2026-A-1: Treat *Formicarius destructus* as a separate species from Black-headed Antthrush *F. nigricapillus*
2026-A-2: Treat *Turdus ardosiaceus* as a separate species from Red-legged Thrush *T. plumbeus*
2026-A-3: Treat Willet *Tringa semipalmata* as two species
2026-A-4: Change the linear sequence of species of *Sialia* (bluebirds)
2026-A-5: Treat *Myiopsitta luchsi* as a separate species from Monk Parakeet *M. monachus*
2026-A-6: Treat Long-tailed Woodcreeper *Deconychura longicauda* as three species
2026-A-7: Treat *Onychorhynchus swainsoni* as a separate species from Royal Flycatcher *O. coronatus*
2026-A-8: Treat Great Egret *Ardea alba* as two or more species
2026-A-9: Recognize Whimbrel *Numenius phaeopus* as two species
2026-A-10: Treat Yucatan Vireo *Vireo magister* as two species

2026-A-1
Treat *Formicarius destructus* as a separate species from Black-headed Antthrush *F. nigricapillus*

YES. I was happy with this proposal last year and it still holds up, especially after SACC has adopted the split. The vocalizations of the two taxa are distinctive and do not overlap in trait space. The songs are not quite as different as those shown by other *Formicarius* that we treat as species (e.g., *moniliger* vs. *analis*), but are quite different for a suboscine, and as mentioned before, are non-overlapping in trait space. The mensural and plumage characters also seem to be consistently different between the two taxa. Regarding B, I am happy to go along with the suggested names, which maintain some connection to the parent taxon name of Black-headed.

YES. As we concluded before, the degree of vocal distinctness between these suboscines clearly warrants species status. (I note though that there are still several other populations of antthrush in South America, especially in *Chamaeza*, that differ even more and are still awaiting study, which hopefully someone will work on soon.) The SACC process of establishing English names was challenging and extended, but ultimately the names that were settled on seem the best available, given the limited morphological differentiation, the lack of satisfactory geographic names, and the match to the specific epithet of *nigricapillus* at least.

YES. It would be nice to have a better sense of the purported geographic gap with regard to vocal variation, but the clearly distinctive songs in conjunction with plumage differences warrant species status. The proposed English names (Black-capped Antthrush for *F. nigricapillus*; Black-hooded Antthrush for *F. destructus*) are consistent with the diagnostic plumage differences and retain a connection to the former parental name Black-headed Antthrush.

YES. I vote in favor with a little hesitation, however. Following the SACC votes, I feel these are distinctive enough in vocalizations and plumage to merit species-level recognition given patterns of species-level phenotypic differences in other *Formicarius* taxa. They are certainly diagnostic in song and plumage. I would very much like to see

genetic data for the nominate *nigricapillus* alongside *destructus*, and also to figure out if they do overlap and what that hybrid zone is like, should it exist. This is a common geographic area for taxon turnover, so there is precedent for species limits to abut here, but more robust genetic data should really be included in this type of proposal. All that said, with the evidence we have on hand, separate species status seems merited.

YES. The strong vocal differences with very little or no overlap in most aspects of the song investigated for this group of suboscines is clearly the most important trait for separating these two taxa as distinct biological species. I think the names Black-capped and Black-hooded Antthrush are appropriate for these two species, and is a nice nod to the original English name for the species.

YES. The evidence (song and morphometric data) is strong to separate *destructus* from *nigricapillus*, even though there is a lack of genetic data. English name: Black capped Antthrush for *F. nigricapillus* and Black hooded Antthrush for F. *destructu*s.

YES. Reasons are given in the motion and in members' comments. Yes to Black-capped Anthrush for *F. nigricapillus* and Black-hooded Antthrush for *F. destructus.*

YES. Based on differences in vocalizations, as well as plumage and morphology. I agree with proposed English names.

2026-A-2
Treat *Turdus ardosiaceus* as a separate species from Red-legged Thrush *T. plumbeus*

YES. I appreciate the reanalysis of the genetic data by the proposal authors, but I still worry that the data are insufficient for species limits given the small number of genes sequenced. The inferences about gene flow could just as easily be due to incomplete lineage sorting. They do, however, suggest that there is a deep split between the two groups. The face patterns are remarkably uniform across the range of each group and consistently different between groups, with the exception of the reduced white malar in Bahamian *plumbeus* which is clearly otherwise allied with the slaty Cuban birds. The songs sound consistently and diagnosably different across the range of both groups, although some examples of Bahamian birds do sound slightly lower-pitched and more like *ardosaceus*, which is worrisome. These plumage, genetic, and vocal differences to me seem on par with those between other recent splits in the genus, such as *T. daguae* and the recent proposal on *T. albicollis* that seem on track to pass with SACC. Any further splits within the western species would need additional studies, especially given the broad hybrid zone in Cuba.

The proposed English common names (Eastern and Western) are acceptable, but I would like to avoid simply prepending geographic names to all our splits, a trend that seems to be becoming widespread (and which I am guilty of). I think a bit more originality in naming is warranted.

I'm happy to follow WGAC and other global lists for the taxonomy of globally distributed species or those with ranges mostly outside our area, but this is a case where WGAC directly went against a very recent and, at the time, well-justified NACC decision on an entirely North American clade, an unfortunate and very much avoidable situation.

YES. The morphological and genetic differences are clearly more typical of two species-level taxa than of subspecies groups, in my view. On further consideration, I also think there is a good case to be made for the three-way split as enacted by HBW and Kirwan et al., though that is considerably less well-supported and will require further study.

I agree that the English names Western and Eastern are less than ideal but they are probably the best we know of and already have a lot of traction. However, given that for the HBW three-way split the name "Northern" was used for the Bahamian nominate daughter split (instead of using the obvious "Bahamian" for *plumbeus* and "Cuban" for *rubripes*), the concept "Western" differs between treatments and thus becomes even less ideal. Anyway, given that we're enacting a two-way split and that no better English names are obvious for these two species, I agree with Western and Eastern.

The two-way WGAC split was voted on and passed comfortably in 2021, and was not subsequently revisited for AviList, as there was a follow-up NACC proposal in the works.

YES. I vote for the split with the caveat that we really should maintain a precedent of nuDNA and multiple loci for taxonomic decisions of this kind. I appreciate that the proposal authors did some more thorough phylogenetic analyses, but in an ideal world, we'd have more robust geographic sampling, multilocus data sets, quantitative vocal analyses etc… mtDNA divergence of 2.3% is not super deep, but these are island taxa that are geographically isolated and likely represent separate evolutionary lineages. The throat patches are quite distinct and appear fixed/diagnosable. While the vocal analyses are qualitative, they read convincingly. The degree of phenotypic differentiation seems on par with other recent taxonomic splits for the region. I'm ok with the English names, they are appropriate even if they are a little uninspiring.

YES. Reasons are outlined in the proposal. I agree that it would be ideal to have better genetic sampling of these taxa so that we don't have to rely just on mtDNA, but I think together with the morphological and vocal differences, the mtDNA evidence acts as a supplement and not the core reason for the split. I agree that the English names are rather bland, but given their prior use in other places, I think they're okay. If a further split was to be adopted at a later time, however, I would certainly prefer something along the lines of what others have suggested, with "Bahamian" for *plumbeus* and "Cuban" for *rubripes*.

YES. I believe there are sufficient genetic, coloration, and song differences. I agree with the proposal.

YES. I agree to a two way split and the English names of Western and Eastern Red-legged Thrush make sense. If there is a move to a three way split, there will need to be more studies with both Cuban subspecies, *rubripes* over much of the country and

schistaceus from Holguin, Santiago de Cuba and Guantanamo provinces, and with nominate *plumbeus* in the Bahamas. Intergrades between the two Cuban subspecies are found in Holguin and Granma provinces (Kirkconnell et al. 2020). Eastern Cuban *schistacea* looks more like Bahamas birds, at least in largely lacking buff in the belly region of Cuban *rubripes* and subspecies (?) *coryi* from Cayman Brac. Subspecies *schistaceus* (from nominate *plumbeus*) also has a red, not black, bill and has more white around the bill. Subspecies *perditus* (meaning "lost or abandoned"; changed from *eremita* for nomenclatural reasons when Tristan Thrush *Nesocichla eremita* was moved into *Turdus*) from the Swan Islands (two small islands off Honduras which total less than three square kilometers) went extinct shortly after it was found in 1887. C.H. Towsend took a series of specimens between 4 February and 25 May 1887. I don't believe it has been recorded since. Ridgway (1907) said that Swan Island birds average larger with shorter toes and white on chin and malar region is usually more extended. This subspecies doesn't merit a mention in Howard & Moore (2017), even as part of the range of *rubripes*, or in HBW. In Clement (2000) on p. 402 under the range for he states under *rubripes* "and possibly formerly on the Swan Islands." He does not mention that Ridgway (1907) treated it as a valid subspecies. Whether a valid subspecies, or not, It looks like the lost and abandoned *perditus* was the perfectly chosen name for this "subspecies."

In the Bahamas, Hallett (2006) says that nominate *plumbeus* is common on Grand Bahama, the Abacos, Andros and New Providence and is uncommon on the Berry Islands and Eleuthera and fairly common on Cat Island. I believe both *plumbeus* and *rubripes* have occurred in south Florida, the former on the Atlantic coast and the latter at Key West. This needs to be checked.

On Cuba, Kirkconnell et al. (2020) detail the ranges of *rubripes* and *schistaceus* and their zone of intergradation. Interestingly they (Kirkconnell et al. (2020) add: "a specimen taken by P. Bartsch on Cayo Caballones in 1930 (Archipelago Jardines de la Reina), which was considered by Buden and Olson (1989) to be an example of *coryi* from Cayman Brac, but Kirkconnell et al. (2020) consider it intermediate between *rubripes* and *schistaceus* (which possibility Buden & Olson *op cit.* also admitted)." Buden and Olson (1989) say it might be now extinct there and Kirkconnell et al. (2020) say that recent surveys of other islands in this archipelago failed to record the species. In the Caymans the species (subspecies *coryi*) is only found regularly on Cayman Brac where Bradley (2000) considered it a common resident. On Little Cayman, it is casual where Bradley (2000) several were noted at the east end, April-May 1992-1994. On Grand Cayman Bradley (2000) notes that three were introduced from Cayman Brac (one escaped) in 1956 and nest building occurred on 15 May 1965 off Walker's Road, George Town (had been observed since 1963).

Kirkconnell et al. (2020) state: "some authors do not consider subspecies *coryi* of Cayman Brac to be valid, but if it is upheld as distinct, a position we support, *T. r. rubripes* becomes a Cuban endemic." For that matter so does *schistaceus* from the eastern third of Cuba. Left 'unhandled' is the 'lost and abandoned' and long extinct *perditus* from the tiny Swan Islands. It would seem they are more likely derived from western Cuba rather than marginally distinct birds on Cayman Brac.

For Eastern Red-legged Thrush, Keith et al. (2003) state that *ardosiaceus* is moderately common and is also "reported" from three major satellite islands but don't say which ones or their status on those islands. Raffaele (1989) says [same subspecies] it is a common resident but is absent from the Virgin Islands. Rafaelle (2020) includes as part of the "common" status subspecies *albiventris* from Dominica, but in googling the subject I get that they are rare and local on Dominica and heavily concentrated in dry woodlands on the west coast of Dominica, south of Portsmouth.

Finally, I wonder if any genetic work has been done on the extinct (by 1932, last specimens taken in 1911) Grand Cayman Thrush *T. ravidus* and if so how it fits into the now split Red-legged Thrush complexes. White-chinned Thrush *T. aurantius*, common and widespread on Jamaica, has been considered the sister species to Red-legged Thrush.

Selected literature:

Bradley, P.E. 2000. The Birds of the Cayman Islands. BOU Checklist No. 19. British Ornithologists' Union, Tring, Hertz, UK.

Buden, D.W. and S.L. Olson. 1989. The avifauna of the cayerias of southern Cuba with the ornithological results of the Paul Bartsch Expedition of 1930. *Smiths. Contrib. Zool* 477:1-34.

Clement, P. 2000. Thrushes. Princeton University Press. Princeton, NJ.

Hallett, B. 2006. Birds of the Bahamas and the Tuks & Caicos Islands. Macmillan Caribbean, Oxford, UK.

Keith, A.R., J.W. Wiley, S.C. Latta and J. A. Ottenwalder. 2003. The Birds of Hispaniola Haiti and the Dominican Republic. B.O.U. Series: 21. British Ornithologists' Union and the British Ornithologists' Club. Tring, Herts, UK.

Kirkconnell, A., G.M. Kirwan, O. H. Garrido, A.D. Mitchell and J.W. Wiley. 2020. The Birds of Cuba. BOU Checklist Series:26. British Ornithologists' Club, Tring, U.K.

Raffaele, H.A. 1989. A guide to the Birds of Puerto Rico and the Virgin Islands, Revised ed. Princeton University Press.

Raffaele, H.A., J.W. Wiley, O.H. Garrido, A.R. Keith, and J.I. Raffaele. 2020. Birds of the West Indies, 2nd ed. Princeton University Press, Princeton and Oxford.

Ridgray, R. 1907. The birds of North and Middle America. *Bull. US Natl. Mus.* 50(4).

YES. I voted to split these in 2022, and the new presentation of the data here reinforces my decision. Although only mtDNA, the genetic differences between the two taxa are more similar to what we see between, rather than within, species. Coupled with differences in morphology and the vocal data, I am comfortable with separating these

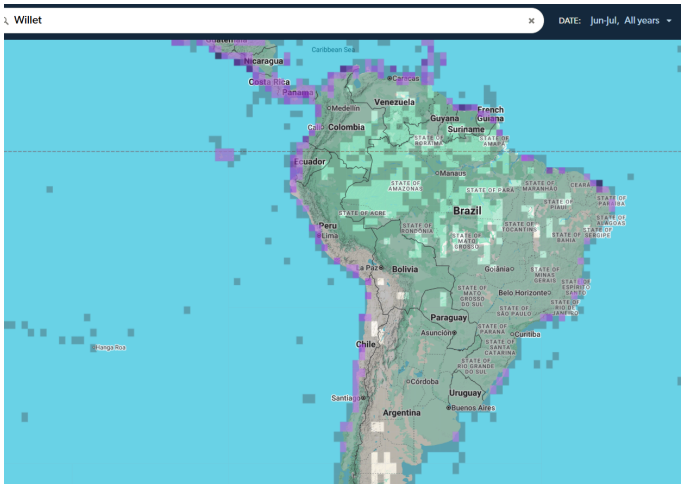
into two species. However, I do agree with others that better genetic sampling (individuals and markers) and more rigorous vocal analysis are still needed.

NO. I appreciate the genetic, phenotypic, and vocal differences presented in the proposal (including links to Macaulay Library recordings), but the proposed split is still based on only two mtDNA genes (with limited sampling from the western group) and a formal analysis of vocal variation is still lacking. If this proposal passes, then Western Red-legged Thrush and Eastern Red-legged Thrush are appropriate English names.

2026-A-3
Treat Willet *Tringa semipalmata* as two species

YES. I agree with the two-way split of Willet and with the English names Prairie and Saltmarsh. In my opinion, the morphological, vocal, and ecological differentiation between these two are indicative of separate species. Few if any other shorebird taxa currently considered conspecific by NACC differ to the same degree, even in the case of the whimbrel(s), although at least two extralimital plovers (e.g., Three-banded Plover *Thinornis tricollaris* and Red-breasted Dotterel *Anarhynchus obscurus*) were split by HBW but not by AviList. The vocal differences between *semipalmata* and *inornata* alone seem to me to be clearly inconsistent with conspecificity. I don't think that the rather shallow genetic divergence between them provides strong evidence against the split, but rather that this seems a case of rapid speciation.

We did not thoroughly address the issue of the resident West Indian populations in the proposal, as so little is known of them. The LSUMZ specimen photos provided are interesting and as far as I can see are consistent with their being *semipalmata*, but I am not convinced these are all resident birds, given the propensity for late/early migration and oversummering birds throughout their wintering ranges:



Nevertheless, while clearly there needs to be focused study on the West Indian breeders, I don't think this is likely to change the overall picture that supports two species, possibly each of them weakly polytypic. Assuming the two-way split passes,

whether subspecies should be recognized at all within the two daughter species is a matter for further research, but seems likely to me.

For English names, in the proposal I backed Western and Eastern for *inornata* and *semipalmata*, respectively, but on further consideration and bearing Jessica's comments in mind, I now prefer Prairie and Saltmarsh to reflect the paramount importance of these habitats in the life cycle of these birds and their segregation during the breeding season, although I would still say that Western and Eastern are acceptable.

YES. There is a lot to digest in this proposal. I went back and reviewed the original NACC proposal 2017-A-10 with associated comments, and have some of the same concerns that were raised at that time. Although I still view this as a borderline case, I am swayed to support species-level treatment by the combination of differences in morphology, habitat, annual cycle, vocalizations, and genetics. Regarding English names, I like Prairie and Saltmarsh for reasons given in the proposal, although Western and Eastern are also acceptable.

YES. Reasons are given in the proposal. Kudos to Pam for doing the heavy lifting on this resubmission and the in-depth information presented therein. I am good with Prairie and Saltmarsh Willet for English names.

YES. Having grown up with breeding nominate *semipalmata* and then living around breeding *inornata* for 6 years, these two birds just feel very different on their respective breeding grounds. The vocal differences are just so striking, plus the ecological differences together with the morphological differences all suggest species status. While the genetic divergence is low, I think the preponderance of other evidence strongly supports species status. I think it is also very intriguing that despite being together on the nonbreeding grounds, there are no known instances of mixed pairings, with either an *inornata* or *semipalmata* following the "wrong flock" to their breeding grounds and pairing up with the other taxon.

YES. Data on morphology, vocalizations, and ecology support this separation. Yes to the English names Prairie and Saltmarsh.

YES. I support considering *inornata* as a species separate from *semipalmata*, and also support the suggested common names of Prairie and Saltmarsh, respectively.

The proposal is excellent and thorough. I am most convinced by the vocal divergence and playback trial results, which, in my view, show divergence equivalent to that of other closely related *Tringa* species. I have no problem with the flight calls being similar, as I highly doubt that they have any role in reproductive isolation. The proposal does point out some differences in the calls, though, which only reinforces my thoughts on vocal divergence. That other shorebirds we consider to be species have strikingly different flight calls doesn't matter in the case of the willets. The rollicking "song" and the flight display would be the premating isolating mechanisms, which is what we should focus on. The slower cadence and lower pitch of the song of *inornata*, and more arched and rapid notes of *semipalmata* are quite different to the ear and in sonograms. Compare those differences, for example, to the comparatively minor differences between *Tringa*

melanoleuca and *T. nebularia* (which I believe are sister species):
https://macaulaylibrary.org/asset/609975681
https://macaulaylibrary.org/asset/649166136
Or even between *melanoleuca* and *flavipes*:
https://macaulaylibrary.org/asset/206482
To me, the song differences in these species are equivalent to, or even less obvious than, those between *semipalmata* and *inornata*. Note, also, that the Douglas (1998) paper quantified the song characteristics and found strong statistical differences in four song parameters measured (song duration, interval between songs, maximum and minimum frequencies), so those differences have been quantified.

Regarding the playback trials, I went back and read the Douglas (1998) paper after seeing it in the proposal. The paper conducted 118 trials and found that *semipalmata* responded to other *semipalmata* 83% of the time, but to *inornata* 22% of the time, which is of course a large difference, but that 22% bothered me. However, it seems that most of those *semipalmata* responding to *inornata* song did so only weakly by vocalizing but not approaching the speaker, which in the world of playback trials would typically not be considered a "full" response and still suggests a vocal isolating mechanism.

I do think that there is ample opportunity for gene flow, with non-breeding *inornata* oversummering in the breeding range of *semipalmata*. Comments on the 2017 proposal brought this up, and I have observed this in the field, where the non-breeding *inornata* are often on beaches near breeding *semipalmata*, yet don't seem to interbreed. That there appears to be no signal of gene flow from the STRUCTURE plot, despite the shallow genetic divergence and apparent opportunity for gene flow, is convincing for species rank. That also suggests that some reinforcement might be happening if subadult *inornata* are sympatric with *semipalmata* in the breeding season.

Regarding the Caribbean populations, there is clearly more to tease apart, but the breeding Willets there appear to be quite localized and uncommon. There are also certain to be many over-summering subadult birds, perhaps of both *inornata* and *semipalmata*, so we do need to be careful in looking at definitively breeding adults for ascertaining the breeding taxon present. Looking through photos of Caribbean birds that are clearly breeding, most of what I have found look to be quite clearly *semipalmata*-like. For example:
https://macaulaylibrary.org/asset/587807001 (the associated checklist even has photos of a chick)
https://macaulaylibrary.org/asset/241535811
https://macaulaylibrary.org/asset/30641301
In my experience, *semipalmata* is very common in the saltmarshes of the northern Gulf of Mexico and the eastern seaboard, but becomes very uncommon as a breeder as one heads south through peninsular Florida. That pattern seems to simply extend into the Caribbean, where it is quite localized.

I am very strongly in favor of Prairie and Saltmarsh for the English common names. Although Eastern and Western are in use in the birding community and in some publications, to my knowledge, they do not have prior use as standardized names. I am also much more in favor of selecting unique, evocative, or otherwise interesting names

whenever possible. Simple geographic monikers like “eastern” and “western” are about as bland as you can ask for, and are problematic in this case when, at least in North America, many “western” birds spend two-thirds of the year in the eastern half of the continent. Although the habitat-based names are only applicable to the breeding grounds, that is the primary distinguishing feature of these taxa. At least, there isn’t much about the morphology that we can use for a good name, and translating the scientific names isn’t ideal (although, having yet another “Semipalmated” shorebird would be fun). I’m not concerned that the Caribbean birds breed in other habitats, as the names won’t be perfect. At least the birds I’ve seen breeding in mangroves in parts of southern Florida seem to associate with *Spartina* vegetation when it’s present, so fitting with the “saltmarsh” name. As for “Plains” vs. “Prairie”, I think Prairie is the better name, which is typically the wet grasslands (vs. drier for Plains) and is where the Willets are actually breeding, even if they are in the broader Great Plains ecosystem. The core of their breeding range is in the Canadian Prairie provinces, too.

YES. I voted to recognize these as separate species in the 2017 proposal based on the genetics, morphology, vocalizations, and playback. The current proposal reaffirms my original decision. For common names, I prefer Saltmarsh and Prairie. Although not perfect (since they are often encountered in habitats other than these), the application of Western and Eastern is not perfect either.

YES. The case for a species split is stronger now than when we previously reviewed a split motion. Unlike the rest of the committee I am not taken with Saltmarsh and Prairie. For me the “prairie “ region in North America lies east of where this species breeds. Where they breed is in wet areas of the Great Plains and the Great Basin, so with reservations, I think Eastern Willet and Western Willet is best. It certainly holds for breeders.

Another committee member raises good points about the infusion of *inornata* into breeding areas of *semipalmata* on the Gulf and Atlantic coasts. Yet, they are easy to differentiate by behavior with *semipalmata* singing, displaying and sitting around on posts, while *inornata* occurs on beaches and in more interior flooded rice fields where *semipalmata* is not. And comments about non breeders (in basic or largely basic plumage) loafing about wintering areas is also important. I would add that southbound *inornata* in alternate plumage are already back from interior migrant spots (e.g., Lake Tahoe) by the end of June and no doubt they are back on the coast then too.

I agree that much more needs to be learned about West Indian breeders. In a dozen trips to Cuba where much time is spent on the coast, I’ve yet to see breeding (displaying, etc.) Willets. The majority of Willets I see are *inornata* on beaches and mudflats. Yet in the Cuban monograph (Kirkconnell et al. 2000) it is termed a common breeder. I would be opposed to any new named subspecies for the West Indies unless much more is detailed.

External comments from Jessica Oswald Terrill:
Thanks to you all for revisiting Willet taxonomy. I do have a few points that may be used to strengthen the proposal.

Page 24. Potential geographic variation/recognition of subspecies ---

If I remember correctly, the outstanding questions regarding what the phenotypically distinct taxa represent within the currently recognized subspecies, e.g., Gulf coast birds, were points used to reject the previous proposal to split Willet (and the lack of genetic sampling of those individuals by Oswald et al 2016). The most troublesome population, in my opinion, are the individuals that are resident in the Caribbean (Bahamas, Greater Antilles, Cayman Islands). The different life history strategy of these residents and their geographic isolation during the breeding season suggests to me this may be a divergent taxon. Based on breeding habitat (saltmarsh, lagoons, beaches), they are ecologically more similar to *semipalmata*, though are there phenotypic differences between these residents and mainland taxa? Is there any reason based on plumage or other morphological characters that they should also be considered a subspecies of *semipalmata*, if it is split? While there is little information regarding these residents, beyond their rarity, I think at least touching on this group more is important for this proposal.

Per VertNet, at LSUMNS there are two willets collected in April from Inagua, Bahamas, and one in July from Cuba. These specimens might be worth a quick check to assess phenotype in hand.
https://www.vertnet.org/occurrence/search?taxonKey=5789298&country=CU&country=BS&basisOfRecord=PRESERVED_SPECIMEN

Photos of these spring/summer Caribbean individuals on Macaulay might be worth a check, too.
https://media.ebird.org/catalog?birdOnly=true&view=grid&sort=obs_date_asc&taxonCode=willet1&regionCode=caribbean2&beginMonth=6&endMonth=7

Common name

I am a proponent of Prairie Willet and Saltmarsh Willet as common names. This is in part because of the conservation importance of these habitats, which was outlined in the proposal. Despite the use of “Western” and “Eastern” by birders, I think the conservation points and clear differences in breeding habitats should take precedence here.

2026-A-4
Change the linear sequence of species of *Sialia* (bluebirds)

YES. Reasons are given in the proposal.

YES. While I appreciate that there is uncertainty in this case, I am persuaded by the arguments in the proposal and am okay supporting this resequencing decision.

NO. The studies cited in the proposal seem somewhat contradictory and do not seem to conclusively support the change recommended, and *currucoides* does seem the obvious outlier in morphology (plumage, shape, and posture). A quick review of vocalizations,

however, suggests to me that *currucoides* is vocally somewhat more similar to *sialis* than either is to *mexicanus*, which could support the proposed rearrangement. I am left wondering though whether it might be preferable to change instead to:

mexicanus
sialis
currucoides

This to me might better reflect the uncertainties and the greater phenotypic similarity between *mexicanus* and *sialis*, but I will be interested in further thoughts from the committee on the matter.

NO. Per the proposal and the Master's thesis by Veale (2023), different mtDNA and nuclear analyses give conflicting phylogenetic data on relationships. While Figure 2 in the proposal shows Eastern Bluebird and Mountain Bluebird as sister based on one analysis of ddRADseq data (with a reduced number of individuals), different analyses of ddRADseq in the same study (Figure 3.6b and 3.6c in Veale 2023, not presented in the proposal) showed a closer relationship between Western Bluebird and Mountain Bluebird. I think for now we should keep the status quo pending more definitive resolution of sister species relationships.

NO. The three taxa in *Sialia* appear to have very, very short internodes—that much is true. I would prefer to wait for the Master's thesis to undergo peer review. Ideally, we need a multi-locus species tree inference that accounts for previous or ongoing gene flow between these taxa. I'm assuming the phylogeny based on SNPs shown is a SNAPP tree. As far as I recall, SNAPP assumes that there is no gene flow between taxa, which we know is not true from documented hybridization. So, before we make a final call on the linear classification, I'd prefer to wait to make sure the analyses presented in Veale (2023) are robust to the assumptions of the inference model and the system at hand.

NO. The data are not enough to support this proposal.

NO. I had voted yes, then pend, as I'm inclined to think the sequence is not right, but I'm not convinced in the motion as to what the sequence should be. Call notes of Eastern are really different from either Mountain, or Western which to my ear sound closer. Hybridization between Mountain and Eastern on the Plains in the Prairie Provinces of Canada and North Dakota have been known for more than half a century. Does that imply a close relationship, at least genetically - not necessarily as evidenced by the well-studied Bullock's and Baltimore orioles. Having Eastern next to Mountain would still seem to be the best solution but where does Western go in this sequence of three?

NO. Pending publication of the research and additional analyses (such as multi-locus species trees).

2026-A-5

Treat *Myiopsitta luchsi* as a separate species from Monk Parakeet *M. monachus*

YES. *Myiopsitta luchsi* is very distinctive in vocalizations and breeding behavior, and moderately distinctive in plumage. Among parakeets, this all points to reproductive isolation. Both the suggested English common names are well established.

YES. Reasons are given in the proposal and amplified in the SACC discussion. To me the vocal and nesting habitat differences are compelling and out of line for subspecies. Yes, some parakeets may have labile nesting preferences, but from all accounts *luchsi* seems quite strict in its unique requirements despite the availability of *monachus*-type nest substrates. And, while of course Psittaciformes show varying degrees of flexibility and learning in vocalizations (some extreme and exhibiting the pinnacle of some types of mimicry among birds, obviously), there does not seem to be much variation at all in the striking vocal differences among the narrowly endemic *luchsi* and the widespread and even invasive *monachus*. The fact that plumage differences are clear but not overwhelming, and that the genetic evidence seems suggestive only, to me this does not outweigh the nesting and vocal evidence.

YES. This is mostly a SACC issue which passed unanimously, and there is sufficient compelling evidence as presented in the proposal to support species-level treatment. Cliff Parakeet is a good name for *M. luchsi.*

YES. This is really an SACC proposal given the taxon's native range, and passed 7-0 there. I am good following their lead.

YES. The two species are allopatric and they are morphologically very different.

YES. I think the vocal differences and differences in breeding behavior, plus plumage differences, all strongly support species status for these taxa. Adopting the split also brings us into alignment with SACC, where it passed unanimously.

YES. The two taxa certainly look different, more so than other species pairs within this family that have been split. Along with habitat and vocalizations, I'm fine with this although I do not know the issues well.

YES. Plumage, vocal, and nesting differences all justify separating them into two species. Treating them as two species also agrees with SACC decision.

2026-A-6
Treat Long-tailed Woodcreeper *Deconychura longicauda* as three species

YES. This is primarily a SACC issue, and I'm happy to go along with their decision here. The vocal data in particular are very convincing that at least the taxon in our region (*typica*) is distinct from the rest, and that is also consistent with the mitochondrial genetic data. I'm less convinced of the differences among the various South American taxa. The nominate Guiana Shield birds seem quite distinctive vocally, but there seems to be some

complexity within the rest of the Amazon basin that I can't wrap my head around. Hopefully, the paper describing the Andean species will also sort out that vocal complexity. However, that is all outside our region, and given that SACC has already voted to split at least the Guiana Shield birds from the Amazonian birds, I will do the same. I'm happy with the English names suggested.

YES. This degree of action seems strongly warranted at this point, despite the remaining uncertainties. One could argue they should never have been lumped in the first place, although on a morphology-based Peters-type approach that is completely unsurprising, and of course they lacked the vocal and genetic evidence. Even with the non-compelling morphological differences that could as well represent subspecific variation, the vocal differences of *typica* to the others cannot be ignored or considered to represent merely motivational states - especially now that we have good online samples and the anecdotal but useful playback trials reported on by Andrew Spencer and Kevin Zimmer. While the vocal differences between the other two exclusively South American taxa are less obvious, they seem non-trivial and have been agreed as such by most SACC members, and are backed up by the limited genetic data.

The English names seem a bit long but really each is only seven syllables, and the use of "Long-tailed" is helpful in this group of very-similar-looking birds by tying them in with the ingrained parent species name. However, as has been noted they are not all that long-tailed, and vocal names don't help in common situations such as seen-only birds or specimen comparisons. Nevertheless, the very extensive SACC discussion resulted in what is in my opinion the most suitable English names thus far suggested.

YES. If this were strictly a NACC issue, then I would vote against this split based on the proposal. My concerns are similar to the two "no" votes on the SACC proposal: limited sampling of individuals for both the genetic and vocal data, only mtDNA data, discriminant analysis of vocalizations doesn't include clade 8 (*typica*), plumage differences are non-diagnostic, no mention of putative contact zones. However, since this is primarily a SACC issue, I reluctantly think we should go along with the split for consistency. Also, *typica* was formerly recognized as a separate species but then lumped with *longicauda* without comment. I am fine with the English names adopted by the SACC.

YES. This is a challenging one for me, and I am also on the slightly skeptical side, but with the strong support from SACC and this being a primarily South American issue, I am more inclined to vote for the split. As others have mentioned, regardless of the other two South American groups, the one group in our region is the most distinct vocally and to me has the strongest argument for splitting. I agree with others that I would very much like to see more than just mtDNA data, but I think the genetic data combined with the vocal differences are what push this over for me. I also agree that further changes may be warranted with more data from other taxa (e.g., the Andean taxon), but I am okay moving ahead with these splits, and if more are required later, then those can be implemented later. I am happy with the proposed English names.

YES. I vote for a three-way split based on vocalizations and genetic data. Also, given that this is primarily an SACC issue, I prefer to go along with their vote to separate these

three taxa. At this point, there is more data in support for separating them than for keeping them as one species.

YES. Entering into a woodcreeper taxonomic debate is something I would rather avoid and offering a different decision from what the SACC has done is something I would rather avoid. All agree that Middle American *typica* is a well-supported split based on multiple lines of evidence, particularly vocalizations. So, split others and follow SACC would be my vote. Obviously further studies may lead to alterations, but that shouldn't affect us.

I'm not thrilled about the English names chosen. They look cumbersome to me. In comparison to some other woodcreepers they don't even look disproportionately long tailed. I'm willing to go along. I do note that Dyer and Howell (2023) use Cherrie's Woodcreeper. Yes, the eponym issue. Any other suggestions, and do we need to keep Long-tailed in the English name?

NO. I hesitate to go against the tide of the SACC vote for a taxon that is largely in their geographic jurisdiction, but I share some of the concerns raised in the SACC comments. Namely, that the genetic data are mtDNA-only and the vocalization analyses have some issues: possible issues with the behavioral context of vocalizations analyzed raised by Lane, and also the DFA is not 'agnostic' in that it seeks to differentiate individuals according to *a priori* groupings rather than a true clustering analysis. It also seems that the Andean taxon is not fully addressed in this proposed split. I do not have much first-hand experience with these taxa in the field, so am going purely off what is presented in the proposal; while I strongly suspect there are multiple species in this complex, I'd prefer to wait for a formal, peer-reviewed, more comprehensive study to properly assess species limits. If it does pass, I am fine with the English names proposed.

NO. There is not enough evidence; using only mitochondrial DNA is a bit risky. I think we need to wait for more data to support this proposal. But if this proposal is approved, I support the proposed English names.

2026-A-7
Treat *Onychorhynchus swainsoni* as a separate species from Royal Flycatcher *O. coronatus*

YES (option B). The genetic data show a very deep split between *swainsoni* and the rest that is consistent with species-level differences. The other taxa could be split, but really need a quantitative analysis of the song (not just the primary call) and the crest coloration, both of which are the likely factors in species recognition. The primary calls of *swainsoni* are clearly the most distinct of the group, being much shorter. In listening to the songs, each of the taxa for which songs are available do sound different, but as mentioned before need a quantitative analysis. It isn't mentioned in the proposal, but there are recordings of the song of *swainsoni* (https://xeno-canto.org/188289 and

https://xeno-canto.org/18210), and it is certainly quite different from the rest. The English names sound good to me, and Tropical would change with further splits.

YES (option B). The WGAC consideration of this issue that resulted in acceptance of a two-way split by AviList occurred just before Reyes et al. (2023) appeared, and thus was based to a considerable extent on Boesman's vocal comparisons and Harvey et al. Now we have Reyes et al. (2023), which supports the main conclusion of a two-way split. While it recommends more, these are not as compelling or well-supported, and further genetic work is clearly needed. And songs seem very infrequently recorded, but possibly the SACC proposal and comments will spur greater efforts to record them. The English names seem the best we can do given the constitution of *coronatus* on the basis of this action.

If there are any deeper splits eventually, I would expect the split to be between the *mexicanus-occidentalis* and the *coronatus* clades unless the former two are shown to differ significantly in song. This would affect the NACC range statements, but for now the obvious split of *swainsoni* from the rest is as far as we should go, and results in minimal change to the NACC Check-list.

YES (option B). This is primarily a SACC issue, so it makes sense to go along with a two-way split for consistency. The available genetic data also support this, as does the qualitative vocal difference. The English names used by BOW and adopted by SACC are fine with me.

YES (option B). This seems a conservative way to move the taxonomy of this complex forward without overreaching. The genetic and vocalization data line up with this split, and it's already been adopted by BOW and supported by SACC. I am good with the English names proposed by BOW and SACC.

YES (option B). I like this approach for this group, where we are able to split off the taxon that we know is distinct and hold off on making a decision on the other taxa until more data are available. Brazilian *swainsoni* is definitely distinctive in many ways, and while the other taxa seem like they are probably different too, it is not as clear-cut to split those at this point.

YES (option B). Recognize two species (as in HBW and Birds of the World): *O. swainsoni* and *O. coronatus*, which would include all other taxa.

YES (option B). I see no reason to differ from others in the two-way split, particularly from a NACC perspective.

YES (option B). Good justification for two way split, based on genetic differences and vocalizations.

2026-A-8
Treat Great Egret *Ardea alba* as two or more species

YES. A definitive yes to the two-way split of *egretta* from the others, based firstly on strong and obvious vocal differences that have long been known but for which we now have abundant online evidence. Species status for *egretta* is also well-supported by the clear differences in breeding soft-part colors, which surely must play a role in mate selection, and probably also by its stronger plumes that are so often and obviously displayed compared to the others. The genetic evidence is subpar and doesn't count for much, but it is beyond me to understand why we don't yet have anything more comprehensive and definitive on the genetics yet for this obviously needy and high-profile complex. As one AviList commenter stated, the genetic data available are insufficient to even speak of paraphyly, and thus that should not be an obstacle to elevation of *egretta* to species status.

As for species status for *modesta*, I still think it should be (and eventually will be) split but I understand the reservations expressed on this matter by AviList commenters thus far. There is much to be resolved about whether *modesta* and *alba* are sympatric, parapatric, or even vary clinally. We simply don't know. The dashed line in the map included in the proposal was simply meant to indicate the considerable uncertainty in our knowledge on this matter. This confusion is understandable when most of the past work was based exclusively on specimens, only a small percentage of which will be in breeding plumage. Compounding the confusion is the fact that people still routinely misidentify *intermedia* as this, and vice versa. With the analysis of non-breeding soft-part colors and structure by Reyt et al. (2025), hopefully people will start to pay more attention and work out some of these issues in the field and based on photos. While we cannot be completely certain that the limited genetics are sending the right message, there is the signal of genetic divergence to contend with, hopefully also something that will garner more work soon. What sways this for me though are the striking vocal differences between *modesta* and *alba/melanorhynchos*. While not as profound as those between *egretta* and the others, it is very evident that *modesta*, despite its relatively small size, produces deeper rattles with longer elements than do *alba/melanorhynchos*. As pointed out by an AviList commenter, these have not been quantified, but that would be easy to do; based on having worked with a large set of downloaded recordings, I have no doubt that doing so would show exactly those differences outlined above. The question, as in so many cases, comes down to whether we accept the obvious based on publicly accessible material or wait for someone to put in the effort and time necessary to publish a quantification. But we don't know for sure whether there might be clinality in voice somewhere in the interior of Asia, so I can appreciate the reservations of others. My vote will remain a Yes to split *modesta*, fully expecting that this will not be the NACC position at this time and that it may take considerably more effort to establish this to the satisfaction of a voting majority.

As for species status of *melanorhynchos*, No at this time to a split, for the reasons in the proposal, although a split could well be the ultimate outcome.

English names: In the event of a two-way split, American Great Egret is very ingrained and I'm not aware of anything better. I don't see a problem with Old World Great Egret, which is descriptive enough and a counter to American, but I am pretty sure it will be met with dislike by many. I don't prefer names based on vocalizations, such as Croaking

Great Egret and Rattling Great Egret for *egretta* and the *alba/melanorhynchos/modesta* complex, respectively; these names are unlovely and seem somewhat demeaning to these beautiful and majestic birds, and the birds are seen and photographed far more than heard and recorded (although they are indeed quite vocal). But a couple of cisticolas bear exactly these names and I haven't heard any complaints about that (although of course cisticola names are even harder to come up with and fewer people would notice or care). No soft-part color names suggest themselves, especially given that breeding bill colors are repeated repeatedly among egrets, and only held for a short time frame.

In the seemingly unlikely (for NACC) event of a three-way split, Western and Eastern Great for *alba/melanorhynchos* and *modesta* respectively are adequate, and wouldn't require any change for American *egretta*.

Follow-up after reading further comments: I would be ok with American Egret given its long history of usage, and probably Great White Egret for the Old World complex, the latter name still being in common usage in for example New Zealand, though it is of rather marginal occurrence there, and New Zealand Birds Online uses 'White Heron'.

Follow-up after PAL-RAG comments: I still vote for both splits, *egretta* (strongly) because it is so obviously different vocally and (to a lesser extent) in soft parts and plumes, and (less strongly) *modesta* because the voice differs too much for it to be conspecific with *alba*, as well as the suggestive genetic results, though I completely agree further data are needed and I never expected this to pass until the case is better resolved.

YES. I agree to separating *egretta* from the rest, but NO to further splits within any of the Old World taxa. I am most convinced by the differences in plume structure, bare part coloration, vocalizations, and flight displays, all of which are clearly related to pair formation and maintenance, and would thus be involved in reproductive isolation. In my view, the degree of the differences is comparable to other ardeid taxa that we currently consider species, such as Little/Snowy Egrets, Cocoi/Gray/Great Blue Herons, the Cattle-Egrets, and the Intermediate Egret complex, although this is admittedly a borderline case.

I have recently been spending lots of time around heron/egret breeding colonies, and have been fortunate to hear and watch many Great Egrets at the nest. When I listened to the sound recordings and watched the limited photos/video of displaying old world birds, I was struck especially by the differences in voice, which I did not recognize as a Great Egret in comparison to my local birds. The breeding colorations, although held only briefly, are strikingly different in *egretta* compared to all the Old World taxa.

The genetic information we have is limited, but at least based on the one mitochondrial gene, seem to be greater than that of the Cocoi/Gray/Great Blue Herons, which are clearly species. I did poke around to see if any newer samples were available on GenBank, and found some from Australia and a few more of *egretta* and *modesta*. I ran a RAxML tree with those data, but it doesn't change the overall picture much. The branch leading to the *alba* clade has very low support, so it forms a polytomy with the rest, but the branch lengths are quite long. So, from what I can tell of the genetic data

(and is consistent with the trees in the proposal), *alba*, *modesta*, and *egretta* all form deeply divergent mitochondrial clades, but the relationships among them are unresolved.

As for English common names, I am strongly in favor of returning to the historical names used for these groups back when we had common names for subspecies. As late as the 1960s, *egretta* was known in most field guides as American Egret (including in my first Peterson guide!). Many Old World lists, including the last BOU checklist and the current BirdLife list, all refer to *alba* (including *egretta* as a subspecies of course) as Great White Egret. There was a time between the use of American Egret and Great Egret that Common Egret was in widespread usage, but that wouldn't apply to any one taxon in particular. I would much rather use these existing names instead of coining new names that simply prepend a geographic region to the current name. I am not in favor of Old World Great Egret in any case.

There are of course plenty of other egrets in the Americas, but that doesn't prevent us from returning to this well-established historical name that has considerable publication history and usage. I did a google scholar search for all these names, and found the following number of results: Great Egret (13,400), Great White Egret (2,480 results, including recent papers from Europe, Africa, and eastern Asia), American Egret (1,470), and Common Egret (684). Just to toss out an alternative; if Great White Egret is truly in widespread use in Eurasia, and Great Egret we know is in widespread use in the Americas, we could stick with Great Egret for *egretta*, even though it goes against our policies. It would be analogous to the Snowy/Kentish Plover naming decision after that split. I will wait, though, to see what folks with more Eurasian experience say about this.

I don't know if we need to vote on this, but if this split does pass, I would accept the old world taxon as occurring in the NACC area based on the specimens from Alaska, recent photographs from western Alaska, and the record from Maryland. There are some birds photographed recently in British Columbia and Washington that are good candidates for *alba* and that would need to be critically evaluated. There is also one record of a bird on Kure Atoll that is either *alba* or *intermedia*, but I suspect the former. If we split the various Old World taxa, these records would need to be evaluated before I'm willing to accept each separately, as I'm still not confident in separating lone non-breeding *modesta*, *alba*, and *melanorhyncha* from each other. These records of cross-oceanic vagrancy, plus many records of *egretta* in the Azores, do point to the potential for gene flow to occur, which I think strengthens the argument for splitting *egretta*. If they are capable of showing up in the range of the other taxa, that should break down species barriers if reproductive isolation was not occurring at some level.

Follow-up after reading the Palearctic RAG comments: I still vote to separate *egretta* and not to adopt any changes for any other taxa in the complex. I understand the desire to wait for more data on this change, but I feel that most strongly for the various Old World taxa. To me, the combination of vocal and breeding morphology differences of *egretta* rise to the level of species. I agree that the genetic data are not great, but regardless of the topology of the tree, *egretta* does form a clade, which is good enough for me, at least for now.

YES. I agree to splitting *egretta* but not to additional splits among Eastern Hemisphere

taxa. I am hesitant even for the *egretta* split since it's just COI data, but it does seem to form a monophyletic group based on the Sasikala et al. (2012) data and is likely a separate evolutionary lineage. The differences in bare parts color are perhaps subtle, but likely important in mate choice. I would prefer to leave the decision for the other taxa to global taxonomic authorities. For English names, I like American Great Egret, but do not like Old World Great Egret due to its Eurocentric, colonial connotations.

YES. I agree to splitting *egretta* but not to splitting the other taxa. I agree with what others have said regarding this split, that the evidence is very strong for splitting *egretta* from the other taxa, given vocal, display, bare part color, and feather structure differences, which all contribute to reproductive isolation and pair formation in this group. I agree that the level of difference between *egretta* and the Old World taxa seem on par with other closely related heron groups. I am a bit torn on the other taxa, but I agree that more data are needed to fully resolve what is going on there. It does seem like there are some clear vocal or behavior differences between *modesta* and *alba*, but these are not as strong as those between *egretta* and the other taxa. Plus, since we lack vocal data from much of interior Asia where these two taxa may (or may not) come into contact, we are not getting the full picture of what these vocal differences might mean. Morphologically, *modesta* also does seem to have somewhat different plume structure, but similarly not as distinct as *egretta* from the others. As others have suggested, I am in favor of adopting American Egret for *egretta* and Great White Egret for *alba*.

Follow-up after reading the Palearctic RAG comments: Despite the recommendation to wait on all splits of this complex until there are more data, as others have mentioned, that seems most applicable to the Old World taxa, with *egretta* being pretty clear cut and separate. Sure, there are some odd discrepancies in the limited genetic data, and more information from South America would be good, but I do not think it is necessary to wait on this split.

YES. I agree to split *egretta* from the others, but we need more genetic data from the other groups.

YES. I agree to splitting *egretta* from the others based on bill color during the breeding season, vocalizations, and breeding plume feather structure. More information is needed to split the other taxa. I prefer American Egret and Great White Egret, based on historical usage as indicated by another committee member. Plus, I would hate to end up having to use another hyphenated common name (Great-Egret). After reading the Avilist comments, I'm still comfortable with splitting *egretta* from the others, and still think there is not enough information to deal with the other taxa in this group.

YES. I agree to splitting *egretta* from the three Old World taxa but not to splitting *modesta* from the others. Another committee member details the issues well between *alba* and *modesta.* We simply don't know whether they overlap in range in northeast Asia, particularly perhaps from Manchuria and Ussuriland (and Amurland), Russian Far East. Postscript (5 June 2026)**:** I just picked up the Princeton Field Guide to the Birds of China by Liu Yang and Chen Shuihua (2024) and they say that nominate *alba* breeds in Heilonjiang and Liaoning (in NE China) and northern Xinjiang (NW China) provinces and migrates through northwest, southwest and the Quinhai-Tibet (Tibetan) Plateau in China.

Subspecies *modesta* breeds in Jilan and eastern Nei Mongol provinces and migrates through north, east, central and SW China and winters in south China. Thus the two taxa might well be sympatric in northeast China in Lianoning and Jilin provinces and perhaps North Korea as well. I must say that photographs from Nills from South Korea of *alba* and *modesta* together show striking size differences and I think there might have been soft part color differences too (I think the leg color). Nominate *alba* certainly breeds to northeast Asia, mostly, or entirely, to the north of *modesta*. Some remain for the winter in Korea and a few are seen in Japan then. Perhaps most *modesta* withdraw south? It would seem that *alba* is unrecorded in Southeast Asia, although it can likely be said that observers aren't prepared to identify one. I see from Rasmussen and Anderton (2005) that nominate *alba* is known from the northwestern portion of the Indian Subcontinent. At least *melanorhynchos* is allopatric from the rest of the taxa in the complex. I will spend next February in Thailand with Paul Holt who has spent a great deal of time in China, much of it living there. He has traveled over much of China and his perspective on *modesta* and *alba* would likely be quite informative. Paul is listed as one of five translators of the Chinese text into English of Yang and Shuihua (1994) Chinese text from the field guide. Frustrating that there is no map in the guide with the provincial lines shown.

I understand the need for a comprehensive taxonomic solution to this issue, but we often don't follow that, particularly when there is only one to split off from our area, with additional ones to sort out elsewhere, particularly from South America which obviously SACC addresses. We sometimes don't follow that as with Solitary Vireos where two subspecies from Middle America (*notius* group) are currently placed with Plumbeous Vireo. To my knowledge they still have not been properly studied and the Solitary Vireo split was decades ago.The Committee voted to split the Marsh Wrens despite the uncertainty of the resident plateau Mexican subspecies.

As for the identification of *alba* type birds from North America, the two Aleutian specimens were identified as *modesta.* I asked Dan Gibson about this (pers. comm.) and how he assigned them to that subspecies. He said by measurements, otherwise he would not have identified them as such. But, given the more northerly distribution of *alba* it could reach the Aleutians or elsewhere in western Alaska too. East Coast records seem likely to be *alba*, although *melanorhynchos* from Africa could occur.

As for English names, I agree with looking backwards for guidance. Great White Egret for the Old World English name and American Egret for *egretta* seem perfectly fitting. I grew up with Common Egret as the English name for *egretta.* If the Old World is split into two or three species that naming issue should be left to them for the English names. Obviously, something with Africa in it for *melanorhynchos.* Subspecies *modesta* is not found in Europe while nominate *alba* is found in both Europe and Asia. In Asia *alba* is found north of *modesta.* Nominate *alba* is, I believe, the largest subspecies, so going back to Great White Egret is not a bad choice.

NO. I originally voted in favor of splitting *egretta* from the other taxa based on differences in behavior, vocalizations, plume structure, and soft part colors, with a comment that it would be nice to have better genetic data. I did not vote in favor of the other splits pending further study (which is outside the scope of NACC). However, after reading

comments from AviList that Terry solicited (thanks!), I am changing my vote to NO based on the consensus that this split is premature (AviList's Palearctic RAG was not in favor of a piecemeal split of just *egretta* at this time). If the proposal passes, I favor American Great Egret rather than American Egret (although that name is historical) to make it clear that it's part of that complex and because there are other egrets in the Americas. I'm not crazy about Old World Great Egret and am open to other options. However, I could live with American Egret and Great White Egret as suggested elsewhere if that is the consensus of the committee.

2026-A-9
Recognize Whimbrel *Numenius phaeopus* as two species

YES. I have voted for this split previously and consider it the better alternative to continued treatment as conspecific. This is based on the genetic and morphological differentiation, and is somewhat countered (unlike the case of the Willet) by the extreme similarity in most vocalizations. The tail-rump presentation differences in courtship between the two groups summarized in the proposal seem likely to be significant in mate selection, in the unlikely event that the two should meet on the breeding grounds. The same goes for the documentation of differences in vocalizing by the male in the aerial climb phase of the flight display. The genetic results of Tan et al. (2023) that suggest similar divergence times between *hudsonicus* vs. *phaeopus, arquata* vs. *tenuirostris*, and even *madagascariensis* provide a good yardstick for treatment of *hudsonicus* as a species, if one accepts such metrics. I agree completely about the need for a comprehensive quantitative analysis of vocalizations, and suspect that differences will be found (as suggested in the proposal), but the lack thereof does not seem an obstacle to me.

One point made in the proposal involves the finding by McLaughlin et al (2020) of genetic divergence between Palearctic and Holarctic *Pinicola enucleator*. This is another issue that should be reevaluated, as these taxa differ fairly strikingly in a number of characters, and thus I don't think it can be held up as a reason not to split Whimbrel.

The English names would seem to warrant further discussion, although I am OK with Eurasian and Hudsonian. I find the latter a bit duplicative of the godwit name, although it mirrors its own specific epithet, and of course it is a second-level eponym (but so is American). Perhaps Eurasian and American Whimbrel? Then there is the issue of whether to use Whimbrel and Hudsonian Curlew, or some other combination, to be considered, but I would certainly prefer for them to have the same 'last' names.

YES. It would be nice to have a formal quantitative analysis of vocal differences, but I think there is sufficient evidence to support this split and it would put NACC in line with AviList. The plumage and behavioral differences (especially rump patch color and its use in courtship and agonistic interactions), strong genetic divergence in both mtDNA and genome-wide SNPs, and qualitative differences in aerial display calls, are sufficient for me to vote in favor of this split. The fact that it was lumped without rationale further argues for restoring *hudsonicus* to species status. I like Hudsonian Whimbrel (better than

American as suggested elsewhere, given its restricted distribution along the Hudson Bay) and Eurasian Whimbrel for English names.

YES. An extremely well-researched proposal, nicely done. The new genomic analyses from Tan et al. (2023) are what I find most convincing, alongside the numerous differences in phenotypic and largely non-overlapping distributions. I also like Hudsonian Whimbrel for the North American taxon.

YES. I vote in favor of this split, but barely. I am most convinced by the genetic divergence and the rump being used in mating displays. I am concerned, though, by what seems to me to be quite similar vocal courtship displays. I think I can hear some different display calls in the nominate birds, but the long whistled call that fades into a trill sounds very similar. I am not worried about the similar sounding flight calls, as I doubt they have any role in reproductive isolation. I'm happy with the English common names proposed and already in use following the AviList split.

YES. I was in favor of the split the first time it came up, and I am still in favor of it. I think the plumage and genetic differences are quite strong despite the lack of vocal differences, plus the apparent courtship/agonistic displays where the white rump is featured prominently for Old World taxa are all strongly indicative of species-level differences. It is especially interesting to me that there is no evidence of gene flow between *hudsonicus* and the Old World taxa, despite what appears to be at least some opportunities for it in Alaska, while there is evidence of extensive gene flow between *phaeopus* and *variegatus* despite them appearing to have allopatric ranges. I am in favor of the name Hudsonian Whimbrel for *hudsonicus* and Eurasian Whimbrel for *phaeopus*.

YES. I agree with the proposal and with the English name: Hudsonian Whimbrel and Eurasian Whimbrel.

YES. I voted for this split in 2022, writing the following at the time: “Strong genetic evidence is now available to split these taxa which appeared to have been lumped without much justification. There are also morphological differences.” The new data of this proposal (especially the Tan et al. 2023 study) reinforces my decision to treat these as separate species.

NO. A nicely done motion. One interesting thing, unrelated to the potential split, is the detailing of *rogachevae* breeding in central Siberia which has been variously attributed to both *phaeopus* and *variegatus.* Those might represent most of those that winter in coastal Thailand with more easterly breeding *variegatus* wintering further east in Australasia and Australia and New Zealand. I voted against this split before and do so again. Why is it necessary to follow Avilist? It seems to me to be a PSC split based essentially on molecular data. But, didn’t Van Remsen state in comments from the previous time we reviewed this issue that genetic data indicated that there was some introgression between New and Old World birds? The rump display, display calls, etc. all seem somewhat marginal and more importantly need better substantiation. The two look very similar. At Gambell on St. Lawrence Island in the Bering Sea, both *hudsonicus* and Asian *variegatus* occur in about equal numbers. Identification usually requires flight views from the right angle where you can see the rump and lower back.

I view the calls as a real problem and I'm puzzled as to why the contact calls are not important for mate selection.That low and characteristic series of trilling notes is the dominant call, even on breeding sites where it is uttered continuously by intruding predators, including humans. The flight song is more elaborate and is conjectured in the BNA account by Skeel and Mallory (1996) to be important for pair bonding. Skeel and Mallory cite Jehl and Smith (1970) and Obst and Spaulding (1994) that pair formation occurs shortly after arrival to the breeding grounds even though they might arrive singly or in small flocks. In Cramp (Birds of the Western Palearctic, 1983), Sutton is quoted that they can arrive on the breeding grounds already paired off. That is likely significant if the more complex "song" delivered in flight is significantly different, but what is the confidence level in the postulated differences? As for the standard call, nearly all species of shorebirds have their own distinct calls. I agree that American and Black oystercatchers have very similar or identical calls and (not surprisingly) hybridization is fairly frequent in northwest Baja California. I see hundreds of the *phaeopus* group every winter (February) in the Gulf of Thailand, Thailand, and can discern no difference from New World *hudsonicus.* One year Kimball L. Garrett joined us and he agreed that there was essentially no difference between the contact calls. All of the other species of *Numenius* to my knowledge have their own distinctive vocalizations. In sum, I continue to think that New and Old World subspecies of Whimbrel are best treated as subspecies of one another, much as we recognize the named subspecies of Rock Sandpiper, nominate *ptilocnemis* being particularly distinctive.

If the split passes (which it has), Eurasian and Hudsonian Whimbrel are fine for English names. The name "Hudsonian" is well ensconced for the English name for this subspecies, but usually with the English name of Hudsonian Curlew. It is too bad that "whimbrel" is too well embedded as an English name. All other *Numenius* are named "curlews."