2024-C-1: Treat *Tyto furcata* as a separate species from Barn Owl *T. alba*
2024-C-2: Treat *Anthus japonicus* as a separate species from American Pipit *A. rubescens*
2024-C-3: Recognize multiple species within the House Wren *Troglodytes aedon* complex
2024-C-4: Treat Rufous-naped Wren *Campylorhynchus rufinucha* as two or three species: (a) treat *C. capistratus* as a separate species from *C. rufinucha* (including *humilis*), and (b) treat *C. humilis* as a separate species from *C. rufinucha*
2024-C-5: Treat recently described Vanuatu Petrel *Pterodroma occulta* as a subspecies of White-necked Petrel *P. cervicalis*
2024-C-6: Replace family name Cettiidae with family name Scotocercidae
2024-C-7: Transfer Mangrove Hummingbird *Amazilia boucardi* to the genus *Chrysuronia*
2024-C-8: Treat *Ramphocinclus sanctaeluciae* as a separate species from White-breasted Thrasher *R. brachyurus*
2024-C-9: Transfer *Phyllomyias burmeisteri*/*zeledoni* to (a) *Tyranniscus* or (b) *Acrochordopus*
2024-C-10: Treat *Phyllomyias zeledoni* as a separate species from Rough-legged Tyrannulet *P. burmeisteri*
2024-C-11: Treat *Tolmomyias flavotectus* as a separate species from Yellow-margined Flycatcher *T. assimilis*
2024-C-12: Treat *Tolmomyias viridiceps* as a separate species from Yellow-breasted Flycatcher *T. flaviventris*
2024-C-13: (a) Adopt a new group name for species in the genus *Tolmomyias*, and (b) adopt a new linear sequence for species in this genus
2024-C-14: Treat *Charadrius atrifrons* as a separate species from Lesser Sand-Plover *C. mongolus*
2024-C-15: Treat *Oenanthe seebohmi* as a separate species from Northern Wheatear *O. oenanthe*
2024-C-16: Reconsider our taxonomic treatment of quail in the genus *Cyrtonyx*
2024-C-17: Transfer *Habia fuscicauda* and *H. atrimaxillaris* to new genus *Driophlox*
2024-C-18: Treat *Colinus leucopogon* as a separate species from Crested Bobwhite *C. cristatus*
2024-C-19: Add Icterine Warbler *Hippolais icterina* to the Main List
2024-C-20: Add Western Marsh Harrier *Circus aeruginosus* to the U.S. list
2024-C-21: Treat *Gelochelidon macrotarsa* as a separate species from Gull-billed Tern *G. nilotica*
2024-C-22: Treat *Automolus cervinigularis* as a separate species from Buff-throated Foliage-gleaner *A. ochrolaemus*
2024-C-23: Transfer Gray Francolin *Francolinus pondicerianus* to *Ortygornis*
2024-C-24: Establish English names for *Campylorhynchus rufinucha sensu stricto*, *C. humilis*, and *C. capistratus*
2024-C-25: Establish English names for barn owls *Tyto alba s.s.*, *T. javanica*, and *T. furcata*
2024-C-26: Change (A) the English name and (B) the type locality of *Puffinus lherminieri*

2024-C-1
Treat *Tyto furcata* as a separate species from Barn Owl *T. alba*

YES. I support this split for the reasons outlined in the proposal, the main factor being the series of *kleek* calls which are regularly given by New World birds, and not (or very rarely) given by at least European birds (*alba* clade), genetically the sister group to the *furcata* clade.

Since the motion was submitted, I finally got a copy of Konig and Weick's 2nd edition of Owls of the World (2008). There is a section in the introduction (pp. 42-63) titled Molecular Phylogeny and Systematics of Owls (Strigiformes) by Michael Wink, Petra Heidrich, Heidi Sauer-Gurth, Abdel-Aziz Elsayed and Javier Gonzalez. They present numerous trees which I will leave to others on the Committee to carefully review. Based on this, Konig and Weick make a number of splits within the *Tyto alba* complex. These include a split of the Australian complex (*novaehollandiae*) and New World birds with further splits of Lesser Antillean taxa along with *T. bargei* from Curacao and *T. punctatissima* from the Galapagos. It appears that there was no genetic analysis of any birds from the *javanica* clade and those birds are placed into the *alba* clade by Konig and Weick (2008). They give the English name of American Barn Owl for *Tyto furcata.*

In the text for American Barn Owl (p. 211) Konig and Weick (2008) do mention the metallic clicking calls in the Vocalisations section and say: "Metallic clicking calls (not in the frequency range of ultrasonic sounds as in bats) may be often heard from flying birds, especially when gliding. Might these be a special form of echolocation?" This seems unlikely. The most recent time I heard this call and watched the bird was in a tight circular fluttery flight around a pool area that was well lit up. Konig and Weick (2008) also say that the calls of American Barn Owl are delivered on a perch and in flight. As we stated in the proposal, this needs further investigation, but calling of perched birds is not familiar to us, or to others that we have communicated with. Brainard Palmer-Ball Jr. from Kentucky said that he had heard recordings from a silo, but obviously the bird was not seen, so could have been perched, or in flight. Konig and Weick compare the vocalizations (including song) as similar to *Tyto* alba, but no analysis was presented as was done in the book on owls (Undiscovered Owls) by Magnus Robb (The Sound Approach) which was published (2015) after this 2nd edition. In the account, Konig and Weick (2008) say under Remarks: "The taxonomy of members of the genus *Tyto* needs further studies. The separation of American from Common Barn Owl is justified by DNA evidence."

YES. Three-way split. Although not yet instituted in Clements, WGAC has voted (narrowly) for the three-way split of *alba*, *javanica*, and *furcata*, and the transfer of *nigrescens* and *insularis* to *glaucops*. I think the presence of a unique male vocalization type in the *furcata* clade, well demonstrated in this proposal, is sufficient to rule out possible conspecifity of *furcata* with the Old World lineages. And the mtDNA analyses show the existence of two strongly diverged Old World clades, which also show some vocal and plumage differences. What will happen if a secondary zone of contact arises will be interesting, but that will be sometime in the future and even then may take years for the interactions to be understood in the context of reproductive isolation.

[Note: spelling should be Aliabadian throughout, not Alibadian]

YES. The diagnostic vocal difference ("kleek" call present in *furcata*, absent in *alba* and *javanica*), combined with the molecular data, supports species-level treatment of *furcata* (American Barn Owl). I also vote YES to recognizing *javanica* for alignment with WGAC.

YES. Three-way split. The evidence presented in this proposal strongly suggests that the populations of Barn Owl in North and South America are clearly separate from those of Europe, Asia, Australia, and Africa, demonstrating convincingly to me that the vocal differences are strong, especially given the *kleak* call appears absent from the *alba* and *javanica* clades, but with similar, analogous calls in various grass-owl species. In addition, the limited evidence of reproductive isolation between the *javanica* and *furcata* groups from the introduction of populations to Lord Howe Island was surprising and fascinating to me. The decision to further split the *alba* and *javanica* groups to me is less clear, although the genetic paraphyly makes the case stronger, but given that these taxa are outside of our region, I defer to the decisions of other global and regional checklist authorities. I vote to adopt "American Barn Owl" for the *furcata* group. I am not a fan of any of the names for the two Old World species, and I would advocate we solicit alternative names for these taxa. If forced to choose from the available names, I'd probably prefer "Eastern" and "Western," but as noted in the proposal, these names have some major flaws.

YES. Three-way split. The existence of a novel vocalization in the New World barn owls argues for species status for that taxon. It is interesting to note, however, that *glaucops* largely shares the same vocalizations with *furcata*, yet they occur sympatrically, suggesting that their reproductive isolation lies elsewhere. The genetic data for the genus look great but are based on a single mtDNA gene, hardly sufficient to be relied upon for species-level decisions. Plumage variation is interesting but hard to pin down what may be important. The repeated evolution of dark facial disks of island forms argues that this character may be subject to local adaptation. Whether it is important in reproductive isolation remains to be seen.

YES. Three-way split. These are deep phylogenetic splits between geographically and phenotypically concordant lineages within Tytonidae. Vocalizations are important in owls more generally, and there appears to be appreciable vocal differences between these putative species. Plumage is known to vary among populations of barn owl in concordance with Gloger's Rule, such that color differences seem to evolve rapidly in response to local conditions. As such, I don't put as much faith in color being related to species limits in this group. It seems clear that the *furcata* group in the Western Hemisphere is distinct from the Afro-European *alba* group. If we recognize that split, then it would behoove us to also recognize *javanica*, otherwise we would have deep paraphyly dating back to the late Miocene. However, as this is outside of our geographic jurisdiction, that should ultimately be the decision of global or local taxonomic authorities for that region.

YES. Three-way split. Reasons are stated in the proposal.

YES. Three-way split. Reasons provided in the proposal.

YES. Three-way split. The reasons were given in the proposal.

YES. Treat these as three species. I voted for this split in 2018, and the data presented here (especially vocalizations) reinforce this decision.

2024-C-2
Treat *Anthus japonicus* as a separate species from American Pipit *A. rubescens*

YES. I agree with the authors that the preponderance of the evidence now shifts towards species status for *japonicus*. I think that the general rarity of breeding pipits in the Bering Sea islands likely limits gene flow between the two taxa, as do the call note differences (even if not massive) and the mitochondrial DNA differences. I'm not as convinced by the playback data, given that it was on a different set of taxa, but I do suppose it points towards call notes being a species-level character in this complex.

One thing that was not discussed in the proposal are breeding plumage differences. One thing that strikes me about the *rubescens* group is the huge amount of variation in breeding plumage, even within subspecies. There are not many photos of breeding *japonicus* available (but perhaps there are specimens in European museums?), but there are a few. I am struck by consistent differences between breeding *japonicus* and Alaskan breeding *pacificus*. In particular, there are some very nice photos of breeding-aspect *japonicus* from Japan in late spring. Those birds show bolder wing bars, broader chest streaks (albeit more limited than in winter), and a more contrasting face pattern.

As for common names, because our guidelines are ambiguous, I am strongly in favor of retaining American Pipit for *rubescens* for the sake of stability. The argument for Buff-bellied Pipit seems to be coming primarily from old world authors, which I don't think should hold a huge influence on our decisions. There is a massive amount of literature (and name familiarity among the public) using the name American Pipit, and it would be a shame to lose that continuity. It is now an even more accurate name without *japonicus* included. I would be interested in knowing more about the history of the English name for *rubescens*, as I'm guessing that Buff-bellied was used at some point in North American literature.

For *japonicus*, I'm ok with either Siberian or Japanese, but given the arguments on breeding distribution made by the proposal authors, I do lean towards Siberian. However, the common-scientific name linkage of having both refer to Japan, even if it's not an ideal name, is very tempting.

YES. While this issue would certainly benefit from additional research, in particular analysis of song as well as additional genetic sampling, I agree with the proposal that the burden of proof now lies in showing that these two represent a single species. What I

found particularly convincing, despite the lack of song analysis, were other studies showing that the distinct call notes were indeed important in aspects of species recognition, making me feel more comfortable with the notion that the differences in the call notes identified by Doniol-Valcroze et al. (2023) represented important, species-level differences.

YES. Differ in genetics, vocalizations, and distribution. Genetic differences between the two are similar to that seen in other members of this group. I agree with the proposed recommendation that the burden of proof now lies in showing that it's not a separate species. Getting data from the contact zone is not possible, so best to have a taxonomy that reflects the data we have.

YES. They have enough genetic, distribution, and vocalization differences. It´s desired to have data from the contact zone, but the data show differences between them.

YES. As noted by others and in the proposal, this case would benefit from additional study (especially the extent and nature of a potential contact zone). However, the combination of vocal and genetic evidence, along with some phenotypic differences, sway me toward accepting the recommendation of the proposal to recognize them as separate species. I am fine with Siberian Pipit for *japonicus* and retaining American Pipit for *rubescens*.

YES. As mentioned in the proposal, differences in non-breeding plumage (although the differences are in non-breeding plumage, some mechanism should be keeping the two groups apart to explain these differences), genetics, and vocal analyses support the split; as well as comparisons with congeneric species. Siberian Pipit for *Anthus japonicus* and American Pipit for *Anthus rubescens s.s.*

YES. The differences in vocalizations are slight and overlapping in PCA space, but are commensurate with species-level differences between well-recognized species in a group where vocalizations are largely conserved. The genetic data also support separate species. I agree that Siberian Pipit is better suited for *japonicus* and that we should retain American Pipit for *rubescens* as the 'daughter' species are roughly similar in their distribution size.

YES. Reasons are stated in the proposal. I agree to Siberian and American pipits.

YES. I found the reasoning in Doniol-Valcroze et al. (2023) to be compelling, particularly in their concluding comments, but given the lack of information from the Chukotka region and adjacent areas to the south, the situation does have a degree of ambiguity. Another comment raises a good point about the alternate plumage of *japonicus.* It seems to be largely unknown, or at least not illustrated (e.g., Alström and Mild 2003). What impressed me about the few I've seen in late May at Gambell, St. Lawrence Island, was the very broad and very dark malar bar along (from one last spring, 2023) with some retained (from basic) white tipped median coverts and white edged terials. One still had pale legs. Lehman (2019) includes photos of ambiguous birds at Gambell. Remember that Monson

and Phillips (1981) cite a late spring bird (specimen) from northern Sonora, Mexico (Naco, 6 June 1958), as belonging to *japonicus*. It was heavily streaked below.

As for the English name, while I don't overly object to Siberian Pipit, the majority likely don't breed in Siberia. They breed in the Russian Far East, so I tentatively favor Japanese Pipit which is an important part of their winter range (Honshu and south) and as noted in the proposal is often used for the English name, at least informally, perhaps reflecting the scientific name. Yes, another case like Japanese Waxwing. On the other hand, I agree that we really shouldn't be advocating an English name for a species that is rare/casual to western North America.

Lehman, P. E. 2019. The Birds of Gambell and St. Lawrence Island, Alaska. Studies of Western Birds, No. 4. Western Field Ornithologists.

Monson, G. and A.R. Phillips. 1981. Annotated Checklist of the Birds of Arizona, 2nd edition. The University of Arizona Press.

Finally home (20 March 2024) to check my library. Portenko (1989) in his Birds of the Chukchi Peninsula and Wrangel Island (Vol. 2) says under Distribution and Status that this species nests in the Chukchi peninsula, but is extremely rare nesting only in hills of Providence Bay and inside the western part of the peninsula on the Amguema. He lists one summer (July 1938) from Providence Bay. The subspecies he lists is *harmsi.* According to Alström and Mild (2003) his taxon was synonymized with *rubescens* by Vaurie (1954, 1959) and Mayr and Greenway (1960). The type specimen is from Tashkent, Uzbekistan, in December. Alström and Mild (2003) say that Portenko (1939, 1960, 1989) extended the range south to Anadyr. Hall (1961) examined a paratype of '*harmsi'* and considered it a junior synonym of *japonicus*. Alström and Mild (2003) did not see any type material of *'harmsi'* but provisionally followed Hall stating that her judgment is supported by Sarudny's [1909] original description which stated that the markings below were as in Meadow Pipit in color, shape, number and distribution, thus not like *rubescens.* Also, I think all of this strengthens the argument that if any *rubescens* are in northeast Asia, they must be rare and it seems likely that most, if not all, breeders there are *japonicus.* The Bering Sea acts as a barrier, apparently, between the two taxa. The Pribilof Islands in the southeast Bering Sea, a place where *rubescens* has bred, is much closer to mainland North America than Asia. I think the statement in Alström and Mild (2003): is compelling: "We have not seen any *rubescens* anywhere in Asia, neither in the field nor in museums, so we firmly believe that if it occurs at all it must be very rare. Per Alström has spent a great deal of time in Asia and made many contributions, and I'm sure he was attuned to any Buff-bellied Pipits he encountered in the field and I suspect his time in museums was easily as exhaustive. I think part of the confusion results from Vaurie (1959) and others who include Asia in the breeding grounds. He extends the breeding range west to the eastern Taimyr Peninsula (Khatanga River) and includes the Commander Islands. Surely this is mistaken. Gibson and Byrd (2007) report no breeding of any subspecies of Buff-bellied Pipit in the western Aleutians and *pacificus* (= *rubescens* for most authorities) breeds regularly in the Central Aleutians only to Seguam Island. Vaurie's (1959) defined range of *japonicus* is south of *rubescens* in Asia. Surely if

the breeding range of *rubescens* extended west some 2000 miles west of the Bering Sea, the subspecies would be much more numerous on Bering Sea islands. Gibson and Byrd (2007) list a few spring specimens as *japonicus,* others as *harmsi,* and others as intergrades. Under Notes they add that high counts of Buff-bellied Pipits in spring are in conjunction with Old World motacillid species and suggest then that these Buff-bellied Pipits originate from the Old World. In the Western Aleutians, spring migration is almost entirely of Old World species; in fall the appearance of New World species is more regular. Lehman (2019) indicates that the species (includes both subspecies) is rare to uncommon in spring and in my 40 some odd spring trips there, any Buff-bellied Pipit sighting is unusual, let alone flocks. Counter this with the Pectoral Sandpiper which breeds west in Russia to the Taimyr region. On St. Lawrence Island (Lehman 2019) flocks of migrants are regularly seen in spring and fall, with the highest count exceeding 500 birds. These birds are likely headed to mainland Russia which at Gambell is less than 50 miles away.

I want to also pass on some anecdotal evidence based on observations of a single *japonicus* that wintered with a flock of *rubescens* in Griffith Park, Los Angeles, CA. Three of us independently noted that the *japonicus* bobbed its tail much more than the accompanying *rubescens.* I passed this on and Pam looked at videos that were inconclusive as to whether there was a difference. North American *rubescens* certainly bobs its tail as well and if there is a difference it is largely a quantitative not a qualitative difference. Still, perhaps it's something that perhaps deserves more investigation.

YES. I agree with the proposal

NO. I disagree that the new data shifts the burden of proof to showing that they are not reproductively isolated. We always need to balance these "burden of proof" arguments with our desire to keep the status quo if the evidence is not there. If we change the status quo, the evidence should be overwhelming, not just barely inching past the fulcrum in a burden of proof argument, especially in cases where critical data are missing. In this case, we potentially could have sympatry or parapatry, but the field analyses have not been done in the region where that may occur. For these pipits we have some good proxies for analyzing reproductive isolation, and that is often all we have because of allopatry, but in this case we could potentially have the gold standard - actual in the field reproductive isolation (or the converse).

2024-C-3
Recognize multiple species within the House Wren *Troglodytes aedon* complex

Note: The initial votes below (in bold type) concern whether or not to recognize one or more additional species in this complex; the letters (a) - (l) are votes on whether to recognize individual species.

YES. With the combined morphological, vocal, and now genetic data from this and the 2022 proposal, I agree that we should move forward with some taxonomic changes.

In my initial reading of the proposal, I was in favor of using the hyphenated 'House-Wren" for the various species in the group, but I now agree that especially for the Lesser Antillean taxa, the name "house" is less appropriate given their stronger association with forested habitats. "House Wren" is very well entrenched, and that name should be preserved for *musculus* and *aedon*, without the hyphen due to the non-monophyly. So, I vote for Northern House Wren and Southern House Wren, although I think Tropical House Wren would be a better name for *musculus*. Perhaps Tropical could be used if there are further splits within *musculus*.

(a) YES. I am a bit hesitant about splitting the mainland birds just because we do have potential geographical contact that hasn't yet been quantified. However, I agree that the genetic data in particular very strongly suggest two species, and these birds are much more definitively separate species than are some of the Caribbean taxa. The situation in central and southern Mexico is fairly complex, and I would like to get some feedback from field observers who are familiar with the group in this region. It's been many years since I've spent time there. In particular, *musculus* is found south of the Isthmus of Tehuantepec (a major biogeographic barrier), but crosses it to the north in the Sierra de los Tuxtlas, which potentially brings it into contact with northern birds. In particular, I believe that there are lowland-breeding House Wrens between the Tuxtlas and the *brunneicollis* from the nearby Sierra Madre Oriental, as well as within the lowlands of the Isthmus of Tehuantepec, and figuring out whether they are in contact seems critical. I'm less concerned with the identity of the montane Oaxacan *nitidus*, as at least based on plumage these seem pretty clearly allied to the *brunneicollis* group. Wintering *aedon* is also found throughout this region, which complicates things in terms of sorting out range overlap and any potential for hybridization.

My other concern is that a few studies have found very deep genetic divergences within other species of small largely resident wrens (e.g., *Henicorhina leucophrys*) that don't appear to correspond to other species-level morphological characters, so I'm not convinced that genetic distance (on its own) is automatically indicative of species status in small wrens. This is also what seems to be happening with the Plain Wrens in the recent proposal to lump that did not pass (deep genetic break with limited or no morphological differences). However, in this case there are concordant song and plumage differences that also point to species status for *aedon* and *musculus*.

For the insular taxa, I agree that multiple species are present, but I'm not convinced that all should be split as single-island endemics. Some adjacent islands are clearly similar to one another, and especially given the less well resolved genetic data for these taxa (more reliant on mtDNA), I am in favor of a more conservative treatment, even if it ends up being piecemeal with further splits in the future. Despite that, I have voted yes on some of these partly because the lack of genetic data for some of the taxa (combined with some nomenclatural issues) almost makes it easier to elevate some to species level. I'm not convinced that this is the best approach, however.

(b) NO, for the reasons given. There is clearly extensive gene flow between *brunneicollis* and northern taxa. Detailed studies in the mountains of southeastern Arizona and northwestern Mexico would be interesting, however. I find the mitochondrial polyphyly to be very interesting (*brunneicollis* sister to montane Central American taxa). It seems that some sort of speciation reversal may have happened when *brunneicollis* came into secondary contact with the *aedon* group.

(c) NO, for the reasons given.

(d) YES, for the reasons given.

(e, f, g, h) I'm not sure how the voting should be structured here, but I vote for splitting *guadeloupensis*, *rufescens*, and *martinicensis* from other house wrens under the name *T. martinicensis*, with *guadeloupensis* and *rufescens* as subspecies. The three seem broadly similar in terms of plumage, and given the lack of data on song and genetics for two of them, I think it safest to consider them as a single species, despite the possibility that they together are not monophyletic. It seems a better treatment than the alternatives of leaving some in with *musculus* or not splitting any of them. The mitochondrial data showing that *rufescens* is sister to Central American *musculus* populations is a very cool biogeographic pattern that almost requires separating *rufescens* from at least the adjacent Lesser Antillean taxa. I am strongly opposed to using Dominica Wren for *rufescens*, as the lack of congenerics on the other islands is solely due to recent human-caused extinctions. A broader geographic name (such as Lesser Antillean or Antillean Wren) would be misleading if the other taxa from the southern Lesser Antilles are also split. A plumage-based name might be better in this case. Reddish or Rufescent Wren could be good names. If going with a plumage name, I'm in favor of Rufescent, which parallels the scientific name and is used by other species with the epithet *rufescens* (e.g. *Thamnistes rufescens* and *Myiophobus rufescens*). Rufous Wren is occupied by *Cinnycerthia unirufa*. However, now that we have a formal approval by the chief of the Kalinago, I am enthusiastically in favor of Kalinago Wren for this species. The ancestral lands of the Kalinago people closely align with the distribution of *T. martinicensis*, and this is a wonderful opportunity to honor and draw attention to the Kalinago people.

(i) YES to splitting *mesoleucus*, but tentatively. I have seen and heard this bird and it is very strikingly different from mainland house wrens in just about every way imaginable. I have watched it creeping around on large branches in the manner of a Black-and-White Warbler and peering around larger branches with its long bill. Despite the lack of genetic data for this one, I can't see how it should stay with *musculus*. However, the plumage is rather similar to *musicus* (and somewhat similar to *grenadensis*), and I could see an argument for considering it conspecific with those taxa. In fact, the three do form a sort of cline from paler birds in the north to more rufous in the south. However, all three do have quite different songs, especially *musicus*. The lack of genetic data for *mesoleucus* makes this a bit complicated (i.e., is it actually part of the *musicus*/*granadensis* group?). So, I would be in favor of either considering it as a single-island endemic, or perhaps conspecific with *musicus* and *grenadensis* (but this would raise some nomenclatural issues given simultaneous publication dates for some of the names). Alternatively, we could consider *mesoleucus* as a separate species despite the lack of genetic data, and

consider *grenadensis* and *musicus* as a combined species separate from *musculus*. This seems rather borderline to me, given that it's relying on the same mitochondrial data and unquantified vocal differences that sank the last proposal.

(j) YES to splitting *musicus*, but see above and below.

(k) YES to splitting *grenadensis* from *aedon/musculus*, but the genetic data are ambiguous with regard to splitting it from *musicus* and perhaps *mesoleucus*. The mitochondrial data seem to show a very close relationship between *musicus* and *grenadensis*, so the split rests mostly on the songs, which admittedly are quite different (especially distinctive in *musicus*). I'm also not sure which of the two taxa would have priority, as they seem to have been described by Lawrence in the same year, and I believe in the same publication?

(l) NO, for the reasons given. I spoke to some of the authors of the Klicka paper, and they pointed out the deep genetic splits within South America that suggested multiple species might exist within *musculus*. I am not fully convinced of this, but it seems like detailed studies of *albicans*, and other South American taxa (especially in the Andes) might be on the horizon, so perhaps there will be more data on this in the future. Either way, it's an issue for SACC to consider.

YES. What a complex set of taxa. I was in favor of splitting them initially based on the Remsen et al. proposal from 2022, but only voted against it after it was pointed out that we required proposals to be based on published information, and so I only voted it down based on a technicality. However, now that there is research that has been published that sheds additional light on the situation, I am happy to vote to split the House Wren. I was certainly surprised by the need to split the Northern and Southern House wren groups, and that break in Mexico is certainly quite abrupt in the genomic data, with no evidence for introgression. I am in favor of splitting most of the island taxa, except I agree that *martinicensis*, *guadeloupensis*, and *rufescens* should be kept together. While I was a little torn on how to treat *mesoleucus* and *grenadensis*, I felt given all else, these are probably best treated as separate species. I agree with the common English names proposed for the new species, and would vote in favor of "Grenada Wren" instead of "Grenada House Wren" to be consistent among the other Caribbean taxa.

As for the name of *martinicensis*, I am in favor of the name Kalinago Wren, given the approval for using this name by the Kalinago Council. I do not like the name Dominica Wren for this species, because even though it currently occurs only on Dominica, it occurred on other islands historically, with its species name reflecting this. Cinnamon Wren would also be an acceptable name for this species.

YES. I voted to split all seven in the 2022 proposal. After reading this proposal and reaching the recommendations, I found that my views on the taxonomy lined up nearly perfectly with the recommendations. For the English names, I agree that "House Wren" should only be used for the mainland taxa that are loosely associated with human habitations (Northern House Wren, Southern House Wren).

(a) YES. Genetic evidence in particular is strong, but vocal, playback, and morphological data also point to separating these as species. English Names: NORTHERN HOUSE WREN, SOUTHERN HOUSE WREN.

(b) NO. I agree with the proposal that this would be purely an mtDNA split with little other data backing it up.

(c) NO. I agree with the recommendations.

(d) YES. Super distinctive in most facets. English Names: COZUMEL WREN.

(e) YES. Split *martinicensis* group, but keep *guadeloupensis*, *rufescens*,and *martinicensis* as subspecies. English Name: I think KALINAGO WREN is best by far. The distribution of the wren closely matches the cultural context, and the Kalinago council's okay is excellent.

(f, g, h) NO. See above.

(i) YES. I agree with the proposal. English Name: ST. LUCIA WREN.

(j) YES. I agree with the proposal. English Name: ST. VINCENT WREN.

(k) YES. I think the evidence presented, especially the morphology, argues for species status. English Name: GRENADA WREN.

(l) NO. I agree with the proposal that it belongs with *musculus*.

YES. In general, the only split that is clear to me is *beani* which is distinct genomically, morphologically, and vocally (with differential responses to playbacks). Plumage and size differences in other island taxa are insufficient without corroborating genetic and/or quantitative vocal evidence as a basis for species-level differences. Specific comments on each sub-proposal are as follows:

(a) NO. The proposal shows a clear break in southern Mexico (Veracruz and Oaxaca region) between the *aedon* and *musculus* clades, with supposed lack of intergradation, but sample sizes are very small (3 *brunneicollis* from Oaxada, 1 *musculus* from Oaxaca, 4 *musculus* from Veracruz; Klicka et al. 2023 supplementary data). Thus, any putative contact between the two clades is unknown. The figures showing acoustic differences are suggestive, but I looked at the supplementary material from Sosa-López and Mennill (2014) and of the 19 *musculus* recordings listed with locality data, 17 are from south of the potential contact area (Costa Rica, Nicaragua, Colombia, Argentina). Likewise, 8 of the 10 *brunneicollis* recordings with locality data are north of the potential contact zone (Mexico: Mexico City and Sonora; USA: Arizona). Given that these two taxa approach fairly closely, further study is needed to adequately assess levels of intergradation, if any.

(b) NO. As noted in the proposal, there appears to be extensive intergradation between *aedon* and *brunneicollis*.

(c) NO. Lack of evidence to support this split.

(d) YES. Genomic and morphological data show *beani* to be distinct, and playback data show the strongest physical response to songs of other taxa. Cozumel Wren.

(e, f, g, h) NO. These may represent one or more species but the current evidence to support a split is weak. Genetic data would help to elucidate distinctiveness.

(i) NO for now. The proposal mentions distinctiveness in its “fairly well-documented voice” but what are those differences? NACC proposal 2022-B states “this one sounds more or less like a typical aedon-type to me.” More information on vocal differences plus genetic data would help to elucidate distinctiveness.

(j) NO. Perhaps the most compelling evidence is the anecdotal statement on voice given in the proposal (copied from the NACC proposal 2022-B), but quantification of differences including playbacks (like with *beanii*) would help elucidate distinctiveness.

(k) NO. As with the other taxa, more evidence is needed in the way of vocal differences. NACC proposal 2022-B states re: songs from online repositories: “ two sound very aedon-like, but the third one somewhat different.”

(l) NO. Reasons are given in the proposal.

If these pass, then I agree with the English names provided in the proposal including Kalinago Wren for *T. martinicensis* given approval by the Kalinago Council.

YES. A very complicated group with variations in coloration and vocalization patterns.

(a) YES. Enough support for the split: *musculus* from *aedon*. I agree with the proposal.

(b) NO. There is gene flow between *brunneicollis* and *aedon*.

(c) NO, for the reason given.

(d) YES. I agree with the proposal.

(e, f, g, h) NO. More molecular data are needed to support this separation.

(i) YES, but weakly. I think with its habitat specialization and quite different plumage is supported with this split, although it would be desirable to have molecular data available.

(j) YES. Even when the genomic data is not available, the songs and the plumage

patterns support this split.

(k) NO. I think we need more data available to support the split.

(l). NO. There is no data available for the split. I agree with the proposal.

YES. Many of these I voted for in the 2022 proposal. Based on further evidence, I continue to vote for those splits presented in both proposals. I agree with the potential English names suggested in the proposal.

(a) YES. Deep division in RADseq data with no evidence of admixture; also ecological and morphological differences.

(b) NO. Despite forming a separate clade with RADseq, there is evidence of admixture with aedon.

(c) NO. As stated in the proposal, vocal differences aren't as distinct; groups with *aedon* in the RADseq data analysis.

(d) YES. I voted for this split in 2022 and continue to support it. Paraphyly of musculus with respect to *beani* doesn't bother me for this island taxa.

(e) NO. However, if the proposals to recognize *guadeloupensis, rufescens,* and *martinicensis* fail, then I vote YES to treat the *martinicensis* group (incl. *guadeloupensis* and *rufescensi*) as a separate species.

(f, g, h) YES. I voted to split each of these based on the information presented in the 2022 proposal and I don't see any information in the new proposal that convinces me to change my mind. Yes, we have little to go on for *guadeloupensis* and *martinicensis*, but I would rather error on the side of splitting them.

(i) YES. Reasons are given in the current proposal and in the 2022 proposal.

(j) YES. Reasons are given in the current proposal and in the 2022 proposal; mainly vocal differences.

(k) YES. Forms a clade and plumage color and bill shape differences.

(l) NO. Reasons are stated in proposal (minimal morphological differences, sequences intermixed with others).

YES. As recommended in the proposal, I vote YES on all but (b) *brunneicollis* of Arizona to central Mexico, (c) *parkmanii* of western North America), (f, g, h) the individual splits of *guadeloupensis, rufescens,* and *martinicensis*, and (l) *albicans* of Trinidad. I guess English names will be a follow-up proposal? The one problematic one is *rufescens*

(extant *rufescens* with extinct *guadeloupensis* and extinct *martinicensis*) if it passes, but the few color-based suggestions offered by previous voters seem like they have potential. I wonder if there is a geographic term for the central Lesser Antilles that includes Guadeloupe, Dominica, and Martinique, but not St. Lucia?

English names: I'm in favor of using Northern/Southern House Wren for the two mainland species, but not using House in the others, which can all adopt the island names except for *martinicensis* due to its polytypy. I marginally prefer Rufescent Wren, given that it includes three different taxa each with slightly different described coloration, and rufescent is not that specific, and that it mirrors the scientific name of the extant taxon.

YES. (a) YES. This is a stark genomic split compared to the clinal differences we see between *brunneicollis* and *aedon*. There are also corresponding phenotypic differences that have been recognized for a long time. The Isthmus of Tehuantepec is a common biogeographic barrier between divergent taxa.

(b) NO. There seems to be clinal variation in ancestry coefficients and plumage characters across much of northern Mexico and the southwestern United States, suggesting that there is considerable ongoing gene flow and connectivity between populations of these subspecies.

(c) NO. Seems to exhibit less pronounced phenotypic variation than others and genomic data suggests that there is ongoing gene flow.

(d) YES. Pronounced phenotypic differences combined with some differences in responses to vocal displays as a potential pre-mating barrier to gene flow suggest this should be a separate species.

(e) YES. Together, these seem phenotypically and genetically distinct, but they do not seem super different from one another, so I'd prefer to keep each one as subspecies of a more broadly distributed 'island' species.

(f, g, h) NO. Do not recognize each one as a separate species. I prefer Cinnamon Wren for *T. martinicensis*.

(i) NO. We do not have sequence data, and phenotypic analyses as of yet are limited. Seems to me would be good to base the decision off of more data. Suggestive habitat specialization and differences from *musculus*, but would like to see peer reviewed data supportive of the split.

(j) NO. Only one individual sequenced and only with ND2. Pronounced phenotypic differences in song, plumage, and morphology are strongly suggestive of splitting, but I would like to see more than one individual sampled and more loci sequenced to get a sense of its phylogenetic position.

(k). NO. Similar to my rationale for (j), I would like to see more individuals from *musicus* sequenced and compared to the *grenadensis* samples. The nodes that unite *grenadensis* as a clade are not strongly supported in the phylogeny, and while there may be some differences in adaptation to urban environments, bill morphology, and plumage, they also seem fairly similar to mainland South America taxa.

(l) NO. For reasons stated in the proposal, it aligns well with South America mainland taxa generically and phenotypically.

YES. A few comments to add to the proposal. One comes from Keith (1997) where he says that P. William Smith did playback experiments at Cacoli, St. Lucia, of *rufescens* from Dominica to *mesoleucus*. P.W. Smith noted the response of singing birds at Cacoli was “weak” (Keith 1997). Also, in Raffaele et al. (1998) there is a comment in the House Wren account that “Lesser Antillean forms do not cock their tails like North American birds. In looking at photos from the earlier motion, I can see this from some photos, while in others the tail is somewhat cocked. Still many talk about the distinctive shapes and a different behavior. The tail posture should be further investigated.

Keith, A.R. 1997. The Birds of St. Lucia. B.O.U. Check-list No. 15, British Ornithologists’ Union.

Raffaele, H., J. Wiley, O. Garrido, A. Keith, and J. Raffaele. 1998. A guide to the birds of the West Indies. Princeton University Press.

(a) YES. I was immediately impressed by the song differences (from the U.S.) of *musiculus* in southern Chiapas (south of the Isthmus) when I first heard them a few decades ago.

(b) NO. I hear no difference in songs from breeding birds in Southeast Arizona and Western Mexico. Southeast Arizona breeders sure look like those elsewhere. Birds farther south are buffier below but still sing typical House Wren songs.

(c) NO. The described ranges of the two main North American races don’t make much sense for a potential species division. I am curious about exactly what the vocal difference I’ve heard described?

(d) YES. I was certainly very impressed when I first encountered the Cozumel Island birds, both visually and especially vocally. We found them most easily in native woodland.

(e) NO. See below.

(f, g, h) YES. Split all three as separate species. Since it seems we are splitting all of the other Lesser Antillean taxa, why not split these three, two of which are extinct. Apart

from the islands being proximal to each other, I was unaware of a close faunal connection between these islands as outlined in the proposal. It seems more odd to treat these three as a polytypic species with three subspecies rather than make an objective guess that they are separate at the species level. I agree that genetic data would be useful but they are island isolates.

(i) YES.

(j) YES.

(k) YES, somewhat more weakly. I can appreciate the closer relationship to mainland birds, the *musiculus* group, but morphology and structural aspects seem pretty distinct.

(l) NO. There's little evidence of endemism at the species level on Trinidad.

YES. (a) YES. Very different resources support this split (genetic, vocal and morphological data). (b) NO. I agree with the proposal. (c) NO. I agree with the recommendations. (d) YES. All the evidence supports this split. (e) NO. (f, g, h) NO. (i) Yes. Weakly. (j) YES. (k) NO. (l) NO.

YES. What an interesting group of birds is the *Troglodytes aedon* species complex. It was fascinating to read the two proposals (2022-B-10 and 2024-C-3) focused on multiple splits within the species complex, which together provide integrative evidence to approach species limits in *T. aedon*.

(a) YES. No genetic intergradations detected between the *aedon* and *musculus* clades, in addition to vocal differences. English name for *T. aedon sensu stricto*: Northern House Wren. English name for *T. musculus*: Southern House Wren. (b) NO. (c) NO. (d) YES. English name for *T. beani*: Cozumel Wren. (e) YES. This is a difficult decision because two of the groups are already extinct. Decision based on geographical affinities and descriptions of plumage coloration. English name for *T. martinicensis*: Kalinago Wren. (f, g, h) NO. (i) YES. English name for *T. mesoleucus*: St. Lucia Wren. (j) YES. English name for *T. musicus*: St. Vincent Wren. (k) YES. English name for *T. grenadensis*: Grenada Wren. (l) NO.

2024-C-4
Treat Rufous-naped Wren *Campylorhynchus rufinucha* as two or three species: (a) treat *C. capistratus* as a separate species from *C. rufinucha* (including *humilis*), and (b) treat *C. humilis* as a separate species from *C. rufinucha*

YES (a and b). Based on the work from the hybrid zone, it is very clear that *capistratus* is a separate species from *rufinucha*. Even from the work of Selander, the narrowness and apparent stability in location of the hybrid zone was certainly suggestive of separate

species status, despite apparent free interbreeding within the hybrid zone. How to treat *humilis* and *rufinucha* is certainly much more challenging, but given the fairly distinct plumage differences, and the differences in the duet pattern between the two, I think species status is warranted for these two taxa as well.

YES. (a) The combination of the very narrow and stable hybrid zone, with the plumage and song differences comparable to other species of *Campylorhynchus* wrens, point to species status for *capistratus*. I have birded in the Tonalá region and not seen any *rufinucha* types there, which does point to their rarity in the hybrid zone. As pointed out in the proposal, the recent photos from the region don't show any clear hybrids, and pure birds within a few dozen kilometers of one another, again pointing to the hybrid zone still being stable, if it's even present any more. The songs are especially different in *capistratus*. **(b)** The song differences between *rufinucha* and *humilis* aren't very striking, but I think I'm picking up some subtle differences in pace, although this could be due to effects of playback. The plumage differences are more subtle than between *capistratus* and *humilis*, but do differ in the same way that other *Campylorhynchus* differ. I am mostly convinced by the allopatry with intervening congenerics, habitat differences, and plumage differences.

I've long suspected that there are narrow hybrid zones between other species of Mexican *Campylorhynchus*, but I have no clue if anyone is working on this. For example, *jocosus* and *humilis* come very close in eastern Morelos, *jocosus* and *gularis* approach one another south of Toluca, and *humilis* and *gularis* in Colima. If that is the case, small narrow hybrid zones in this group would not be unusual. What does give me pause is the free interbreeding *within* the *capistratus/humilis* hybrid zone, but there are clearly very strong post-zygotic isolating mechanisms limiting gene flow. The paraphyly within *capistratus* does bother me somewhat, but this is a single gene tree, so I'm not sure that it's telling us much. This could also be due to introgression with other species of *Campylorhynchus*.

As for common names, I think we need a separate proposal. Rufous-naped Wren should probably be abandoned, although it would be nice to keep the parallel with the scientific name for *rufinucha*. However, Veracruz Wren is used by Clements, and is appropriate for that taxon, as it's endemic (or nearly so) to that state. Rufous-backed Wren is a good descriptor for *capistratus* and is also used by Clements. *C. humilis* is the one that probably needs a new name, as Sclater's isn't an option any more. West Mexican Wren could work, but isn't very exciting, and *C. gularis* is also found in western Mexico.

YES (a and b). Zone of free hybridization between *capistratus* and *rufinucha* was narrow to begin with when originally studied. Moreover, recent studies show that the hybrid zone no longer exists. Also split *rufinucha* and *humilis* based on evidence at hand. The argument for keeping them together relies on a hypothetical meeting of the two forms; however, we already know they have evolved differences reflective of separate species. Duets differ among the three.

YES (a and b). The evidence in the area of sympatry/parapatry clearly indicates that *humilis* and *capistratus* are reproductively isolated. Nice sampling through 50 years to

give a better picture. Although there is no test ground to see how *humiis* and *rufinucha* would behave if they came in contact, comparing morphology plumage, and vocalizations to other *Campylorhynchus*, separate species status seems appropriate.

YES (a and b). Vocal differences in the duets, along with congruent genetic and phenotypic differences and the narrow contact zone between *humilis* and subspecies *nigricaudatus* of the *capistratus* group, support species status.

YES (a and b). Reasons are given in the proposal.

YES. (a) The narrow hybrid zone, which seems to shrink over the years and which implies the reduction of gene flow between *humilis* and *capistratus nigracaudatus* supports the split; in addition to morphological, vocal, and genetic differences. **(b)** The split is supported by morphological, vocal, and genetic differences.

YES (a and b). For a change this is a well-researched complex with a very interesting history that seems to illustrate the pace of change in a hybrid zone. The morphological differences (especially size) alone make the *humilis* and *capistratus* groups unlikely conspecifics, and the narrow to disappearing hybrid zone reinforce this. For *rufinucha s.s.*, the less-different plumage and size appears equivocal but the song pushes it over the line for me. English names: Veracruz Wren is fine for *rufinucha*, and Rufous-backed OK for capistratus; both are appropriate and have prior usage. For *humilis*, I think West Mexican Wren might be the best we can do for a non-eponym. Granted, there are several other wrens in West Mexico, some endemic to the region, so maybe someone can suggest other more helpful names.

YES. (a) The hybrid zone is small and diminishing, suggesting that hybrids are increasingly rare and that introgression does not move far into the parental types of either *capistratus* or *rufinucha*. (**b)** Although only based on mtDNA, *rufinucha* and *humilus* are well differentiated. The duets are also quite different. If the taxa to end up meeting in the future, as is suggested in the proposal authors' recommendation, then we can revisit and see how these differences hold up as reproductive barriers.

YES. (a) A fascinating story, particularly with the hypothesis about how *Campylorhynchus chiapensis* fit in. The fact that Hellmayr lumped this taxon too is certainly an indicator about his overall lumping philosophy. (**b) NO** for now. I find no mention in the proposal about the birds in the northern part of the range of *humilus* (interior Colima). Howell and Webb (1995) and Brewer (2001) discuss these birds as differing in their plumage in at least some having spotting on the sides of the chest and barring on the flanks This sounds like intermediacy towards nominate *rufinucha.* These interior *humilus* do not receive subspecific recognition. I wonder if the songs of these birds were compared. I'm just raising a potential red flag from what I've read, so my no vote is more of a question.

Brewer, D. 2001. Wrens, Dippers and Thrashers. Yale University Press.

Howell, S.N.G. and S. Webb. 1995. A guide to the birds of Mexico and northern Central America. Oxford University Press.

Further note from proposal co-author: Regarding the previous comment about birds in interior Colima, Ku-Peralta et al. (2020) included sampling from El Chical, interior Colima, in their study of vocal duets in *C. rufinucha*. Their results indicated that duets of these birds were perfectly typical of *humilis* (see their Figure 3 below). Moreover, if the birds from Colima were truly intermediates, one would expect this to be an area geographically proximate to nominate *rufinucha*. Instead, Colima appears to be the part of the range of *humilis* that is furthest away from the range of *rufinucha.*

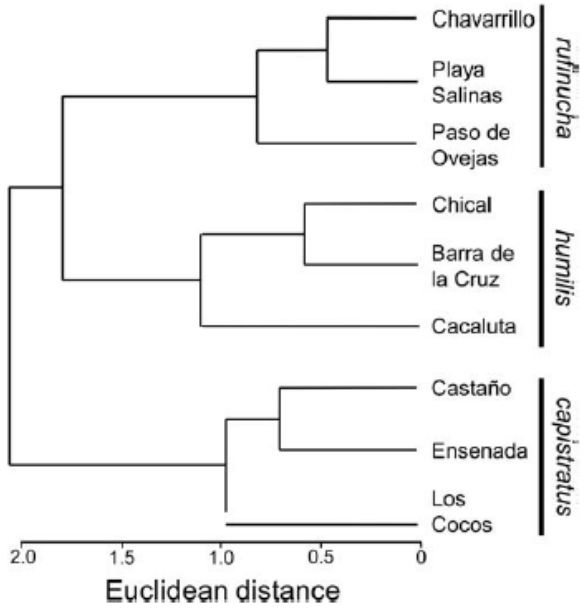


FIGURE 3. Dendrogram resulted from the cluster analysis at the level of sites. Values on the horizontal axis represent the average Euclidian distance (similitude) between sites.

2024-C-5
Treat recently described Vanuatu Petrel *Pterodroma occulta* as a subspecies of White-necked Petrel *P. cervicalis*

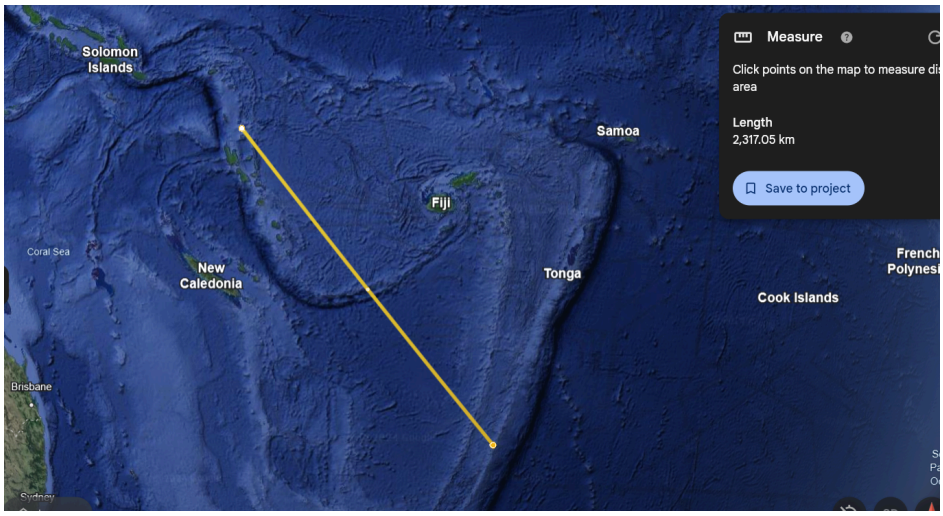
YES. I vote in favor but with reservations. I would like to see broader agreement among regional or global taxonomic authorities on this one, given that it is far outside our purview. With WGAC lumping them, I suppose we should follow suit. That said, these really do seem like subspecies under the BSC, given the extreme similarities in plumage and only subtle differences in size. A formal analysis of vocalizations and genetics would be critical for really sorting out the taxonomy of these taxa.

YES. Given the lack of diagnosability in many individuals, and only slight differences in size, these seem consistent with subspecies-level differences, at best. Further work may demonstrate clearly definable vocal or genetic differences between *cervicalis* and *occulta*, but until then, I agree it is probably better to treat these at the level of subspecies.

YES. There is not enough evidence and data available, so I agree with the proposal on treating *P. occulta* as conspecific with *P. cervicalis.*

YES. I would really prefer to see genetic data, but the phenotypic differences seem slight and seabirds in general tend to exhibit less population genetic structure, so perhaps subspecies is a more accurate treatment pending more information. According to an earlier comment made, this measure was passed by WGAC and seems more of a global issue, so I am inclined to follow their recommendation.

NO. I don't think that a 20% size difference in *Pterodroma*, which is typified by cryptic species, is best accommodated at the subspecies level. Moreover, this is amplified by the major breeding range discontinuity (mainly Macauley Island in the Kermadecs, in the temperate zone for *cervicalis* vs. Vanua Lava in the Banks Islands of Vanuatu for *occulta* in the tropics some 2300 km away), with no intervening breeding grounds known. In addition, Macauley Island is low-lying (reaching just over 200 m elevation) and there *cervicalis* breeds on "gentle slopes" among boulders, while *occulta* is only known to breed above 500 m.



Though WGAC voted for the lump, I'm not in favor of it on these and the (minor) plumage grounds, because I think these taxa exhibit characteristics more typical of species in this genus (think of the Macaronesian taxa), they have been recognized as separate species by both IOC-WBL and Clements in recent years, and a lump would just have to be reversed once more data come in. We know that recordings of both exist even if they aren't online, and it may be just a simple matter of getting ahold of them and making comparisons.

NO. There is clearly more work to be done here before consensus can be reached. However, the range discontinuity (2300 km as noted in the comment) combined with breeding season differences (6 weeks earlier and higher elevation in *occulta*) suggest that gene flow is unlikely. For now I think it's best to retain the status quo pending further data (especially vocal) that might shed light on the likelihood of reproductive isolation.

NO. The Vanuatu Petrel is smaller than the White-necked Petrel, there is variation within groups in the white/black coloration of the underwing, but there are no analyses on vocalizations and genetics. Shirihai and Bretagnolle (2010) measured specimens and analyzed plumage color variation; they mentioned that vocal and genetic analyses are needed before commenting on the taxonomy, I will take the experts' opinion. The IOC and Clements treat them as separate species.

Shirihai, H., and Bretagnolle, V. (2010). First observations at sea of Vanuatu Petrel *Pterodroma* (*cervicalis*) *occulta*. Bulletin of the British Ornithologists' Club 130: 132–140.

NO. The disjunction in breeding range, slight plumage differences, slight size differences, and lack of vocalization data argue that we should keep the original species status for *occulta* until we know more.

NO. I'm on the fence, but I'm voting "no" given that described differences and the fact that the split is already accepted by Clements and IOC (I am aware that WGAC might change this). I also think keeping them separate will motivate future research to clarify the nature of what may be a cryptic species with a limited population size. Additional genetic and vocalization data would be great.

2024-C-6
Replace family name Cettiidae with family name Scotocercidae

NO. Per the addendum to the proposal, it appears that Cetiidae is a valid name and the change to Scotocercidae is not required.

NO. As stated in the addendum, no change is needed due to the previously overlooked conditional but valid use of Cettiinae by Coues (1903), backed up by the use of Cettiidae by Chigi (1912).

NO. The addendum to this proposal makes it clear that Cettiidae is a valid name.

NO. Cettidae is a valid and available name.

NO. After additional evidence was found (see Amended Proposal), it appears that Cettiidae is indeed a valid name that is available for the family, based on its use by Coues (1903) and Chigi (1912), a name also used by Alström et al. (2006) and Dickinson and Christidis (2014). Therefore, no change is needed.

NO. I agree with the addendum, Cettiidae is a valid name.

NO. The amended proposal clears the path to use Cettiidae.

NO. According to the proposal's author, additional evidence was found that indicates

Cettiidae is in fact a valid name and no changes are needed under the ICZN code.

NO. I had missed voting on this motion, so with the addendum I am pleased that the path of least resistance can be adopted, which is “no change.”

2024-C-7
Transfer Mangrove Hummingbird *Amazilia boucardi* to the genus *Chrysuronia*

YES. There are other topological differences between the McGuire and Albertazzi trees (aside from the position of *coeruleogularis* LSUMZ B-29053), including the position of *versicolor* FMNH 395409 and *grayi* ANSP 5064, which gives me pause. But, despite this, I think it’s best to transfer *boucardi* to *Chrysuronia* and place it before *coeruleogularis* for the time being. The available data indicate that it’s at least part of the *Chrysuronia* clade.

I see no reason to change the English name at this time, given that “hummingbird”, “sapphire”, and “emerald” don’t have much taxonomic meaning at this point. Any changes of this nature should be done across many species (if at all) to maintain naming consistency.

As for *luciae*, I strongly suspect that it is truly an *Amazilia*. It is essentially a pale, washed-out version of *Amazilia tzacatl* or *A. yucatanensis*, and sounds a lot like both of those species in both calls and flight calls (and somewhat in song). Without genetic data, it should be kept in *Amazilia* for now anyways.

YES. This change is made clear by its inclusion in the phylogeny of Albertazzi et al. (2022). I also agree with tentatively retaining *luciae* within *Amazilia* rather than making any moves without a firmer basis. Hopefully someone will resolve its position soon.
I don’t think any English name change is needed nor advisable at this time, either to the specific name “Mangrove”, the habitat to which it is endemic, and which distinguishes it from most other hummingbird species, or to the group name “Hummingbird”, since we are far from reconciling group names in the newly constituted *Chrysuronia*.
[Note: *boucardi* is found in mangroves the entire length of the western coast of Costa Rica, not just the northwest.]

YES. The new genetic data support this change. I agree with the recommendation to leave *luciae* as is pending genetic analysis.

YES. Phylogenetic evidence suggests that *Amazilia boucardi* should be transferred to the genus *Chrysuronia*. I agree with the suggested common name in English, Mangrove Hummingbird.

YES. Reasons are given in the proposal.

YES. The reasons were given in the proposal.

YES. Reasons are given in the proposal.

YES. New phylogenetic analyses that include the heretofore excluded *boucardi* indicate that it should be transferred to *Chrysuronia*.

YES. Transfer to genus *Chrysuronia* based on this species' position in the new phylogeny. I also agree with the new placement in linear sequence.

2024-C-8
Treat *Ramphocinclus sanctaeluciae* as a separate species from White-breasted Thrasher *R. brachyurus*

YES. One without comment. For English names, St. Lucia Thrasher and Martinique Thrasher.

YES. The combination of vocal, plumage, and genetic differences are quite convincing to me. These differences are comparable to those in other thrasher species, especially other species of Caribbean thrashers such as *Cinclocerthia*. It is discussed briefly in the proposal, but the plumages are really quite different. In *brachyurus* the dorsal coloration is considerably paler, and there is a scaly "frosted" pattern on the crown that is especially strong on the supercilium. On *sanctaeluciae*, the dorsal is a much darker and evenly rich brown, creating a very different overall look that is immediately diagnosable, especially when combined with the cleaner white underparts that contrast more with the dorsum.

Check out, for example, this photo of *sanctaeluciae*
https://macaulaylibrary.org/asset/214222601
vs. one of *brachyurus*
https://macaulaylibrary.org/asset/415367101

The sample sizes of the vocalizations are of course quite limited, but the harsh scolding calls are very different, being about three times the length and subtly two-noted in *sanctaeluciae*. I think I can pick out some differences in the song pattern, but with only two recordings, I'm not sure that it's a reliable difference. There are some additional sonograms of various call types illustrated in the detailed study of Gros-Desormeaux, et al. (2015), but unfortunately no comparisons with *sanctaeluciae.* The deep genetic split seals the deal for me. Given that both taxa are endangered, I think we need to adopt the taxonomy that best reflects the available data, even if those data are imperfect.

As for common names, I'm not a big fan of the very long compound names like St. Lucia White-breasted Thrasher, but I really think that's the way that we should go. These have some prior usage in literature referring to the subspecies (e.g. Gros-Desormeaux, et al. 2015; Mortensen et al. 2017). The shortened geographic versions could work (e.g. St. Lucia Thrasher) but are not ideal given the radiation of Caribbean thrashers, with multiple thrasher species present on each island. Both islands have two other species with the group name thrasher, although admittedly neither of them are single-island

endemics, plus the tremblers. It would be worth reaching out to ornithologists on both islands to see if they have any other local names for the two taxa. I poked around a bit, and found the following: on Saint Lucia the bird is known as “Gòj Blan” in Saint Lucian Creole, while on Martinique it is known as “Moqueur Gorge Blanche” (Mortensen et al. 2017). Both names translate to White throat or White-throated Thrasher. The conservation plan for the bird on Saint Lucia is called the Gòj Blan Plan. So, given that the local names in both languages refer to the white underparts I think it is critical to retain that aspect in the new names. Another option would be to coin novel plumage-based names for the daughter species that also highlight the white underparts. Just to throw some out there, the brighter white underparts that contrast more with the dorsum in *sanctaeluciae* could be highlighted by a name like Snowy/Ivory/Pearly-breasted Thrasher. For *brachyurus*, we could highlight the duller underparts with Pale-breasted Thrasher, which is admittedly not very exciting. This taxon does have a unique but subtle scaled look to the crown/supercilium, but that doesn’t highlight the white throat/breast. My order of preference then is 1) the long compound names, 2) novel plumage based names, and 3) the purely geographical names.

Gros-Desormeaux, J.-R., T. Lesales, and A.-G. Tayalay. 2015. Behavioral observations on the White-breasted Thrasher (*Ramphocinclus brachyurus brachyurus*): conservation implications. Acta Ethologica 18:197–208.

Mortensen, J.L., M.N. Morton, P. Haynes, J. Tschirky, M.-L. Felix, and J.M. Reed. 2017. Current status of the Endangered White-breasted Thrasher (*Ramphocinclus brachyurus*), a dry forest songbird endemic to Saint Lucia and Martinique. Journal of Caribbean Ornithology 30:39–48.

YES. I support this mainly because the typical calls (mostly on ML) seem to differ consistently (as mentioned above), with the long scolds given by *sanctaeluciae* and short ones by *brachyurus*, and the genetic results of DaCosta et al. (2019) which show fairly deep divergence. Of course longer and better series of recordings and playback experiments would be ideal but I think they are unlikely to change this picture. There is some variation in the plumage traits at least judging by many photos of each, but I don’t think this outweighs the other types of evidence. Song certainly needs more study. Despite their rarity both taxa are rather readily found in their very limited habitats. As a side note, though they are demonstrative and routinely flare their wings they don’t recall tremblers (which habitually do tremble) in appearance and behavior, to me anyway.

For English names, I vote for Martinique Thrasher and St. Lucia Thrasher (but note that ISO 3166 spells out the Saint. We will likely need to consider adopting Saint over St. more broadly but for now for consistency St. is fine). I’d also be OK with Martinique White-breasted Thrasher and St. Lucia White-breasted Thrasher, given how familiar and distinctive that part of the name is.

YES. The combination of phenotypic differences, deep genomic divergence, supposed vocal differences, and limited dispersal (i.e., no movement between islands) together suggest that they are best treated as species. I’m also not a fan of long compound names, but agree that retaining “white-breasted” in the name is probably best given that there are other thrasher species on both islands.

YES. I agree with the proposal, there is enough evidence that these two subspecies are two independent lineages and should be considered two separate species, based on deep genetic divergence, differences on morphology and plumage coloration and a low capacity for dispersal. I vote for the English names Martinique White-breasted Thrasher and St. Lucia White-breasted Thrasher.

YES. At their closest these islands are only 33 km apart, a rather small distance. Because hurricanes and volcanoes are regular occurrences in the Lesser Antilles, I would expect occasional dislodged *Ramphocinclus and some* gene flow between these islands. However, the large genomic distance argues for very little gene flow, even in such close proximity. Given this and the morphological differences, I think these should be considered separate species. I don't like the overly long compound names at all. I think we will get hammered for such clunky English names. St. Lucia Thrasher and Martinique Thrasher are fine, if uninspired. I think we get a little too hung up on having English names that are perfectly diagnostic. Even if there are other thrashers present on the islands, those others (Scaly-breasted and Pearly-eyed) are rather distinctive names. California Thrasher is perfectly apt for *Toxostoma redivivum* even if there are four other thrashers that are regular in the State.

YES. For me, the deep genetic divergence combined with field studies indicating limited dispersal are the most compelling evidence for the split. The islands are pretty close together, yet there is no evidence of historical or contemporary introgression between the forms. There are concordant phenotypic differences as well; I am less inclined to put weight on vocal differences as these are learned and highly variable in Mimidae, but the plumage certainly differs. As for English names, I would prefer St. Lucia White-breasted Thrasher and Martinique White-breasted Thrasher.

YES. Genetic and phenotypic differences. I prefer dropping "White-breasted" for simplicity. However, I do feel reluctant about that after reading Oscar's comments about local names including the white-breasted. I agree that it's important to reach out to locals working on these birds before changing these names.

YES. Reasons provided in the proposal. Preferred English names: Saint Lucia White-breasted Thrasher for *Ramphocinclus sanctaeluciae* and Martinique White-breasted Thrasher for *R. brachyurus*.

YES. Based on the genetic, vocal, and morphological evidence provided in the proposal. As noted in another comment, these taxa have likely had many opportunities to interbreed due to hurricanes and other natural events causing birds to move between islands, yet they remain genetically distinct. For English names, I strongly favor St. Lucia Thrasher and Martinique Thrasher. The long compound names seem cumbersome and wholly unnecessary.

NO. The morphological differences seem minor and to me seem in line with subspecies-level differences. Although the genetic divergence is very deep, I do wonder if the population bottleneck (and possibly others that have gone undocumented) has contributed to this deep divergence, giving a false sense of how old these two taxa

actually are. Insular taxa are always difficult to assess, and I am not strongly opposed to splitting these taxa, but just leaning against it at the moment. If the two are split, I would advocate for simply “Martinique Thrasher” and “St. Lucia Thrasher,” and just dropping the “White-breasted” part of the name.

2024-C-9
Transfer *Phyllomyias burmeisteri/zeledoni* to (a) *Tyranniscus* or (b) *Acrochordopus*

YES (b). Obviously there is a long way to go to straighten *Phyllomyias* out generically, but the separation of *burmeisteri s.l.* (including *zeledoni*) into *Acrochordopus* seems an easy start. Those warty tarsi are so peculiar (what’s that all about anyway?) that its/their placement with taxa that don’t have this feature seems problematic, especially in a group for which morphological differences tend to be understated, to put it mildly.

YES (b). Transfer *Phyllomyias burmeisteri/zeledoni* to *Acrochordopus* in agreement with the SACC given that the geographical range of the species is more widespread in South America.

YES (b). I agree with the proposal

YES (b). Separating *Acrochordopus* from *Tyranniscus* is borderline unnecessary, but given the distinctive morphology and in keeping with the taxonomy of the SACC, I think we should follow suit.

YES (b). I think we should transfer *Phyllomyias burmeisteri* to *Acrochordopus* for conformance with SACC, per the recommendation in the proposal.

YES (b). This is largely a SACC issue, so I defer to their treatment.

YES (b). I do somewhat agree with the comments above that placing these taxa in *Acrochordopus* instead of a broader *Tyranniscus* is a bit of an overly narrow genus circumscription. However, the three species in *Tyranniscus* do form a neat little clade of phenotypically similar species, with very distinctive (and shared) wing patterns and similar face patterns. However the songs of all species in this group (*Tyranniscus* and *Acrochordopus*) are somewhat similar. So, given that it’s a bit of a toss-up, I would prefer to go along with what other taxonomic authorities are doing and recognize *Acrochordopus*.

YES (b). Following SACC, transfer to *Acrochordopus*. However, if it were not for SACC vote I would be in favor of putting all of these in *Tyranniscus*.

NO. While I generally vote in favor of decisions to align NACC and SACC lists, I am opposed to the creation of an additional genus for two species that are very clearly supported as closely related to another group of small tyrannulets that were already all

previously considered part of the same genus. Although the bumpy leg morphology is distinctive, it did not prevent the species from being grouped in *Phyllomyias* previously, and I do not think it should prevent it from being grouped in *Tyranniscus*.

2024-C-10
Treat *Phyllomyias zeledoni* as a separate species from Rough-legged Tyrannulet *P. burmeisteri*

YES. This seems to be the best course based on present data, which shows a fairly deep divergence among the two main groups and a moderate level of vocal divergence that seems comparable to differences between other currently recognized taxa of tyrannids. Of course there are details that need to be further studied and there may ultimately be more species recognized but the data don't yet point to that outcome. I also agree with using the names that have already gotten some traction, White-fronted for *zeledoni* and Rough-legged for *burmeisteri*.

YES. Following the proposal's recommendation, elevate *zeledoni* to species rank, including the *leucogonys* group. This treatment considers differences in vocalizations and in plumage coloration, in addition to the most recent phylogenetic evidence. It also agrees with the SACC. White-fronted Tyrannulet is the common name currently used for *zeledoni*.

YES. This split seems warranted based on the pronounced vocal differences between the two groups. I am not a huge fan of retaining "Rough-legged Tyrannulet" only for *burmeisteri*, given that both groups have the distinctive "rough-legs." I'd almost prefer a name that includes "rough-legged" for both, despite how clunky that would be.

YES. I agree to treat *zeledoni* as separate species including *leucogonys* on the differences of the vocalizations. White-fronted Tyrannulet.

YES. The vocal differences are dramatic and go along with the deep genetic split. White-fronted Tyrannulet is a good English name.

YES. The combination of clear vocal differences and deep genetic divergence support this split, which will bring NACC in line with SACC and other global authorities. White-fronted Tyrannulet.

YES. There is concordance in both vocal and genetic data suggesting that we should follow SACC's lead in both elevating *zeledoni* to species rank and adopting White-fronted Tyrannulet for this taxon.

YES. Reasons are stated in the proposal. White-fronted Tyrannulet.

YES. Vocal differences and follows SACC's treatment. White-fronted Tyrannulet.

2024-C-11
Treat *Tolmomyias flavotectus* as a separate species from Yellow-margined Flycatcher *T. assimilis*

YES. I vote to split for the obvious reasons of very distinctive voice and level of genetic divergence. As noted by others, this is unlikely to be the end of species-level splitting in *Tolmomyias assimilis* but it is a safe one. The English name Yellow-winged Flycatcher/Flatbill that has been used by IOC-WBL and Clements for a while now and has been adopted (though with much dissent) by SACC is reasonable and somewhat familiar, and other name options do not seem enough of an improvement to be worth the added confusion that yet another name change would bring.

YES. Considering *T. flavotectus* as a separate species from *T. assimilis* is a necessary change based on phylogenetic data. Adopt the English name Yellow-winged Flycatcher for *T. flavotectus*.

YES. Splitting *flavotectus* is necessary based on the phylogeny of Harvey et al. (2020). For the reasons outlined in the proposal, I do not like the “Yellow-margined Flycatcher” for *flavotectus*, as I think it would cause too much confusion to adopt that name for a taxon with such a limited range compared to *T. assimilis sensu lato*. I think I prefer “Yellow-winged Flycatcher” even if “Yellow-edged” is slightly more appropriate; I think “edge” is vague and it is not immediately clear what “edge” is being referred to.

YES. The genetic evidence supports that *flavotectus* is unrelated to *T. assimilis*, so this would align the NACC taxonomy with the SACC. English name: Yellow-winged Flycatcher for *T. flavotectus* and *T. assimilis* Yellow-margined.

YES. Pretty much a slam dunk, with the distinctive cadence and notes of *flavotectus* along with its placement on the tree (sister to all other *Tolmomyias*). Yellow-winged is a blah name and not really that descriptive. But it’s better than Yellow-margined with its convoluted history. Of course, it would be nice to align with SACC.

YES. This split conforms to the phylogeny, is supported by vocal differences, and puts NACC in line with SACC. I prefer Yellow-winged for reasons given in the proposal, plus Yellow-edged is confusingly similar to Yellow-margined in my opinion.

YES. The phylogenetic analyses support the split, which is further corroborated by consistent vocal differences. I prefer Yellow-winged Flycatcher for *flavotectus* and Yellow-margined Flycatcher for *assimilis*.

YES. Reasons are stated in the proposal.

YES. *flavotectus* is not closely related to *assimilis* based on the phylogeny. Also YES to the English names.

2024-C-12
Treat *Tolmomyias viridiceps* as a separate species from Yellow-breasted Flycatcher *T. flaviventris*

YES. Reasons are given in the proposal: major vocal differences and relatively deep genetic divergence. Apparent sympatry (if proven) would make the case even stronger. The English names Ochre-lored Flycatcher/Flatbill for *T. flaviventris* and Olive-faced Flycatcher/Flatbill for *T. viridiceps* are reasonable and also reasonably well-established, so any other choice would seem unnecessarily confusing.

YES. Phenotypic (plumage coloration and vocalizations) and genetic data support elevating *viridiceps* to species rank. This split agrees with the SACC. Adopt the English name Ochre-lored Flatbill for *Tolmomyias flaviventris*.

YES. Reasons are outlined in the proposal.

YES. There is enough evidence of the divergence based on the vocal and morphological data.

YES. Reasons given in the proposal: major vocal differences and relatively deep genetic divergence. Largely out of our NACC area so best to follow SACC on splitting off *viridiceps*.

YES. Reasons are given in the proposal, and this puts NACC in agreement with SACC on a mostly extralimital issue. Adopt the English names Ochre-lored Flatbill and Olive-faced Flatbill.

YES. This is an unpublished thesis, but the data strongly suggest two separate species that differ in vocalizations and morphology. Also good to follow SACC on a largely extralimital issue. Yes to English names Ochre-lored Flatbill and Olive-faced Flatbill.

YES. Reasons are stated in the proposal.

YES. Based on morphological and vocal differences and also brings us in line with SACC. Yes to the proposed English names.

2024-C-13
(a) Adopt a new group name for species in the genus *Tolmomyias*, and (b) adopt a new linear sequence for species in this genus

YES. I agree with the use of flatbill given that it is taxonomically (mostly) informative,

already widely adopted, and opens up the field for naming future splits. YES to the resequencing.

YES. I agree with the reasons given in the proposal. New sequence is a necessary change that follows the most recent phylogeny.

YES. Given the sister relationship between *Tolmomyias* and *Rhynchocyclus*, and the long history of usage of “flatbill” prior to 1955, changing the names of the *Tolmomyias* to “flatbill” from “flycatcher” seems warranted. Yes to the new linear sequence.

YES. Adopt the group name "Flatbill" for species in the genus *Tolmomyias*, useful in the field, and the new linear sequence.

YES. Although the unrelated *Ramphotrigon* species are also called Flatbills, it is much more informative to call *Tolmomyias* Flatbills as well because they are sister to the Flatbills in *Rynchocyclos*. Yes to the new linear sequence.

YES. Good reasons for these changes are given in the proposal.

YES. Use ‘flatbill’ for the Engilsh names of *Tomomyias*, lots of good reasons given in the proposal. Change to linear sequence is necessary given the new phylogenetic information at hand.

YES. Reasons are stated in the proposal.

YES. Change in linear sequence is necessary based on phylogeny and flatbill name is more informative.

2024-C-14
Treat *Charadrius atrifrons* as a separate species from Lesser Sand-Plover *C. mongolus*

YES. This change has been adopted by various global taxonomic authorities, and I think that we should follow suit. Because it is not a true parent-daughter split, our naming guidelines are a bit looser, and Lesser could be an option. However, I again think we should follow suit and go with Siberian and Tibetan.

I see that the whole genome data indicate paraphyly of *leschenaultii* with regards to *mongolus*, so a lump could perhaps be considered there. I would not be surprised if there is ongoing gene flow between those two taxa. Something for other taxonomic authorities to sort out though.

YES. Clearly the two-species solution isn’t correct. I don’t think a lump of these three is reasonable, either, as *mongolus* at least has a somewhat different voice, and

leschenaultii and *atrifrons* occupy different breeding habitats, either in parapatry or perhaps sympatry, with *leschenaultii* being a desert breeder while *atrifrons* breeds above timberline. YES to English names Siberian and Tibetan, already widely adopted and reasonably appropriate (despite the Russian Far East issue).

YES. Phylogenetic evidence based on mitochondrial and genomic data shows that *Charadrius mongolus* is not the sister clade of *C. atrifrons*, making the split of both taxa necessary. I agree with the recommended English name for *C. mongolus*, the species that can be found within NACC jurisdiction, Siberian Sand-plover.

YES. Reasons are outlined in the proposal.

YES. I agree with the separation of *atrifrons* from *mongolus* according to the proposal.

YES. I agree with the concern regarding using cladistic analysis of skeletal elements for species-level taxonomy, but this change is supported by genetic/genomic data as well as some vocal and phenotypic. This is an extralimital issue that brings NACC in line with global treatments. I agree that we should follow the English names used by Old World authorities.

YES. Given that this is largely extralimital to NACC, I am inclined to agree with the authors and support the decision made by global / Eurasian taxonomic authorities and split *atrifrons* from *mongolus*. However, I will note that the sampling is somewhat sparse, and many of the individuals were sampled on migratory routes rather than during their breeding season, so we don't know where those individuals breed and there may be some admixture between these forms with more rigorous sampling of the contact zone. While there are phenotypic differences concordant with phylogeographic structure, the crown age of the group is still rather young at 2 mya. But given that *leschenaultii* has long been recognized as a separate species, it makes sense to split *atrifrons* and *mongolus*.

YES. I vote in favor based on phylogenetic position of these three taxa. Yes also to proposed name that agree with global lists.

NO. I don't think cladistic analyses of skeletal elements should have any bearing on species level decisions; in addition, many of Livezy's characters are difficult to interpret (*fide* D. Steadman). That leaves vocalizations and mtDNA. We all know that analyses of mtDNA can yield spurious results with regard to evolutionary history, especially at the species level. In addition, paraphyly at the species level is not a huge concern (e.g., GW Teal, ravens). The vocalization data perhaps does argue for species status, but one co-author of the proposal was a bit skeptical.

2024-C-15
Treat *Oenanthe seebohmi* as a separate species from Northern Wheatear *O. oenanthe*

YES. I find the evidence pretty convincing and I see that Lars Svensson has split it in the latest (3rd) edition of his field guide published last year. As for the English name, Atlas Wheatear is a great name as it well-reflects its breeding range and at least peripherally its year round range. But, here we go. It's not our place to give an English name to a bird that does occur remotely near North America. Even worse, in this case the odds of this largely resident or short distance migrant occurring in the AOS area is near nill. The matter should be left to Europeans and I've contacted the editor of British Birds about what English name is likely to be adopted. Svensson used the English name of Seebohm's Wheatear and if that becomes the English name widely adopted, then that's the name we should use.

YES. I am not very convinced by the available evidence, but we should go along with the taxonomic treatment adopted by the regional and global authorities for this one, for both the species treatment and the English name. The total lack of mitochondrial divergence is worrisome, but given the obvious plumage differences there must be some nuclear DNA divergence. Perhaps there was some mitochondrial haplotype capture due to gene flow. It looks like Clements (presumably following WGAC?) uses Atlas Wheatear, so I'll vote for that name. *Oenanthe oenanthe* should absolutely retain the name Northern Wheatear, as that name is very well entrenched.

YES. Recognizing it is a judgment call but still in my opinion better treated as a full species for the reasons given in the proposal. YES to Atlas Wheatear (Clements followed IOC on this, as WGAC hasn't involved itself in English names decisions), which already has wide usage (with rather more Google hits than Seebohm's), and I haven't seen resistance to its adoption. But for any countries/regions that prefer Seebohm's Wheatear that can still be an option for eBird users, anyway.

YES. Follow the global treatment for the extralimital *O. seebohmi* and elevate it to species rank. Adopt the English name Northern Wheatear for *O. oenanthe*.

YES. Reasons are outlined in the proposal.

YES. I agree with the proposal.

YES. I agree we should follow the global treatment for the extralimital *O. seebohmi* and elevate it to species rank. If this was a species in the NACC region, I may demand better evidence, but I think it is best to adopt the treatment of the local authorities.

YES. I don't find the available evidence very convincing to support species-level treatment of *seebohmi*, but given that this is an extralimital taxon I think it's best to conform with global treatments. Adopt the English name Atlas Wheatear for *seebohmi* and retain Northern Wheatear for *oenanthe.*

YES. Seems a borderline case with some weird evolutionary dynamics that should be studied more. This really only occurs in NACC area as a vagrant, so I think we should follow the global authorities in their preferred treatment.

2024-C-16
Reconsider our taxonomic treatment of quail in the genus *Cyrtonyx*

YES. This is a difficult case. It is more difficult to merge species than to split them. However, I think that we should change the taxonomic treatment in the genus *Cyrtonyx*. Molecular and phenotypic data (plumage coloration, vocalizations, and intermediates) support considering the quail in the genus *Cyrtonyx* as one single species.

YES. We certainly need better geographic sampling for this system, and there very well may be more than one species involved, but for now I prefer lumping and treating these as a single species. We know that plumage patterns are quite variable in quails and related taxa, and the genomic and phenotypic data at hand suggest these are weakly differentiated with intermediate individuals regularly occurring. While 2 species is the current status quo, a single species taxonomy has been supported in the past and seems a better position for the time being until we have more data.

YES. The available data indicate that a one species treatment is best. In particular, if there was a long history of isolation between our two currently recognized species, this should have been picked up with the available genetic data. It was not. To me, there is enough data at present to consider these one species.

NO. There are too many unanswered questions, and I would prefer to maintain the status quo until a more extensive analysis is conducted, especially with more genetic samples of each taxon that include contact zone, and with analysis of vocalizations. I listened to the recordings in the links provided, and I do hear some differences between *montezumae* (s.l.) and *ocellatus*. To me, the male buzzy call of *ocellatus* has a rising component at the end, compared to the overall descending call in *montezumae*. For the female call, *ocellatus* is considerably slower-paced and with fewer notes than in *montezumae*. Having just one recording of *sallei/rowleyi* makes any conclusions regarding their vocalizations much tougher, but that recording does sound more similar to those of northern *montezumae* types (more descending).

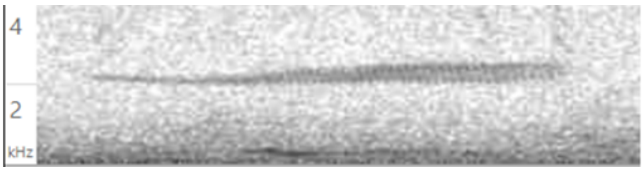
As for the genetics, the position of *sallei/rowleyi* in the tree is confusing. It seems like the ML tree has them sister to *ocellatus* but the quartet tree has them sister to *mearnsi*, the latter of which makes no biogeographic sense. Given that Salter et al. considered the svdquartets analysis to be essentially a polytomy, the ML tree with a paraphyletic *montezumae* seems to be the one to go with. However, given that the plumage of *sallei* is much more similar to *montezumae*, I would prefer to maintain them in that species for now (despite the paraphyly), although splitting *sallei/rowleyi* as a third species does seem like a viable alternative. My issue with splitting them (or transferring *sallei/rowleyi* to *ocellatus*) is the purported hybrids between *montezumae* and *sallei* (“*merriami*”). I would like to see some data from this potential contact zone before elevating or transferring *sallei/rowleyi*, which at this point we know little about.

The plumage of *ocellatus* is so distinctive, especially the color and pattern of the underparts (but also the dorsal streaking), and combined with the vocalization differences that I think I'm hearing, it does seem like a separate species, despite the paraphyly. I agree with the proposal that these differences are less than those shown within species of *Colinus*. However, *Colinus* is an outlier in the family in terms of intraspecific variation, and *Cyrtonyx* is phylogenetically closer to the *Odontophorus*, which do not show extreme amounts of intraspecific variation. In fact, the plumage differences between *ocellatus* and *montezumae* are on par with those between *Odontophorus* species, except perhaps *O. speciosus* which does show some intraspecific variation.

Further note from proposal author: Although the underparts of *ocellatus* are distinctive from those of *mearnsi-montezumae-sallei*, note that *sallei* shares the orange dorsal streaking with *ocellatus*, as can be seen in the photos in the proposal. This is true to some extent of *merriami* as well, and is one of several features that show different break-points or points of intermediacy between the taxa in this complex.

As noted in the proposal, the principal calls of the different taxa are similar but there's individual variation in a variety of features, and although calls show average differences between taxa in several features, they don't appear to be diagnosable. For example, male "descending" calls of *montezumae-mearnsi* usually descend slightly, but calls that ascend at the end are present in the Macaulay Library and presumably on xeno-canto, such as this one of *mearnsi* from Cochise, Arizona (ML242504681):

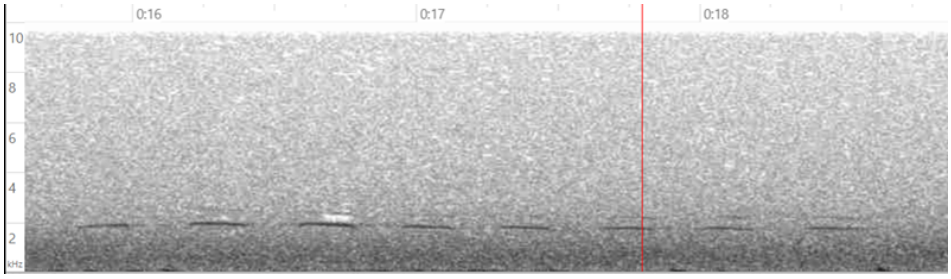
and this one from Texcoco in the state of Mexico (*montezumae*), which is almost entirely ascending after a brief descent at the beginning (ML453173871):



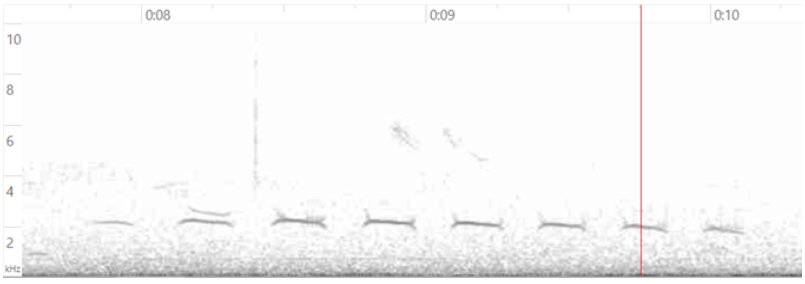
Likewise, although most male calls of *ocellatus* rise slightly at the end, one can find descending calls with no rise at the end (ML23663021):

The female calls are also quite variable, including in pace and number of notes. This 8-note call of *mearnsi* that our own Nick V recorded in Cochise (ML516727591) is very similar to the following call of *ocellatus* from Honduras, except that the one from Honduras (ML203867601) is actually slightly *faster*. Nick's recording from Arizona also includes a similar but shorter (5-note) call at the beginning of the clip.

Mearnsi



ocellatus



Differences in pitch of male calls are perhaps the most pronounced, with those of *ocellatus* typically around 3 kHz and those of *montezumae* typically closer to 2 kHz, but again there's overlap, as in this call of *ocellatus* from Honduras (ML228389051):

compared with this call of *mearnsi* from Arizona (ML476011151):

A quantitative analysis of the male calls might result in differences in PCA space between *ocellatus* and *montezumae-mearnsi*, but what to do about *sallei-rowleyi*? The only song of *sallei* (see the sonagram in the proposal) is around or slightly below 3 kHz, more like typical *ocellatus*, but is a slightly descending call with no rise at the end, more like typical *montezumae-mearnsi*. The intermediacy in two features of this call is similar to the intermediacy in the plumage of *sallei*, which is more like *ocellatus* in its dorsal streaking but more like *montezumae-mearnsi* ventrally.

NO. I don't really like any of the options, but after initially favoring a one species treatment, I would prefer to await additional studies, particularly of calls and additional genetic studies before disturbing the status quo. Lumping Ocellated Quail, a species of major international interest, would be unsettling for many and I would hope could be supported by pretty overwhelming evidence.

NO. Really tough decision on this obviously borderline case. I too would hate to reverse the long-standing specific treatment of *ocellatus* on the basis of what we know now (although this is definitely a reasonable course of action) and then have to re-reverse it. It's encouraging to see that a genetic paper on the topic came out just after the WGAC decision to split three ways, and although the genetics didn't settle the issue at all, at least people are working on it. Hopefully this proposal and the discussion and uncertainty will stimulate further work, especially further well-documented sound-recording and analysis. It would be valuable to have a better perspective on the levels of intergradation, and whether we just happen to have a few specimens of hybrids at zones of contact or whether the birds themselves simply don't care much about these plumage differences that are so obvious to us (as in Northern Flicker, for example). I agree that the differences pale compared to those in Northern Bobwhite but also that they are far above those in most species of *Odontophorus*. So, my NACC vote is for no change at this time, although I could equally support the three-species treatment and may eventually concede that the single-species treatment is warranted.

NO. As others have noted, this "NO" vote is to maintain the status quo *for now*. It does seem that changes in the taxonomy of these quail are needed, but I do not think we have enough information to go on to make an informed decision one way or the other. In light of the genetic results of Salter et al. (2022), and despite the apparent paraphyly of Montezuma Quail, I still think it is best to maintain the status quo: even though the ML tree shows *rowleyi* and *sallei* as successively sister to *ocellatus*, since none of the trees seem to agree, and one analysis shows a polytomy, the branch lengths at the nodes are very short, and it seems likely that additional information added to these analyses could lead to even different results, so to me the ML tree should only be taken with a grain of salt.

NO. We need more data and more studies.

NO. Although it is ideal to avoid paraphyly in taxonomy, at the species level paraphyly does not always indicate species incongruence with species limits using the BSC. Without more thorough analyses of vocalizations, genetics, and behavior I think it is best to keep the status quo. I would think that the taxa of concern are kept in captivity and some experiments with mate choice would be possible.

NO. I prefer to maintain the status quo for now pending the publication of additional data - in particular more genomic data with better sampling (especially across a potential contact zone) as well as a formal analysis of vocal variation within and among populations.

2024-C-17
Transfer *Habia fuscicauda* and *H. atrimaxillaris* to new genus *Driophlox*

YES. 1 without comment.

YES. Reasons are given in the proposal. *Habia* is clearly paraphyletic, and merging all into *Chlorothraupis* is not a viable solution. I do think that merging *Chlorothraupis* and *Habia* (*rubica*) could be an option. Female *Habia rubica* don't look too different from a *Chlorothraupis*, but that's not a voting option and doesn't affect the recognition of *Driophlox.*

YES. A new genus is clearly required for the *Habia* species other than *rubica*. As for the possibility of merging *Chlorothraupis* into *Habia*, which I gather we aren't specifically voting on, this is intriguing and may prove to have merit, but they seem different enough in morphology and vocalizations that I'm still unconvinced about this potential move.

YES. Phylogenetic evidence indicates that a change in the genus of *Habia fuscicauda* and *H. atrimaxillaris* is necessary. I agree with transferring both species to the newly proposed genus *Driophlox*.

YES. Moving *Habia fuscicauda* and *H. atrimaxillaris* to a new genus is clearly necessary based on the results of Scott (2022). While not a voting option, I also would support the idea of moving *Habia rubica* alone into *Chlorothraupis*.

YES. The genus *Habia* is paraphyletic on the basis of the UCE data, so moving to *H. fuscicauda* and *H. atrimaxillaris* to a new genus is necessary.

YES. We need to address the clear paraphyly in *Habia*. Recognizing *Driophlox* for *fuscicauda* and *atrimaxillaris*, instead of merging in another genus, is warranted, especially since the sister to *Driophlox* is unclear. Retaining *Habia* for *rubica* is preferred over uniting with the quite disparate *Chlorothraupis.*

YES. A change is clearly needed and I agree with the proposal's recommendation to transfer *H. gutturalis, H. atrimaxillaris, H. cristata*, and *H. fuscicauda* to *Driophlox* while retaining *Habia* for *H rubica.*

YES. The arguments presented based on the phylogeny and phenotypic variation in the group are convincing to transfer *H. gutturalis*, *H. atrimaxillaris*, *H. cristata*, and *H. fuscicauda* to *Driophlox*.

ABSTAIN - 1.

2024-C-18
Treat *Colinus leucopogon* as a separate species from Crested Bobwhite *C. cristatus*

YES. 1 without comment.

YES. A very tentative vote in favor. The differences between *leucopogon* and *cristatus* seem to me on par with those between *virginianus* and *nigrogularis*. Despite the extreme plumage variation in both groups of bobwhite, the differences in both cases are in the patterns of melanin-based plumage on the face and throat. The degree of genetic differentiation in both pairs of taxa also appears to be similar, which is comforting. Note that the pattern of very long terminal branches in the tree is a common artifact of UCE phylogenies, especially when based on data from low-quality or fragmented samples like toepads. The length of the subtending branches is what does seem comparable in *leucopogon*/*cristatus* vs. *virginianus*/*nigrogularis*. Critically, it appears that the two taxa that approach each other most closely in Costa Rica (*dickeyi* and *mariae*) are more closely related to other (geographically proximate) taxa in their respective groups. What gives me pause is the extremely similar songs of *leucopogon* and *cristatus*. I am not picking up any consistent differences between the two groups. However, I also don't hear consistent differences between *virginianus* and *nigrogularis*, so for the sake of consistency, I'll vote for considering both pairs as full species. Perhaps we can revisit this in the future and reconsider the species status of *nigrogularis* and *leucopogon* at the same time. For English names, Spot-bellied and Crested seem quite well entrenched, so I'll vote for those, despite the fact that both groups have both spotted bellies and crests.

YES. Although not a super strong case, I think it is preferable to keeping *leucopogon* lumped with *cristatus*, which would also seem to dictate lumping *nigrogularis* with *virginianus*. And YES to keeping the long-recognized though imperfect names Crested Bobwhite for *cristatus s.s.* and Spot-bellied Bobwhite for *leucopogon*.

Not sure this was mentioned in the proposal, but SACC has long implicitly treated leucopogon as a separate species (see SACC note):
Sibley & Monroe (1990) considered Colinus cristatus to form a superspecies with North and Middle American C. virginianus and Middle American C. nigrogularis. Some authors

(e.g., Hellmayr & Conover 1942, Sibley & Monroe 1990) have considered Central American C. leucopogon to be conspecific with C. cristatus; they form a superspecies (Stiles & Skutch 1989). REFS and Dickinson & Remsen (2013) treated leucopogon as a subspecies of C. cristatus. SACC proposal needed.

YES. Interesting group of birds with wide geographical variation, which is represented in the numerous subspecies described within the *Colinus* species. The phylogenetic results correspond to several plumage traits, which were used originally to describe *C. cristatus* and *C. leucopogon* as separate species. Treating *leucopogon* as a separate species from *cristatus* is consistent with species ranks in the other *Colinus* species. I agree with the recommended English names, Crested Bobwhite for *C. cristatus* and Spot-bellied Bobwhite for *C. leucopogon*.

YES. I agree with the proposal that this is very much a borderline case, and I do not really feel strongly one way or another on these taxa, however, given the degree of difference between *leucopogon* and *cristatus* as compared with *virginianus* and *nigrogularis*, I do think it is necessary that we split what we currently treat as *Colinus cristatus*. I listened to a number of recordings in Macaulay Library, and had a hard time separating the two, but then, I had a hard time separating any of the *Colinus* from each other, as all sound quite similar.

YES. I agree with the proposal that this is a borderline case, very difficult group.

YES. The plumage differences between the two nearest subspecies (*leucopogon dickeyi* in Costa Rica and *cristatus panamensis* in Panama) are much more dramatic than the written accounts and photos in the proposal show. Here are some photos from eBird:

dickeyi: https://macaulaylibrary.org/asset/72831311

panamensis: https://macaulaylibrary.org/asset/432322411

These differences, coupled with genetic differences and possibly vocal differences argue for species status.

YES. For reasons outlined in the proposal including monophyly based on genomic-level data and consistency with the *virginianus* and *nigrogularis* split.

YES. I agree with the proposal in that the genetic data and commensurate phenotypic differences compared to other species within the genus supports splitting *C. leucopogon* from *C. cristatus*.

NO. This is clearly a borderline case, but I would like to see the vocal data published and then revisit this issue.

2024-C-19
Add Icterine Warbler *Hippolais icterina* to the Main List

YES. 2 without comment.

YES. Reasons are stated in the proposal.

YES. Reasons are given in the proposal.

YES. I vote to accept Icterine Warbler based on the photos by Rodney Ungwiluk at Gambell, St. Lawrence Island, on 22 September 2022. An odd shaped Old World warbler, big body, but attenuated rear end, flat crown, shortish tail, long wings. The yellow plumage in this genus narrows it down to Melodious Warbler (*H. polyglotta*) and Icterine Warbler (*H. icterina*). The long wings, reaching the upper-tail coverts and about as long as the tertials, and the pale contrasting edging to the tertials, secondaries, and greater coverts are distinctive for Icterine. The fresh plumage indicate a HY bird.

YES. Straightforward addition to the main list based on good evidence.

YES. Reasons are stated in the proposal.

YES. Photographic evidence from a bird in Alaska supports the addition of *Hippolais icterina* to the main list of North American birds.

YES. The reasons are given in the proposal.
YES. As per the proposal.

2024-C-20
Add Western Marsh Harrier *Circus aeruginosus* to the U.S. list

YES. 2 without comment.

YES. Reasons are stated in the proposal. I hope the specimen was saved.

YES. Reasons are given in the proposal. I too hope that there is an archived specimen or at least some parts of the bird.

YES. The Maine RBC's report summarizes the encounters there, and also presents confirmation that the bird 11 weeks later in New Jersey was almost certainly the same bird. The bird-strike report adds a grisly bow to the record. No problems with the identification, as this species in immature plumage presents a rather distinct pattern with solid browns and creams, and a near lack of streaking. Molt and plumage pattern indicates a female molting into first basic plumage. It doesn't seem harriers are kept by falconers, and a vagrant record on the Atlantic coast is very plausible. The English name is Western Marsh Harrier in the NACC (no hyphen).

YES. Very well documented with photos and verification from the feather lab.

YES. Reasons are stated in the proposal.

YES. Photographic and genetic evidence confirms the presence of Western Marsh Harrier in the United States.

YES. The reasons are given in the proposal.

YES. As per the proposal.

2024-C-21
Treat *Gelochelidon macrotarsa* as a separate species from Gull-billed Tern *G. nilotica*

YES. 4 without comment.

YES. Reasons are given in the proposal. Gull-billed Tern and Australian Tern are good names.

YES. Reasons are stated in the proposal. I support the use of Gull-billed Tern for *G. nilotica* and Australian Tern for *G. macrotarsa*.

YES. While we only have mtDNA for our genetic data, there are various differences in ecology, molt, plumage, and distribution that support bringing our checklist in line with global authorities.

YES. This is a borderline case. Although I think I would be a little more hesitant if this was a taxon in our area, I think enough evidence is presented for us to go along with the global trend. I support the continued use of Gull-billed Tern for *G. nilotica*.

YES. Differences in plumage, morphological measurements, breeding and molting times, feeding behavior, and vocalizations support the split of *G. macrotarsa* from *G. nilotica*. I agree with the English name Gull-billed Tern for *G. nilotica*.

YES. The combination of differences in morphology, molt, breeding, and putatively voice support species-level treatment that brings NACC in line with global authorities for this extralimital taxon.

YES. The integrative data show these similar-looking taxa to be quite distinct morphogically and ecologically.

2024-C-22
Treat *Automolus cervinigularis* as a separate species from Buff-throated Foliage-gleaner *A. ochrolaemus*

YES. 3 without comment.

YES. While not the strongest of cases, I think the approach taken by WGAC is the right one, given the stronger vocal differences between *cervinigularis* and *ochrolaemus* than the differences that led to the split of *exsertus*. Although more genetic data would be nice, I think the fairly deep genetic divergence plus the mtDNA findings of *exsertus* as sister to *cervinigularis* to the exclusion of the *ochrolaemus* group is sufficiently strong for me to support splitting *cervinigularis* from *ochrolaemus*. I support adopting the names Fawn-throated Foliage-gleaner for *cervinigularis* and Ochre-throated Foliage-gleaner for *ochrolaemus*.

YES. We considered *exsertus* a separate species from *hypophaeus/cervinigularis* based mainly on playback trials. The difference in vocalizations between the *cervinigularis* and *ochrolaemus* groups is more dramatic than between *hypophaeus* and *exsertus;* thus, we should consider the *cervinigularis* (with *hypophaeus*) separate from the *ochrolaemus* group. The genetic results kind of lean toward species status, but mtDNA is often a poor marker for determining species limits.

YES. Phylogenetic evidence (mtDNA) shows that *cervinigularis* is not the sister clade to *ochrolaemus*. Although having information from the contact zone should be a research priority, I support the split based mainly on the differences in vocalizations. I agree with the proposed English names, Fawn-throated Foliage-gleaner for *A. cervinigularis* and Ochre-throated Foliage-gleaner for *A. ochrolaemus*.

YES. Reasons are given in the proposal. I do want to mention a few additional things that were not included in the proposal. First, it appears that some WGAC members were under the impression that the Smith et al. (2014) findings were based on Ultraconserved Elements, rather than a single mitochondrial gene, so that may have strengthened their argument to split *cervinigularis*. Second, there are quite a few eBird records of *Automolus* between the canal zone and far western Panama (i.e. the potential contact zone), but just one from western Veraguas has photos (https://ebird.org/checklist/S134726258). Given the lack of specimens from this region (at least none were available to Cory, Hellmayr, or Ridgway, or at the LSUMNS), these photos are of particular interest. The Veraguas bird looks to me to be a fairly typical *hypophaeus*, which would imply that the contact zone with *pallidigularis* is between western Veraguas and the canal zone, and thus might be fairly narrow. There unfortunately appear to be no recordings from this region.

YES. The vocal and mtDNA evidence clearly favors a three-species treatment. As I recall, we recognized this with the earlier split of *exsertus* but considered it a primarily SACC-region issue. Of course, there will be issues to be sorted out with the potential contact zone in Panama.

NO. I'm wary that weird things can happen with mtDNA in contact zones, so I'd rather see more genetic data from the contact zone before we split these. Maybe the mtDNA gene tree that constitutes much of the evidence for the split does not reflect the species tree. Maybe there's hybridization and introgression that we haven't detected yet. While the vocalizations do seem to differ, this is purely qualitative and based on our perception. The other phenotypic differences seem relatively minor.

NO. I think it's premature to split these based primarily on mtDNA gene trees, without data from the contact zone and a formal study of vocal differences.

2024-C-23
Transfer Gray Francolin *Francolinus pondicerianus* to *Ortygornis*

YES. Reasons are given in the proposal. Transferring to *Ortygornis* makes sense based on branching times relative to other genera, and has been adopted by various global checklists.

YES. Reasons are given in the proposal. Maybe we are dissecting the old *Fancolinus* a little too much, but given that it was the most diverse avian genus, I recall, a new taxonomy would be welcome, especially if the genera branched at similar ages.

YES. It is primarily an extralimital species, and I agree with following global checklist authorities.

YES. Reasons are given in the proposal, and it makes sense to conform with global authorities in this case.

YES. This change has already been adopted by global checklists and it makes sense to follow their lead here.

YES. Reluctantly. I am always a fan of more inclusive genera, but in this case, to align with global taxonomic authorities on a group that is largely extralimital to our area, I will vote to accept these changes.

YES. Makes sense based on phylogenies and agrees with global lists.

YES. Reasons are given in the proposal.

YES. My thoughts are to follow other global authorities for a marginal and confined introduced species. To not follow these authorities would require pretty firm reasons to take a different path.

YES. For alignment with the phylogenies and global lists.

2024-C-24
Establish English names for *Campylorhynchus rufinucha sensu stricto*, *C. humilis*, and *C. capistratus*: Veracruz Wren, Russet-naped Wren, Rufous-backed Wren

YES. I like Veracruz and Rufous-backed. West Mexican is really blah, but Russet-naped is equally bad and we just used it for *Aramides*. Such a lively conspicuous bird must have lots of fun local names. Without better alternatives, I ultimately think that Rufous-naped is the best suited English name.

YES. I vote for the following English names: Rufous-backed Wren for *C. capistratus*; Veracruz Wren for *C. rufinucha*. Russet-naped Wren for *C. humilis* if there is no better name based on local common names.

YES. Veracruz Wren seems like a good name for *C. rufinucha*. Rufous-backed Wren seems fine for *C. capistratus*. Per the proposal and other comments, I'm not crazy about any of the names for *C. humilis* but I favor Russet-naped Wren among the alternatives provided in the proposal.

YES. I also like Veracruz for *C. rufinucha* and Rufous-backed *C. capistratus*. As for *C. humilis*, I'm not a huge fan of the available options so far, so if someone comes up with a better option, that would be great. I'm coming up short, especially for plumage-based names. Of the ones suggested, I like Russet-naped the best, so I'll vote for that one for now. It does have the nice parallel with *C. capistratus* and with the parent species, and it accurately describes the duller rufous nape of the species. As for geographic names, I had initially thought that West Mexican was a good choice, as it parallels the range of the Chachalaca, but there's the complication of the more westerly/northern *C. gularis* that has a more similar range to the Euphonia by that name. That said, with *Ortalis wagleri* having a similar distribution as *C. gularis,* West Mexican is an acceptable option to me. The range of *C. humilis* is more centered on the state of Oaxaca than Guerrero, but even Oaxaca Wren is not a great option. Certainly not as good as Veracruz Wren is for *C. rufinucha*. I'm not aware of a name for the biogeographic region where *C. humilis* is found. The people of the region where *C. humilis* is found are the Mixtec and Zapotec. I haven't been able to find any sources on their names for birds, although I'm sure that plenty exist.

YES. I like Veracruz Wren and Rufous-backed Wren for *C. rufinucha* and *C. capistratus*, respectively. I'm not overly excited by the names for *C. humilis,* but I am fine with Russet-naped Wren. While none of the names are particularly inspiring, Russet-naped and West Mexican both are still better fits than many other common English names that have been in use for other species for years.

YES. Veracruz for *rufinucha* and Rufous-backed for capistratus. Of the suggested names

for *humilis,* I agree with the suggestion that Russet-naped is best. It is a different name from Rufous-naped, so shouldn't be a problem for conveying this split.

YES. I like Veracruz for *rufinucha*, Russet-naped for *humilis*, and Rufous-backed for *capistratus*. While they are fairly routine, I like the parallel structure of having a color plus body part combination for the English names.

YES. I agree to Veracruz for *rufinucha*, Rufous-backed Wren for C. *capistratus* and Russet-naped Wren for *humilis,* although I also like the Western Mexican Wren for *humilis*.

YES. I like West Mexican Wren, which has the advantage of immediately placing it geographically with respect to the other former members of *C. rufinucha*. However, of course there are plenty of other wrens (including other regional endemics) in western Mexico, so I can understand why this wasn't the first choice of others. I am OK with Russet-naped, although I think it is somewhat confusing with the former Rufous-naped, and the definition of russet is a dark reddish-brown, while photos show a rather pale rufous on the names of most individuals.

YES. The only name I'm enthusiastic about is Veracruz Wren. Russet-naped Wren (for *Campylorhynchus humilus* is fine, except that it is a nouveau name. Steve Howell says that the name most associated (e.g., HBW) with this wren is Sclater's Wren, named after the renowned English ornithologist, Philip Lutley Sclater (4 November 1829-27 June 1913). Rufous-backed Wren for *C. capistratus* works well for some subspecies, less well for others, those with more striped backs, but with an underlying rufous coloration. I'm not aware of another English name that is in use. Steve Howell says that the isolated population (subspecies *castaneus*) in the Argun Valley of northern Honduras is distinct vocally and may well be a separate species, so perhaps the issue (more splits) will be revisited in the years ahead.

2024-C-25
Establish English names for barn owls *Tyto alba s.s.*, *T. javanica*, and *T. furcata*: Western Barn Owl, Eastern Barn Owl, American Barn Owl

YES. The only name that I can really get behind is American Barn Owl for *T. furcata*. I don't like 'Western' and 'Eastern' for the other taxa (reasons given in the proposal), although I am reluctantly voting for those in order to align with other global lists.

YES. I'm in full agreement with others. I like American Barn Owl for *T. furcata*, but I'm not a fan of 'Western' and 'Eastern' for the other taxa. I was also not a fan of those names for the Cattle Egrets. I'll go along with whatever the global or regional taxonomic authorities decide on. I will strongly vote *against* Common Barn Owl for *T. alba*, though, as it is just as common as other species of Barn Owl.

YES. Like others have noted, and the proposal has noted, “Western” and “Eastern” Barn Owl are not great names for *T. alba* and *T. javanica*, but given that these taxa have not occurred in our region, I think it is best that we defer to other authorities on these names, as it does not really seem like our place to decide these names. I am fine with American Barn Owl for *T. furcata*.

YES. American, Western, and Eastern. Like others, I’m voting reluctantly for Eastern and Western since they are extralimital and there is not a suitable alternative.

YES. I vote for the following English names: Western Barn Owl for *T. alba s.s.*; Eastern Barn Owl for *T. javanica*; American Barn Owl for *T. furcata*.

YES. I vote for Western for *T. alba ss* and Eastern for *T. javanica* and for *T. furcata* American.

YES. I vote for Western and Eastern Barn Owl for the Old World species and American Barn Owl for the widespread New World species. This follows global usage and in an Old World context “west” and “east” makes sense, as does “American.” Someone can explain to me why no hyphen is needed, but I always support not using hyphens, if possible.

YES. Although not perfect, the names American, Western, and Eastern are well-established and useful, and align with global lists. Without better alternatives, I vote for these names.

2024-C-26
Change (a) the English name and (b) the type locality of *Puffinus lherminieri*

YES. (a) 4 for Sargasso Shearwater without comment; 3 find Caribbean Shearwater also acceptable. (b) 4 without comment.

YES (Sargasso Shearwater). For (a), I was initially strongly in favor of Caribbean, and I still think it’s a perfectly fine name. I also strongly disagree with many of the points raised in the external comment. First off, there is no reason not to name a seabird after the breeding islands / region, if it is generally unique to the region. The fact that there are ‘already’ four other species with the name Caribbean also does not diminish the appropriateness of the name for the *Puffinus lherminieri*. The name “Caribbean” is anything but mundane. It is one of the most biologically unique parts of the planet! I am also strongly opposed to making decisions based on what a ‘majority’ of birders (in this case almost exclusively those in the United States) perceive as the ‘typical’ range of the species. By that same logic, Bermuda Petrel would be an inappropriate name just because many pelagic birders detect it off of North Carolina.

All that said, I do think that Sargasso is a better name. It is more evocative and it applies to a unique aspect of the ecology of the species. Yes, I'm sure that other shearwater species utilize *Sargassum*, but it does appear that *P. lherminieri* is especially tied to the *Sargassum* habitat. I also strongly suspect that it is tied to *Sargassum* in the breeding season, as the Great Atlantic *Sargassum* Belt passes through much of its Caribbean breeding range. Yes, various species of *Sargassum* are found worldwide, but the habitat in the Sargasso Sea is quite unique. I agree that Sargasso is better than Sargassum, although I note that other species unique to the *Sargassum* habitat are called Sargassum, such as Sargassum fish and Sargassum Swimming Crab.

YES to (b) for the reasons given.

YES (Sargasso Shearwater). (a) Sargasso is a fine name and better than the rather bland Caribbean. However, I have been on many pelagic trips off Florida and I have never detected a tight association of *lherminieri* with *sargassum.* Given that the Sargasso is also used for the rather nebulous geographic region that encompasses much of the range of *lherminieri,* it is suitable. (b) YES.

YES (Sargasso Shearwater). (a) I have no strong feelings toward this, but find Sargasso Shearwater a good name for the bird, but equally would be fine with Caribbean Shearwater. I also prefer Sargasso Shearwater over Sargassum Shearwater. I agree with another committee member that the perception of a majority of birders from North America should not be a driving force in this decision, and place more weight on the opinions of Caribbean ornithologists and birders. I personally find names related to the island/location of breeding for seabirds to be quite interesting and evocative, moreso given their propensity to wander extremely long distances during the non-breeding season; on the East Coast of the US, if more seabirds were named for their breeding location, it would paint a fascinating portrait of the dispersal abilities and epic journeys of the birds we take for granted, and fascinating to think that all of the common seabirds come from wildly different places: Antarctic (Wilson's) Storm-Petrel, Caribbean (Sargasso/Audubon's) Shearwater, Tristan da Cunha (Great) Shearwater, and North Atlantic (Manx) Shearwater. (b) YES.

YES (Sargasso Shearwater). (a) I prefer Sargasso, but would also be good with Caribbean. I think Sargasso is a more specific description of the species' habitat and natural history. (b) YES.

YES (Sargasso Shearwater). (a) Change the English name to avoid confusion with previous taxonomic treatments. I vote for Sargasso Shearwater. (b) YES.

YES (Sargasso Shearwater). (a) Sargasso is fine with me. (b) YES.

YES (Caribbean Shearwater). (a) I find parts of this proposal annoying, specifically about what birders want and a name that is poetic, evocative, etc. Given the overall issue with English names these days, I'm perfectly happy with "bland," "mundane," or

“vanilla.” Caribbean Shearwater is a perfectly good name and describes nearly all of the breeding range for the species. Another committee member gives examples for other English names that describe the breeding range and I’ll add Hawaiian Petrel (*P. sandwichensis*) and is the perfect counter to the phenotypically and formerly lumped Galapagos Petrel (*P. phaeopygia*). This is only shearwater that *breeds* in the Caribbean region and that is determinative for me.

Sargassum Shearwater has problems as is noted, it is a genus that is found widely in the tropics, not a species, the Caribbean and adjacent Atlantic one being *Sargassum bacciferum.* Sargassum-weed Shearwater seems awkward and not poetic. Sargasso Shearwater is better and is partly accurate, at least for the west part of the “sea,” but only applies to a small part of the non-breeding range. When all is said and done, I believe that Caribbean Shearwater is the best English name to be adopted. Caribbean expresses the breeding range and much of the range for the non-breeders too. Antillean Shearwater is good for the breeding range. I’m not overly troubled that many come up the Atlantic coast to off Maryland, more uncommonly to casually farther north.

I see the votes are in favor of Sargasso, but most are nearly as content with Caribbean Shearwater, and lack the strong convictions expressed in the motion regarding English names.

(b) I favor the change for the reasons stated.

YES (Sargasso Shearwater). (a) YES to Sargasso Shearwater, although I think that Caribbean Shearwater is a close second. The former describes their habitat association, while the latter their primary breeding grounds. (That said, I’ve mostly seen them away from noticeable Sargassum concentrations and not in the Caribbean.). (b) YES. Reasons in the proposal.