2023-D-1:

Add Bat Falcon *Falco rufigularis* to the U.S. List

YES. 1 without comment.

YES. I will defer to the experts on the ID of this one because I was personally unable to get close enough to get a diagnostic photo of the Santa Ana bird, as you can see here:

YES. Bat Falcon has been a long expected stray to the Texas. Recent eBird records from Monterrey are only 125 miles from Santa Ana NWR. Nothing in the plumage or behavior indicate captive escape/release, and it appears to be rarely used in falconry (https://www.facebook.com/falconryfund/posts/species-spotlight-the-bat-falconthe-bat-falcon-falco-rufigularis-is-a-brilliantl/2526547174334808/) . Identification is straightforward. Only confusion is the rare Orange-breasted Falcon, an unlikely stray from its range from southern Mexico into Amazonia. Bat Falcon is smaller, with less massive bill and legs, less white fringing on breast feathers, and little to no orange on upper breast and throat."

YES. Identification not in doubt, and an expected vagrant.

YES. Reasons are stated in the proposal.

YES. This seems straightforward.

YES. This addition is not a surprise and it was confidently identified.

YES. Photographic evidence confirms the presence of *Falco rufigularis* in Texas.

YES. Seems pretty clear cut. Location in the US not far from typical range.

YES. Reasons are in the proposal.

2023-D-2:

Add Lilac-crowned Parrot *Amazona finschi* to the U.S. List as an established introduced species

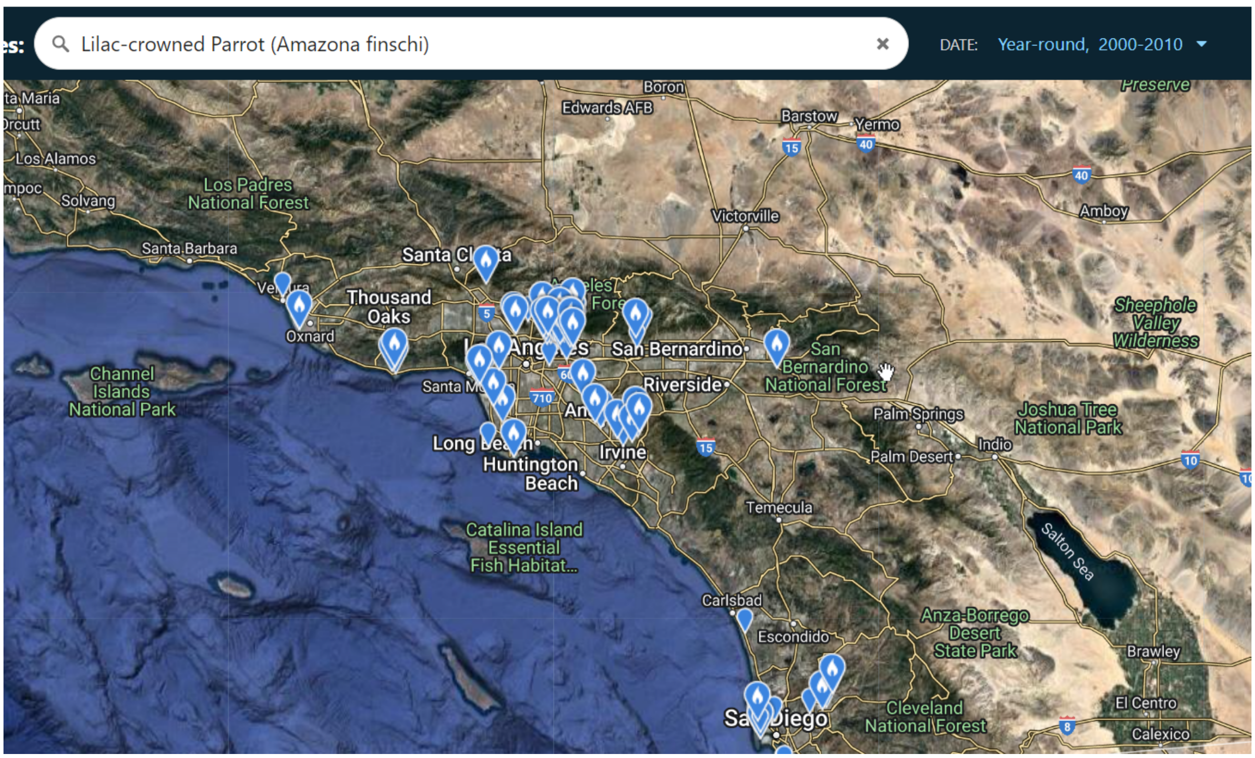
YES. 1 without comment.

YES. Deferring to CBRC and ABA.

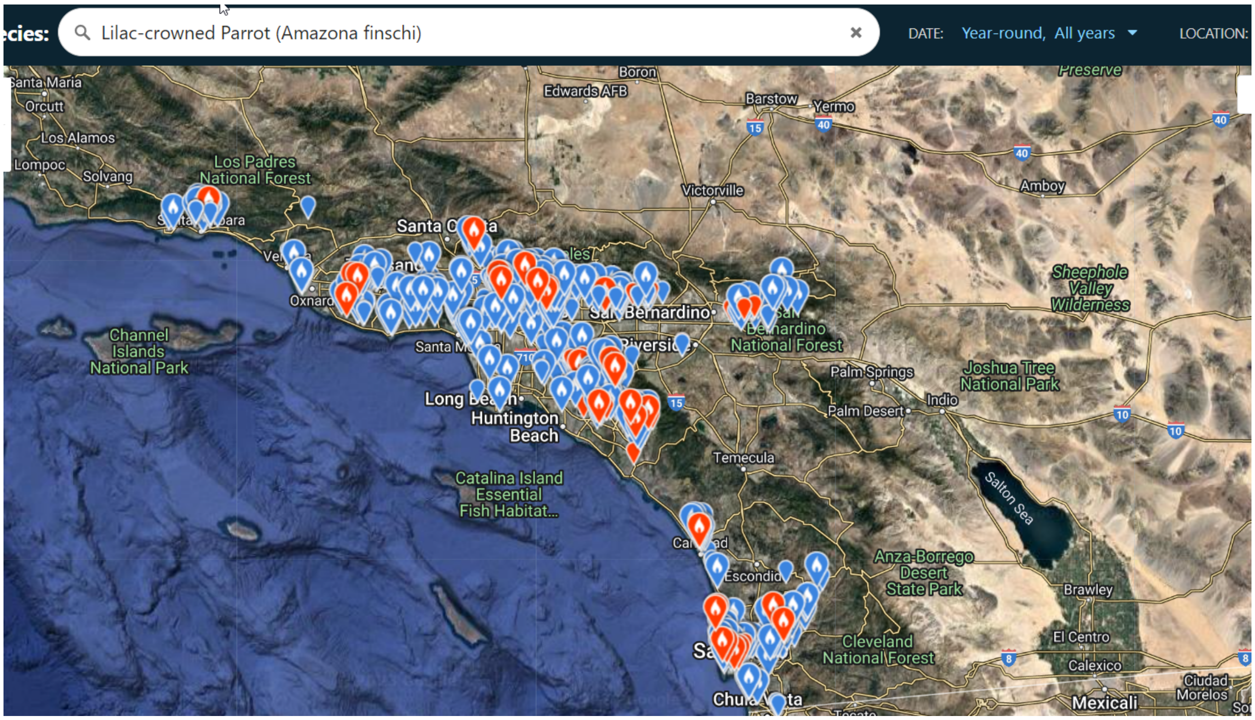
YES. The Southern California population meets all the ABA criteria for establishment (https://www.aba.org/criteria-for-determining-establishment-of-exotics/).

YES. Not sure if we have access to the CA proposal, but if you look at eBird records from 2000-2010 they were already in many of the same places and have clearly spread and multiplied since then.

Here's the eBird map restricted to 2000-2010:



And the current eBird map:



YES. I defer to the California BRC and ABA CLC on this.

YES. Reasons are given in the proposal.

YES. It meets our accepted criteria for addition.

YES. Add *Amazona finschi* to the U.S. list.

YES. Fairly easy to see here in San Diego and likely underreported in San Diego; many Lilac-crowneds are misreported as Red-crowneds on eBird.

YES. I usually find it hard to get overly involved in these types of issues and would rather defer to the state committee and the ABA CLC.

2023-D-3:

(a) Add Red-masked Parakeet *Psittacara erythrogenys* to the Main List as an established introduced species, (b) Change the linear sequence of *Psittacara*

YES. Add to main list, deferring to CBRC and ABA, and change linear sequence.

YES. In addition to the California population, the population in Florida is on the cusp of being accepted. The FOSRC is waiting for a more detailed population survey.

Because the three populations in California are completely allopatric and almost certainly non-interacting, you cannot use sum totals throughout the State to see if criteria are met. I will therefore focus solely on the SF population because it appears to be the largest. In SF, the population seems to have grown from a rather localized population on Telegraph Hill to being widespread throughout the city and also in northern San Mateo County (eBird). A few records elsewhere as well, on Alcatraz (24!), in Marin, Palo Alto, and Sunnyvale. It seems the population has greatly increased, no doubt through nesting and not more releases. The ABA criteria state that populations optimally should be in the 100s to be considered established, and the SF population has reached that minimal threshold with 250-300 recorded on CBCs. The smaller but persistent populations in LA and San Diego are icing on the cake, and I think that the species should be added.

YES. I haven't seen the proposal but this species is clearly well-established in California and Florida, where I've seen several in a well-known site.

YES. I vote for adding it to the main list and to changing our linear sequence to follow Remsen et al. (2013).

YES. Reasons for both adding the species to the main list and revising the linear sequence are given in the proposal.

YES. It seems to meet our criteria, and the new sequence makes sense.

YES. Add *Psittacara erythrogenys* to the main list as an established introduced species, and change the linear sequence as required.

YES. Already added to ABA Checklist. The population in San Francisco has been around for many years and was famously featured in a documentary about 20 years ago. Common bird here in San Diego. I routinely see them in my backyard (as far back as at least 2010), with some flocks as large as 45 birds.

YES. Linear sequence needs to change to match Remsen et al. (2013)

YES. Identification between this species and Masked is not as straight-forward as it seems, and both are now established in California. In introduction cases like this, I usually defer to the state committee and the ABA CLC.

YES. This species seems well-established for decades now. Makes sense to add it to the main list and change sequence accordingly.

2023-D-4:

Add Rufous-tailed Rock-Thrush *Monticola saxatilis* to the Main List.

YES. 2 without comment.

YES. Add to main list, deferring to CBRC and ABA.

YES. Although there are a number of similarly patterned *Monticola* species, none show the white interscapular panel showed by *saxatilis*. The photos show this well. The bill is also quite long, like *saxatilis*. Although this species was not on anyone's list of possible vagrants to show up in Alaska (or elsewhere in North America), it is a very long-distance migrant, and has shown up far out of range (in Japan) a couple of times. Also there seems to be a propensity for these central Asian breeders to show up in Alaska in late spring, likely overshoots of their breeding range. The location absolutely discounts any possibility of captive release/escape or ship assistance.

YES. Obviously no ID issues and negligible chance of unnatural means of arrival there. Sometimes birds go the wrong way and just keep going.

YES. I defer to the Alaska Checklist Committee and ABA CLC on this.

YES. Reasons are given in the proposal.

YES. This bird was confidently identified.

YES. Agrees with ABA Checklist Committee. Well photographed.

YES. Easy ID and no issues on origin. I was surprised that this one turned up, but yes species from Central Asia seem to be turning up more and more. This one gets a little farther east.

2023-D-5:

Add Lesson's Seedeater *Sporophila bouvronides* to the Main List

YES. 1 without comment.

YES. Published, diagnostic photo and reasonable case made for natural vagrancy.

YES. Details in proposal.

YES. And what about the one in 2021 in Quebec - Lesson's Seedeater in Québec - American Birding Association? [Note that this record was not accepted by the Quebec rare birds committee.]

YES. I agree that unless there is compelling evidence to suggest the bird was a trapped bird, that it is safe to accept it on the Check-list. Given that it is a medium-distance migrant that breeds relatively close by in Colombia and Venezuela, this seems like an obvious candidate for vagrancy to Costa Rica (plus the records from Panama).

YES. Reasons are given in the proposal.

YES. With the numerous records from Panama, this documented record indicates addition is warranted.

YES. Add *Sporophila bouvronoides* to the main list, multiple records and photographic evidence support the addition.

YES. I'm a little wary of this one since *Sporophila* are often kept in captivity. However, keeping them as cage birds isn't well known in Costa Rica (according to the proposal), and there are additional records of this bird in Panama. Regarding the sequence: We didn't have *bouvronides* in our 2014 tanager tree, but we have it in our unpublished tanager UCE tree and it is sister to *lineola*, so that agrees with its placement next to *lineola*.

YES. I am uncertain as to what the Quebec committee has done with their record but it should be mentioned [it was rejected]. The fact there are multiple Panama records shows a pattern of dispersal. Recall that the north shore of the St. Lawrence River in Quebec recorded the first Amethyst-throated Hummingbird, so records very far out-of-range happen. Perhaps for a Lesson's Seedeater too. We had an immature South American seedeater here in Inyo County along the Owens River which was photographed, but remains unidentified. Of course, there are origin issues involved, but it makes one wonder. These tanagers perhaps can travel far.

2023-D-6:

Change the linear sequence of genera in the Grallariidae

YES. Our protocols for sequencing require this change. The difference between *Hylopezus* and *Mrymothera* is so thin, so their placement could go either way (status quo is *perspicillatus* then *dives*).

YES. Reasons are given in the proposal.

YES. Justified in the proposal.

YES. The committee protocol requires the sequence change.

YES. Reasons are outlined in the proposal.

YES. This is warranted. Nice to have this aspect of our sequencing protocols refreshed in my mind.

YES. This is a logical change based on the topology of the well supported phylogeny.

YES. Change required based on the phylogeny and our protocol.

YES. Reasons are outlined in the proposal and I appreciate having the internal protocols of priority outlined as well.

YES. Checked.

2023-D-7:

Treat Cordilleran Flycatcher *Empidonax occidentalis* as conspecific with Pacific-slope Flycatcher *E. difficilis*

YES. Outstanding, comprehensive proposal that should leave no doubt that they may behave like separate species in the Warners based on a small N of mated pairs, but in their previously unstudied contact zone, all data point towards one massive hybrid swarm. If Ned Johnson were to see these new data, I predict he would have considered them conspecific.

YES. I'm glad to see that this problematic issue has finally been comprehensively addressed. For its time, Johnson's work was phenomenal but what was then a borderline split is now shown to be unwarranted.

YES. This comprehensive and persuasive proposal truly shows that the reproductive isolation breaks down in many parts of the broad overlap between *occidentalis* and *difficilis*. Johnson's original data set from the Warners offered, for its time, evidence for the split, but the small samples and extent of overlap outside the Warners would give us caution today.

YES. This is an impressive and very well-researched proposal that clearly lays out and documents the history of these taxa and the rationale for why they were originally split, and ultimately why they should be lumped. The extremely broad area of introgression across Washington, Idaho, and Montana where there are no diagnosable parental species points to a very broad “hybrid swarm” where selection does not appear to be acting. This zone of introgression is beyond anything typically seen in a hybrid zone, and definitely fits better with an isolation-by-distance model.

YES. This is an excellent proposal that covers the background in detail and makes a strong case for re-lumping Pacific-slope and Cordilleran flycatchers back to Western Flycatcher. I have some caveats, mostly that there are still unpublished data from Andrew Rush’s 2014 dissertation at UC Berkeley which focused on vocal divergence (including playback experiments) and genetic variation in this complex; Chapter 2 is most relevant to this proposal. There are some interesting findings, especially of behavioral asymmetries. Yet, I think that the available data do not support retaining them as distinct species.

YES. The evidence here is overwhelmingly supportive of recognizing these taxa as being the same species. Subspecies *are* within the scope of NACC, and I appreciate the authors’ inclusion of this level of analysis.

YES. I enjoyed reading this proposal and all the history of research on the *Empidonax difficilis*/*occidentalis* complex. The impressive accumulation of information about these birds highlights the importance of including samples that represent the entire range of the taxa of interest. As presented in the proposal and in chapter 2 of Andrew Rush's dissertation, the exclusion of the primary contact zone exaggerates the divergence between the two forms, *occidentalis* and *difficilis*. Evidence suggests a model of isolation-by-distance and supports treating *E. occidentalis* as conspecific with *E. difficilis*.

YES. I find the case for lumping to be convincing, particularly given the breadth of the range of birds giving mixed vocalizations. Maintaining these two as separate species, but not the others is no longer valid and lumping them and returning to Western Flycatcher seems by far the best path forward. It will certainly help the birding community that have largely given up on telling these two apart over large parts of their western range. Just one thing to add, I remember from decades ago spending time in western Mexico and hearing only Cordilleran type male position notes in the Sierra Madre Occidental, but only Pacific-slope male position notes at lower elevations. The single ‘*tseet*” contact notes have always sounded very similar to my ear, perhaps slightly stronger (“sharper”) in Cordilleran. The California state list will drop by one.

YES. What a well-written proposal—it was a joy to read. I would encourage the authors to publish this summary of the complex as a note in a peer-reviewed journal somewhere. With the new insights from the work on vocalizations and population genomics of the complex, *E. difficilis* and *occidentalis* are best treated as a single species under the BSC. The pop gen patterns are quite fascinating: the fact that the Sierra Madre del Sur population is more divergent from its nearest neighbor than comparisons between the formally presumed contact zone between *difficilis* and *occidentalis* is quite telling for me. The vocalizations seemed an important line of evidence for pre-mating barriers to gene flow when these species were split, but those differences do not hold up to increased geographic sampling. I also agree with the authors and think we should adopt their subspecies classification—kudos for including that.

2023-D-8:

Establish English names for various newly split species

YES. I agree to all for reasons stated for each. I don't see any other reasonable choices other than the ones listed for each. Leaftosser – count me in for retaining Tawny-throated – nice solution.

YES. The leaftosser names are not great but nothing better has suggested itself.

YES. I agree to all but D-8-d. I strongly disagree with Middle American and South American for the *Sclerurus* split. Both South America and Middle America have other *Sclerurus* species, so those names are inaccurate as well as being boring. I'd rather go with almost anything: even Plain Leaftosser and Drab Leaftosser are better (I will let others choose which English name belongs with which taxon), especially if they are temporary. And Middle American would not be temporary with future splits, as those would only involve taxa outside our area. We will get hammered if we go with these names, even if it is temporary.

YES. I agree to all except the original name for *mexicanus* in D-8-d (Middle American Leaftosser). Although I disagree that future splits in this complex would only involve taxa from outside our area (*pullus* is a candidate to be split from *mexicanus s.s.*), I do agree that Middle American Leaftosser (also South American Leaftosser, although this is not our problem) is a poor name. Because nuclear data indicate that the South American forms of *S. mexicanus* are sister to *S. rufigularis* rather than to the CA forms of *S. mexicanus* (see the excerpt from Harvey et al. 2020 below), it occurs to me that we could use the "not sister taxa and should never have been considered conspecific" exception to retain the name Tawny-throated for *S. mexicanus*. The IOC is currently using Tawny-throated for *S. mexicanus s.s.* (but see Pam's vote above), although I don't know if this was the reasoning or not. I have modified the proposal to recommend adoption of Tawny-throated Leaftosser for *S. mexicanus* and South American Leaftosser, the SACC name, for *S. obscurior*.

YES. (a) Adopt Hispaniolan Nightjar for *ekmani* and Cuban Nightjar for *cubanensis*;(b) Adopt American Goshawk for *atricapillus* and Eurasian Goshawk for *gentilis*; (c) Adopt Velvety Manakin for *velutina* and Blue-crowned Manakin for *coronata*; (d) I am fine with adopting South American Leaftosser for *obscuriour* and Middle American Leaftosser for *mexicanus*; (e) Adopt Cuban Palm-Crow for *minutus* and Hispaniolan Palm-Crow for *palmarum*; (f) Adopt Puerto Rican Euphonia for *sclateri*, Lesser Antillean Euphonia for *flavifrons*, and Hispaniolan Euphonia for *musica*; (g) Adopt Ecuadorian Seedeater for *aequatorialis* and retain Blue Seedeater for *concolor.*

YES. I agree to all except (d) which it seems requires more discussion given the comments above. The reasoning to use 'Tawny-throated' makes sense to me.

YES. I agree with all of the recommended English names in the proposal.

YES. I agree to all for reasons stated in the proposals, except d, based on comments above regarding the name.

YES. I agree to all except for the Leaftosser, where I agree that retaining Tawny-throated is fine.

YES. I agree to all except (d), in agreement with comments above.

ABSTAIN. I find nothing objectionable here.

2023-D-9:

Change hummingbird subfamily name from Topazinae to Florisuginae

YES. 1 without comment.

YES. Essentially a mandatory correction under the Code.

YES. No option here.

YES. Mandatory adherence to code.

YES. Reasons are stated in the proposal.

YES. This change is required under the Code.

YES. Follow the Code and change Topazinae to Florisuginae, which has priority.

YES. Mandatory correction needed.

YES. This seems to be a straightforward correction.

YES. The code.

2023-D-10:

Treat *Gygis microrhyncha* as a separate species from White Tern *G. alba*

YES. What a roller-coaster history. That the two were once widely sympatric is sufficient evidence for treating them as two species, and the vocal data, as limited as they are, are consistent with species rank for *microrhyncha*. I am also unaware of such strong bill shape or tail shape variation between any tern populations considered as a single species. The situation between *microrhyncha* and *candida* sounds similar to those moving hybrid zones with one species replacing the other, as in Golden-winged vs. Blue-winged warblers, and American Black Duck and Mallard.

That Yeung et al. (2009) decided not to consider any of the taxa valid because of shared mtDNA haplotypes shows the perils in using mtDNA as a metric for taxonomy, as if mtDNA were a proxy for the entire genome. Even more recently, Thibault and Cibois (2017) seem to have made the same mistake, as if the morphological differences they documented had no genetic basis and that a couple of mtDNA genes are the only genes in the genome. I look forward to a time when statements like “do not differ genetically” require the phrase be qualified by “with respect to the # genes analyzed”.

Also, I think we should also consider Pratt’s (2020) recommendation to treat *candida* as a third species ASAP. This may not have been adopted by any recent classification, but Pratt made a good case. Also, I strongly recommend a separate proposal on English names that considers Pratt’s suggestion for a group name “Fairytern” (or “Fairy-tern”), because genetic data are trending towards *Gygis* not being true terns. This is from Pratt’s abstract:

> “It also reviews historical English vernacular names and proposes fairytern as a group name for these members of the newly recognised subfamily Gyginae. This name maintains popular tradition but requires a minor exception to some current naming conventions. Proposed English names are Atlantic Fairytern, Common Fairytern, and Little Fairytern. The name White Tern should now apply only to the historical single species, and Fairy Tern remains for *Sternula nereis*.”

I wince at using “Fairytern” when a true tern, *Sternula nereis*, is called Fairy Tern, but I think we should give Pratt’s ideas a fair hearing. Also, by our conventions, the names suggested in the proposal should be hyphenated, i.e. “Common White-Tern” and “Little White-Tern”. Also, a point to consider is making the “Tern” lower case to signal that it these are not Sterninae, as we do for “Silky-flycatcher”. All in all, a separate proposal on English names is needed.

YES. A conflicted yes. I read Pratt’s paper after reading opposing comments. For me it comes down to the very different bill shapes and what that means ecologically. I agree that the mtDNA data do not shed much light on the situation.

NO. A reluctant no based on present data. I think it’s likely that more than one species is involved but I don’t think this paper conclusively shows that, even discounting the apparently massive hybridization documented that now appears to be endangering *microrhyncha*. I don’t doubt that subfossil remains suggest sympatry in the past but how definitive is this? What are the sample sizes on which this is based? Olson (2005; https://repository.si.edu/bitstream/handle/10088/1567/Gygis_microrhyncha_BBOC.pdf?sequence=1&isAllowed=y) refers to Olson ms. These taxa nest in single pairs with one chick, so there can hardly be many remains at any one location (I admit I haven’t chased down all the relevant papers though and could still be convinced of the past sympatry.) A lot of the rationale given by Pratt seems somewhat anecdotal. I agree that bill shape, basal feathering, and basal coloration are radically different in SOME birds anyway, and that differences exist in outer primary shaft color and tail shape, but it seems that some birds in ML photographs are in molt and thus show a confusing array of tail shapes in places where they wouldn’t likely be hybrids. The genetic data available, even if only mtDNA, don’t offer much hope that *microrhyncha* and *candida* will prove to be well-diverged, but we won’t know without further work. I also couldn’t convince myself of real differences in vocalizations even between *alba* and *candida*, other than a seemingly lower pitch for the grating calls of *alba*, but due to the “white noise” nature of these calls even this is tough to measure and compare. I hope that someone will carry out a thorough integrative analysis of these that would allow us to have more confidence about these potential splits. Of course with all-white birds (as with all-black birds) comparisons are especially problematic and different species can easily get swept under the rug (think of American and Old World Great Egrets, which sound completely different, and yet no one has produced what should be a relatively straightforward analysis).

NO. This was a very challenging case, and definitely tests the limits of our definitions of species. It definitely seems like these two taxa acted as good biological species historically, but the current pattern of rapid expansion of *candida* whereby it hybridizes extensively and replaces *microrhyncha* across most of its known range to me suggests a total breakdown of reproductive isolation. The differences in bill shape and tail shape are definitely very pronounced and unusual, but these characters, combined with apparent differences in vocalizations, do not seem to be contributing to isolation between these taxa. I do agree with others that a case could also be made for splitting nominate *alba* of the Atlantic from the Pacific and Indian Ocean taxa, as the vocal evidence is stronger.

NO. I'm inclined to agree with the proposal and other comments that these taxa do not appear reproductively isolated given the hybridization and replacement of *microrhyncha* by *candida.* Another committee member also makes a good point about the fossil record and putative past sympatry, which is not clear to me (the statement in Olson 2005 is pretty broad without any specifics). There are some clear phenotypic differences, but I think we need more data before making this split.

NO. I did go back and forth on this one and read other comments as well as Pratt's paper. Although there are morphological differences indicating past differentiation and probably species status, currently there is widespread gene flow as one taxa swamps out the other. Thus, they need to be considered one species given our use of the Biological Species Concept.

NO. Interesting case, but current levels of gene flow preclude consideration of them as separate biological species.

NO. This is a borderline case. There is a history of sympatry without interbreeding, although the fossil evidence is not clearly presented. Both *candida* and *microrhyncha* can be recognized visually by the shape and color of the bill, the shape of the tail, and the size of the body. Surely the hybridization could be mixing up the mtDNA and therefore the results reported by Yeung et al. (2009). However, to assess the possibility that *candida* and *microrhyncha* are separate species, it is necessary to know the precise geographic location and extent of the (genomic) hybrid zone.

NO. With so much hybridization, I am reluctant to recommend a split. Other terns, notably Black Noddy and Gull-billed show significant geographical variation. I would add at least the alternate plumage of *longipennis* Common Tern too.

NO. Interesting case! There could be multiple species in here given the distinct phenotypic differences. One possibility is mitochondrial capture via ancient hybridization, or complete mitochondrial introgression of one form into the other. Would be really interesting to look at some genomic / autosomal data to characterize the patterns, but I'm not comfortable splitting it given the data on hand. Also, Pratt (2020) does suggest some vocal differences, but it seems that those sample sizes are a little bit small and I'd like to see more formal analysis of vocalizations from throughout the range of the putative species before that overrides the extreme similarity in mtDNA that we have on hand. I'm a little confused about the patterns in the morphological data and whether or not there are phenotypic intermediates—my reading of the text on page 46 of the proposal set suggests that there are phenotypic intermediates between *candida* and *microrhyncha*, further suggesting that these are a single BSC species.

2023-D-11:

Establish English names for the newly split *Chlorothraupis carmioli* and *C. frenata*

YES. 2 without comment.

YES. I agree to both suggestions.

YES. Keep Carmiol's Tanager for the nominate, as the name is so long-established and no other name is readily available (to my knowledge) and for which the eponym reflects the scientific name. Also keep Yellow-lored Tanager, as the yellow lores seems to be the only field mark it gives us. I don't know that changing to Yellowish-lored is worth it, since they are usually quite yellow, at least in the sample of ML images I looked at.

YES. I do not think we have ever formally stated that the nominate subspecies of the taxa has more ownership to the English name using that patronym than do other subspecies following a split.

YES. Reasons are given in the proposal.

YES to continue using Carmiol's Tanager for *Chlorothraupis carmioli*, its use is covered by the NACC guidelines for English names, reflects the scientific name, and promotes stability. And YES to use Yellow-lored Tanager for *C. frenata*.

YES. Reasons are stated in the proposal.

NO to (a), YES to (b). (a) While the NACC guidelines on English names do specifically mention true phylogenetic parent/daughter species in avoiding the retention of an English name for one of the resulting species, the guidelines also say for other splits that the old name "may be" retained, but it is not necessarily standard. Given the fact that *frenata* actually seems to have the (slightly) larger range of the two resulting taxa, I still think it would be appropriate to establish new names for both resulting species. While there are not necessarily any good options available for *carmioli*, I would prefer Yellowish Tanager from the original proposal as a possible option, as I think it contrasts well with Yellow-lored and Lemon-spectacled Tanagers. (b) I think Yellow-lored Tanager is appropriate for *frenata*.

ABSTAIN.