2026-C-1: Treat Two-barred Crossbill *Loxia bifasciata* as a separate species from White-winged Crossbill *L. leucoptera*
2026-C-2: Add two species to the Checklist, one species to the U.S. list, and five species to Appendix 1
2026-C-3: Adopt English names for genus *Mitrephanes* and species *M. phaeocercus s.s.* and *M. aurantiiventris*
2026-C-04: Change the linear sequence of the Certhioidea (Sittidae, Certhiidae, Polioptilidae, and Troglodytidae)
2026-C-05: Add Audouin's Gull *Ichthyaetus audouinii* to the Main List
2026-C-06: Add Little Swift *Apus affinis* to the Main List
2026-C-07: Add Squacco Heron *Ardeola ralloides* to the Main List
2026-C-08: Add White-winged Swallow *Tachycineta albiventer* to the Main List
2006-C-09: Treat Little Swift *Apus affinis* and House Swift *A. nipalensis* as a single species

2026-C-1
Treat Two-barred Crossbill *Loxia bifasciata* as a separate species from White-winged Crossbill *L. leucoptera*

YES. I am a bit on the fence about this proposal, but a combination of evidence leans me toward voting in favor of a split. The mtDNA data are not well-supported, but there are genomic data - although not published separately - that are at least presented in a published book that shows that *L. l. bifasciata* is not closely related to *L. l. leucoptera*. As noted in comments by the Palearctic RAG, the recovered sister relationship makes sense biogeographically. It would be great to see those data published. There are also morphological and vocal differences, although unfortunately the described differences are more qualitative than quantitative. The Palearctic RAG voted in favor of this split, and I do not see a compelling reason to differ.

YES. I too am a little on the fence about this proposal, but agree that the combination of vocal, morphological, genetic, and ecological data support recognizing both taxa as separate species. In some ways, I feel much better about recognizing these two as separate species than other crossbill taxa. I am particularly intrigued by the genomic data presented in Benkman (2025). As noted in the comments of the Palearctic RAG and other committee members here, it is not surprising that the mtDNA phylogeny shows *leucoptera s.s.* as sister to *megaplaga*, and I would not base my decision to split based solely on the paraphyly there, as insular taxa often can be embedded within other species. However, the genomic data tells a very different story (although it would be nice to see that published in a peer-reviewed article).

YES. The combination of genomic, vocal, ecological, morphometric, and plumage differences all convince me of species rank for these two taxa. Although none of these characters are extremely different, they all point to species-level differences among Fringillidae species. I am hesitant to use yardstick approaches here given the mess that is the Red Crossbills, but in comparison to the differences shown by (for example) Hispaniolan *megaplaga*, these differences seem to me to be comparable. Certainly, if the genomic data hold up, then the polyphyly of our current treatment would almost necessitate the split of *bifasciata*. I do hope that those data get published. I am surprised by how few recordings there are of *bifasciata*, but it makes some sense given how uncommon the taxon is, especially in comparison to how common *leucoptera* can be in parts of North America. I do hear the vocal differences described by Young and Spahr, and they are comparable (to my ear) to the vocal differences shown by *megaplaga*.

The English common names are widely used in the two regions for these two taxa, so it makes sense to me to adopt them here (White-winged for *leucoptera* and Two-barred for *bifasciata*).

YES. I'm particularly struck by song differences, which seem major to me. I'd need to spend more time on calls to be sure but at least the flight calls seem different; I think there is overlap in the trumpet calls though. The multiple morphological differences, taken together, are more typical of full species than of subspecies, too. The 2007 genetic results also suggest species status, but more modern techniques have not yet been applied. To me, the song differences are strong evidence, supported by the other lines of evidence.

YES. I vote strongly in favor of this proposal - something I have been arguing within NACC for more than a decade. I find it very difficult to believe that the case for splitting *Loxia sinesciurus* is better than splitting off *Loxia leucoptera bifasciata* from nominate *L. leucoptera leucoptera.* The morphological differences alone are pretty distinct, especially the coloration of males and the bill structure. The upper white wingbar is also differently shaped between the two taxa. Long ago I recall hearing a recording of wintering *bifasciata* in the Netherlands, a rarity there but many appeared in an invasion. I recall they were contact notes, not songs, but regardless I would not have been able to recognize them as nominate *leucoptera.* Given the lack of variation within the large ranges of both taxa, the split seems straight-forward to me as there are no populations showing intermediate characters, say in Alaska or northeast Asia. As noted, nominate *leucoptera* has made it to Iceland and Bermuda (termed rare by Amos 1981; several 1969, 1974) and to the Bering Sea at Gambell on St. Lawrence Island (casual, four records) by Lehman (2019). Lehman (2019) says that *bifasciata* is found north in northeast Asia to the northern Sea of Okhotsk, but he notes that Tomkovich (2008) indicates it has wandered further north to the western Anadyr River basin. I find it interesting the mention of nominate *leucoptera* being tied largely to black spruce stands. I haven't looked carefully at habitat preferences for *bifasciata* in northern Eurasia but the Princeton Birds of Europe, 3rd ed. (Svensson 2023) states that it prefers larch cones but will often take rowanberries in winter. Svensson (2023) states that it breeds regularly only in Russia but in some years will nest in Finland and Sweden.

One further thought. If *L. sinesciurus* is really resident in the Front Range of the Rockies, that sort of alters the overall dynamic that has been presented. Two isolated populations of this type. Is this supposed to be a recent arrival and, if there, how many other isolated populations of Cassia Crossbills occur away from south-central Idaho?

Literature cited:

Amos, J.R. 1991. A guide to the Birds of Bermuda. Published by the author.

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

Svensson, L. Birds of Europe, 3rd ed. Princeton University Press.

Tomkovich, P.S. 2008. [Birds of the upper Anadyr River (Chukotka Autonomous Area)] *Archives Zool. Mus. Moscow State Univ. 49:1010158.* [In Russian, with English abstract and title.]

NO. I could be persuaded otherwise, but at this point I don't think there is enough evidence in favor of treating these two taxa as separate species rather than continuing to treat them as one. To me, the morphological differences aren't that compelling and vocalization data are limited. The mtDNA does show a closer relationship between *megaplaga* and *leucoptera*, but the support values aren't great. The unpublished genomic tree (Benkman 2025) also shows this relationship. If those data were published, I would be more comfortable with a split. However,

what are we to make of the genomic tree showing *bifasciata* closer to the Red Crossbills than to *leucoptera*/*megaplaga*. This relationship conflicts with the mtDNA tree. I guess I would want to see the genomic data published (or at least more details about that study) before committing to splitting the two taxa. More analyses of vocalizations would be helpful as well. If the proposal passes, those English names are good.

NO. This seems rushed given that there are relevant studies and associated data forthcoming—why not wait for those data to undergo peer review before drawing conclusions on them? I worry this group may be 'oversplit' if our goal is to recognize species as independent evolutionary lineages. They have recent ancestry, and while there are some body size and bill morphology differences that may be associated with foraging ecology, these can evolve rapidly with few loci of large effect (i.e., *Acanthis*). I would prefer to wait and assess the forthcoming whole-genome data given how complex the eco-evolutionary dynamics of this group are. If the proposal passes, I am fine with the suggested English names.

NO. I prefer to wait until the data being generated are published. I think this will strengthen this proposal. It is already clear that they are different both morphologically and vocally; it is preferable to wait for the molecular data to have a better-supported proposal.

2026-C-2

Add two species to the Checklist, one species to the U.S. list, and five species to Appendix 1

YES. I vote in favor of all of the proposed additions.

YES. I vote for all the recommendations. I do think that the *Apus pallidus* is likely correct, especially given the strong support by the various experts that were consulted on that record, but given that the ABA did not vote to accept that record, I think we should follow suit.

YES. I vote for all recommendations in the proposal.

YES. I vote for all of these additions, already vetted by the ABA.

YES. However, I wonder about the crake, if it might even have been a Sora? Do we really add species to the Hypothetical list that are so poorly documented that they could even be a common NACC-area bird?

YES. These additions appear well-vetted.

YES. I agree with the proposal.

YES. I agree with all additions, but the reasoning on the Pallid Swift by the ABA CLC is suspect. I doubt if any subspecies of *Apus apus* has fledged young that are migrating by early June. This includes eastern *pekinensis.* The breeding population in Beijing (*pekinensis*) is one of the best studied populations of any swift species. Peter Pyle needs to be queried on this. I consider the record to be acceptable, but agree to deferring to ABA. In short the issue should be revisited.

The Baillon's Crake was seen only in flight and was not photographed, so the sighting is not acceptable, even by ABA standards, at least formerly which required photos, although they (ABA CLC) seems to have forgotten this. The record is accepted by eBird so I see some use including this record in the Appendix. Species in the Appendix are there for a variety of reasons and I don't think there should be any assumption that they were correctly identified.

2026-C-3
Adopt English names for genus *Mitrephanes* and species *M. phaeocercus s.s.* and *M. aurantiiventris*

YES. I agree to adopting Tufted-Flycatcher for *Mitrephanes*, including for *olivaceus*. This provides a great and coherent name that differentiates a distinctive genus from all the other flycatchers, and also frees up the use of various names such as "Yellow-bellied" for use with these species, which would otherwise conflict with existing names in other flycatchers.

For the species names, I am strongly in favor of plumage-based names, in part so that they align with the widely used name for *olivaceus* and also because we have species here that are actually quite distinctive in their plumages (unusual for a flycatcher). In the interest of selecting an accurate color-based name, I checked Ridgway (1907) to see what he said of the head and underpart color for *phaeocercus* and *tenuirostris*. He gives the following descriptors for *phaeocercus*: "sides of head and under parts tawny-ochraceous or russet", and for *tenuirostris*: "Pileum often strongly tinged with cinnamomeous. Chest between tawny ochraceous and clay color, fading into slightly yellowish buff on abdomen." So, on that basis my first preference is Tawny Tufted-Flycatcher for *M. phaeocercus*, followed by Cinnamon. If there is opposition to plumage-based names, I like Pinewoods, but the other habitat names are ok too. I am still strongly opposed to Mexican, as it is inaccurate and does not align with the other names in the group, even if it has a little bit of prior usage. For *aurantiiventris*, I'm ok with either Yellow-bellied or Yellowish, with slight preference for the former to align at least somewhat with the scientific name. After reading the additional suggested names, I am more in favor of Lemon-bellied, as it is closer to the true belly color of many of the populations. It would be a little odd to have the Latin name *aurantii* (orange) on a bird with the English common name Lemon (at least they're both citrus?). Although Pale-bellied is perhaps somewhat more accurate, it is boring in my opinion, especially for such a cool bird. I'll edit my order of preference to Lemon-bellied Tufted-Flycatcher, followed by Yellow-bellied, Yellowish, then Pale-bellied.

YES. I agree with adopting Tufted-Flycatcher for *Mitrephanes*, for reasons discussed in the proposal. I'm a bit torn on the names of the species. I can't seem to settle on a symmetrical set of names for the species, which I think is totally fine as many closely related species don't have these. In this case in particular, especially given that *aurantiiventris* seems likely to be split further in the future, two separate plumage-based names for those two species would be challenging to come up with; I think both Lemon-bellied Tufted-Flycatcher and Yellow-bellied Tufted-Flycathcer are great names for *aurantiiventris* that also includes *berlepschi*, especially given that habitat and geographic names are especially tough for this group of taxa. I have a slight preference for Lemon-bellied Tufted-Flycatcher in this case. I still like Pinewoods Tufted-Flycatcher for *phaeocercus*. While I rank the order of preference for the other names as Mexican, Tawny, and Cinnamon, I do not feel very strongly about that order and would be fine with plumage based names as well.

YES. I am strongly in favor of Tufted-Flycatcher for *Mitrephanes*, as outlined in the proposal. I'm

uneasy about differing from SACC on a species which has not occurred and will very likely not occur in the AOS area, but surely they will come around with a common group for the genus which presently constitutes three species.

For the species, I do not like habitat names as some northern populations move around into habitat for half the year (winter) that does not include conifers. Unless there are compelling and easily defined habitat names for the other species, let's stick to color names.The overall color of the northern populations, now *M. phaeocercus*, look to me more cinnamon than my understanding of the color "tawny," particularly nominate *phaeocercus.* I looked at photos of Cinnamon Flycatcher *Pyrrhomyias cinnamomeus* and I see a close match.There aren't many species with "cinnamon" in the English name. Let's celebrate the few that are truly cinnamon. "Tawny" to me has always seemed somewhat vague, not as bad as "buff" but down that same road. Let's see we have Tawny-collared Nightjar and Tawny Pipit and none of those are colored like this northern group of Tufted Flycatchers. I prefer cinnamon, so Cinnamon Tufted-Flycatcher for *M. phaeocercus.*

For *M. aurantiiventris* Yellow-bellied Tufted Flycatcher is OK, but…. I talked to Kimball L. Garrett this morning about new English names for this complex and he suggested Pale-bellied Tufted Flycatcher, stating that it has one fewer syllables for what was already a somewhat clunky name. I think there is an additional reason. The birds on Cerro Tacarcuna in northeast Panama, as noted in the split motion, are an undescribed population. Looking at those photos, the belly does not look at all yellow but rather more dingy white or grayish. Looking at the topo map, Cerro Tacarcuna is some 105 km from Cerro Pirre and the two ranges are separated by some 50 km of lowland swamp forest. They could easily be a separate undescribed subspecies, one without a yellow belly. Cerro Pirre birds do have a yellow belly and seem more closely aligned with *berlepshi.* The entire issue of the Cerro Tacarcuna birds is detailed in Appendix B of the motion to split this species into two or three species.
So, for slight brevity and an acknowledgment of the Cerro Tacarcuna population, I vote for Pale-bellied Tufted-Flycatcher for the birds from Costa Rica to northwest Ecuador.

YES. I vote for Tufted-Flycatcher as the group name for *Mitrephanes.* I prefer to stick with plumage-based names and rank Cinnamon or Tawny above the other suggested names for *M. phaeocercus*. I like the alternative suggestion of Pale-bellied for *M. aurantiiventris* (especially given the comment that not all populations may have a yellow belly) but can also go with Yellow-bellied. The alternate Lemon-bellied is acceptable too.

YES. I vote for Tufted-Flycatcher to improve alignment. I prefer to use plumage-based names. For *M. phaeocercus s.s.*, Tawny seems most appropriate but Cinnamon is ok as well. Pinewoods is better than Mexican which I rank last. I'm not a fan of Mexican for the name since the species occurs outside of Mexico as well. For *M. aurantiiventris*, I like the suggested name Pale-bellied because some populations don't have a yellow belly. For the other two names, I rank Lemon-bellied over Yellow-bellied to avoid confusion with Yellow-bellied Flycatcher. I rank Yellowish last.

YES. I vote for Tufted-Flycatcher as the group name for *Mitrephaes*. Nice that it is already in use and the rationale provided in the proposal is excellent. Given that this group name emphasizes plumage, perhaps the English names should too, in which case my preference would be Tawny Tufted-Flycatcher for *M. phaeocercus* and I like the suggested Lemon-bellied Tufted-Flycatcher for *M. aurantiiventris*.

YES. I vote for Tufted-Flycatcher as the group name for *Mitrephanes*. I prefer to use

plumage-based names: English name Tawny Tuffed-Flycather for *M. phaeocercus* and Lemon-bellied Tufted-Flycatcher for *M. aurantiiventris*.

2026-C-4
Change the linear sequence of the Certhioidea (Sittidae, Certhiidae, Polioptilidae, and Troglodytidae)

YES. Reasons are given in the proposal.

YES. Reasons are discussed in the proposal.

YES. Reasons are outlined in the proposal.

YES. Reasons are given in the proposal.

YES. Reasons are stated in the proposal.

YES. Reasons are provided in the proposal.

YES. Reasons are given in the proposal.

2026-C-5
Add Audouin's Gull *Ichthyaetus audouinii* to the Main List

YES. Reasons are given in the proposal. A straightforward identification.

YES. Reasons are discussed in the proposal. The photos are diagnostic for this species.

YES. Reasons are outlined in the proposal. I need to get a last date of its occurrence.

YES. Reasons are given in the proposal.

YES. Reasons are stated in the proposal.

YES. Reasons are provided in the proposal.

YES. Reasons are given in the proposal.

2026-C-6
Add Little Swift *Apus affinis* to the Main List

YES. Given that we now consider *nipalensis* and *affinis* as conspecific, following passage of Proposal 2026-C-9, I am happy to accept both of these records. The only other confusing species is *horus*, which I think is ruled out by the depth of the tail fork.
Initial comments when considering *nipalensis* and *affinis* as separate species: I was initially ready to accept this record, but after reviewing the references that I have on hand and looking

into the identification more, I really think that this one would greatly benefit from outside input or a peer-reviewed publication to support the identification. I am not sufficiently experienced with these species (aside from a few brief observations) to comment on the identification with any authority, but from what I can tell in my review, *affinis* tends to have a shorter squared tail that can be shallowly forked or forked with rounded tips in some populations (maybe more so in African birds), a broader rump patch, and is overall slightly paler than *nipalensis* at least in some populations. In looking at photos, *nipalensis* gives the impression of an elongate bird with slim wings and long body, while *affinis* looks short and compact. The two Barbados records are three weeks apart on opposite sides of the island, so it's likely that they are different individuals. The 3 June bird looks to me to be long-bodied and slim with a forked tail (not rounded at the tips) and a narrow rump band, so perhaps a bit closer to some *nipalensis*? One of the observers on the eBird checklist actually has it entered as *nipalensis*, although that could be an error. The 24-25 June record does look more compact and has the shallow tail fork with rounded tips to the fork and looks more compact, but it does seem to have a somewhat narrow rump band, although I can find photos that match this from Africa. So, on the basis of the overall shape and shallow tail fork, I could be convinced that this latter record is *affinis*, but I also think it would be worth waiting until next year and soliciting opinions on the identification from a variety of people more experienced with these species.

YES. I vote in favor with the caveat that I have no experience with this group and am relying on the recommendations in the proposal. I echo other comments, hoping to hear more from others who have more experience with these species.

YES. I share the caveats and feel unqualified here. Perhaps Pam can address House Swift *Apus nipalensis* from Southeast Asia, Sundas and the Philippines. I see them annually at Mae Tang, northern Thailand and they are close. They seem more sharply black and white to me with no paleish on the forehead. A House Swift in the Lesser Antilles seems like a bit of a stretch, but swifts travel far. If we acknowledge that it had a slight fork, Horus Swift *Apus horus* from sub-Saharan Africa, local and mainly East and southern Africa, could be considered. It seems to have more of a tail fork than the Barbados bird. Outside review would be good, but whom - someone with knowledge of rather small African *Apus* swifts with not obviously forked tails? If someone knows of someone, we could pend and send it out for review. At present I do not see a compelling reason to question the original identification. With the lump of House and (*A. nipalensis*) and Little Swifts ((*A. affinis*) the decision to accept should be straight-forward.

YES. I originally voted no based on the tentative recommendation in this proposal to accept the record as well as caveats from other committee members concerning further review of the record. However, now that proposal 2026-C-09 to lump *A. affinis* and *A. nipalensis* has passed, questions of identification between the two taxa are no longer relevant.

YES. Originally, I was reluctant to vote yes, given the difficulty of determining which of the two "species" this observation belonged to. Now that we are considering *A. affinis and A. nipalensis* as a single species, I change my vote to yes.

YES. I agree with the proposal.

NO. The photos are compelling and it very well could be *Apus affinis,* but there does seem to be uncertainty both on the side of the proposals and reviewers.

2026-C-7
Add Squacco Heron *Ardeola ralloides* to the Main List

YES. It would have been nice to see a breakdown on the identification of this notoriously tough-to-identify genus, but from what I can see, the paler back, fine neck streaking, and lack of a dark loral stripe all point to *ralloides* over confusion species such as *grayii*.

YES. I have zero experience with this genus, but in some reading and browsing photos in ML, the two November records are definitely more consistent with *ralloides* versus *grayii* or *bacchus*; the latter two species often show some darker shade to the tips of the primaries, which the Guadeloupe bird does not show. Further, the head streaking on both November birds seems consistent with *ralloides* in its very fine nature, versus the thicker and darker streaking usually seen for the other two species.

YES. I am clueless about identifying non-breeding plumages in this notoriously difficult group when they are in that plumage. Philip Round (Thailand) has never mentioned any characters between *A. bacchus* and *A. speciosa* in separating non-breeding plumaged birds. On the other hand, in alternate plumage they seem to be straightforward and the alternate plumaged Martinique bird fits Squacco, I think. Does *A. grayii* have a dark loral stripe? Really, all that's needed to add the species to the Main List is to accept one record. I think that criterion is met with the Martinique bird. Depending on the comments from members, we can say that we accept the Martinique record and the other two are likely *A. ralloides* on probability, but identifying non-breeding plumage between most of the *Ardeola* is very difficult.

YES. I don't have any experience with this group but accept the Martinique record based on its clear identification as indicated in the proposal.

YES. Reasons are stated in the proposal.

YES. I don't know the group well, but seems like a solid identification and record.

YES. Reasons are given in the proposal.

2026-C-8
Add White-winged Swallow *Tachycineta albiventer* to the Main List

YES. I'm surprised that we haven't voted on this one yet. There are quite a few records now from the Windward Islands.

YES. Reasons are given in the proposal.

YES. In regard to the comment that we haven't voted on this yet, this falls on me. I have been communicating with Paul Buckley about records from the Lesser Antilles. I think we are now up-to-date, except that Paul has appraised me of records from Barbados of both Hen Harrier *Circus cyaneus* and Pallid Harrier *Circus macrourus*, both of which, I believe, have been endorsed by Dick Forsman, but something more is needed in either the review or the writing up process. Pallid Harrier would be new for the Main List. We have the wing specimen of Hen Harrier from the western Aleutians, AK, which has genetic evidence. It was recently added to the main list. Time to check with Buckley again.

YES. Reasons are given in the proposal.

YES. Plenty of records and this species even appears to be breeding.

YES. Reasons are provided in the proposal.

YES. Reasons are given in the proposal.

2026-C-9
Treat Little Swift *Apus affinis* and House Swift *A. nipalensis* as a single species

YES. Reasons are outlined in the proposal. The proposal clearly demonstrates that the supposed morphological differences between the taxa do not hold up, and the limited genetic data support lumping as the two are either very closely related sister taxa or paraphyletic with respect to each other.

YES. This is an excellent proposal that provides good justification for lumping the two species.

YES. The thorough proposal changes the burden of proof for those that continue to believe the two groups should be treated as different species. Behavior (nest sites mostly on buildings), very similar or identical vocalizations and overlapping morphology dictate treating these two groups as a single species. I am puzzled by option 2 if the lump does not pass. I looked at photos of Horus Swift (*Apus horus*) and White-rumped Swift (*Apus caffer*) and the Barbados bird(s) are not those species on the tail fork alone, so I assume there will be no further question about the identification of the Barbados bird(s).

YES. Reasons are given in the proposal. The available data show few or no diagnosable characters, including for vocalizations. The genetic data indicate paraphyly or extensive gene flow. It seems possible that there are multiple species in this complex, but I don't think that the current two-species delineation is the correct one. I would prefer to consider these as conspecific pending additional data. Plus, it is primarily outside of our purview, so I also prefer to go along with the regional taxonomic authorities as recommended in the proposal.

YES. Proposal provides evidence that they can't be separated based on morphology. Genetic data are equivocal. Additional data may show otherwise, but treating these as a single species is the best option given the data.

YES. There does not seem to be any evidence of the putative sympatry that was the basis for the original lump. I've briefly reviewed vocalizations rangewide, and do not find any obvious vocal differences from Africa through eastern Asia, in strong contrast to some other *Apus* taxa. From west to east, though, there are strong differences in degree of tail furcation, with eastern birds having elongated, well-forked tails almost recalling Pacific Swift, but this difference may be clinal. Further study is needed to substantiate any splits in *Apus affinis s.s.*

YES. I agree with the proposal, there are enough records from different sources. The data available do not support the separation of the species.