2026-B-1: Change the group name of *Tympanuchus cupido and T. pallidicinctus* from Prairie-Chicken to Prairie-Grouse
2026-B-2: Treat *Cistothorus paludicola* as a separate species from Marsh Wren *C. palustris*
2026-B-3: Treat Great Horned Owl *Bubo virginianus* as two or more species
2026-B-4: Treat Southern Beardless-Tyrannulet *Camptostoma obsoletum* as two or more species
2026-B-5: Treat Tawny-crowned Greenlet *Tunchiornis ochraceiceps* as four species
2026-B-6: Treat *Leiothlypis ridgwayi* as a separate species from Nashville Warbler L. *ruficapilla*
2026-B-7: (a) Recognize the genus *Nannus* for three species of wren; (b) Subsume *Thryorchilus* into *Troglodytes*
2026-B-8: Treat West Indian Woodpecker *Melanerpes superciliaris* as two or more species
2026-B-9: Treat Russet-crowned Motmot *Momotus mexicanus* as two species
2026-B-10: Treat Tufted Flycatcher *Mitrephanes phaeocercus* as two or three species
2026-B-11: Subsume *Leiothlypis* into *Oreothlypis*
2026-B-12: Treat Fea's Petrel *Pterodroma feae* as two species
2026-B-13: Treat Yellow Warbler *Setophaga aestiva* as two species
2026-B-14: Change the group name of species of *Myioborus* from Redstart to Whitestart
2026-B-15: Treat Chaffinch *Fringilla coelebs* as two or more species

2026-B-1
Change the group name of *Tympanuchus cupido and T. pallidicinctus* from Prairie-Chicken to Prairie-Grouse

NO. Although I recognize that the phylogenetic data place these two species within a monophyletic "grouse" clade, the "Prairie-Chicken" names are extremely well entrenched in the popular and scientific literature. I do tend to agree with the proposal that changing the name to Prairie-Grouse would remove the potentially pejorative connotations associated with them being "just a chicken" (which is unfortunate, because chickens are gorgeous). However, I would want to see considerable public input on this name change before making it, especially given the wide popularity of these species in the conservation, hunting, birdwatching, and scientific communities.

NO. I agree with the proposal that "grouse" is a better moniker, primarily because they are in a clade of other grouse and the other species in the genus is referred to as a grouse. However, I also agree with the comment that the name "chicken" is heavily entrenched in the literature and society and that this change should get broader public input. Thus I am voting no for now. Perhaps the author of the proposal would care to undertake a poll?

NO. Although I am highly sympathetic with the proposal author's point of view, I echo the comments made by others regarding the deeply entrenched nature of the name "prairie-chicken". Also, the word "grouse" does not necessarily have entirely positive connotations, meaning "complaining" or "grumbling", except inexplicably (?) in Australia (where the group is lacking) where it can mean "excellent" etc. Thus I am not sure that such a disruptive change would be really worth it in the end, in terms of getting people to

care more about them and in aid of their conservation.

NO. As others have mentioned, I too am sympathetic to the desire to change the name, but ultimately, I do not think it is appropriate to change the name. The name is indeed very deeply entrenched in not just the ornithological community, but also the conservation and agency communities, and any name change for these species could be deeply disruptive. Any further proposal to change the name of these species would require public input from all the relevant stakeholders here, of which there are many.

NO. I do not like how the proposal is written. I think we need to wait and think about whether this change is really necessary.

NO. Prairie-chickens, as an English name, are too well established within the birding community and importantly within the public at large and the usage has been established for centuries. If we are all seeking a more appropriate name, the list of species to change would be very large indeed.

NO. I agree that “grouse” is arguably a better, more accurate English name, but this change would likely also be very disruptive.

NO. The name “Prairie-Chicken” is so entrenched among the public, hunters, wildlife biologists, etc., a change of this nature would be too disruptive. We have so many other English names that have to be changed out of necessity, we should limit such changes when we can. I’m also not convinced the name “chicken” is pejorative.

2026-B-2
Treat *Cistothorus paludicola* as a separate species from Marsh Wren *C. palustris*

YES. This is an excellent, thorough proposal that provides multiple lines of evidence (song, plumage, genetics, molt, migration route) in support of a split between Eastern and Western populations of Marsh Wrens. I am fine with the proposed English names and with hyphenating Marsh-Wren for the reasons given.

YES. All lines of evidence support the split, which has been a long time coming. The proposal sums it all up well, and leaves little doubt that the *paludicola* and *palustris* groups are better treated as separate species. Some may consider a 180-km hybrid zone as too broad for species status, but considering the size of their respective ranges the hybrid zone is tiny. Another potential sticking point (as the proposal authors point out) is the Mexican *tolucensis*, which is rather poorly known. The song does appear to align *tolucensis* with *paludicola*, but the plumage of *tolucensis* is considerably richer below than is typical of any other population, and it seems to typically have especially extensive black on the mantle (as indicated in the original description). It also has a highly isolated resident range, and it’s not always the case that populations on the Mexican Plateau group with western US/Canada taxa (think Eastern Bluebird). Thus, I won’t be surprised if a more comprehensive analysis that includes *tolucensis* shows it to be a third clade, but for now the best evidence we have is the *paludicola*-like song.

As for English names, I agree with adopting Western and Eastern Marsh Wren/Marsh-Wren for *paludicola* and *palustris*, respectively. While these names will be considered boring by many (and they're not wrong about that!), again (as with the vireos) we are dealing with species for which really good descriptive names are not obvious (especially when *tolucensis* is included in *paludicola*); the geographical region-based names are indeed useful; and the songs are not so very different or simple that obvious song-based names suggest themselves or would be very helpful. As for hyphenation, I think for now we should hyphenate to indicate the sister-species relationship, although I won't be surprised if AviList doesn't hyphenate. This seems such a minor issue to me, although many feel strongly about it one way or the other.

YES. This is a well researched and well written proposal that covers the intricacies of this system. I was especially swayed by the studies of the hybrid zones which, while the two do hybridize, it appears rather limited. I was very interested in the presentation of the Figure 7C, which shows a strongly bimodal distribution of ancestry values from the core of the hybrid zone, which is strongly indicative of either selection against hybrids or selection for assortative mating. As others have noted, even though the names are not terribly exciting, I think Eastern and Western Marsh Wren/Marsh-Wren are appropriate for these two species given the lack of other very strong habitat or plumage-based names to use. As for hyphen use, I'm somewhat ambivalent, but I do not think it is strictly necessary.

YES. I agree with the proposal, there is enough evidence (song, genetic, distribution and plumage) to split *Cistothorus paludicola* from *C. palustris*. English names: Eastern and Western Marsh-Wren.

YES. The combination of genomic and song evidence from the hybrid zone is very convincing. The level of genetic divergence is very high, especially in comparison to other species with similar continental hybrid zones that we consider species. Likewise, the fact that F1 and F2 hybrids appear to be uncommon, even within the hybrid zone, is strong evidence of reproductive isolation. That the song types also largely match the genomic background (with a few exceptions) is strong evidence that, despite song learning, these two are behaving as species. It would be nice to see more comprehensive genomic sampling from the Nebraska contact zone (the "wrenless corridor" region), but the contact is more extensive in Saskatchewan, so I would expect the Nebraska contact zone to behave similarly, or isolation here to even be more complete, given the low density of breeders and likely older contact.

The winter ranges of the two species will need to be worked out, as will the fine details of the plumage-based identification, but the details in the proposal give us a great start. There do seem to be some subtle differences in the call notes (slightly lower-pitched and harsher in *paludicola*, higher-pitched and more of a "chip" note in *palustris*). In a quick search through available photographs, I found some clear *palustris* as far west as eastern New Mexico, but it's not clear to me how far east *paludicola* gets in the winter.

I agree that *tolucensis* should be considered part of *paludicola* for now, based on song similarity, but given the tendency for birds to speciate in the central Mexican high-elevation marshes, I would not be surprised if those are shown to be a distinct

species with additional research. A clarification, based on the comments from the committee member below: I am fine with splitting these piecemeal. I don't think we should wait to split what appear to good species because we are not sure about the species status of *tolucensis*. If we eventually get data that show *tolucensis* to be a distinct species, then we can address that issue at that time. I do think that the proposal outlines why it should go with *paludicola*, for the time being, and I agree with that assessment. Yes, it is much more richly colored, but it also has the introductory "tuk" notes to the song, like *paludicola*. It also appears that *paludicola* reaches the same marshes in winter, so there is an opportunity there for gene flow (although unlikely).

If the proposal does pass, I agree with Eastern/Western Marsh-Wren. Like with the Warbling-Vireos, I can't think of a better name. Even though Eastern/Western isn't really novel or exciting, it's at least accurate.

If this proposal passes, the linear sequence of *Cistothorus* based on the data from Imfeld et al. (2024) should be: *paludicola, palustris, stellaris, platensis*, which is at odds with our current sequence.

YES. The separation of these forms into two species is justified by vocalizations, genomics, migration routes, and plumage color. Although there is hybridization, this seems limited and likely all F1s. I agree that Eastern and Western are boring, but they are accurate. And the hyphen is necessary since the two taxa form a monophyletic clade.

YES. The data from the contact zone in Canada were especially convincing for me. The plumage differences are apparent in the photos provided, it would be nice to have those quantified though to better understand how intraspecific and interspecific variation compare.

NO. I'm fine with splitting Eastern and Western Marsh-Wrens for all of the evidence outlined. There are other things too not mentioned in the motion. Eastern Marsh-Wrens are late fall migrants in the East. In northern Kentucky at places where they do not breed (e.g. at the Sloughs, west of Henderson), they are found in wet areas in mid-October. Fall movement in Western Marsh-Wrens is considerably earlier. I wonder if this is because I believe Eastern Marsh Wrens sometimes (frequently) have two broods from different geographical areas. I need to double check on this and check on what Western Marsh-Wren subspecies do .The morphological characters outlined fit my impressions. Eastern birds do look darker capped and more richly colored, except for *griseus* from the coastal Southeast which just looks dull and gray. Once you learn the songs they really are quite different. Kroodsma stated quite matter of factly some 30 years ago to me that they were clearly different species.

My problem with this split is with the designation of Mexican *tolucensis* with the Western Marsh-Wren group from the Plateau as others have commented. More study is needed.I received correspondence from Steve Howell within the last week. He had heard of the proposal and wondered what they would do with the above-mentioned group which is isolated from eastern and western North American populations? In his Mexican manuscript for his new Mexican field guide (with Dale Dyer) which he hopes to submit

this summer, he illustrates *tolucensis* and it is dark and richly colored, the morphological differences more striking than Eastern and Western Marsh-Wrens (but note *griseus* is pretty different). The breast of *tolucensis* is a rich buff color. Howell also points out multiple differences with contact calls. From the motion and from member's comments, it would appear that this taxon (group) needs more study. Steve pointed out to me that a split, as outlined now, would recall the split of the Solitary Vireos where the Middle American breeding taxa, which look more like Cassin's, were placed somewhat arbitrarily (not studied I am told) into Plumbeous. Some 30 years later, this unsettled issue remains. So, I would prefer to pend the motion and figure out whether *tolucensis* should be split as a third species. The decision to place it within the western species seems arbitrary. Have the songs of *tolucensis* been carefully analyzed?

2026-B-3
Treat Great Horned Owl *Bubo virginianus* as two or more species

YES. I agree to separating *magellanicus* and *nigrescens* from *virginianus*. Those two are vocally very distinct, and despite some of the incongruent samples in the trees, they do seem to mostly form clades separate from the rest. The one sample of *magellanicus* that falls out with *nigrescens* in the mitochondrial tree is likely a signal of gene flow (also supported by Dsuite analyses in the Ostrow paper supplement), but that doesn't bother me too much given the very different vocalizations between the two and versus *virginianus/nacurutu.*

I'm much less convinced about *nacurutu,* and tentatively vote NO on that split. The vocalizations sound very similar to *virginianus*, at least in overall pattern. I do hear the differences and they are consistently different across the range of each, so in that sense, I see the arguments for splitting *nacurutu*. The genetic data, though, are very messy. Even with the mislabeled samples being resolved, it is still not clear to me that *nacurutu* forms a clade. All the trees give conflicting answers on its placement, although I do suspect that the "Fresh tissues only" tree is the correct nuclear species tree, which has *nacurutu* as sister to *virginianus*. The UCE "All samples" tree, the UCE quartet tree (Ostrow paper supplement), and both mitochondrial trees give different answers about where *nacurutu* sits in the phylogeny, including some that make *virginianus* paraphyletic. Given its similar vocalizations to *virginianus*, it may be better to keep it with *virginianus* until that is sorted out. That said, it is a SACC issue and we should perhaps go along with their decision.

As for English common names, I think we should go along with SACC so Magellanic Horned Owl for *magellanicus* (although I note that AviList has this as Lesser Horned Owl) and Paramo Horned Owl for *nigrescens*. If *nacurutu* is split, I'm happy with Tropical Horned Owl. Given that Great Horned Owl is so incredibly well-entrenched, I see no reason to modify that name for *virginianus sensu stricto*.

YES. The proposal provides rationale for splitting North and Central American *Bubo virginianus* from South American taxa, especially based on vocal differences. The consensus from SACC is to split *magellanicus* and *nigrescens* from *virginianus,* and I'm

fine with that. I'm not convinced about *nacurutu* but will go along with SACC's decision (7 in favor, 1 against) since this is their issue. I agree to the proposed English names including Great Horned Owl for North/Central American *Bubo virginianus*.

YES. I support the four-way split, which is clearly warranted for *magellanicus* (long split by IOC etc.) and *nigrescens*, for which the vocal differences have only recently become clear based on new recordings. Also, *nigrescens* is much darker overall than *nacurutu* of the adjoining lowlands, and given its very high-elevation habitat this is not clearly just an expected difference based on biogeographic rules. As others have pointed out, *nacurutu* is a less obvious candidate for species status, but first note that the distributions of the *virginianus* group and the *nacurutu* group are much more widely allopatric than generally thought (see eBird map for example). In fact, although the latest printed *Check-List* gives the distribution as if continuous, in the 42nd NACC supplement (AOU 2000:850), the distribution statement of the *virginianus* complex was modified based on Olson (1997) to indicate the gap between Nicaragua and Colombia. There are a few evidently unconfirmed reports in between, but there is certainly no reason to believe there is a contact zone between the *virginianus* group and *nacurutu*. Both Costa Rica and, perhaps to a lesser extent, Panama are so well known ornithologically that any undiscovered populations seem improbable. Second, the song of the *virginianus* group is very similar throughout its vast and highly disparate range, while those of *nacurutu* differ in several details that may seem minor to us but are similar in magnitude to the differences between songs of *Bubo bubo* and *Bubo ascalaphus*, for example. In addition, the *virginianus* group, despite their extreme heterogeneity in size and plumage, always (as far as I can see) have yellow irides while those of *nacurutu* are usually bright orange (perhaps 10% of individuals of *nacurutu* in ML photos appear more yellow-eyed). In sum, despite the fact that *nacurutu* is more similar to the *virginianus* group than are *magellanicus* and *nigrescens*, I think species status is warranted for it, and this is not just for the sake of going along with SACC (although doing so as a rule is highly preferred).

Strong YES to maintaining the use of Great Horned Owl for *Bubo virginianus sensu stricto*. Its range is positively huge, even compared to those of *nacurutu* and *magellanicus*, and in much of that range English and therefore the long-established use of the name predominates, whereas that is less so in South America (although local guides, for example, will need to learn the new names for the splits there). In addition, I believe the largest birds in the complex occur in the *virginianus* group. The question of hyphenation, as "Horned-Owl", arises, but SACC elected not to use the hyphen and it would be best to align here. However, it seems odd to me to adopt "Marsh-Wren" but not "Horned-Owl".

YES. I agree with the proposal which provides evidence of the split for *Bubo virginianus*. I also agree with the English names.

YES. This is definitely a tricky situation, but ultimately I am swayed by the evidence to split all the taxa here as separate species. I am also happy to defer to SACC on this issue, since all the relevant taxa are exclusively South American. I like the English names proposed for the South American taxa, and also agree that Great Horned Owl should be maintained for *virginianus*.

YES. I support this proposal, following SACC. Keep Great Horned Owl for North America.

YES. There are still some complexities and nuances to work out for sure, but I am on board with the SACC split and Great Horned Owl for the English name of the northern taxon.

YES. Good evidence for the split of North & Central America from others and I'm ok with going along with SACC for the other decisions. Yes to retaining the English name Great Horned Owl. As others have noted, this taxa has a large distribution, so it's ok to retain current name.

2026-B-4
Treat Southern Beardless-Tyrannulet *Camptostoma obsoletum* as two or more species

YES. This is mostly a South American issue based on a well-sampled study of vocalizations that supports the recognition of multiple species. I am fine with Panama Beardless-Tyrannulet for the species in the NACC region for the reason given in the proposal.

YES. I agree with the multi-way split that SACC has now adopted, especially as NACC-region *flaviventre* is one of the more vocally distinctive, and thus least controversial, taxa proposed for elevation to species status in the *obsoletum* complex.

The English name Panama Beardless-Tyrannulet seems about as good as it gets for this not especially distinctive-looking species that appears to be largely confined to Panama (and western Costa Rica). eBird points make the distribution look continuous or nearly so between Panama and northern Colombia, but there's a break in ML media in southeastern Panama and adjacent Colombia, and it's not yet clear as far as I know whether *flaviventre* occurs in far northwestern Colombia, but that would not be surprising.

I also think that NACC needs to reconsider the split of *thyellophilum* from *imberbe*, which we rejected in 2025. Part of the rationale for some no voters was that there are more divergent vocalizations within *imberbe sensu lato*, but with the 6-way SACC split that is no longer the case. However, there still remains the issue of whether there is a contact zone and if so what happens there.

YES. I share some of the concerns brought up by the comments on the SACC proposal, but given that it is almost entirely a SACC issue, I defer here to their final decision on species delimitation here. I am also fine with Panama Beardless-Tyranulet for the species in the NACC region.

YES. There are clearly multiple species-level taxa within the broadly defined *C.*

obsoletum, and kudos to Lima & Vaz for starting to sort out this complex issue. I am not convinced that the various South American splits adopted by SACC are good species, but for the ones that matter for our purposes (*flaviventre*, *caucae*, and *pusillum*), I hear consistent and fairly uniform vocal differences as described in the publication. The terminal trilling/buzzy note in *caucae* and *pusillum* appears to be lacking in *flaviventre*, so that's good enough for me, for a suboscine.

Does anyone know the reasoning behind using the noun "Panama" instead of the adjectival "Panamanian", as we do with almost all other English common bird names that are based on country names? I know that we have Panama Flycatcher for the *Myiarchus*, but that one always struck me as odd, given that for most (all?) other countries, we use the adjectival form (e.g., Costa Rican Warbler, Colombian Crake, Mexican Duck). I'm happy with Panama/Panamanian Beardless-Tyrannulet for *C. flaviventre.*

As for Panama vs Panamanian, I think it's best to maintain internal consistency with AOU (1998) so that Panama Beardless-Tyrannulet follows the precedent set by Panama Flycatcher. My guess is that Panama was used for the flycatcher in keeping with non-ornithological adjectival uses such as Panama Canal and Panama hat.

YES. I agree with recognizing multiple species in *Camptostoma obsoletum*. The proposal is strong and I agree with Panama Beardless-Tyrannulet for the NACC region.

YES. I agree with the proposal, following SACC.

YES. I agree to following the lead of SACC as it's largely under their jurisdiction.

YES. Good evidence for splitting these taxa; mostly an SACC issue. I'm fine with either Panama or Panamanian Beardless-Tyrannulet.

2026-B-5
Treat Tawny-crowned Greenlet *Tunchiornis ochraceiceps* as four species

YES. If this were strictly a NACC issue, I'd be on the fence for reasons given in the proposal and SACC comments: genetic data strongly show divergence but hard to integrate that with plumage and voice. However, it is mostly a SACC issue that received strong support, so I am voting in favor mostly for consistency. Regarding English names, I'm not crazy about "Brownlet" but I defer to the SACC and will go along with that recommendation (Ochre-crowned Brownlet) for the species that occurs in the NACC region.

YES. Although the situation is hardly clear-cut, as many others have noted, the fact is that there are at least four genetically highly diverged lineages of which the *ochraceiceps* group forms one. The plumage and song characteristics do not appear to closely conform with these lineages. Still, their division is an improvement to the long-standing single-species treatment, and as others have noted, further analysis seems likely to

strengthen the case and perhaps even result in recognition of more than four species.

As for the English group name Brownlet, I think it is a good solution to a complex problem, as there aren't enough good names for all the greenlets out there (and likely more splits are forthcoming); the *Tunchiornis* species aren't at all green, but various shades of brown; it gives this genus their own group name, emphasizing their distinctiveness, echoing the use of greenbul vs. brownbul in Africa.

YES. As others have mentioned, I am not very excited by this split because of some of the unresolved issues still remaining. However I will defer to SACC although I do take issue with the names that were proposed/adopted by SACC. I think these names are incredibly clunky and in this case, I think better options are available. The names adopted by the Clements/eBird checklist (Clements et al. 2025) include the name Ochre-crowned Greenlet for *ochraceiceps*, which fits well with the plumage and scientific name of the bird. I *don't* think it is necessary to maintain the name "Tawny-crowned Greenlet" for the complex because the members of this complex are already the only species in the genus, so maintaining that connection seems less important here. Not that it occurs in our region, but retaining "Tawny-crowned Greenlet" is especially odd for *luteifrons*, which pretty much lacks the tawny/ochre/rufous crown or forehead patch.

Addendum based on further English name proposal: I am fully supportive of the descriptive names for the species (i.e., Ochre-crowned) but I am not a fan of "brownlet" for this group. It just strikes me as sounding "wrong" and I am left with an overall negative impression from it. I'd prefer to stick with Greenlet.

YES. I find the extremely old divergence times and what appear to me to be fairly consistent plumage differences to be convincing. In listening to the sound recordings available online, it is clear that there are vocal differences (ascending vs. descending notes, 1 vs 2 notes, etc.) despite some odd recordings here and there. There is clearly more work to be done, but I find the four-species treatment to be a good first step.

I'm not sure how the voting is structured for the English names, but I am much more in favor of the name selected for *T. ochraceiceps* by AviList (Ochre-crowned Greenlet) than the one selected by SACC (Northern Tawny-crowned Greenlet). Ochre-crowned is simpler, more memorable, and mirrors the scientific name. It is also somewhat tied to the parent name of Tawny-crowned, so it maintains some of that connection. I am strongly in favor of Ochre-crowned Greenlet for *T. ochraceiceps.* Addendum: the three genera of greenlets (*Pachysylvia*, *Hylophilus*, *Tunchiornis*) do not form a clade (Slager et al. 2014), so I don't see any need to maintain Greenlet for all three. However, I'm also not generally in favor of "improving" names simply for the sake of improvement, and Brownlet just sounds a bit "wrong" to me. I do wonder, though, if that's just because I'm accustomed to Greenlet. Given that SACC has adopted the name, and most of the species are "theirs", I suppose I'll go along with that decision and vote YES.

YES. The genetic data are strong, even when the plumage and song do not match with the phylogeny.

YES. I'm more than willing to follow SACC, particularly for a group which I know little

about. Obviously, it would be ideal if the vocal data would fit in with the genetic divergence. I have no strong opinion on what English names are given but am inclined to defer to SACC on this, so will vote for Ochre-crowned Brownlet.

YES. As others have mentioned, there's some additional data that would provide more clarity, but this is largely an SACC issue. I like Ochre-crowned Greenlet for the English name.

YES. Strong genetic evidence; other evidence not as strong. Mostly an SACC issue. I prefer the name Ochre-crowned Brownlet, following SACC. .

2026-B-6
Treat *Leiothlypis ridgwayi* as a separate species from Nashville Warbler L. *ruficapilla*

YES. First, I don't believe there is any contact zone in their respective breeding ranges. Nashville Warblers of any type are rare in Alberta. Subspecies *ridgwayi* does occur rarely in the southwest part of the province west of Calgary in what could be called the eastern slope of the Rockies, while presumed *ruficapilla* has appeared (singing birds) on territory northeast of Edmonton. Basically, *ridgwayi* breeds only to the west slope of the Rockies to southeast British Columbia and *ruficapilla* breeds west only to about central Saskatchewan and they are only rather numerous in the eastern portions of the province (e.g. Duck Mountain Provincial Park). Ralph Browning, author of an earlier motion to split these two, complained about this. There is no contact zone to study. These taxa are allopatric. The populations are thus separated by some 500 miles.

The call notes are distinct, with *ridgwayi* sounding identical to Virginia's Warbler and very similar to Lucy's. Eastern Nashville has a duller, less sharp call. I hear little variation in eastern songs, but *ridgwayi* is variable with some versions sounding close to Virginia's songs (thus quite different from *ruficapilla*) while others are closer to *ruficapilla.* I am left wondering if *ridgwayi* is closer to Virginia's Warbler than Eastern Nashville. And like Virginia's, *ridgwayi* frequently bobs its longer tail unlike shorter-tailed *ruficapilla.*

Unlike allopatric *ruficapilla* and *ridgwayi* Nashville Warblers, Virginia's and *ridgwayi* Nashville are parapatric, if not sympatric, in the eastern Sierra Nevada. Virginia's is a very local breeder in the eastern Sierra Nevada of Inyo and Mono counties and more frequent in the "dry mountains" farther east, while territorial *ridgwayi* Nashville spill over from the west side of the Sierra Nevada where they are common. Studies for contact zones of these two would likely be more productive than looking for any contact zone between the two Nashville subspecies. These two Nashvilles are strongly allopatric from each other.

In some ways, not splitting these two is better as separating any stray would be problematic unless in the hand where measurements could be taken. It is interesting that of two Alaska records, substantiated with specimens, one from Middleton Island is *ruficapilla* and one from St. Matthew Island in the Bering Sea is *ridgwayi.*

I voted for this split with Browning's previous motion and my views haven't changed. As for English names, I like Calaveras Warbler, as the species is a common breeder on the west side of the Sierra which is much of Calaveras County. Having a real species as opposed to the county known for its "jumping-frogs" would be good. The AOU also used this English name through the 4th edition of the Check-List. And Nashville well reflects a good stop over, namely it is rare east of the Appalachians as a migrant, particularly in spring. They are common in Tennessee, Kentucky, Ohio, etc. Subspecies *ruficapilla* is a circum-Gulf migrant.

In reading over the comments, subspecies *ridgwayi* is more Pacific (CA to S British Columbia, W Montana and N Idaho), so "Western" would be a poor choice for the English name. I believe in giving long-established English names (like Calaveras Warbler) weight and that county is in the heart of its breeding range, even a county that ends at the Sierra Nevada rather than extending to the east side where Calaveras Warbler is much scarcer, particularly as a breeder. I'll add that the breeding range of *ridgwayi* Nashville rather closely matches that of *Vireo cassinii.*

YES. I voted to separate these two allopatric forms in 2017 and my position is only strengthened by the current proposal. The two forms are allopatric and differ in plumage coloration, morphology, genetics, behavior (tail-bobbing in *L. r. ridgwayi*), and vocalizations (especially call note). There is at least some evidence of response to playback. Level of mtDNA divergence is similar (2.0%) to that seen among other species of warblers. Genomic-level studies show no evidence of admixture as well as different demographic histories for each subspecies. Although some of the comments indicate the need for genetic samples closer to the contact zone, these two subspecies don't really come into contact and genomic variation in *L. r. ridgwayi* was not related to geography. The only study to include multiple individuals of both species in a phylogenetic analysis remains the mtDNA study of Klein et al. (2004). In contrast to what is stated in the proposal, this study found the two forms are not just paraphyletic but polyphyletic. The proposal also implies that the mtDNA study of Lovette et al. (2004) contrasts with this pattern and found the two subspecies were monophyletic with respect to other warblers. However, Lovette et al. (2004) does not address phylogenetic relationships. Lovette et al. (2004) was using mtDNA to separate subspecies of warblers from each other and didn't include a phylogenetic analysis of other closely related warblers. The fact that the two subspecies are reciprocally monophyletic with respect to each other in Lovette et al. (2004) agrees with the Klein et al. (2004) study. Weir and Schluter (2004) show paraphyly, but they only included one other related species (*virginiae*), so it doesn't address the possibility of polyphyly. All that said, my decision is not based just on the mtDNA. Given the sum of the evidence (including genomic DNA, behavior, call note, distribution), it makes more sense at this point to separate these two forms than continue to consider them one species. If we were to decide to split, I would prefer providing new names for each.

NO. This proposal sums up the issue nicely, and I agree that for now it's best to retain the status quo of subspecies rather than full species. The strongest separation is with the genetic data, but the sampling was limited to New York (*ridgwayi*) and Washington plus n=1 from Oregon (*ruficapilla*), so there is no sense of how the two subspecies vary

genetically across their respective range.. Furthermore, the playback responses were asymmetrical, with *ridgwayi* showing no song discrimination. Further work is needed to justify splitting these taxa. If the split passes, then Calaveras Warbler and Nashville Warbler are suitable names.

NO. Having heard about this issue for some years now, I expected the data to be more convincing. The difference in call notes, which seems striking on the limited sample I checked, certainly suggests two species, but it seems to be a case where a formal analysis is needed to validate the differences. Other lines of evidence are more equivocal; the differences in song do not seem that obvious, and the birds themselves exhibit puzzling, asymmetrical responses to playback. Thus I agree with the recommendation that, given the lack of clarity in this situation, more study is needed. However, it is not necessarily the case that further study will result in information consistent with a two-species treatment.

It does not seem likely that we will have to deal with English names at this time, but if we do I think that serious rethinking of the names by way of a proposal would be needed. Calaveras is just a county in California, and Nashville is just a stopover! If they are indeed split, why not take the opportunity to finally come up with more suitable names? There is some breeding range size asymmetry but not so much as to warrant retention of Nashville Warbler for just the eastern population, especially as this is such a common and familiar bird in western North America. And Western Nashville Warbler? That is such a nonsense name. I'm not sure what would be better, though, as there are so many warblers and good names are at a premium.

NO. The available playback data quite strongly suggest to me that these are the same species. Although they test only the pre-zygotic barrier, they indicate that western birds, at least, do not distinguish between hetero- and homo-specific songs. Even the discrimination for the eastern birds has some overlap. The calls do seem somewhat divergent, and the plumage is subtly different. However, I would want to see data from nearer the contact zones, and hopefully even including the *ruficapilla-virginiae* contact zone (although they seem to be largely separated by the Snake River Plain).

NO. The evidence in the proposal is not very strong.

NO. Nice proposal, I agree that it is better treated as a single species given the data on hand.

NO. It certainly seems like something interesting is going on with these taxa, especially with the very distinctive all notes, but as the proposal lays out, I think we are far from adopting a species split for them at this point.

2026-B-7
(a) Recognize the genus *Nannus* for three species of wren

YES. I support the proposal of recognizing *Nanus* and agree that alternative options are

worse. At some point we might want to consider including adding subspecies *pallescens* from the Commander Islands to Pacific Wren and taking it out of Eurasian Wren. I believe Kevin Winker has commented that it is closely related to Aleutian birds.

YES. I support recognizing the genus *Nannus* that includes three species currently in *Troglodytes* (*T. troglodytes, T. pacificus and T. hiemalis*) for reasons given in the proposal. I agree with the recommendation that recognizing *Nannus* is preferable to expanding *Troglodytes* to include all of *Troglodytes* s.l., *Thryorchilus*, *Cistothorus*, and *Ferminia*.

YES. I agree with the recognition of *Nannus* rather than the alternative of creating a greatly expanded *Troglodytes*. My main objection to the latter course would be the inclusion of *Cistothorus* in the same genus as all the others, thereby creating a genus more morphologically disparate than most of those in the Troglodytidae.

YES. A rare case where I actually prefer further breaking up a genus rather than moving to merge multiple groups together. In this case, I think *Cistothorus* deserves recognition as a genus separate from *Troglodytes*. While there is still some uncertainty around the exact relationships of *troglodytes*, *hiemalis*, and *pacificus*, they do seem to consistently fall outside of *Troglodytes*, so I do think it is most appropriate to make this change (and one that seems long overdue).

YES. The phylogenetic data are convincing that *Troglodytes*, as currently defined, is paraphyletic. The genomic sampling could be better, but the findings seem consistent across at least some nuclear loci. Like the other committee members, I am not in favor of subsuming *Ferminia*, *Cistothorus*, and *Thryorchilus* into a very broad and old *Troglodytes* just to retain *"Nannus*" in *Troglodytes*. It truly pains me to lose *Troglodytes troglodytes* though! Are we sure that the subsequent designation of *aedon* as the type species for *Troglodytes* was done correctly? It does seem like an odd choice, when *troglodytes* was right there.

YES. I agree with the proposal, to recognize the genus *Nannus* and include the three *Troglodytes* species: *T. troglodytes*, *T. pacificus* and T*. hiemalis*.

YES. Recognizing *Nannus* seems the best solution to the paraphyly present among the currently recognized taxa.

YES. Recognizing *Nannus* would be the least disruptive option.

2026-B-7
(b) Subsume *Thryorchilus* into *Troglodytes*
YES. 1 without comment.

YES. The molecular data support merging *Thryorchilus* into *Troglodytes.*

YES. Given the unequivocal genetic evidence presented in the proposal that *Thryorchilus* is a close sister to *Troglodytes sensu stricto*, or even embedded therein, it seems we should also address the issue of its continued recognition. Shouldn't there

have been three voting options: lump all into *Troglodytes*; recognize *Nannus* and all the other genera; and recognize *Nannus* but lump *Thryorchilus* into *Troglodytes*? The genus *Thryorchilus* Oberholser, 1904 was erected for *browni* on the basis of its short tail with only ten rectrices (vs. 12 in others), as well as its proportionately larger feet. Given that we treat e.g. *Gallinago delicata*, with its 16 rectrices, in the same genus as other snipes with up to 26 (!) rectrices, I don't think that the basis for recognition of *Thryorchilus* is very defensible. According to Dickinson & Christidis (2014; https://www.aviansystematics.org/checklist?viewfamilies=174#footnote_53), *Thryorchilus* wasn't even recognized by AOU until the 1983 *Check-List*.

YES. Based on the available data, it does make most sense to merge *Thryorchilus* back into *Troglodytes*.

YES. Thanks to Martin Stervander for the reanalysis of the available genetic data. Those data suggest that *Thryorchilus* is either embedded within *Troglodytes* or sister to it with shallow divergence. Given that it is behaviorally and vocally quite similar to some species in *Troglodytes* (although an analysis of these characters would have been good), I'm comfortable merging *Thryorchilus* into *Troglodytes*. If anyone is interested in the original diagnosis for *Thryorchilus*, it is here: https://www.biodiversitylibrary.org/page/7730154#page/232/mode/1up

Based on the available phylogenetic data, *Thryorchilus* should go at the start of *Troglodytes* in the linear sequence, followed immediately by *rufociliatus* and *ochraceus*, and *Nannus* should go right before *Cistothorus.* However, I just compared the tree in Imfeld et al. (2024) to the linear sequence in our checklist, and our genus-level sequence is wildly off from what Imfeld et al. (2024) found. Our current sequence is: *Salpinctes, Microcerculus, Catherpes, Hylorchilus, Pheugopedius, Cantorchilus, Henicorhina, Cyphorhinus, Uropsila, Thryophilus, Campylorhynchus, Thryomanes, Thryothorus, Troglodytes, Ferminia, Cistothorus, Thryorchilus*. If the changes in the current proposal are adopted, and following the data from Imfeld et al. (2024), I believe that the linear sequence should be: *Salpinctes, Catherpes, Hylorchilus, Microcerculus, Thryomanes, Thryothorus, Campylorhynchus, Nannus, Cistothorus, Ferminia, Troglodytes, Uropsila, Pheugopedius, Cantorchilus, Henicorhina, Cyphorhinus, Thryophilus*. This is also consistent with the earlier data from Barker (2017), except for the positions of *Microcerculus* and *Uropsila*. Also, *Thryophilus rufalbus* should go after *Thryophilus pleurostictus*, and the linear sequence of species within *Campylorhynchus* needs a complete overhaul.

YES. The molecular data support merging *Thryorchilus* into *Troglodytes.*

YES. Genetic evidence doesn't support a separate genus.

YES. Based on the genetic evidence this is warranted and I don't see a compelling to maintain the genus *Thryochilus.*

2026-B-8
Treat West Indian Woodpecker *Melanerpes superciliaris* as two or more species

YES. As in the proposal, a strong YES to the split of *caymanensis*, and a somewhat less convinced YES to the split of *nyeanus*, for which less is known.

I prefer Grand Cayman Woodpecker for *caymanensis*, as it is limited to that island, but Cayman Woodpecker is OK too as it is unambiguous and echoes the specific epithet. San Salvador Woodpecker would be fine if *nyeanus* passes. If the *caymanensis* split passes, and whether or not *nyeanus* is split, I think West Indian Woodpecker is OK (but not great) for the remaining taxa, which still occupy the bulk of the range of the complex. In case anyone is considering "Greater Antillean Woodpecker", the Bahamas are not technically part of the Greater Antilles, so that would not be a good option.

YES. A weak/tentative YES, although I am curious to hear others' thoughts on this. While the Grand Cayman population is indeed the most distinct, given the variation in the group, the differences do not necessarily seem to be at the species level. However, I am ultimately swayed by the vocal differences and the size differences between *caymanensis* and the other taxa. The vocal and other differences between the other taxa are definitely not enough for me at this point to vote for additional splits within this complex. For English names, I have a slight preference for Cayman Woodpecker over Grand Cayman Woodpecker, but would be fine with either depending on how the rest of the committee votes. I definitely prefer retaining West Indian Woodpecker for the other species.

Addendum after reviewing AviList votes: I am sticking with a weak/tentative YES. I am not swayed by the argument against splitting by the shallow genetic divergence, as I think using divergence, especially for insular taxa, is misleading and not useful. I am also not concerned about waiting to study the other taxa in the system, as I feel perfectly comfortable splitting out *caymanensis* first while awaiting further study of the other taxa. I understand the concern about the small sample sizes and potential for relying on too few individuals in the vocal analyses of *caymanensis*, but those concerns aren't enough to fully sway me against voting for the split.

YES. I was strongly in favor of both *nyeanus* and *caymanensis* while reading most of the proposal, until I got to the genetics section which gave me pause. The only tree that sampled multiple taxa in the complex (*caymanensis* and *superciliaris*) shows extremely low genetic divergence. I can kind of reconcile this with it being just a few loci, and I'm guessing likely dominated by the mitochondrial signal. That said, of the other "*Centrurus*" woodpeckers, only *hoffmannii* and *rubricapilla* are as messy in the tree, but those two form a hybrid zone in central Costa Rica so gene flow would be expected here. I doubt that ongoing gene flow is happening in the *superciliaris* complex, so it seems reasonable to conclude that these are extremely recently diverged taxa (at least *caymanensis* and *superciliaris*). I'm fine splitting recently diverged species, especially with small, isolated island populations where founder effects and rapid sweeps could accelerate speciation, and this seems to me to be one of those cases. I'm very much on the fence, though, and if other committee members present good arguments for maintaining the one-species treatment, I could easily be swayed to that position. To me, the plumage, vocalization,

and size differences between *superciliaris* (including *blakei* and *bahamensis*)*, nyeanus*, and *caymanensis* are on par with those of other "*Centrurus*" woodpeckers.

As an aside, based on the Shakya et al. (2017) tree, it looks like we need to rearrange the linear sequence of *Melanerpes* (at least for *erythrocephalus*, *radiolatus*, *uropygialis*, *striatus*, and *superciliaris*).

For the English common names, I am strongly in favor of Grand Cayman Woodpecker for *caymanensis*. The only other two Cayman Islands endemic birds that we rank at the species level are both endemic to Grand Cayman Island, and both have the English common name of "Grand Cayman" (Grand Cayman Thrush *Turdus ravidus* and Grand Cayman Bullfinch *Loxigilla taylori*). So, for consistency, I advocate for Grand Cayman Woodpecker for *Melanerpes caymanensis*. If *nyeanus* is split, I will vote for San Salvador Woodpecker. For the remaining birds, I think it's most prudent to keep West Indian Woodpecker, as the two taxa being split are extremely small, isolated populations, and I can't think of a better name for *superciliaris*. I am not in favor of using Cuban Woodpecker for *superciliaris* if it includes the northern Bahamian populations. We already have enough "Cuban" birds that are also found in the Bahamas, so there's no need to add to those misnomers.

Addendum after receiving AviList votes: YES to *caymanensis*, NO to *nyeanus* (originally voted in favor of this split). I understand the hesitation here, especially with *nyeanus*, but I keep coming back to the striking visual differences between *caymanensis* and *superciliaris*, especially in the head pattern, which is the main plumage difference separating species in the "*Centrurus*" clade of *Melanerpes* woodpeckers. That, combined with the broad buff back barring, gives *caymanensis* a very different appearance that I find hard to reconcile with them being the same species. The lack of a big vocal sample size doesn't concern me, as vocal differences in this clade don't seem to matter too much for species limits. At least in the species pairs that hybridize (*carolinus*/*aurifrons* and *hoffmannii/rubricapillus*), I struggle to detect differences in the primary vocalizations. So, the fact that there are any vocal differences at all points to these being species. That the vocal differences are correlated with body size is of course expected. I fully understand those wanting to wait for more or published data, but I lean towards making taxonomic changes with the presently available data, instead of waiting for the possibility of future improvements in data quality or extent. The lack of genomic data is concerning, but hasn't stopped us from making changes before.

To answer the question of another committee member, the "*Centrurus*" clade is fully embedded within *Melanerpes* (Shakya et al. 2017), and would require splitting Melanerpes into three genera (*Melanerpes s.s.,* a monotypic genus containing *M. striatus*, and an expanded *Centrurus* that includes also the species in the old genus *Tripsurus*).

YES. I agree to splitting off *caymanensis,* and also to Grand Cayman Woodpecker as it follows other cases as pointed out and it is confined to Grand Cayman. It's nice to state where they are in the Caymans, one island only. NO to splitting the San Salvador *nyeanus.* More study is needed of this rather poorly known taxon. If further splitting takes place, it is my belief that Cuban nominate *superilliaris* will be the one to split. The wing

spreading and wing flashing for courtship/aggression is such a frequent occurrence and seemingly quite different from Bahama taxa. My experience is only with *blakei* from Great Abaco and from decades ago, but I don't recall seeing this, and I see it on a near constant basis from Cuban birds. More importantly, Bruce Hallett who visits the Bahamas frequently, hasn't noted that behavior. At present, I favor retaining West Indian Woodpecker as the English name for the combined Cuban and Bahama subspecies.

Updated comments (17 July 2026). I looked at photos of Cuban nominate *supercilliaris* and Grand Cayman *caymanensis* and it is hard to reconcile them being the same species, especially with regards to the other "*Centrurus"* woodpeckers where some species are far more similar than these two isolated taxa are. Again, there is no rationale for the lump, so the bar for restoring what was, separate status, for me doesn't have to be necessarily as high. I continue to believe that behavioral studies will show distinct differences. It would be nice if someone could study the Bahama and Grand Cayman taxa in relation to the nominate Cuban subspecies.Take a movie camera. That said, I can appreciate the need to have more detailed studies, but please add behavior into the mix. As an aside, is there a good genetic rationale why *Centrurus* does not belong in its own clade? Vocally and morphologically they sure look like a large group from a clade. And we are asked to believe that *Melanerpes lewis* is also part of that genus. Really? I've read Short's comments (under Relationships and Classification, p. 43), and I'm left unsettled.

YES. Plenty of evidence to split *cayamensis* from others. Grand Cayman is the most appropriate name and matches designations of other endemics on the island. NO on the others; more information is needed before making other splits.

NO. 1 without comment.

NO. I originally voted in favor of splitting *cayamensis* and prefer Grand Cayman Woodpecker due to its endemism. NO to the others - I would rather see a quantitative study to fully address the putative intraspecific vs interspecific variation in this system. I'd also like to see more population genetic data and rigorous pop gen analyses. While they are allopatric I still think it is informative and helps to contextualize the evolutionary history of what seems a pretty complex system.

Addendum after reviewing AviList votes: In reading the Avilist comments and reconsidering the proposal, I am swayed that this is better treated as a system where sufficient data are currently lacking to treat *cayamensis* as a species. I agree that a more formal, peer-reviewed analysis that fully considers phenotypic and genetic data is the best way to go in what is generally perceived as a complex, border-line case of species delimitation.

NO. This is a complicated proposal where the taxonomic situation is not as clearcut as I would like. I originally voted to accept the recommendation of the proposal to split *caymanensis* based primarily on vocal differences as presented. However, in further consideration I think that this is a situation that would benefit from more formal study with published data on morphological, vocal, and genetic variation before making a decision. It would be nice to see a real quantitative study of vocal variation along with more

genetic sampling and whole genome sequencing. If the proposal passes, I prefer Cayman Woodpecker (simpler and used historically) but Grand Cayman Woodpecker is acceptable too. If the proposal to split *nyeanus* also passes, then San Salvador Woodpecker is fine but I'm open to other suggestions.

NO. I suggest waiting for published genomic data and more information on this complex system.

NO. I vote NO to separating *Melanerpes caymanensis* from *M. superciliaris*. I consider the phenotypic data to be borderline and to suffer from some of the reservations raised by other voters, including the duplication of the small body size of *caymanensis* relative to *superciliaris* in other subspecies of *M. superciliaris* (Fig. 3 of Silva Núñez 2019 indicates that *bahamensis*, *nyeanus*, and *blakei* are as small or smaller than *caymanensis*); the effect of body size differences on the higher pitch of the *kwirr* and other calls of *caymanensis* (and in the other small subspecies, which are not proposed for splits); pseudoreplication in the vocal data for *caymanensis* (multiple strophes of the *kwirr* call were used from the same individuals due to the lack of recordings of sufficient numbers of individuals); and variation within subspecies, apparently including *caymanensis*, in the amount of black on the face. The tiny genetic divergence puts all the weight on the phenotypic characters (and makes the burden seem greater than if there were no genetic data at all). My general feeling is that in a complex borderline case like this the data should be formally analyzed and published to enable better evaluation of the evidence. The proposal is extremely thorough and does a remarkable job of presenting the available data, much of which was gathered expressly for the proposal, but the lack of published information (and the anecdotal nature of some of it) is also a weakness of the proposal.

2026-B-9
Treat Russet-crowned Motmot *Momotus mexicanus* as two species

YES. The combination of phenotypic and vocal differences along with the lack of rationale for lumping lean me toward accepting this split. Guatemalan Motmot and Mexican Motmot are appropriate names.

YES. Based on morphology and voice, taxon *castaneiceps* seems on a par in terms of degree of differentiation within the former broad *Momotus momota* complex, now treated as multiple species. In addition to the head plumage differences, *castaneiceps* is quite a bit larger overall and in wing length than is *mexicanus*, contra the usual ecogeographic pattern in size variation. The vocal differences, although subtle, are reminiscent of those shown by *Melanerpes* [*superciliaris*] *caymanensis* in differing in frequency and pulse rate.

The English names Mexican Motmot for *mexicanus sensu stricto* would be most appropriate, given its wide distribution in Mexico; though it is not the only motmot in Mexico, and it is not quite endemic to Mexico, while another species is actually endemic to northeastern Mexico, *mexicanus* is much the most widespread motmot in the country,

and the name Mexican Motmot would echo the specific epithet. Another, more specific, option would be West Mexican Motmot, which would be a close second in my view. Guatemalan Motmot would be great for *castaneiceps*, as it is truly endemic to that country.

YES. After thoughtful consideration, I support the recommendation in the proposal. The phenotypic characteristics presented (plumage coloration, morphological measurements, vocalizations) show how *Momotus m. castaneiceps* is consistently different from the other three recognized subspecies within *M. mexicanus* (*vanrossemi*, *mexicanus*, and *saturatus*). The degree of differentiation among the three northernmost subspecies is less than the differentiation of *castaneiceps* vs. the three subspecies. Geographically, *castaneiceps* inhabits the valley of the Río Motagua, a dry valley that is isolated from other dry valleys by the mountain range "Sierra de las Minas". Some other dry valleys are located between the southern range of *M. m. saturatus* and *castaneiceps*, but Sierra de las Minas could be acting as a strong barrier confining *castaneiceps* to the valley of the Río Motagua. Additionally, the split would align with the original consideration of *castaneiceps* as a separate species (which was merged with *mexicanus* without rationale) and with previous splits in Momotidae (e.g., *Momotus momota*). The English names suggested by the authors of the proposal seem like great options, Mexican Motmot for *M. mexicanus* and Guatemalan Motmot for *M. castaneiceps*. If this proposal is approved, we would have one extant endemic species in Guatemala!

YES. This is definitely a tricky case because the differences don't seem *that* big between the taxa, but these two groups do indeed fit the pattern in the context of other motmots that have recently been split. These differences, plus the biogeographic isolation of *castaneiceps*, support its recognition as a separate species. I like the English names Mexican and Guatemalan Motmot for *mexicanus* and *castaneiceps*, respectively.

YES. I support this split but with reservations. Like other members have said, this is a borderline case. I find the plumage differences to be more convincing than the vocal differences, and those plumage differences are on par with those of other motmots that we now consider separate species. The vocal differences are very subtle; with both members of the duet giving high- and low-pitched calls in each taxon, I hear a lot of overlap in pitch when listening to recordings. These differences are certainly not the same degree of vocal divergence between, for example, members of the "Blue-crowned" motmot complex. I like the proposed English common names.

YES. I think the morphological and song data are sufficient to *recognize Momotus casetaneiceps* as separate species from the *Momotus mexicanus* complex. We currently have genomic, coloration, and morphometric data for this complex and hope to publish them soon.

YES. The proposal is convincing, particularly considering other recent splits within motmots. Thanks to another committee member for the details on the geography in Guatemala. I think the alternate suggestion of West Mexican Motmot has merit as it is the only motmot in West Mexico.

YES. I support this split based on plumage differences and to some extent morphology

and vocal differences as well as geography. If the unpublished data show a different pattern, I am fine with waiting. Mexican and Guatemalan Motmot are appropriate names.

NO. While this may be a solid split, I'd prefer to wait for the integrative dataset alluded to above to have a better quantitative assessment of genomic and phenotypic variation in the complex.

2026-B-10
Treat Tufted Flycatcher *Mitrephanes phaeocercus* as two or three species (*note that the English names section of this proposal was superseded by 2026-C-3*)

YES to A and B (3 species). Thanks for all of the detail in this proposal. The combination of plumage and vocal differences supports the proposed split into three species. It would be great to get genetic data with good sampling across these populations. I think we need a separate proposal regarding English names. I do agree, however, that Tufted Flycatcher or Tufted-Flycatcher should be part of the name.

YES to A and B (3 species). It's hard to believe that this issue has gone unaddressed by NACC all this time! Although there was a proposal (2018-A-8) that specifically addressed the split of *olivaceus* (following SACC, which had always treated this as a species), there was no mention or discussion of splits within the remaining *phaeocercus* complex at that time. The "position note" vocalizations of the northern *phaeocercus* group are simply extraordinarily different from those of the *aurantiiventris* and *berlepschi* groups. Even if they didn't differ morphologically or in any other way, that alone would be disqualifying in a suboscine for conspecificity, had it been known to Webster or other taxonomists. There are strong plumage differences as well, way out of line with what we see within universally recognized flycatcher species. The vocal differences between the *aurantiiventris* and *berlepschi* groups are less profound, but still beyond the usual for flycatcher species.

English names: Unlike so many cases with suboscines and other mainly drab-plumaged birds, there seems to be an abundance of pretty good names available for this complex. I could go for the range-based names Mexican (although *phaeocercus s.s.* occurs south to Nicaragua), Costa Rican (although *aurantiiventris* occurs widely in Panama too), and Choco (for *berlepschi s.s.*). Plumage-based names, e.g. Tawny (for *phaeocercus s.s.*) and Yellow-bellied (echoing its specific epithet, *aurantiiventris*) would be OK but the latter name would also describe *berlepschi*, for which no other obvious plumage-based name comes to mind. The habitat-based names suggested in the proposal could work, too. Whatever the case, I do think it would be ideal to use names for all the daughter taxa that contrast with each other and therefore provide maximal informational value. For starters I'll vote for the geographic names. I agree that the group name Tufted-Flycatcher should be used, and that it should be enacted for Olive Flycatcher, too, which otherwise gets lost in a plethora of similar-looking birds in multiple families.

YES to A and B (3 species). This is a thorough and very detailed proposal, and while I would like to see genetic data, I don't think it is necessary to actually vote on this split as

I don't expect it would actually change anything. The combination of plumage and vocal differences, together with habitat differences (especially striking for *berlepschi*), support the split. To me the split of *aurantiiventris* and *berlepschi* is possibly the weakest, but I think the vocal differences are probably enough to warrant species status (especially when all the call types are taken together), and the ecological differences of *berlepschi* again are striking and suggest the two are sufficiently divergent.

As for names, I also prefer retaining Tufted Flycatcher/Tufted-Flycatcher for the complex (and if possible adding it back to *olivaceus*); while we'll probably need a separate proposal, I prefer the habitat-based names for these three species (e.g., Pinewoods, Cloud-forest, and Choco), but geographic names would also be okay (e.g., Mexican, Costa Rican, and Choco).

YES to A and B (3 species). I agree with the proposal, even though genetic data are lacking.

YES to A and B (3 species). This proposal was detailed but very easy to follow, thanks! Genetic, plumage, and strong vocal difference support separation of *M. phaeocercus* from the others. Plumage, moderate vocal differences, and to some extent habitat differences support separation for *M. auratiiventris* and *M. berlepschi*. A three way split would better align how these taxa are treated to how other species of flycatchers are split. An additional proposal is needed to deal with English names.

YES to A, NO to B (2 species). I have lots of field experience with the *phaeocercus* and *aurantiiventris* groups, and the differences between these two groups, especially for a suboscine, are massive. The plumage, size, shape, position note, and dawn song all clearly point to them being distinct species. The position note and primary song of *olivaceus* are similarly diagnostic and clearly point to *olivaceus* being a distinct species (not that we're voting on it here, just as a matter of comparison). For a suboscine, the level of divergence certainly doesn't have to reach that high a bar, but in the case of *aurantiiventris* versus *berlepschi*, I'm just not hearing that big of a vocal difference. The position notes have basically the same shape, but seem to differ somewhat in pitch. However, there seems to be some variation here in the recordings that I'm listening to. For example, this position note recording of *aurantiiventris* from the eastern edge of its range (https://macaulaylibrary.org/asset/302766) sounds strikingly similar to that of *berlepschi* from Valle de Cauca (https://macaulaylibrary.org/asset/264163). It is certainly not as different as *olivaceus* and *phaeocercus* are.

For the dawn song, *aurantiiventris* and *berlepschi* sound like they have basically the same overall pattern, just a little bit slower in *aurantiiventris*. Certainly, differences in note pace in a suboscine can be important, but it's just not that striking to me. For example, compare this recording of *berlepschi* (https://macaulaylibrary.org/asset/90360501) to this one of *aurantiiventris* (https://macaulaylibrary.org/asset/184565).

Likewise, the plumage of *aurantiiventris* and *berlepschi* is broadly similar, with the tawny chest and yellow belly. Yes, the chest is paler in eastern Panama (https://macaulaylibrary.org/asset/460996591) but it's darker again in the Chocó of Colombia (https://macaulaylibrary.org/asset/634196572) and then paler elsewhere in

Colombia, like in Antioquia (https://macaulaylibrary.org/asset/639339113). So, I'm not even confident about where to draw the boundaries between these if we consider them to be species based on plumage. Certainly, *aurantiiventris* does average darker and with more contrast between the tawny and olive of the head, but given the variation in the *berlepschi* group, *aurantiiventris* could perhaps fit within that range of variation. Given also that we lack genetic data for *aurantiiventris*, and those are presumably forthcoming in the next round of suboscine sequence data, I would prefer to wait on this split.

Regarding the English common names, I prefer a habitat or plumage-based name as there is actually some variation in these characters (unusual for a “drab” flycatcher). I do agree with the proposal that “Tufted-Flycatcher” is preferable to distinguish them from the many other flycatchers. That does mean that the name could become a mouthful, but I’m ok with that in this case. For *phaeocercus*, I could live with Pinewoods, Pine, or Tawny (Mexican is inaccurate, as it extends south through half of Central America). For *berlepschi* + *aurantiiventris*, Yellow-bellied or Tawny-chested are good choices. I’m not a fan of Cloud-forest, as that biome encompasses the range of the entire species complex, not just *berlepschi* + *aurantiiventris.* If *olivaceus* is Olive Tufted-Flycatcher, then Southern doesn’t make much sense for *aurantiiventris* or *berlepschi*, as they are the “central” species. Below are my ranked votes: *phaeocercus*: 1 Pinewoods, 2 Tawny, 3 Pine, 4 Pine-forest, 5 Cinnamon, 6 Northern; *aurantiiventris* (incl. *berlepschii*): 1 Yellow-bellied, 2 Tawny-chested, 3 Cloud-forest, 4 Southern. Although it's outside our area, I do like Olive Tufted-Flycatcher for *olivaceus*.

YES to A, NO to B (2 species). I need to look at this more and wonder about SACC thoughts. I do feel that “Tufted” needs to be retained in the English name. I wondered about Talamacan Tufted Flycatcher but the range in Costa Rica extends to other ranges to the northwest and with the settled two species treatment for now extends south into Ecuador. I’d like to see more discussion on English names.

Update on 1 July 2026 - I have carefully reread the proposal and the comments of NACC members. I am in full agreement about splitting the northern *phaeocercus,* particularly given the striking “position note” differences. Too often year-round (?) contact calls aren’t given sufficient consideration. I’m now much more uneasy about splitting *berlepschi* from *aurantiiventris*. The most compelling reason to wait is that the situation in easternmost Panama is unsettled. In particular what to do about that ‘undescribed’ population from Cerro Tacarcuna which are morphologically closer to *aurantiiventris* unlike the birds on Cerro Pirre which are morphologically closer to typical *berlepschi*? With reference to the eastern end of the range of *aurantiventris* (per another committee member’s comments), are we speaking of Cocle and western Panama (Cerro Campana) provinces or possibly a bit further east? These latter birds were recognized as a separate subspecies *vividus* which apparently is now widely treated as a synonym of *aurantiiventris.* I haven’t read the rationale for the lump, but if the vocalizations are more intermediate than perhaps that issue should be reconsidered. To conclude, given the unease about the vocal differences between *aurantiiventris* and *berlepschi* and the uncertainly about the population on Cerro Tacarcuna I think it is best to hold off on a three-way split at present, a conclusion that SACC might well reach too.

English names - I like keeping Tufted Flycatcher in all English names and if it is a

two-way split for now, I like the suggestion of Northern Tufted-Flycatcher and Southern Tufted-Flycatcher.

YES to A, NO to B (2 species). While this proposal is very in-depth and I appreciate the work that has gone into it, I don't feel comfortable splitting *aurantiiventris* and *berlepschi* without a more integrative data set that looks at population genetic data. I agree that a more in-depth discussion of the English names is merited.

YES to A, NO to B (2 species). I vote YES to separating *Mitrephanes aurantiiventris* from *M. phaeocercus* based primarily on the striking differences in vocalizations, but NO to treating *berlepschi* as a separate species. The vocal evidence for the distinctiveness of *berlepschi* is weaker, there is no genomic data to bolster the case, and the question of how to treat the Tacarcuna population in Panama needs to be resolved.

2026-B-11
Subsume *Leiothlypis* into *Oreothlypis*

YES. While this would be flip-flopping on a fairly recent decision, I think the split of the genus wasn't necessary. I'd prefer an expanded *Oreothlypis* in this case where the monophyly of these two clades is well-supported (and always has been) to having a genus for only two species. While it would cause some instability, these treatments have not been very deeply entrenched, and it would be reverting to something we had changed only 7 years ago. Further, although these two clades are somewhat deeply split, I don't think the degree of plumage or song diversity is any different from what is already in *Setophaga*; plus, both *Leiothlypis* and *Oreothlypis* have very similarly shaped bills, being straight and very sharply pointed.

NO. Both options are reasonable based on the phylogenomic data (retain both genera or merge *Leiothlypis* into *Oreothlypis*) but the proposal gives a good rationale for maintaining the status quo.

NO. The relatively deep split between the two *Oreothlypis* species and the *Leiothlypis* clade, plus the relatively bright plumage and buzzy songs of the former versus the drabber plumage and chippier songs of the latter, lead me to agree that it is best to retain the two-genus treatment, which is also favored by the stability argument.

NO. I agree with the proposal, the stability of classification is better to support.

NO. Reasons are given in the proposal. *Oreothlypis* and *Leiothlypis* are both diagnosable, and I don't like the idea of bouncing back and forth between these two treatments multiple times since the 2009 supplement. If we're going to make this change, it should be unambiguously the correct one to avoid further reversals of this treatment. The Astral tree shown in the proposal and other trees in the publication all have some odd branch lengths in this area, especially with regard to the very short branch subtending *peregrina*. I'm sure the topologies are all correct for the nuclear trees, but I'm not as confident about those branch lengths. A time-scaled tree would perhaps help here.

NO. The previous comment states the case well. With the poor *Vermivora* we seem to be tampering with the taxonomy too often.

NO. The proposal summarizes the situation well and I agree with their recommendation.

NO. The new phylogeny still matches the generic classification with regards to these two genera. For stability, we should continue to maintain the two genera.

2026-B-12
Treat Fea's Petrel *Pterodroma feae* as two species

YES. This is mainly an Old World issue but the split is supported by a combination of genetic, vocal, and striking phenological (timing of breeding) differences. This also makes the treatment consistent with external authorities. Thus, I support replacing *P. feae* with *P. deserta* on the NACC list. Regarding English names, we should go with the names that are currently widely established - Desertas Petrel for *deserta* and Cape Verde Petrel for *feae*.

YES. Given the differences in breeding phenology and in migration strategy (long-distance migrant in *deserta* vs. short-distance, near-resident status in *feae*), species status would seem likely. Add to that the somewhat subtle vocal differences, as well as the considerable degree of allopatry, and species status seems to be the better treatment for *deserta* rather than retention as a subspecies of *feae*. Note also that *madeira*, with which *deserta* is nearly parapatric, is a high-montane breeder and also has a different breeding phenology despite occurring at almost the same latitude.

For English names, the well-established Desertas Petrel for *deserta* and Cape Verde Petrel for *feae* are obvious choices. If Desertas weren't already so well-established, I could go for "Bugio Petrel", especially as it doesn't breed on Deserta Grande, and also the fact that there is an unrelated Deserta Island in Faro, just off mainland Portugal.

YES. I agree with what others have said, and am especially swayed by the vocal, phenological differences, and migratory differences in splitting these two taxa. It seems to be becoming increasingly clear that many seabird taxa are not particularly deeply diverged, as temporal isolation seems to be able to happen very quickly without the accumulation of other differences, but which nevertheless lead to strong reproductive isolation. I like the names Desertas Petrel for *deserta* and Cape Verde Petrel for *feae*, following other global taxonomic authorities.

YES. I agree with the proposal. Data from song and genetics support this split.

YES. If this were a proposal for primarily North American taxa, I would be strongly in favor of considering all three as conspecific, but because this is primarily outside our purview, and it seems that all the other taxonomic authorities have adopted the three-species treatment, I will go along with that treatment in the interest of taxonomic agreement. To me, these are barely diverged subspecies, with minor differences in

plumage and morphometrics. Critically, the vocalizations are not drastically different, or at least if there are differences, they aren't very clear to me. To my ear, *madeira* sounds rather intermediate between *deserta* and *feae*, with the longer notes like *feae* and the upslurred ending of *deserta*. The breeding allochrony is interesting, but not on its own indicative of species rank.

YES. I agree with the previous comment but it's not our area so I am willing to go along with Old World authorities. As for the English name, I realize that I'm astray here but I'm wondering about Cabo Verde for the group of islands where they breed and which is now an island nation. The point in Senegal was known as Cape Verde, now it seems to be Cap Vert. There are no Cape Verde Petrels there. Anyway, I don't really propose that, but Cape Verde for the island group is antiquated.

YES. I vote in favor because it's mostly outside of NACC jurisdiction. They seem much more like subspecies or geographically distinct populations to me, but I'll defer to WGAC. Nice proposal!

YES. I support this based mainly on differences in time of breeding and the fact that these taxa are outside our purview. Yes to the names Cape Verde and Desertas.

2026-B-13
Treat Yellow Warbler *Setophaga aestiva* as two species

YES. I am on the fence on this proposal, given that more data are sorely needed as pointed out in the recommendation. However, there are clear differences between "Northern" and "Mangrove" warblers, especially in the song with non-overlapping PCA clusters and anecdotal evidence that *aestiva* did not respond to songs of *petechia*. This also makes NACC consistent with AviList, which also swayed my decision to accept the proposed split. Regarding English names, I prefer Yellow Warbler and Mangrove Warbler.

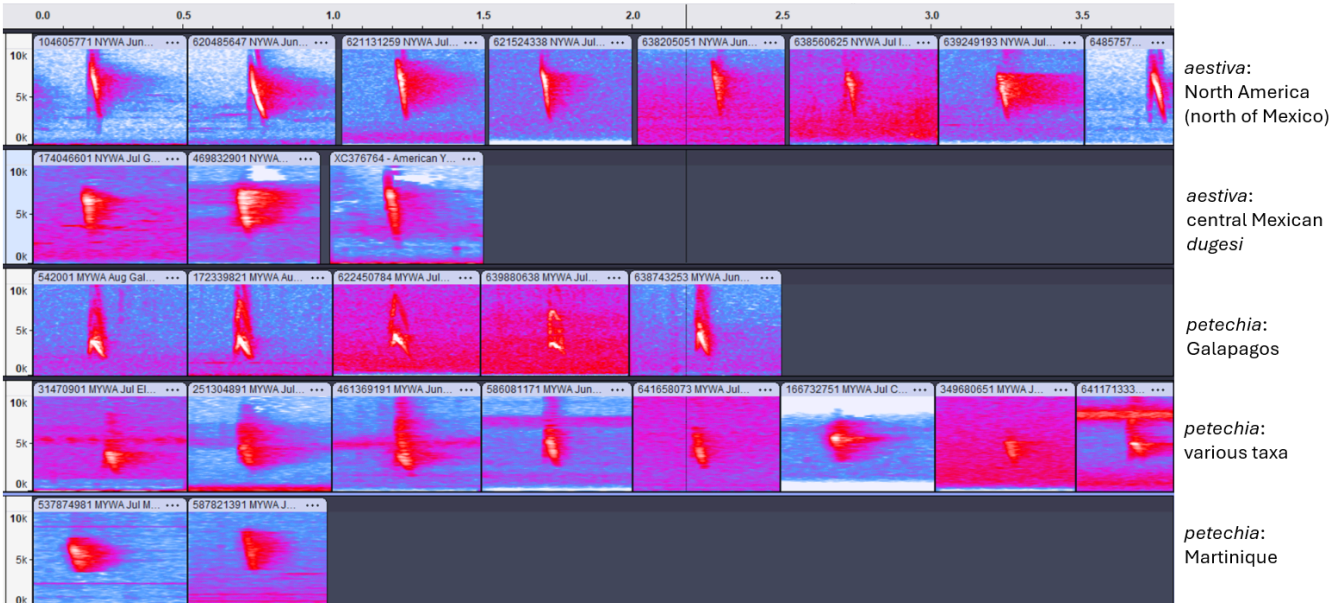
YES. The disparate distributions, morphologies, and ecologies of the various component taxa of the *petechia* complex render this case very complex, in contrast to the contiguous breeding distribution of the *aestiva* complex which is far more uniform in morphology and ecology (except perhaps on the Mexican Plateau). To sum it up, the *aestiva* group is broadly associated with freshwater wetlands and willows for breeding habitat, while most members of the *petechia* group are associated with mangroves, except on small islands where they may be more catholic in habitat choice. However, none of the populations of *petechia* penetrate far inland on larger landmasses, despite being broadly distributed along coasts from northern Mexico, southern Texas, and southern Florida to northern South America (although they do occur right along the Panama Canal).

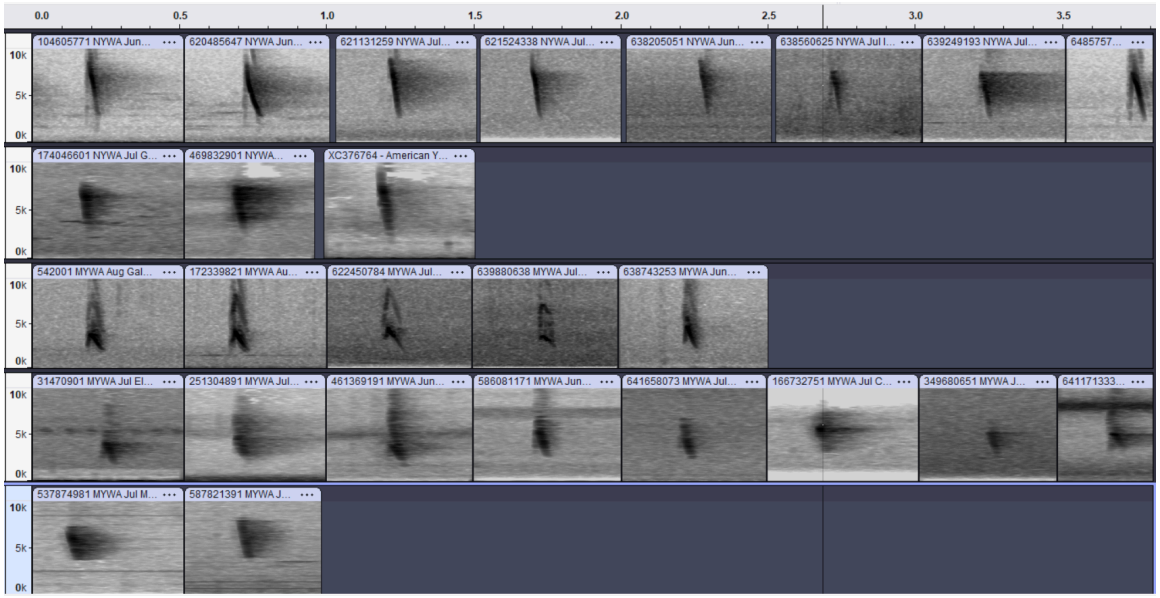
Furthermore, throughout the range of the *petechia* complex, *aestiva* winters commonly side-by-side with them in mangroves. In Mexico and northern Central America, several taxa (the *bryanti* group, referred to incorrectly as the *castaneiceps* group in the 2025

Clements revision, corrected in the 2026 draft) with distinctive chestnut-headed, hardly-streaked males occur in mangroves, while not far away in the adjacent uplands of Mexico, subspecies *sonorana* and *dugesi* are resident around wetlands and other wooded habitats (judging from June-July eBird photos). As far as I know there are no known instances of members of the *aestiva* group remaining to breed in mangroves anywhere in the vast range of *petechia* (if anyone does know of such cases, please correct!). It seems to me that this degree of habitat segregation is unparalleled among conspecific parulid warbler taxa.

The genetic data, while still less comprehensive than would be preferable, nevertheless demonstrate that the *petechia* group is sister to the *aestiva* group, despite the disparate plumages of taxa of the *petechia* group, which includes several taxa ("Golden Warblers") that lack chestnut heads except for a small crown patch.

In terms of vocalizations, clearly a large-scale study is needed but song differences and differential response have been documented between some populations at least. Something I didn't see in the proposal is that typical call notes clearly differ, as described by Morton and Stutchbury (1991): a sharp *chip!* in the *aestiva* group and a soft *chup* in the *petechia* group. The introductory paragraph for *petechia* in eBird states "slightly more ringing and metallic [than *aestiva*]; huskier and lower-pitched on Galapagos". However, this apparent difference may be somewhat less clear and requires confirmation, judging by the descriptions in Juarez et al. (2025; BOW, Sounds and Vocal Behavior), although I suspect misidentifications may cloud the picture. And of course, we know there are call differences between western and eastern Nashville Warblers, and between "Audubon's" and "Myrtle" warblers, so typical call differences are not currently being treated as make-or-break in parulid species limits decisions, although certainly they are contributory. Below is a screenshot of typical call notes of various taxa (limited to June-August recordings to minimize chance of using misidentified recordings):





(Same set of recordings below in grayscale.)

Clearly, *aestiva* (top row) have much broader-band, sharper call notes that have a much higher maximum frequency than those of *petechia*. Two of the three central Mexican ones (second row from top), however, have slightly lower maximum frequencies than most northern *aestiva*, but this would need confirmation with larger samples. I did not find any examples of *aestiva* calls that are low-pitched like those of the *petechia* group. Galapagos calls (third row) are noticeably hoarser and more drawn-out, and with stronger harmonics than those of the other *petechia* taxa (fourth row), all of which have much lower upper frequencies than all those of *aestiva*, with the surprising exception of the two recordings available of calls from Martinique (fifth row). Given that Martinique *ruficapilla*, with its wholly chestnut hood, is the outlier in plumage among Antillean taxa, perhaps this taxon bears further scrutiny.

With respect to the possibility of splitting within the *petechia* complex, I don't think we are there yet. The situation is too confused and there are what seem to be hybrid zones (although these seem narrow and may not rule out species status). In the Paraguana Peninsula, birds described as subspecies *paraguanae* look intermediate between the chestnut-headed *chrysendeta* of the Guajira Peninsula to the west, and the golden-headed *cienagae* to the east, from central coastal Venezuela. And then there is the situation in Cozumel, in which the mangrove population seems likely to be a hybrid swarm between chestnut-headed *bryanti* of the Yucatan peninsula mangroves and inland Cozumel golden-headed *rufivertex*. Furthermore, the golden-headed Galapagos/Cocos *aureola* was considered to be relatively deeply diverged in one study but to be fairly recently diverged, probably from Central American birds (which however are chestnut-headed), in another (summarized in https://www.birds.cornell.edu/clementschecklist/updates-and-corrections-october-2025#splits). On the other hand, Galapagos birds seem to differ the most distinctly in call notes (something that needs to be confirmed). And, Martinique *ruficapilla* stands out among Antillean taxa for its chestnut head, although it is surrounded by golden-headed taxa.

As for English names, I gather there is popular resistance to adoption of Northern and Mangrove Yellow Warblers and a strong preference for the “old” names Yellow and Mangrove, or for American Yellow Warbler and Mangrove Warbler. Rereading our justification for the former name set in the Clements updates, I still maintain that the rationale for use of Northern Yellow and Mangrove Yellow is sound. On the other hand, we could invoke our range size asymmetry guidelines, especially if year-round range is included, in which case it would make sense to retain Yellow Warbler for *aestiva*. However, that still leaves the really very widespread *petechia* with a name (“Mangrove Warbler”) that has been used differently (to the exclusion of “Golden Warbler”) for a long time. Given that English names, especially widespread and familiar birds, are primarily for the masses, not for specialists, but that on the other hand they have high potential to lead to data quality problems, perhaps this is a case where a broader survey of public preferences would be advisable. In the meantime, I prefer Northern and Mangrove Yellow Warblers, with Yellow Warbler and Mangrove Warbler a close second. I don’t think American Yellow Warbler is an improvement, since *petechia* is also wholly American.

YES. An admittedly borderline case, but I think the available evidence now tilts in favor of recognizing two separate species for the northern migratory and tropical sedentary taxa. However, much more research is definitely needed for this system, especially genetic and vocal studies to clarify the extent of the differences between these taxa and to further understand the extent of reproductive isolation. As outlined in the proposal, I prefer the English names Northern Yellow Warbler and Mangrove Yellow Warbler, as I believe it is inappropriate to use “Yellow Warbler” and “Mangrove Warbler” for these species as those names have historically applied to different entities; from a data perspective, avoiding those names would prevent confusion. I know many birders (myself included) will continue to use the shortened “Yellow” and “Mangrove” warbler for these groups in the field, but I strongly believe the official English names should be “Northern Yellow” and “Mangrove Yellow.”

YES. I agree with the proposal and with other comments to split *S. aestiva* from *S. petechia.*

YES. Reasons are given in the proposal. I will, though, make another plug for using Yellow Warbler and Mangrove Warbler for the two new species, assuming the proposal passes. I understand the logic behind selecting Northern Yellow Warbler and Mangrove Yellow Warbler, including connections to the “parent” name, avoiding data quality issues, and following our naming guidelines. However, it strikes me that we are letting all these things get in the way of what, in my view, is an objectively better set of names that will, as another committee member said, be the names that are used “by the masses” anyway, even if Northern Yellow Warbler and Mangrove Yellow Warbler are selected by this committee. I’m sure that there will be some data quality issues in the short term, but those will sort out after a few years once people learn the circumscription of what constitutes “Yellow Warbler” *sensu stricto*. I know it’s fewer words and syllables, but I’m struck by the similarity to the Sharp-tailed Sparrow naming post-split. After a few years as “Nelson’s Sharp-tailed Sparrow” and “Saltmarsh Sharp-tailed Sparrow”, the committee simplified the names, in part because people were using “Nelson’s Sparrow” and “Saltmarsh Sparrow” anyway. Why not just skip ahead now and adopt the shorter

and more memorable names Yellow Warbler and Mangrove Warbler?

YES. I have finally come around on this, but reluctantly. Other comments are particularly interesting. These groups just seemed to have evolved into something pretty different, although I'm still not sure they fit what we think of as "species." I can say that *gundlachi* in Cuba react pretty strongly to "Northern" Yellow Warblers. I don't hear distinct differences in the chip calls, or songs. I supposed the closest Northern Yellow Warblers subspecies to Mangrove Warblers are *dugesi* and *sonorana,* but as noted by another committee member they breed in different habitats.

Yellow Warbler is so inculcated into the lexicon that I would rather keep it and go with Mangrove for the combined "Golden" and "Mangrove" groups. Yes, some subspecies (e.g., on the Galapagos Islands) are found in places where there are no mangroves. I believe the situation on Isla Cozumel is complicated with *bryanti* preferring mangroves in the Yucatan and the Isla Cozumel subspecies being a generalist. I'm sure the situation has been unfolding since the arrival of *bryanti* on the island. Still, most populations are confined to mangroves.

YES. There is enough evidence at this point to split *S. aestiva* into two. For names, I prefer Yellow and Mangrove for their simplicity. Although this violates our rules of retaining one of the names, perhaps this can be justified by their prior use by older authors.

NO. I would prefer to wait to see more genetic data to fully understand the evolutionary dynamics of population structure and gene flow in the system.

2026-B-14
Change the group name of species of *Myioborus* from Redstart to Whitestart

YES. This proposal articulates the argument very nicely in favor of (finally) adopting the group name "Whitestart" for species of *Myioborus*. Interesting to read that history.

YES. This is one of those all-too-rare cases where we can quite painlessly match the name to a monophyletic group and to a universal characteristic of that group, rather than maintaining yet another phylogenetically misleading and at best useless name. I think the vast majority of the users of bird names will be pleased should this change pass, and don't believe it will lead to much confusion.

YES. I was a bit hesitant to vote for this initially, as I was unsure how this change would align with our policy of not changing names just because a better name might be available in order to maintain stability. However, I am swayed by the argument that this name change is in line with our policy to fix common names when there is a taxonomic issue, albeit we are a bit late in adopting this change in light of the research that has shown that the genus *Myioborus* is not closely related to the American Redstart (*Setophaga ruticilla*). That being said, I also do agree that "whitestart" is a much better name for the genus, and it has the added benefit that it has already gained a lot of

traction in the community.

YES. I vote in favor of adopting Whitestart for all members of *Myioborus*.

YES. I support this emphatically. Whitestart is an accurate and far better name for the members of this genus. I appreciate the amount of detail that went into the proposal, and I strongly agree that Whitestart is the best path forward. With a nod to desiring stability in English common names, we have an opportunity here to not just "improve" a name, but to correct a completely inaccurate name. One thing that is not clearly stated in the proposal is the origin of the word "start" in these bird names. It comes not from the movement of the tail per se, but rather from the Old English *steort* which simply means "tail". It has, of course, been used primarily for birds that move the tail in such a way that flashes the coloration for which the bird is named. There is no species of *Myioborus* that has a red "start". It is white in all species. That "redstart" was used for the group at all is, as the proposal mentions, a holdover from when *Myioborus* were thought to be closely related to *Setophaga ruticilla*, which we now know is not the case. So, I do think that this is somewhat of a taxonomic issue. We are, in part, updating the name based on phylogenetic information that shows these two groups are not particularly closely related within Parulidae. This also has the beneficial side-effect of reducing the number of birds that share a name with the "true" *Phoenicurus* redstarts.

YES. This seems to have widespread support and good rationale overall. Thanks for the detailed proposal and treatise on the history of the nomenclature at hand.

YES. Whitestart is a better name generally that is now in more widespread use. Furthermore, we now know that *Setophaga* is not closely related to *Myioborus*, so the change aligns with taxonomy.

NO. We've been here before and I always felt that "whitestart" was contrived. I have seen no groundswell of support within the birding community for "whitestart." I rather think they will just be annoyed. In fact the only one where I do see support for a name change is to change Ring-necked Duck to Ring-billed Duck, a motion that was submitted a number of years ago. I realize I'm reflecting my age with this no vote. I don't really like changing English names, except in the case of *Piranga* tanagers which aren't tanagers. I'm left feeling that there is nothing new presented from what we saw before, other than a change of committee membership. I'll just add that those colleagues I know best are strongly opposed to "whitestart" and are saddened by the change.

2026-B-15
Treat Chaffinch Fringilla coelebs as two or more species

YES. This issue is largely outside of NACC's purview so I agree that we should go along with the global consensus. We also should follow the adopted English names (e.g., Clements 2025).

YES. The Recuerda et al. paper provides a solid basis for this action, although the case for 5 species (vs. 4 or 6) is somewhat debatable and divergence times are pretty recent.

For English names, I prefer the geographically based ones already adopted, although I'm not too thrilled with "Common Chaffinch"---It is indeed common over large areas, but of course not over most of the world. However, good alternatives are not obvious, as it's too widespread eastward through central Asia for "European" to be really appropriate, but not widespread enough in Asia for "Eurasian" to be a great fit.

YES. I am happy to defer to the global taxonomic community on this decision as it involves species that do not regularly occur in the NACC region. I do agree, however, that some of the splits seem a bit shaky, but on the whole they seem well-supported.

YES. The genomic data support this proposal.

YES. Given that it is largely outside our purview, I will go along with the 5-species treatment and the suggested English common names. The split of the African and European taxa seems the weakest to me, but again, it is outside our purview.

YES. Reasons are stated by other committee members. I'm OK with Common Chaffinch for an English name as it is the most widespread and really is "common."

YES. Reasons are given in the proposal.

YES. I vote for this based on the genetic study and given that this is outside of our jurisdiction.