2025-A-1
Treat extralimital *Pachyramphus salvini* as a separate species from Black-and-white Becard *P. albogriseus*

YES. An elegant resolution to this long-standing taxonomic conundrum. The vocal and genetic data and what seems like local sympatry all clearly point to species status. I vote to elevate *salvini* to species rank and adopt the common names selected by SACC, Cryptic Becard for *salvini* and continued use of Black-and-white Becard for *albogriseus*. This of course results in no changes to the names of the NACC area species.

YES. Nice proposal. Multiple lines of evidence (genetics, voice, morphology) and putative sympatry support elevating *salvini* to full species. This will put NACC in line with SACC and other checklists. I support the English names adopted by SACC of Cryptic Becard for *P. salvini* and Black-and-white Becard for *P. albogriseus*.

YES. The paper on which this is based seems to me a model for such cases, with its great analysis of disparate data types that all point to the same conclusion: *salvini* is a cryptic species, for which the English name Cryptic Becard is appropriate and already widely adopted. Retention of the English name Black-and-White Becard for *P. albogriseu*s s.s. is also appropriate as it is more widely distributed and thus familiar over a larger area.

YES. Phylogeny, morphology (size and plumage), and vocalizations support the split of *Pachyramphus salvini* as a separate species from *P. albogriseus*. What an amazing way to present species limits! I agree with the English name Black-and-white Becard for *P. albogriseus*; the decision on the English name for *P. salvini* corresponds to SACC.

YES. The data on genetics, songs, and morphology support the split. I agree with the proposal. English name Cryptic Becard for *P. salvini* and Black-and-white Becard for *P. albogriseus*.

YES. I agree with the split of *Pachyramphus salvini* from *P. albogriseus*, and with adoption of the English name Cryptic Becard for *P. salvini*. This aligns NACC with SACC.

YES. A strong integrative data set supports recognizing *P. salvini* as a separate species from *P. albogriseus*. This will bring the NACC and SACC taxonomies in alignment. I support using the English name Black-and-white Becard for *P. albogriseus.*

YES. Good detective work. My first question is whether I will see Cryptic Becard at or near Araucana Lodge southwest of Cali? I have only previously been in South America for two days, in Ecuador awaiting my trip to the Galapagos long ago. I enjoyed reading the entire back and forth within the SACC. While I agree with retaining the English name of Black-and-white Becard for the other species, and adopting Cryptic for the South American *P. salvini*, I wonder how many will wonder if the bird is cryptically colored rather than the taxonomy being cryptic. Then again we have the scientific name of *Sturnella neglecta* for Western Meadowlark. More to the point, the new English name for *P. salvini* should be entirely the discretion of SACC. For the record I do not like the name “banded” of any sort when referring to wingbars. It just invites confusion, in this case just referring to one wingbar. I was OK with Greater Pied and Lesser Pied.

YES. Puts us in line with SACC and other lists. Multiple data sets support the split (genetics/polyphyly, vocalizations, morphology). I’m fine with using Cryptic Becard and Black-and-White Becard.

2025-A-2
Transfer Pale-eyed Pygmy-Tyrant *Lophotriccus pilaris* to *Atalotriccus*

YES. As stated in the proposal, we could either transfer *Lophotriccus pilaris* to *Atalotriccus* now, in alignment with global checklists, or wait for a more comprehensive revision of the clade. I vote to make the change and then reassess when such a revision is published.

YES. I agree that the transfer to *Atalotriccus* is somewhat equivocal, but it appears to have reasonable justification and has been adopted widely elsewhere, so it seems least disruptive for NACC to make the change.

YES. Either option is fine with me, but I lean towards making a change now, even though we will likely (hopefully?) make another change soon. We know that *pilaris* does not belong in *Lophotriccus* because of priority, and *Atalotriccus* is technically a correct placement (being monophyletic), even though it should clearly be considered part of a different genus based on physical similarities.

NO. This is a difficult case. On the one hand, it would be advisable for the NACC classification to coincide with global lists. However, on the other hand, the SACC's decision to wait for evidence that allows the taxonomy of this group of flycatchers to be updated seems reasonable to me. I will lean towards taxonomic stability, so I recommend waiting for a more detailed and focused study of this group of organisms and waiting to know the decision of the SACC, given that it is a clade mainly distributed in South America.

NO. I prefer to wait, as SACC suggests, to see a publication on the taxonomy and nomenclature of this group. It is very difficult and complicated for me to decide based on only these results.

NO. As I am generally not a fan of monotypic genera, I would much prefer to wait until a broader decision is made on the limits of the different genera of this group, especially if there is a chance that the decision we make would be somewhat quickly changed in the broader upheaval. I would much prefer to see the Pale-eyed Pygmy-Tyrant included as part of broader genus, perhaps *Oncostoma*. While the proposal does point out that Pale-eyed Pygmy-Tyrant is somewhat distinctive, the fact that it does share similarities with these other small flycatchers has me strongly preferring to group them together in some way rather than break all these up into excruciatingly small genera that have little meaning.

NO. I would rather wait for a comprehensive taxonomic rearrangement of this clade based on the Harvey et al. dataset and perhaps other phenotypic data. We may be alone in not having this species in the monotypic *Atalotriccus*, but that is likely short lived and I think it will be more disruptive to have multiple taxonomic changes in a relatively short time.

NO. I'm with others and apparently SACC to await more genetic data and in the meantime maintaining "stability" until there are more data, recognizing that a change will hopefully be made soon. Adopting a single species genus knowing that a more precise placement will likely later come, makes me uneasy.

NO. It is better to wait until the whole clade is dealt with, rather than make a decision on this one taxon. A comprehensive approach is needed.

2025-A-3
Revise the taxonomy of the genus *Gygis*: (a) recognize subfamilies Gyginae and Anoinae

YES. 1 without comment.

YES. The phylogenetic data all support the recognition of Gyginae and Anoinae.

YES. Multiple studies support this division, and the change would align NACC with global checklists.

YES. This subfamilial division seems mandated by available data.

YES. The data support the existence of Anoinae and Gyginae. I agree with the proposal.

YES. The subfamilies Gyginae and Anoinae are necessary if we recognize Rhynchopinae and Sterninae.

YES. I very much enjoyed reading this proposal and the appendices. Given the topology of the currently recognized subfamilies, this is the most parsimonious solution. Note that it is misleading to refer to extant lineages as 'basal' lineages.

YES. Phylogenetic analyses show that Gyginae and Anoinae are separate groups from Sterninae; the results warrant their status as separate subfamilies.

YES. Great proposal; I enjoyed reading it. Clearly distinct monophyletic subfamilies; agrees with the phylogeny.

Revise the taxonomy of the genus *Gygis*: (b) treat *G. candida* and *G. microrhyncha* as separate species from White Tern *G. alba* (also see comments on proposal 2025-D-1 regarding English names)

YES. For *microrhyncha,* I am most convinced by the analogy of similar situations of genetic swamping in Black Stilt, Hawaiian Duck, White-headed Duck, and Golden-winged Warbler, all of which are generally now considered good species. The historical sympatry is an interesting line of reasoning. I would like to know more about the time scales involved in that assessment. Do we know for certain that they were actually temporally sympatric? Or was one present and then the other one replaced it 100 or 500 years later? I don't know if this sort of temporal resolution is possible with those subfossil records. Regardless, the structural differences especially (but also vocalizations) seem to me to be species-level characters. The bill size, bill shape, and lore feather pattern differences all lead to a very different looking bird, and rather than extensive intermediates over a large geographic region (as I would expect of subspecies), we see clear genetic dominance of one taxon over the other. This seems indicative of postzygotic reproductive isolation. Also, If we are going to consider *microrhyncha* to be distinct, then I suppose the same should go for *alba*, just by yardstick extension. I'm not even sure which two would then be considered conspecific: *candida* and *alba*, or *microrhyncha* and *alba*? Presumably the latter given the bill structure and lore feathering similarities, despite the size differences.

I like the reasoning for the proposed English common names, and especially Pratt's arguments in favor of Fairytern. Common, while not ideal, seems most appropriate for the widespread (and expanding!) *candida*. Little and Atlantic are perfectly adequate for the other two. I'm not concerned about confusion with *Sternula nereis*, especially if we used the non-hyphenated "Fairytern".

YES. Different lines of evidence (historic sympatry, bill morphology, plumage coloration, tail shape, vocalizations) support this treatment despite some evidence for intergradation. It would be interesting to do a more comprehensive genomic study to examine levels of hybridization and potential genetic swamping. I am ok with the proposed English names

Common Fairytern (*G. candida*), Little Fairytern (*G. microrhyncha*), and Atlantic Fairytern (*G. alba*), although I worry that this will be confused with "Fairy Tern." I am also open to other suggestions.

YES. My vote is somewhat reluctant as I still don't see that there is any real analysis of vocalizations, and the evidently broad hybridization of *candida* and *microrhyncha* can be interpreted to support conspecificity. However, I went back and listened again to vocalizations and believe I can hear some differences between the three in line with Pratt's descriptions of them, but this surely needs analysis. The differences in bill morphology are striking, and the other morphological differences support species status as well. If considering this situation to be somewhat similar to that of Golden-winged and Blue-winged warblers, or that of Ruddy and White-headed ducks, then I can see species status as justified. WGAC/AviList voted for the three-way split, as well. English names: I'm not really happy with any of these and, while recognizing that a perfect solution is unlikely. I think we'll need a more in-depth E names proposal and discussion. Addendum: I am not in favor of fairytern or fairy-tern. It is too confusing with the very entrenched Fairy Tern (highly familiar to people in Australasia, anyway), for one thing. (In fact, recently this very issue confused me when people called out Fairy Tern on a pelagic trip in New Caledonia!). The names used by AviList 1.0 will be Atlantic, Common, and Little White Tern and that's what I vote for, while acknowledging they are not inspired names. They are already in fairly common usage, too (except as Noddy, in Howell works).

YES. I agree with the proposal, split *Gygis alba* into three species.

YES. Phenotype (plumage, bill morphology) and geographic distribution support the split. The contact zone in the Marquesas Islands requires research. Adopt the English names suggested in the proposal.

YES. More genetic data would be helpful. However, the data we have show morphological (bill, tail) differences, at least some evidence of vocal differences, and fossil evidence showing historical sympatry of the two Pacific forms. Thus, I'm comfortable for now splitting these. I liked the proposed use of fairytern in the English names, and I appreciate taking the opportunity to return to names in local use. The proposed English names for each form are fine with me as well.

NO. I believe this case came across the committee's docket fairly recently, and I am still wary of the apparent extensive and widespread hybridization that has led to *microrhyncha* being restricted to the Marquesas Islands, when it formerly occupied a much wider range. While the apparent sympatry historically would certainly suggest that *microrhyncha* and *candida* operated as separate species under the BSC historically, the situation seems different now, and the two no longer appear to act as separate biological species.

NO. I appreciate the lines of evidence that support what may very well include multiple biological species (i.e., morphological differences, subfossils suggesting sympatry, vocal differences). However, given that there is some strong possibility of hybridization, I think it would be prudent to make our decision based on molecular data that can provide an objective assessment of genetic differentiation and any historical or ongoing introgression within the group. There seems to be little to no published, peer-reviewed analysis of DNA differences in the complex—I prefer to wait for that line of evidence alongside the excellent summary of other data provided here by Pratt. English names: I like the rationale for using ‘fairytern’ rather than ‘fairy-tern’.

NO. I tentatively vote against splitting subspecies *candida* and *microrhyncha* as separate species. NO to splitting Atlantic nominate *alba* as a separate species. I am uneasy about this split for a number of reasons, particularly splitting nominate *alba* from *microrhyncha*, although I haven’t tried to parse the calls. I appreciate the comment indicating there should be a more comprehensive and careful review.

As for genetic studies we have Yeung et al. (2009) and supplemented data by Thibault and Cibois (2017) stating that only one taxon is in the Pacific. I have not looked at Cerny and Natala (2022) or Thomas et al. (2004) which is not listed in the references to the motion. As for nominate *alba* Yeung (in litt. to Pratt) saying that it is very different genetically but this conclusion remains unpublished. Within the Marquesas we have intergrades only in the northwest part of the chain on tiny Hatuta’a and maybe Elao islands (separated by only 3 km) and oddly apparently on Mohotani in the southeast portion of the chain. The studies that have been done are not apparently based on field work but rather by an examination of specimens and by examining photos from D. S. Sargeant taken in 2013. Determining the extent of hybridization/introgression based on this evidence seems pretty conjectural. Looking at the remoteness of at least Hatuta’a and adjacent Elao, getting to these islands (perhaps by yacht or sailing ship?) must be difficult. Instituting conservation measures, even in telling pure birds from hybrids/intergrades seems even more difficult to undertake. I guess my question is has anyone looked at birds carefully throughout the islands of the archipelago? From northwest to southeast, the entire archipelago is some 165 km. Also, if *candida* (or intergrades) are only known from the small islands of Hatuta’a and Elao in the northwest and Mohotani in the southeast, Howell and Zufelt (2019) suggest that this is caused by competition for nest sites. There is something odd about all of this. How could *candida* be restricted to these few small islands within the Marquesas and not have spread to the larger islands? White Terns do range far from shore when foraging so what would stop them from occupying the larger islands where “pure” *microrhyncha* is present and there might be room for both taxa to nest? After all, *candida* is the great “invader". Then again, the two taxa being essentially parapatric, or sympatric with some (or lots) of interbreeding, doesn’t sound like subspecies to me. Perhaps they are species, but again, what’s stopping the invasion of *candida* throughout the Marquesas? The distance between the small islands of northwestern part of the chain (Hatuta’a and Elao) and the larger Nukuhiva is only about 50 km, seemingly an easy reach for *candida* and intergrades. It’s been about a half century since *candida* and intergrades were first noted from the small islands. It’s only about 16 km (10 miles) from Mohotani to the larger islands of Hivaoa or Tahuata.

One comment wondering whether *candida* and *microrhyncha* were really broadly sympatric in the Pacific some 1500 or more years ago (subfossil data) leaves one questioning if this was maintained over decades/centuries or just reflects the gradual replacement of *candida* from *microrhyncha,* thus subfossils would be present even at the same time period. Am I understanding this correctly? Comparisons are made to Blue-winged and Golden-winged warblers and to Ruddy and White-headed ducks, the comparison I thought of. In the latter case, the introduction to Spain of Ruddy Duck was apparently accidental (why, they are not particularly edible?). Trying to wipe out the Ruddy Ducks in Europe is certainly desirable. I can appreciate the problems in protecting Black Stilts, but in both cases the taxonomic issues don’t involve NACC. With the case of Blue-wings and Golden-wings there is still some partitioning going on, by range (Blue-wings absent in the northern part of Golden-wings’s range) and by habitat. Golden-wing seems to maintain purity, at least in places, in early successional habitats where Blue-wings are also found. We have an ongoing debate about Yellow-rumped Warblers where one would think based on morphological, vocal, and genetic data that there were two species involved, yet in the area of overlap, apparently largely (completely?) hybrids are only found. The committee is divided about 50/50 with half feeling it doesn't meet the BSC concept of a species and half feeling that the restricted hybrid zone argues for species recognition. Actually, the comparison that came to mind for me was for the Neanderthals and Cro-Magnons, the latter gradually replacing the former, with the former last holdouts being caves fringing the Mediterranean some 30,000 years ago, or at least that's what I learned at some point. Perhaps the Marquesas are the last shelter for *microrhyncha*.

I wondered about how much these taxa moved around and looked at Pyle and Pyle (2017) and White Terns are commonly recorded at sea throughout the Hawaiian region with little seasonal or distributional patterns and are regularly seen on one day pelagic trips with some 25-30 per day. If this is true in the Marquesas, surely a 3 km barrier between tiny Hatuta'a and much larger Elao would hardly prevent mixing or somewhat longer distances between other islands within the Marquesas. All the more reason for field work there to see what's really happening.

In summary, until we know the extent of hybridization going on in the northwest Marquesas (plus Mohotani to the southeast within the chain), this all seems a bit speculative, this despite the rather distinct morphological differences.

I did some googling about the Marquesas and came across a newsletter from Island Conservation. In the February 27, 2017 issue, there is an article by Richard Griffiths. He and his team spent three days on Elao and Hatuta'a in the northwestern portion of the Marquesas Islands. They address the conservation issues there (numerous) with introduced mammals, erosion and the like. They did a bird survey and in the article Richard notes he got his lifer Phoenix Petrel. He discusses Black Noddy Terns being a nuisance for sleeping on Hatuta'a. There is no mention of White Terns. Perhaps a group like this one (headquartered in Santa Cruz) should be contacted about the taxonomic issues with White Terns and the need to conduct censuses within the Marquesas in assessing the population of both of these taxa, plus intergrades. In Niethammer and Patrick (1998) there is a statement stating that Pratt et al. (1987) in justifying the split of *micorhyncha* say it is not based on published systematic evidence, but rather is an effort to increase awareness of this population. I looked up what Pratt et al. (1987) said and in the introduction to Fairy-Terns he states in part: "Although the most recent study [Holyoak and Thibault 1976] recognizes only one species, both we and other ornithologists to whom we have talked believe that at least two species may be involved. We recognize two tropical Pacific species mainly to encourage birders to look for both forms, and thus to aid our knowledge of their distributions and interactions. We do so realizing that the two may prove conspecific." Pratt et al. (1987) called *microrhyncha* Little Fairy-Tern but treated it then as a subspecies of *G. alba.* I'm not sure what has changed for him to modify that stance other than incrementalism. While I appreciate Doug's concerns and agree with his goals, some of his motivations concern me. On the other hand these taxa are already being split by various authorities so perhaps the issue will rise to public attention, regardless of what AOS does.

I'm troubled by the splitting, but I am also a bit troubled by the lumping of *leucopes* from the Pitcairn island group (Henderson and Ducie islands with intergrades on Oceno). Remsen once argued that morphological differences, even if minor, justified subspecies recognition, if consistent, as apparently it is with *leucopes.* Olson seems to have acknowledged that *leucopes* deserved recognition in his unpublished manuscript. I think it odd that *leucopes* is merged as a subspecies because the differences are "minor" yet nominate *alba* is recognized as a species where the differences may be minor too.

Recognizing the South Atlantic subspecies as a species seems even more speculative. Nominate looks much more like *microrhyncha* overall except for the size and, slightly, the tail fork and apparently the calls for whatever significance that means. Talking about this issue with Louis Bevier who spent much time working on the BNA account (Niethhammer and Patrick), he mentions that within the *alba* group (*alba* and *microrhyncha*) the rectrices are not pointed and the tail is short so that the primaries extend well beyond the tail at rest, while in the *candida* group the rectrices are pointed, except the middle pair, and tail is relatively long so that at rest the tips of the primaries and the tip of the tail are about equal. Regarding size, the birds resident on St. Helena are significantly smaller than the rest of those breeding on the few South Atlantic islands, so much so that Olson wondered if they might warrant separate subspecies status. I haven't checked the measurements, but how do the morphometrics of the smaller *alba* from St. Helena compare to the larger *microrhyncha* from the Marquesas? Rowlands et al. (1998) states in the St. Helena White Tern account (pp. 171-174) under Taxonomy (p. 174) "...However, the significantly smaller size of St. Helena birds compared with those found in Ascension Island (nominate race), Ilha da Trindade or Fernando de Noronha, may warrant raising them to a subspecies status (Olson 1975:28)." They add: "The downy chick found by NPA (Plate 44), dark and mottled brown, was in marked contrast with downy chicks in the Indian Ocean (Carajos Shoals) that were more grey and less mottled (BWR)." So, this indicates another path to investigate (downy chicks). Indian Ocean birds are *candida.* If the downy chicks are distinctly different, what about the juvenal (or juvenile) plumage? I looked at the photos and text in Howell and Zufelt (2019) and he has photos of juveniles of all three taxa (no downy chicks). Under the introductory text for the complex he says "Juv. has variable gingery-brown edgings and wash to upperparts which fades quickly and is rarely noticeable at sea." He has two photos of *candida*, one with lots of this pale gingery color, one with little and there is a photo caption that states "juvs. Variable." The one photo of *microrhyncha* has this color too, but possibly in the scapulars the color is darker, as it might be in the one flight photo showing the dorsum difference of a juvenile *alba* where some juvenile scapulars are retained. If the downy young and/or juvenal plumages are consistently different between *candida* and *microrhyncha* that would be an additional argument for separate species recognition, but if the latter is more like *alba* it could be argued that *microrhyncha* should be merged with *alba* as done by Pratt et al. (1987). Could it be that there was once a widespread pan-oceanic *alba* type which is now more local with nominate *alba* being especially separated, and then the invasive (and more different) *candida* gradually taking over the Pacific? I'm well into areas where I might be completely off on the wrong track, but again more comprehensive taxonomic studies of all taxa of White Terns would no doubt be helpful.

I'm comfortable with Fairy-Tern for a new English name, even though "indexers" will be unhappy. On the other hand Fairytern is fine too. Common is fine for *candida*, at least in this time frame as it is the only taxon in the Indian Ocean and the predominant taxon in the Pacific. Little Fairy Tern is fine for *microrhyncha* though I'm opposed to this split and wonder how much smaller they are then St. Helena Atlantic Fairyterns. Actually the "Fairytern" construction is perhaps helpful as Little Fairy-Tern is perhaps confusing with *Sternula nereis* (Australian Fairy Tern) and *Sternula albifrons* (Little Tern).

Lastly I am profoundly grateful for the discovery and sharing of Storrs Olson's unpublished

1990's manuscript. Thanks to Helen James, Doug Pratt and others (?) for its discovery and sharing it in the motion. Storrs is greatly missed.

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2025-A-4
Treat *Myiarchus flavidior* as a separate species from Nutting's Flycatcher *M. nuttingi*

YES. Nice proposal that clearly lays out the evidence for elevating *flavidior* to full species based on vocal (including behavioral responses to playback), phenotypic, ecological, and possibly genetic differences as well as sympatry without hybridization. Regarding English names, I have read the pros and cons of Salvadoran Flycatcher and am ok with that name although I'm open to alternatives.

YES. A very convincing proposal that makes it hard to argue against this split.
English names: The reasons stated in the proposal for adopting the name Salvadoran Flycatcher, which already has some traction, seem sound, but of course it occurs widely in Honduras too, plus a bit of Nicaragua, and judging from the map it might turn up in southeastern Guatemala too. Addendum: YES to Salvadoran Flycatcher. Although another committee member makes a good case for not using Salvadoran, I don't see a better option, and I don't think it's a bad name. Of course, a great many species have names much more tenuously based than this, and I like the idea of naming at least one species

after this country.

YES. The vocal differences, together with the observations of birds in sympatry or near sympatry/parapatry and no evidence of interbreeding (no mixed pairs), all seem very solid evidence that the two are acting as separate species. The genetic evidence is certainly intriguing as well, but I would be curious to see how that relationship would hold up with additional loci sampled. YES to Salvadoran Flycatcher. Although the points raised against the name are valid, the other points in favor of it to me outweigh the drawbacks of it. As far as "birds named for places" goes, this is far more accurate and appropriate than the vast majority out there, including some recent names that we've voted on. That being said, I am not strongly attached to the name, and would be happy to consider other options that are presented.

YES. The evidence of the differentiation between *M. flavidior* from *M. nuttingi* is clear, even when they have sympatric distributions. Genetic and morphological data support the split. English common name: Salvadoran Flycatcher for *flavidior*.

YES (weakly). The geographic distribution, including areas of sympatry, vocalizations (calls and songs), and slight differences in plumage coloration, support *Myiarchus flavidior* as a distinct species from *M. nuttingi*. The genetic data are so far inconclusive. Although Sari & Parker sequenced three mitochondrial regions and one nuclear intron, the phylogenies they present, both by Maximum Likelihood and Bayesian, are based on only two mitochondrial regions, ND2 and cytb, and the two phylogenies present different relationships for *M. n. nuttingi* and *M. n. flavidior* (the subspecies *M. n. inquietus* was not included). Unfortunately, Harvey et al. (2020) did not sample *M. n. flavidior*, only *M. n. nuttingi* and *M. n. inquietus*, taxa that turned out to be sister groups according to genomic data. The available data on sympatry areas are valuable and should be formally analyzed and published. It is essential to study the contact zones of *M. n. flavidior*, both with *M. n. nuttingi* and *M. n. Inquietus*. English name: NO to Salvadoran Flycatcher. Figure 8 in the proposal presents sampling locations in El Salvador, Honduras, and Nicaragua; this figure may give the impression that the "core range" of *flavidior* is located in El Salvador, as mentioned by the authors. However, *flavidior* occurs from Oaxaca and Chiapas in Mexico to western Nicaragua. Therefore, the geographic range of *flavidior* in El Salvador could well represent between 1/3 and 1/4 of the total range of the taxon. If the English common name refers to the geographic range of the taxon, we need a name that considers most of that range. YES to keep Nutting's Flycatcher for *M. nuttingi*.

YES. Reasons are given in the proposal and in my comments below. The issues with the genetic data raised by the other committee members are certainly concerning. However, I am thoroughly convinced by the drastic (to me) vocal differences that are apparently maintained in sympatry. That alone basically requires a split under the BSC, especially for a suboscine. I do have vocalization data from all suboscine passerines (which I really need to publish), and I did a quick clustering analysis of vocal variation in *Myiarchus*. Unsurprisingly, that showed that *flavidior* and *inquietus/nuttingi* are very different in their songs, to a larger degree than other good species of *Myiarchus*. I went and re-read the Lanyon (1961) paper cited in the proposal, just to make sure the proposal didn't overlook something on vocalizations there, given that Lanyon was an excellent observer and well aware of the importance of vocalizations. I was surprised to find that his sonograms show the clear vocal differences between *flavidior* and *nuttingi/inquietus* but that he apparently failed to realize that, for example, the long twittering call is unique to *flavidior*, that the whistled note is longer in *flavidior*, and that the burry/rolling call of *nuttingi* is absent in *flavidior* (these are all shown in his Figure 11 and are labeled by locality). So, it does seem that he overlooked the vocal differences, which I'm sure would have led to very different conclusions on his part regarding species status. One caveat; Lanyon did note what he called intermediates (based solely on size, again overlooking the vocal differences) in Oaxaca, but in looking at his measurement data, it looks to me like *flavidior* and *nuttingi* overlap completely in mensural characters so I'm not sure how he was classifying these as intermediates. He does also have a nice illustration of the tail pattern differences (Figure 2) noted by Howell et al. English name: YES to Salvadoran Flycatcher, but I acknowledge the concerns raised by other committee members. I am certainly willing to consider other name suggestions, but preferably in consultation with the lead proposal authors. A few points in favor of Salvadoran. First, many (most?) geographic bird names are imperfect, and we had no issues with the name Guatemalan Flicker for *Colaptes mexicanoides* which has a comparable distribution in the highlands from Chiapas to Nicaragua. Second, all other Central American countries (except Belize) have a bird with the country name, and aside from Honduran Emerald, none are endemic to that country. One (Panama Flycatcher, another *Myiarchus*) extends from Costa Rica to Venezuela and Ecuador! If we are to consider another name, the options that I see are 1) something based on the specific epithet *flavidior*, which means "yellowish", but I find such a name to be rather bland (and Yellowish Flycatcher is already taken), or something based on the bioregion in which it is found. That *flavidior* is endemic to the Central American Dry Forest biome I actually find to be another point in favor of species status, and a name based on that bioregion is worth considering. I am unaware, however, of a name for the region in either English or Spanish. Perhaps someone else knows of a good name for this region? The other species endemic to this bioregion (Blue-tailed Hummingbird, White-bellied Chachalaca, Pacific Parakeet, Giant Wren, Rufous-backed Wren) mostly have plumage-based names, except for the parakeet. I suppose Pacific Flycatcher could work. I was never a fan of "Pacific" for a species name, but at this point, we do have various species with that name extending from Alaska (*Troglodytes*) to Ecuador (*Myiopagis*).

YES. I can't conceive that the strong vocal differences mean anything other than separate species as with Alder and Willow flycatchers and Tropical and Couch's Kingbirds. Within *Myiarchus* tail patterns can differ between subspecies, notably within La Sagra's Flycatcher where *lucayensis* from the Bahamas (southeast to Great Inagua; absent from Turks and Caicos islands) differs in tail pattern from nominate *sagrae* from Cuba and Grand Cayman. I believe their calls differ too, but need to recheck this. If so, perhaps they warrant separate species status. Both have occurred in the southeastern U.S., *lucayensis* nearly annual in south Florida, nominate *sagrae* from Dallas County, Alabama; specimen from 14 September 1963). English name: I am OK with Salvadoran Flycatcher for an English name, particularly if we give a justification with the English name chosen (*i.e.* the center of the range), something that should be done more often. Given their small size, El Salvador doesn't get much named for them in the natural world. I don't like "Pacific" for the English name but I'm certainly open to other suggestions. The fact that it is the only *Myiarchus* in El Salvador (according to the authors of the motion; only breeding *Myiarchus*) and the type specimen is from this country perhaps adds additional strength to the arguments for the suggested English name.

YES. I vote in favor based on differences in vocalizations plus habitat, plus the two forms occur in sympatry without interbreeding. Although the genetic data are not conclusive, they do not conflict but rather support the other evidence. English name: NO to Salvador Flycatcher based on other comments on distribution; however, I don't have a better name to suggest.

NO. While there is strong evidence for vocal differences and habitat differences, the genetic data are lacking to conclusively say that these are reproductively isolated. The figure shown in the proposal and the Neotropical Birding article is a cladogram—that is to say that the branch lengths are not proportional to the number of nucleotide substitutions. Therefore, the claim in the proposal that '[the molecular data] also show a very deep split between the taxa, strongly indicative of species status (Sari and Parker 2012).' is unsubstantiated. The inferred phylogeny is also based solely on mitochondrial DNA (ND2 + cyt b). The node that renders Costa Rica and El Salvador populations as polyphyletic is not strongly supported, and we might expect genetic clustering to happen between those sites simply due to isolation by distance. The vocalization and habitat data are highly suggestive of multiple species, but I would prefer a comprehensive, well-sampled population genetic study—especially from sites where these putative species co-occur—to indicate that these lineages are reproductively isolated. While the Howell et al. (2024) study observed positive associative mating, even a small number of genetic migrants per generation can lead to genetic similarity between diverging lineages. As we recently saw with the *Empidonax* complex, tyrannid species limits can be complex. I would prefer to await a more comprehensive molecular analysis of the group that is paired with these excellent phenotypic and observational data. English name: If the species split passes, I am ok with Salvadoran Flycatcher. I understand the drawbacks, but was swayed by comments from Roselvy Juarez.

External comment on English names from proposal author Roselvy Juarez: Because most *Myiarchus* look very similar and share homologous vocalizations, it is not easy to pinpoint a feature unique to *flavidior*. When analyzing options, we also considered avoiding names that can be misleading to biologists and ornithologists alike. When identification relies on the combination of subtle field marks, naming the bird after a single field mark could have the unintentional effect of suggesting that field mark as key.

The name Pacific is perhaps too generic and doesn't single out this species since many similar *Myiarchus* are also found on the Pacific slope of Central America. Brown-crested Flycatcher, for example, is often more common in the Pacific lowlands of northern Central America than *flavidior*. Older literature suggests that there are two distributional 'bands' of NUFL in northern Central America: *flavidior* in the Pacific lowlands up to 300 m and *nuttingi* in the interior between 750 and 1,700 m. Our field work in Honduras has found that this is not accurate and that *flavidior* also occurs in the interior, at least as high as 800 m, while we have found *nuttingi* as low as 500 m (personal data RJ and JVD, manuscript in preparation). Names derived from the American Dry Forest Biome are not representative of *flavidior* either because in several locations of El Salvador and Honduras, multiple species of *Myiarchus* occur sympatrically in dry forests.

We thought about names derived from the vocalizations, but like names derived from plumage features, these too turned out not to be suitable. Sad Flycatcher, in relation to the smooth overslurred call, is no longer available, and sad whistled notes are not unique to this *Myiarchus*. Piping/whistling Flycatcher, in relation to the other common call of *flavidior*, perhaps is not an option either because some other *Myiarchus* (e.g., Dusky-capped, Yucatan, and Panama Flycatchers) have their own version of a piping twitter.

An alternative name is Cuzcatlan Flycatcher, from a Nahuatl word, because the Castellan version is occasionally used interchangeably as Salvadoran (Cuscatleco). A reason against using this name, besides that it is even more restrictive than Salvadoran Flycatcher, is that it is very close to the word Cuscatlán, the current name for the smallest department of El Salvador. Furthermore, the pre-Columbian Nahuat state Cuzcatlan does not encompass the whole El Salvador, and *flavidior* was described from a specimen collected outside of the area formerly known as Cuzcatlan. For the two reasons highlighted above, some Salvadorans may not feel represented by the name Cuscatlán/Cuzcatlan; even spelling the name gets confusing!

In summary, we still feel that Salvadoran Flycatcher is our best option. Plus, it aligns well with names adopted for other rather nondescript *Myiarchus*, e.g., Panama, Venezuelan, Grenada, Puerto Rican, and Galapagos Flycatchers, named after geographic regions.

2025-A-5
Revise the linear sequence of *Dumetella* and *Melanoptila* (Mimidae)

YES. 2 without comment.

YES. A small change to conform with our linear sequence guidelines. The ddRAD data are much more reliable than mtDNA for the tree topology.

YES. This minor change in linear sequence conforms to our guidelines and is supported by a large ddRAD data set.

YES. Well-supported minor change to conform with guidelines.

YES. I agree with the proposal because it is supported by genomic data

YES. The molecular data and geographic distributions of these taxa support this change.

YES. Changes in the linear sequence are supported by phylogenetic analysis.

YES. This change is based on new data and rules regarding sequencing related to distribution of equivalently positioned taxa.

2025-A-6
Transfer Slaty-winged Foliage-gleaner *Philydor fuscipenne* to new genus *Neophilydor*

YES. I vote in favor for all the reasons in the proposal, and especially following the sentiments of Zimmer in his SACC proposal comments. Looking at the two trees (especially the Harvey tree), we may also need to move *Neophilydor* in the linear sequence.

YES. Reasons are given in the proposal. This also aligns NACC with SACC.

YES. This species cannot be maintained in *Philydor*, hence the new genus is well-justified.

YES. While I generally do not like creating successively smaller genera, I see no good alternative to placing this species (together with Rufous-rumped Foliage-gleaner) anywhere but a new genus, as the relationships with other taxa are not necessarily well-resolved.

YES. The strong evidence supported a new genera *Neophilydor* for *fuscipenne* and *erythrocercum*. I agree with the proposal.

YES. Many of those included in this suboscine lineage have been recognized for a long time. This proposal received strong support from SACC, and this seems the easiest solution.

YES. Reasons are stated in the proposal and SACC comments. This aligns with SACC.

YES. Reasons are stated in the proposal and SACC comments and their previous decision.

YES. Conforms with the phylogeny and is already adopted by SACC.

2025-A-7
Transfer Little Ringed Plover *Charadrius dubius* to *Thinornis*

YES. I'm happy to follow the global checklists and recognize *Thinornis*, especially since we only have one species in our area, and it is accidental. I find Schodde's first two arguments compelling, but the weak support values *within* the Old World clade should not be a reason to recognize *Thinornis*. The old age of the clades and that they represent separate zoogeographic radiations are acceptable reasons for me. However, we should be extremely hesitant about making additional genus-level splits in this group, as we're already approaching what I feel are over-split genera in the family.

YES. Reasons are given in the proposal. This also aligns with WGAC.

YES. This forms a reasonably well-defined, deeply diverged clade that makes zoogeographic sense.

YES. I do not agree with this change, as it does not seem necessary to further split *Charadrius* into such successively smaller genera, but I think we should follow WGAC in this instance since it largely concerns a group of birds outside of our region.

YES. I agree with the proposal to follow the revised WGAC decision and transfer *dubius* to *Thinornis*.

YES. This is largely extralimital and the rationale of having two separate, reasonably sized genera that correspond to distinct zoogeographic reasons is compelling.

YES. For proposal 2024-A-3, I followed the recommendation and voted to keep *Charadrius dubius* in the genus *Charadrius*. However, I noted that we should wait and consider recommendations from global authorities since it is a mainly extralimital species. Given that the WGAC has decided to transfer *C. dubius* to *Thinornis*, I vote YES to the genus transfer.

YES. The zoogeographic case for this split doesn't make sense unless we are talking about species in the Old World vs. the New World. And their appearance sure seems varied. I will state that the one species that has occurred in the western Aleutian Islands several times sure sounds pretty different from other *Charadrius.* I'm not surprised they belong to a different genus. I have seen several Long-billed Plovers (in northern Thailand in winter), but don't believe I've ever heard one call.

YES. I vote in favor to conform with WGAC's most recent decision on this case, which only applies to an extralimital species for us.

2025-A-8

GENERAL COMMENT: A difficult batch of proposals. My sense is that although we are seeing extremely rapid improvements in sequencing technology, sampling breadth, and computational methods, it still feels like we're in the era where we're sorting out the reliability of each marker type. This is perhaps similar to the early stages of the mitochondrial data revolution for taxonomy, which we of course later realized was reliable only to a certain degree. I would therefore rather proceed conservatively with these changes, with the realization that many or even most of them will eventually be implemented. For this proposal, then, I will vote to accept linear sequence changes supported by multiple studies/data types and consistently high node support values. That last part, however, is perhaps a poor measure. We now know that maximum likelihood bootstrapping methods, especially, overestimate node support values with massive numbers of loci. The above issues are of course especially problematic for the short internode distances in the deeper parts of the trees, which tend to correspond to order (or higher) relationships. I was comforted to see that these criteria do mostly correspond with the recommendations made by the proposal authors.

Make changes to our linear sequence of families and orders: (a) Flip Pterocliformes-Columbiformes and Cuculiformes so that Cuculiformes precedes Pterocliformes-Columbiformes in the linear sequence

YES. Nodes involving orders are difficult to resolve. However, I think we should follow the phylogenies by Stiller et al. 2024 and Prum et al. (2015), even when the node support is low.

NO. 1 without comment.

NO. This is a borderline case with short internode distances and conflicting relationships with different data types. I would prefer to wait on this one, with the realization that it is very likely correct.

NO. This is not well-resolved so I think it's better to wait on making this change.

NO. Given the weak support for this relationship in Stiller et al. and Prum et al., and other relationships recovered in other studies, I think it is better to wait on making this change until we have a better handle on what is going on here.

NO. I prefer to keep the linear sequence until there is new evidence with high support.

NO. Weak support for this relationship. Some other partitioning or inference strategy could reveal a different set of relationships and render this incorrect.

NO. Only Prum and Stiller trees support this, and without strong support; let's not move unless we have stronger data.

Make changes to our linear sequence of families and orders: (b) Change our current linear sequence of Rallidae-Heliornithidae-Aramidae-Gruidae to Aramidae-Gruidae-Heliornithidae-Rallidae

YES. 1 without comment.

YES. Multiple studies and data types arrive at the same or similar relationship, and the internode distances are long, making these conclusions more reliable.

YES. Different studies show similar phylogenetic relationships.

YES. Reasons are given in the proposal.

YES. I agree with the proposal. Multiple studies support this linear sequence change.

YES. This topology is supported by multiple genome-scale data sets, and the number of species in each of these families supports this change.

YES. Reasons are mentioned in the proposal.

YES. Multiple lines of support.

Make changes to our linear sequence of families and orders: (c) Flip Strisores (Caprimulgiformes-Steatornithiformes-Nyctibiiformes-Apodiformes) and Gruiformes-Charadriiformes so that Gruiformes-Charadriiformes precedes Strisores in the linear sequence

NO. 1 without comment.

NO. A long-intractable relationship. It seems to me that we're getting close to a confident answer on the relationships among Strisores, but we're not quite there yet.

NO. This is not well-resolved so I think it's better to wait on making this change.

NO. Reasons are outlined in the proposal.

NO. I prefer to wait for more studies on the relationship of this group.

NO. Relationships supporting this change seem tenuous.

NO. Reasons are mentioned in the proposal.

NO. Not enough consistent evidence to support this change.

Make changes to our linear sequence of families and orders: (d) Flip Pelecanidae-Ardeidae and Threskiornithidae so that Threskiornithidae precedes Pelecanidae-Ardeidae in the linear sequence

YES. 1 without comment.

YES. Same reasoning as in (b).

YES. Different studies show similar phylogenetic relationships.

YES. Reasons are given in the proposal.

YES. Enough studies support this change.

YES. This is straightforward based on a strongly supported relationship.

YES. Reasons are mentioned in the proposal.

YES. Consistent support for this switch across studies.

Make changes to our linear sequence of families and orders: (e) Flip Cathartiformes-Accipitriformes and Strigiformes so that Strigiformes precedes Cathartiformes-Accipitriformes in the linear sequence

NO. 1 without comment.

NO. Same reasoning as in (c).

NO. This is not well-resolved so I think it's better to wait on making this change.

NO. Reasons are outlined in the proposal.

NO. I prefer to wait for more studies on the relationship of this group.

NO. The node supporting this change is not strongly supported.

NO. Reasons are mentioned in the proposal.

NO. Not enough consistent support within or across studies.

Make changes to our linear sequence of families and orders: (f) Flip Bucconidae and Galbulidae so that Galbulidae precedes Bucconidae in the linear sequence

YES. 2 without comment.

YES. Minor change to conform with linear sequence guidelines.

YES. Reasons are given in the proposal.

YES. I agree with the proposal.

YES. Strongly supported change in multiple phylogenies.

YES. It is timely to make this change.

YES. Consistent support across studies and this flip agrees with the number of species. Of course, position based on number of species has no biological meaning, it's just an agreed upon convention.

Make changes to our linear sequence of families and orders: (g) Flip Regulidae and Dulidae-Bombycillidae-Ptilogonatide-Mohoidae so that Dulidae-Bombycillidae-Ptilogonatide-Mohoidae precedes Regulidae in the linear sequence

YES. Reasons are outlined in the proposal.

YES. Reasons are mentioned in the proposal.

NO. All three studies have found different relationships, and there is also a significant issue of a lack of sampling in the most recent study, which lacked several relevant families in the group that could result in long-branch attraction.

NO. This is not well-resolved so I think it's better to wait on making this change. Also, lack of sampling of some families in the Stiller et al. (2024) study is an issue.

NO. I prefer to wait for more studies on the relationship of this group.

NO. This node is not well-resolved, I would prefer maintaining the status quo in that case.

NO. Given the discrepancies in the studies, maintain stability and await further information.

NO. I would argue that there is not enough consistent support.

2025-A-9
Transfer Spotted Dove *Streptopelia chinensis* to *Spilopelia*

YES. I vote in favor but without strong conviction. It seems to me that the only phylogenetic tree that requires the recognition of *Spilopelia* is the BEAST analysis by Bruxaux (2018), which is not peer-reviewed (although I know we've been lax on this requirement recently), but I worry that something odd is going on when one of the mitochondrial trees supports this clade while the other analysis does not. Also, do we have a sense of the ages of these clades? The sister group is considered a single genus (*Columba*), so the clear alternative to recognizing *Spilopelia* is to merge *Spilopelia* and *Nesoenas* into *Streptopelia*, and they are clearly all allied species. That said, recognizing *Spilopelia* would eliminate any of the polytomy issues from the other studies, *senegalensis* and *chinensis* are a clade, these seem to be a fairly deep split (but what is the time scale?), and they are largely outside our area.

YES. Reasons are given in the proposal. This also aligns NACC with other global checklists.

YES. While not absolutely mandated, I'm not comfortable with *mayeri* and *picturata* being in *Streptopelia. Nesoenas* is recognized for these, and *Spilopelia* pretty much has to be as well.

YES. Reasons are outlined in the proposal. The support for an expanded *Streptopelia* is not particularly strong, so moving the Spotted Dove to *Spilopelia* seems to be the best approach here.

YES. I agree with the proposal.

YES. I am fine with following WGAC's lead in this largely extralimital case.

YES. Phylogenetic evidence supports transferring *Streptopelia chinensis* to the genus *Spilopelia*. Although the phylogeny did not incorporate thorough sampling of *Streptopelia*, as mentioned in the proposal, the monophyly of *Streptopelia s.s.*, *Spilopelia*, and *Nesoena*s is strong. Since *S. chinensis* is an introduced species in the NACC area, I support adopting this change that global lists have already incorporated.

YES. To back up a bit, Gibbs et al. (2001) have a nice review of the genus *Streptopelia* (p. 240). Following Goodwin (1983) they divide the genus into three groups, the first including the four true turtle-doves (*S. turtur, S. orientalis, S. lugens* and *S. hypopyrrha*), the 2nd group includes the various collared-doves for which *S. tranquebarica* of South Asia is the most distinct. The third group holds *S. chinensis* and *S. senegalensis.* They state "The third group holds two species, *chinensis* and *senegalensis*, which are relatively strongly differentiated from the rest of *Streptopelia* and are probably the earliest offshoot of the group. Separating this latter group and putting it back into its own genus has a background story.

A few other comments, Gibbs et al. (2001) under *C. mayeri* say that the species is sometimes placed in *Nesoenas.* Under *C. picturata*, Gibbs et al. (2001) say that this species is sometimes placed in *Streptopelia.*

Back to the introduction to the genus, under this third group, Gibbs et al. (2001) state that under *S. chinensis* "some of the races of *chinensis* are as different from each other as many good species but are treated as a single species because of the existence of several intermediate races. I would add that the vocalizations (song) of the group from South Asia (*suratensis*) and China (nominate *chinensis*) to me sound basically alike. Within our area, they are common and widespread throughout many of the Hawaiian islands (nominate *chinensis*). They (also nominate *chinensis*) were formerly widespread over much of Southern California except for the southeastern deserts where there are a few records. It was locally established in the Central Valley and are also at Avalon, Santa Catalina Island. They have now mostly disappeared from all but Santa Catalina Island. The reason for the disappearance is believed to involve Cooper's Hawks which have become fairly common urban nesters and apparently Spotted Doves haven't learned how to avoid them.

I"m puzzled by the result found in Bruxaux (2018) that Ring-necked Dove (*S. capicola*) is sister to Yellow-legged Pigeon (*S. pallidiceps*). Did I read this right?

References:

Gibbs, D., E. Barnes, and J. Cox. 2001, Pigeons and Doves, A Guide to the Pigeons and Doves of the World. Yale University Press, New Haven and London.

Goodwin, D. 1983. Pigeons and Doves of the World. 3rd Edition. British Museum (Natural History). London.

YES. Due to lack of resolution and consistent monophyly with *Streptopelia*, a stable approach going forward is probably to move to *Spilopelia*. This also conforms with other checklists for this introduced species.

2025-A-10
Treat Plain Xenops *Xenops minutus* as three species

YES. The three clades are vocally quite distinct (especially important in suboscines of course), and correspond to very distinct genetic clusters. Critically, the songs are consistent across the range of each group, even where the ranges somewhat approach the other vocal clusters. The playback experiments also support species treatment. I'm happy to follow SACC and WGAC for the English common names.

YES. Vocal and genomic differences, and behavioral responses to playbacks, support recognition of three species. This also aligns NACC with SACC. I am fine with the English names adopted by SACC.

YES. As elaborated by the SACC proposal and votes, this three-way split is definitely warranted. I'm not a big fan of "Plain-Xenops" but I can see its utility and it has already been adopted widely. This group name together with the geographic names, as already adopted by SACC, seems the best solution.

YES. I vote to adopt the treatment of SACC so that we are in alignment, although I am not a big fan of the hyphenated "plain-xenops" for all three species.

YES. I agree to follow SACC and to treat *Xenops minutos* as three species and adopt the suggestion of the English names: *Xenops minutus* – Atlantic Plain-Xenops; *Xenops genibarbis* - Amazonian Plain-Xenops; *Xenops mexicanus* – Northern Plain-Xenops.

YES. Strong molecular evidence is corroborated by vocal differences and this change was unanimously supported by SACC. The "Plain-Xenops" hyphenated name is a little awkward, but I am ok with those for the English common names.

YES. Phylogenetics, plumage coloration, and vocalizations support treating *Xenops minutus* as three species. Following on SACC, adopt the English name Northern Plain-Xenops for *Xenops mexicanus*.

YES. I agree with the three-way split. "Northern" is fine for the northern species, the one in our area of jurisdiction. It is up to the SACC to determine their English names, and they have unanimously selected Northern Plain-Xenops, Amazonian Plain-Xenops and Atlantic Plain-Xeonops. I wondered why Atlantic for *X. minutus*? In terms of the area of the coast, *X. genibarbis* seems to occupy as much, if not more, of the Atlantic coast of South America. Since it is the most southerly taxon of these three, why not Southern Plain-Xenops? Besides there is a certain symmetry with Northern House Wren and Southern House Wren.

YES. Vocal and genomic differences support the split; aligns with SACC.