there may be few if any genetically pure Pied Stilts in New Zealand." They (*ibid*) treated all *Himantopus* as one polytypic species, except for Black Stilt and this may have been done for conservation reasons.

For the above reasons, option (a) is a consideration. We would be deciding not to recognize the casual (to Alaska) nominate subspecies as a separate species from *mexicanus*. We have already taken a stand that there is only one *Himantopus* species in the New World. The matter of what to do with *leucocephalus* and *novaezelandiae* can be left for another day and hopefully determined by others. After all, if they do have different calls that warrants investigation.

Hayman, P., J. Marhant and T. Prater. 1986. Shorebirds, an identification guide. Houghton Mifflin.

2023-B-7a:

Treat *Chlorophonia sclateri*, *C. flavifrons*, or both as separate species from Antillean Euphonia *C. musica*: Treat *C. sclateri* as a separate species from *C. musica*, retaining *flavifrons* as a subspecies of *C. musica* (two-species treatment, version 1)

NO. I don't see much evidence in the proposal to support elevating *sclateri* to full species. The plumage differences are interesting but not sufficient by themselves, plus a rigorous quantitative study of plumage variation is lacking. Additional data on vocal differences (including playbacks) as well as genomic data would also help to tease out species-level differences in this group.

NO. The plumage of three subspecies in the *Chlorophonia musica* complex appears to be sufficiently different to warrant species status in the subfamily Euphoniinae, as stated in the proposal. However, only *C. m. sclateri* has been included in a phylogeny and the evidence provided in the proposal does not include any quantitative analysis. Genetic and quantitative data on plumage and voice are necessary to make a decision.

NO. I'm voting for (c), see below.

NO. Although the study by Imfeld et al (2020) resolves the relationships among *musica*, *cyanocephala* and *elegantissima*, within *C. musica* there is no molecular study that includes several samples, nor coloration, morphometry and song. There is variation in color and song patterns, but a formal study is required to accept this proposal.

NO. There is no evidence that *flavifrons* and *musica* are conspecific: they differ in plumage more than do most euphonias, for example. See comments under part (c).

NO. See comments under part (c).

NO. While the plumage differences seem compelling and species limits could very well be involved, I'd prefer to see a deeper treatment in a comparative framework than just a numeric yardstick on relative plumage divergence. Hudson & Price (2014) showed why this approach can fail in allopatric taxa. They also showed rather compellingly that we are oversplitting allopatric taxa relative to our treatment of taxa that achieve secondary contact and sympatry. We have numerous rather stunning examples of divergent island phenotypes that are better treated as subspecies, and given the broader evidence, we probably need more.

NO. Favor the treatment outlined in (c), see below.

NO. See comments under (c) below.

2023-B-7b:

Treat *Chlorophonia sclateri*, *C. flavifrons*, or both as separate species from Antillean Euphonia *C. musica*: Treat *C. flavifrons* as a separate species from *C. musica*, retaining *sclateri* as a subspecies of *C. musica* (two-species treatment, version 2)

NO. Same comments as 2023-B-7a regarding *flavifrons*.

NO. See comments on 2023-B-7a.

NO. I'm voting for (c), see below.

NO. See comments on 2023-B-7a.

NO. There is no evidence that *sclateri* and *musica* are conspecific: they differ in plumage more than do most euphonias, for example .See comments under part (c).

NO. See comments under (c) below.

NO. See comments on 2023-B-7a.

NO. See comments under (c) below.

NO. See comments under (c) below.

NO. See comments under (c) below.

2023-B-7c:

Treat *Chlorophonia sclateri*, *C. flavifrons*, or both as separate species from Antillean Euphonia *C. musica*: Treat both *C. sclateri* and *C. flavifrons* as separate species (three-species treatment).

YES. Not only is plumage strikingly different from the others in both sexes, in my opinion voice, at least of *flavifrons*, differs too. They are broadly similar but the Hispaniolan and Puerto Rican birds have a lower pitched, flatter whistle and longer, lower-pitched, more nasal “jink-jink” call notes than recordings from St. Lucia, in which there are shorter, sharper, more upturned call notes and a higher-pitched, more descending whistle. I’ve seen all these birds in the field, but with difficulty, as they are surprisingly skulking for euphonias, and usually one hears them much more readily. The plumages differ to the level that one really wonders why they are considered conspecific. While I agree that it would be best if there was a comprehensive integrative study of the entire group, I agree with the authors of this excellent proposal and think the evidence for species status for these two is at least as good as that for other species, given the conservative nature of plumage variation in the group, and that the burden of proof should be on maintaining them as conspecific. Not to treat them as separate species seems to me an example of highly inconsistent treatment both within this group and for Caribbean birds in general.

YES. As noted in the proposal, genetic data for allotaxa don’t really say much about species limits unless the group is actually not monophyletic, in which case splits would be required anyway – what would more genetic data tell you about species limits? Also as noted in the proposal, plumage differences among these three taxa are off the charts compared to for-certain species-level differences in the sister genus *Euphonia* (in which this group was placed until recently). Go check out a plate of euphonias – you’ll see that it’s actually hard to remember which yellow/blue-black plumage variation goes with which species. Note also that nominate *musica* is more similar to the two mainland species that we treat as separate species than it is to the other two subspecies of *C. musica*, as noted in the proposal. As for lack of quantitative analyses of plumage variation, just look at the photos of the specimens; a quantitative analysis would just reinforce the conspicuous differences between *flavifrons* and the other two and would essentially be a waste of time. The differences between nominate *musica* and *sclateri* are less conspicuous but nonetheless strong by comparison with differences among most euphonias. As for lack of known vocal differences, I agree that this is a problem; however, after browsing through the recordings of those complex, jumbled songs (as expected for fringillids) and their sonograms, I've talked myself into thinking I see and hear major differences among the songs, which you can check out for yourselves by looking at the note shapes and spacing in the sonograms. Of course a formal analysis would be the way to go (and better yet, playback experiments), but at this point from the recordings I think I'm ready to go out on a limb and defend the position that there is no evidence that the songs are the same. I also like to think I see and hear differences in call notes. So, although there are no quantitative data indicating that the vocalizations differ, neither are there quantitative data that the voices are the same. As for whether all this is sufficient to overturn NACC status quo, see my comments on *Himantopus*; as in that genus, here we have no data to support the status quo other than historical momentum, and would not be creating a novel classification: the 3-species treatment is both venerable (Ridgway) and current (HBW and BOW). In sum, I think that burden-of-proof is on maintaining the current NACC classification.

YES. I agree with the recommendations of the proposal that the phenotypic differences among the taxa in the *musica* group are commensurate with species level differences among Euphonias in general. Given how over-lumped many Caribbean taxa were during the Bond/Eisenmann days, this feels like another case over-zealous lumping.

YES. I find the comments from other committee members compelling to split these West Indian island taxa given to my eye rather striking morphological differences. I understand the cautionary flags but were the demands, say, with the multi-species split within Antillean orioles as rigorous? I don't remember vocal analysis, playback experiments, etc. Others have made some effort to analyze calls and hear differences between the taxa. Are there contrary arguments by those who have listened to the calls?

YES. Reasons are stated in the proposal.

YES. Many Caribbean taxa have been overlumped, masking important biodiversity. This appears to be another case. There are striking differences in plumage, especially between *flavifrons* and the other two. In addition, the difference between *sclateri* and the other subspecies is striking enough to consider this taxon as a separate species, given the variation exhibited among *Chlorophonia* & *Euphonia* species. The vocalizations are complex, yet some committee members also noted differences in vocalizations among the species. Although additional vocal analyses would be warranted, the plumage differences are remarkable enough in my opinion to argue for species status. Another important factor is that the plumage of *musica* more closely resembles the mainland species *C. elegantissima* and *C. cyanocephala* than it does its Caribbean counterparts.

YES. This is a challenging situation, but I agree with the comparative approach taken here for this group. While "yard-stick" measures are generally not appropriate or useful for birds, in this case, where plumage differences across the entire group of euphonias and chlorophonias are so minor, the differences we see across the *musica* group seem striking, and certainly on par or greater than most species-level differences in the larger clade.

YES. Compared to the minor plumage differences typical of different species of euphonia, the plumage differences between *flavifrons* on the one hand and *musica* and *sclateri* on the other are striking, and the differences between *musica* and *sclateri* also exceed those typical of euphonia species. It would be helpful to have additional data (e.g., formal studies of vocalizations), but in my view the plumage differences are sufficient to shift the burden of proof to those who would keep them conspecific.

NO. Same comments as 2023-B-7a regarding both *sclateri* and *flavifrons.*

NO. See comments on 2023-B-7a.

NO. See comments on 2023-B-7a.

NO. See comments on 2023-B-7a.

2023-B-8:

Treat *Corvus minutus* as a separate species from Palm Crow *C. palmarum*

YES. I agree that playback experiments, careful field documentation of tail-flicking behavior (and its presumed absence in *minutus*), and publication of evidence on differences in eggs would be preferable to a lack of these data. However, I think we have plenty of evidence in front of us in online format that shows a lack of tail-flicking in *minutus* (several videos of calling birds) and clear and consistent differences in voice for large samples of both taxa, both of which are inconsistent with subspecies status for *minutus*.

YES. This one should be a slam dunk. As noted in the proposal (which is terrific and should be published if this proposal somehow doesn't pass), these two differ in just about every aspect you can measure. Even using comparative genetic distance data (which I personally think is highly flawed), these two differ as much as do other species pairs in the clade. Further, they seem to differ as much or more than does *sinaloae* from *imparatus*, a split endorsed relatively recently by NACC (albeit I might be the only survivor of that committee) based only on the vocalizations. In *Corvus*, it's all about voice in terms of defining species limits. It's no surprise that the Tobias et al. scheme doesn't generate enough points to hit the magic 7 threshold. How could it? Because plumage variation among species in *Corvus* is minimal: conservative plumage evolution in this group makes it *almost impossible* for two similar taxa to get 7 points (and I need to dig out what the HBW scores were for *sinaloae* and *imparatus*) even though Boseman's analysis produced the maximum of 3 points for voice. At the more familiar scale, widely sympatric American and Fish crows differ primarily in voice, and silent birds cannot be distinguished in most cases; even in the hand, you need calipers and wing formula. So, in the *Corvus* context, these two have to be treated as separate species. Again, this is not a novel classification, and is endorsed by Caribbean ornithologists who have direct experience with this species pair.

Although I am conservative when making changes that would produce novel classifications, my standards differ when (1) current data conflict with AOS classification; (2) AOS classification was changed from Ridgway-Hellmayr era classifications simply by decree, with no explicit rationale, much less data; and (3) other major classifications have returned to the older classifications. I do not understand stubbornly sticking to the AOS status quo in such situations, when, as in this case (minus Johnston), there are no actual data or analyses to support our classification. Of course we all want more and better data, always, but at some point, burden-of-proof shifts, in my opinion, on maintaining the status quo.

YES. Various lines of evidence (i.e., vocalizations, morphology, behavior, habitat, egg pigmentation, and genetics) suggest that *palmarum* and *minutus* are differentiated and can be considered separate species. If the split passes, I vote for the English names Cuban Palm Crow and Hispaniolan Palm Crow.

YES. The differences in vocalizations, while subtle, do appear to be similar to differences in other *Corvus* species. In addition, the differences in behavior and habitat use are intriguing. I am not inclined at this point to put much stock in differences in egg color until knowing sample sizes involved, as there can be some substantial variability in egg color and speckling within a species. If the proposal passes and the two are split, I vote for the English names Cuban Palm Crow and Hispaniolan Palm Crow.

YES. A very well-presented proposal. As others mention, species level differences in *Corvus* almost entirely rely on differences in vocalizations. Even though most of these vocalizations are learned, and available media show some variation in both species, the vocal differences elucidated in the proposal seem consistent and on par with differences between other sister species in *Corvus*. In addition, possible pronounced differences in display behavior, a genetic distance equal to sister taxa pairs in *Corvus* that are considered species, and the over-zealous lumping of Caribbean taxa in the Bond/Eisenmann era point to considering these taxa species..

YES. I listened to calls and they sure sound different and consistent to my ear. For *minutus* I think of Raven-like calls, and I don't think of Ravens with the calls from the Cuban birds, a species which I see (quite local) and hear regularly in eastern Cuba. I think it is also worth noting again that Cuba and Hispaniola share little these days in terms of polytypic West Indian species with different subspecies in Cuba and Hispaniola (Antillean orioles, Spindalis, Quail-Doves, and Nightjars).

I would add that in listening to calls recently on a recording of Northwestern Crows from Alaska, they sounded "slightly different" to my ear.

YES. Vocal data that indicate that *minutus* and *palmarum* differ as much as some sympatric species of *Corvus*, and supplementary evidence from behavior, morphology, and genetics, combine to persuade me that these are best treated as separate species.

YES. Preponderance of evidence suggests originally lumping of these two taxa was in error. The taxa differ in vocalizations and behavior. Comparative genetic distance supports a decision to treat these two as separate species.

NO. While I agree that the vocal data are suggestive of species-level differences, I would like to see a quantitative analysis of the vocalizations as well as playback experiments to see how each taxon responds to calls of the other. I don't see much else in the proposal that clearly justifies elevating *minutus* to full species status at the moment.

NO. I prefer to wait for a formal study to split *minutus* from *palmarum*. Although there are differences between them, no recent studies about songs, morphometrics or molecular differences can support the split.

NO. Splits in oscine passerines based primarily on vocalizations when so much of that is culturally inherited still put me on edge. That, and the general oversplitting of allopatric taxa leave me wanting more compelling evidence that we should split yet another one.

NO. This proposal largely relies on vocalization differences to justify the proposed split. The behavior and morphological differences seem slight, even if they are diagnosable, I'm not convinced that these are species under the BSC. I recognize it's a subjective call with allopatric taxa, but given the unpublished, qualitative nature of much the data presented here make me err on the side on of a conservative taxonomic treatment in recognizing the status quo. Furthermore, we recently lumped two taxa in Northwestern and American Crow that presumably differed in vocalizations, so while vocalizations are important for Corvidae writ large, it may not be as ubiquitous as we once thought.

2023-B-9:

Treat *Cyanocorax luxuosos* as a separate species from Green Jay *C. yncas*

NO. Reasons are given in the proposal for maintaining the status quo pending further study, especially of plumage and vocal variation.

NO. I agree with the recommendation of the proposal. The available evidence is limited and does not support a split at this time. Phenotypic, genetic, and vocal analyses with extensive geographic sampling are necessary to assess species limits in the Green Jay.

NO. I think a case can perhaps be made for species status for *luxuosus* but it isn't that clearcut, especially with the Venezuelan birds looking much like northern ones. Further study clearly needed.

NO. I agree with the proposal. We need a formal study to separate *C. luxuosus* and *C. yncas*.

NO. I suspect that a formal analysis will show that two or more species are involved. In contrast to some of the other proposals in this batch for which we have substandard data, in this case we have no data to act on, only anecdotes and assertions. The two groups differ more strongly in plumage than do some pairs of jay species we treat as separate species (e.g. California vs. Woodhouse's scrub-jays). Speaking of anecdotes, some birds at the extreme N end of the range of the *luxuosus* group, i.e., in our Rio Grande Valley, have almost completely yellow underparts, like Andean birds – check out the variation in amount of ventral yellow in this series, including one that is nearly all yellow: https://ebird.org/checklist/S102535890

NO. I agree with the proposal that additional samples and comparisons are needed before splitting these taxa. I had not realized just how similar the Venezuelan and

Central American birds were in terms of plumage; there seems to be enough overlap in plumage variation in these groups to be consistent with subspecies. I agree that the vocalizations do sound different to my ear, but I would like to see a more detailed analysis in the case of a species with such a varied vocal repertoire.

NO. As the proposal relates, new evidence to make this split is not very compelling and further study is warranted.

NO. For the reasons outlined in the recommendation. More study needed.

NO. Looking at the extremes of the two groups, one would wonder how they could be considered conspecific, given the differences in plumage, size, habitat choice, and behavior. And these two groups are geographically well separated, missing entirely in Central America south of Honduras. The extremes are the examples I am most familiar with as well. But taking all subspecies into consideration, and looking at the range of plumage variation, habitat, vocalizations, etc., the story gets much murkier. I would be very surprised if these two groups were not considered separate species eventually, but the data are still insufficient to make that case in the present moment.

NO. Insufficient data at the present, but an interesting system that may warrant a split in the future with more information.

2023-B-10:

Transfer Tiny Hawk *Accipiter superciliosus* to the newly described genus *Microspizias*

YES. The morphological data combined with available molecular data support the removal of *superciliosus* out of *Accipiter*. As noted in the proposal and comments for the parallel SACC proposal, moving this species (plus *collaris*) to *Kaupifalco* may be another option but there are sound phenotypic and biogeographic reasons for not doing that. Nice job by Sangster et al. (2021) in researching the generic names.

YES. The move out of *Accipiter* is mandatory and the case for the new genus seems sound.

YES. Phylogenies and anatomy support the removal of the Tiny Hawk from the genus *Accipiter*. I agree with the recommendation in the proposal, the new genus *Microspizias* suggested by Sangster et al. (2021) is the best choice.

YES. I agree with the proposal to move Tiny Hawk from *Accipiter* to the new genus *Microspizias*, as *M. superciliosus*.

YES. Per comments in the SACC version.

YES. Reasons are outlined in the proposal.

YES. It is nice to see this trashcan genus finally being well addressed. The evidence here is unequivocal.

YES. Structurally, it doesn't even look like an *Accipiter.*

YES. The molecular and osteological analyses agree that *superciliosus* does not lie anywhere close to other Accipiters. Plumage convergence and probable mimicry in raptors is truly amazing. Now that a name is available and older names have been found to not be connected with *superciliosus*, we should definitely move it to *Microspizias*.

YES. This is a logical change to genus-level taxonomy based on new phylogenetic information. *Microspizias* fits the English name too.

2023-B-11:

Treat *Accipiter atricapillus* as a separate species from Northern Goshawk *A. gentilis*

YES. Molecular data combined with vocal differences and phenotypic traits support recognizing Nearctic and Palearctic groups of "Northern Goshawk" as separate species.

YES. Molecular and vocal analyses along with plumage coloration and patterning support treatment of *Accipiter gentilis* as a separate species from *A. atricapillus*. If the split passes, I agree with the English names recommended in the proposal, American Goshawk (*A. atricapillus*) and Eurasian Goshawk (*A. gentilis*).

YES. It seems to me there is now a great deal of evidence for North American and European goshawks not being each other's closest relatives, and nothing that argues convincingly for conspecificity. Sangster (2022) states that (as usual) no published rationale for conspecificity was published by Peters or AOU. And he cites four molecular studies that all point toward relatively deep divergence. Geraldes et al. (2018) did not focus on the question of their conspecificity but nevertheless stated:

"Within northern goshawks, both nuclear and mitochondrial datasets are congruent in showing a pattern of close relationships within North America compared to the distant relationship between North American and European populations. Analyses of the nuclear dataset with Admixture and PCA reveal a similar pattern of strong differentiation between European (A. g. gentilis) and North American samples of northern goshawks (Supporting Information Figure S3)."

In morphology, adults differ strikingly in iris color, crown color, and underparts barring, and all those I checked on eBird were easy to identify to continent (I did not attempt immatures). There is a little variability in iris color in birds in adult plumage but all those that are really red are North American, while none of the pale yellow-eyed adults are.

While vocalizations are different but only to an unspectacular degree, this still provides more support than otherwise to the split.

YES. I agree with the proposal, there is enough evidence (DNA and vocals) for the split of North American Goshawk populations from Palearctic populations.

YES. All data are consistent with treating these two as separate species. I also vote for American Goshawk: not only the former AOU name but also recommended by Sangster et al.

YES. Reasons are outlined in the proposal.

YES. I'm persuaded. Another factor I've been considering is that North American Goshawks for the most part seem to be forest birds. I seldom see them, even though I live not far from where they breed. And they seldom seem to thermal around like Cooper's and Sharp-shinned. The best time to find them is in late summer and early fall when the young are about to have fledged, or have fledged, and are begging, a loud call that is audible for a half mile or so. In Europe, I've seen this species in towns, the sort of situation where I see Cooper's Hawks here. Ferguson-Lees and Christie (2001) comment that since the late 1980's they have adapted to parks in several large European cities (they give Amsterdam and Cologne as examples). They do mention that juveniles throughout the range are more apt to be found in open areas than adults and in thinking about that the few I've seen in open areas in late fall and winter, were juveniles. In any event given the different calls and distinct plumage differences, the case for splitting seems strong. I would add that I can hear almost no difference in the calls of Gundlach's Hawk (*Accipiter gundlachi*) and Cooper's Hawk (*A. cooperi*). In fact from limited experience I've had a better response in play-back from playing Cooper's rather than Gundlach's. Structurally these two look the same.

YES. The calls sound quite consistently different to me, with the New World calls having a raspy, *Larus*-like quality, and the Old World calls being more ringing, maybe closer to *Dryocopus*. Molecular analyses show a moderately deep split, and morphological differences are appropriately distinctive, relative to other closely related *Accipiter* species. All told, I feel that they are best considered separate species.

YES. These are deeply divergent and polyphyletic (more than paraphyletic) in their mtDNA. To me, if certain populations of Nearctic *A. gentilis* were more closely related to Palearctic *A. gentilis*, then they would be paraphyletic. The fact that Palearctic *A gentilis* are more closely related to *meyerianus*, *hentsii*, and *melanoleucus* than Nearctic *A. gentilis* is strong evidence that this is a taxonomic revision that should happen. Couple this deep polyphyly with differences in vocalizations and other phenotypic differences and this is strong evidence for a split in my opinion.

NO. Divergence here is relatively young (well below the average time to speciation in birds), and we do not have a solid basis yet upon which to evaluate vocal differences in these birds (their possible importance is present, yes). Paraphyly in mtDNA is proving to be rather common in perfectly good biological species, and I will predict that a holarctic population genetics study will reveal historic gene flow at a not insubstantial level.

2023-B-12:

Treat *Aphelocoma sumichrasti* as a separate species from Woodhouse's Scrub-Jay *A. woodhouseii*

Proposal postponed.

2023-B-13:

Treat *Delichon lagopodum* as a separate species from House Martin *D. urbicum*

YES. (a) There appears to be good evidence to support this split, including phenotypic and vocal differences, genomic divergence, and apparent breeding sympatry. (b) I am fine with the recommended English names.

YES. (a) It seems surprising that sympatry in such clearly differently plumaged taxa was overlooked for so long. (b) Yes to the recommended E names.

YES. (a) Breeding sympatry, UCE phylogeny, phenotype, and vocalizations support treating *Delichon lagopodum* as a separate species from *D. urbicum*. (b) Adopt the English names suggested in the proposal.

YES. (a) I completely agree with the proposal, breeding, UCE, morphology and vocalizations support the split, *Delichon lagopodum* from *D. urbicum*. (b) Adopt the names suggested in the proposal.

YES. (a) The sympatric breeding with no evidence of interbreeding is solid evidence that the two represent separate species. (b) Yes to the proposed English names.

YES. While there are several things here that raise some questions (e.g., small sample sizes in the field, diagnosability does not seem definitive unless in the hand, and single individuals in the UCE study), multiple sites of sympatry and the totality of the evidence suggests species limits and support splitting.

YES. All evidence indicates two species, and this is not a novel classification. Thanks for getting us caught up on this one.

YES. For the reasons outlined in the proposal and yes to the English names. The record (April 2002) of *urbicum* from Tobago which was obtained from Buckley *et al.* (2009) is unknown apparently to the Chair of the Trinidad/Tobago records committee. I did miss a record of *urbicum* from Guadeloupe, so that should be added to the casual section.

Buckley, P.A., E.B. Massiah, M.B. Hutt, F.G. Buckley and H. Fl. Hutt. 2009. The Birds of Barbados. B.O.U.Checklist No 24. British Ornithologists' Union/British Ornithologists' Club.

YES. (a) Breeding in sympatry with no indication of introgression is good enough for me. (b) Yes to the proposed English names.

YES. (a) Assortative mating in sympatry is good evidence of reproductive isolation under the BSC, paired with the new information from UCE phylogeny that these are not sister taxa suggests these should be split. (b) Yes to the proposed English names.