2024-B-1
Change Japanese Bush-Warbler to Japanese Bush Warbler

YES. A bookkeeping change to conform with the non-monophyly of the "Bush Warblers".

YES. For the reasons outlined in the proposal.

YES. This is a required change per NACC guidelines.

YES. Remove the hyphen from the English name Japanese Bush-Warbler for the reasons explained in the proposal.

YES. Non-monophyly of the various "bush-warblers" requires the name to lose its hyphen, as per our naming guidelines.

YES. I agree with the proposal.

YES. Removal of the hyphen is necessary on the basis of using it only for monophyletic groups.

YES. Change necessary due to non-monophyly, as others have stated above.

YES. Reasons are stated in the proposal.

YES. Following our rules on group names, the hyphen needs to be removed.

2024-B-2
Treat *Sula brewsteri* as a separate species from Brown Booby *S. leucogaster*

YES. What a cool proposal! I have lots of field experience with both *plotus* and *brewsteri* (including at breeding colonies) and I must say that this split wasn't even on my radar. I think the combination of strong positive assortative mating in recent secondary contact, and plumage differences that are comparable to other *Sula* species pairs, are strongly indicative of species status. The handful of hybrid pairings seem to be in cases where one sex of one species was absent (so there were no real opportunities for choice in a mate), and if I recall these hybrid pairings are at lower rates than in *S. granti* and *S. dactylatra*, which we consider good species. The experimental manipulations by López-Rull et al. show quite convincingly that interspecific aggression provides a prezygotic isolating mechanism.

The authors did a great job sorting out the plumage characters that differentiate especially female *brewsteri*, which will help a lot with getting the distribution sorted out, especially given how many records of *plotus* there are on the west coast of the Americas. Looking through available photographs online, I am unable to find any records of *plotus* from this region (away from Hawaii) aside from those mentioned in the proposal from the Revillagigedos. Perhaps if this range expansion continues, we will see more records of *plotus* on the west coast of North America. Sorting out the identification characters of immatures seems like it will be critical to assessing this.

One potential complicating factor, however, is potential contact between *brewsteri* and *leucogaster*. With the creation of the Panama Canal, there is a potential for these two taxa to cross easily between oceans, and in fact I've seen Brown Boobies in Gatún Lake, and in a quick search of eBird photos I found a nice adult male *S. brewsteri* on the Caribbean side of the canal. There are also some really interesting photos of female Brown Boobies off the southern Pacific coast of Costa Rica that do bear a passing resemblance to *leucogaster*, although I'm not fully confident in separating females of these two taxa. Steve Howell (in litt., and maybe also in a book?) has suggested that *plotus* and *leucogaster* are also separate species, and I suspect he may be correct. Unfortunately, *plotus* and *leucogaster* do not come into contact, although I believe both occur as vagrants in South Africa. To my eye, the slimmer bill and bare colors of the face are actually most similar between *brewsteri* and *leucogaster*, while the larger bill and extensive blue face of *plotus* is the most distinctive of the three. I would be very much in favor of seeing a proposal to split *plotus* in the near future.

I don't see any reason to deviate from the English name of Cocos Booby suggested by the authors. They have thought about this more than anyone, put a lot of work into the research, and should be the ones to choose the name. I suppose that there could be concerns about a perceived close relationship between Cocos and Nazca Boobies given that the names refer to similar geographic regions, but I don't think that's a big concern or that it should sway our decision about an otherwise good name. Rather, I think it provides a nice parallel with Nazca Booby and highlights the endemism of what is now four range-restricted booby species in the eastern Pacific. Per our guidelines, I think the widespread *S. leucogaster* should retain the English name Brown Booby.

YES. I knew nothing about this other than the distinctive adult male plumage of *brewsteri.* Since both *plotus* and *brewsteri* are spreading both east and west respectively, I'm left wondering about the colonization of the Brown Booby on Sutil Rock, off Santa Barbara Island. Some 100+ are (or were) present. I don't know the latest details but an AP article in 2017 quoted National Park biologists stating that in 2017, some 102 had been counted and four active nests were found. The article includes a photo (by Andrew Yamagiwa) of an adult male *brewsteri* sitting on a nest. I have seen comparable numbers on my visits (on pelagic trips) there and most are adults. I must say that only a few of the adults have shown the characters of adult male *brewsteri.* I was left wondering, why aren't there more? So, after reading this comprehensive and excellent motion, including identification characters, I'm left wondering if some there (and elsewhere) might be *plotus.* If *plotus* and *brewsteri* are nesting together on Isla San Benedicto in the Revillagigedo Islands off West Mexico, why not on Santa Barbara Island? I've not heard one word from any of the CA birders about this issue. With assortative mating taking place between *plotus* and *brewsteri* this makes the case for splitting *brewsteri* is strong and this fits the genetic studies which shows that *brewsteri* is the most differentiated.

I agree that those that did the studies and in this case published deserve considerable consideration about the suggested English name, and I'm fine with Cocos Booby, a nice symmetry with Nazca Booby. It looks like a split in this case is just as valid as the split of Nazca Booby.

YES. This is a fantastic proposal that nicely lays out the argument for re-elevating *brewsteri* to full species. The pairing patterns clearly show a preference for assortative mating in the context of increasing sympatry, and those data nicely complement genetic and plumage differences as well as evidence for reproductive isolating mechanisms. I agree with others that those responsible for research leading to a taxonomic change should be given preference regarding English names as they have thought the most about the situation. I am fine with the recommended name Cocos Booby which has a nice parallel to Nazca Booby. Nice job!

YES. An excellent proposal that hits the key points in why these should be treated as separate species: morphological differences, near complete assortative mating in sympatry, aggressive behavior experimentally shown to prevent mating based on head color. I would not be surprised if soft part colors also played a role.

YES. Treat *Sula brewsteri* as a separate species from *S. leucogaster*. There is evidence of strong assortative mating as well as aggression against individuals with different plumage coloration (*brewsteri* females against *brewsteri* males with heads artificially colored as *plotus*). Yes to Cocos Booby.

YES. Excellent and thorough proposal that demonstrates that *S. brewsteri* acts as a biological species. The review of patterns of pairing was particularly convincing in demonstrating positive assortative mating, especially in instances where individuals of *brewsteri* at the edge of their expanding range were left unpaired even when many unpaired *plotus* were also present. Even in well-established biological species, hybridization is most frequent under similar circumstances, and the lack of hybridization in most cases for these boobies is particularly telling. Yes to the name Cocos Booby for *brewsteri*.

YES. Given all the lines of evidence pointing unequivocally toward species status it seems surprising that this has gone mostly under the radar until now. (Note however that Howell and Zufelt already split the Brown Booby three ways in *Oceanic Birds of the World*.) Great proposal that really lays out the evidence.

Interestingly, there is or was a well-known hybrid Brown x Blue-footed booby on Santa Barbara Island, which I and others photographed in Sept 2022 (https://ebird.org/checklist/S120689387). Now an adult, it has drab olive feet that seem unlikely to be attractive to a mate. I see now there is a paper in Marine Ornithology on this: TAYLOR et al. 2013. Hybridization from possible sexual mis-imprinting: molecular characterization of hybridization between Brown *Sula leucogaster* and Blue-footed Boobies *S. nebouxii*. Marine Ornithology 41: 113–119. Anyway, we can expect occasional hybridization in such circumstances even among non-sister species but the refusal of sister taxa to hybridize when given the chance as has been documented for *brewsteri* is what's really significant.

I agree with the English name Cocos Booby, as it highlights an important region and island for the population, and is nicely parallel with Nazca Booby. None of the other suggested English names work very well, in my opinion.

NB: I changed my vote to East Pacific Booby because I think there will be a lot of pushback and ridicule of the name Cocos Booby. Googling coco/cocos should give pause for adopting this name, given its many slang usages, especially in combination with the group name.

YES. I agree with the proposal, *S. brewsteri* has enough morphological and behavior character differences from *S. leucogaster* to support the separation of these species. For the English name, I support the Cocos Boody, because it is inclusive of the total area of distribution.

2024-B-3
Treat Common Redpoll *Acanthis flammea* and Hoary Redpoll *A. hornemanni* as a single species: (a) lump *A. hornemanni* into *A. flammea*, (b) lump *A. cabaret* into *A. flammea*

Note: An addendum with expanded information on the assortative mating studies and assessments from other committees was added to the proposal after the original votes. Both pre-addendum and post-addendum votes and comments are included below. The final (post-addendum) votes are listed first.

YES (a and b). Post-Addendum: Interesting additional information, and a vexing issue indeed. At this point I still vote to merge into a single species given the genomic data, phenotypic continuum, behavioral observations, and variability in the mating behavior studies. **Pre-Addendum:** This excellent proposal provides a strong argument for merging these taxa into a single species based on multiple independent studies. Details are given in the proposal, but the genomic data, continuous phenotypic variation, and geographic/ecological overlap all support the single-species treatment. It would be nice to see more behavioral work done on mating behavior, but I agree with the proposal that such a study would most likely not affect the overall picture.

YES (a and b). Post-Addendum and Pre-Addendum: I agree with the proposal, until now the published data show that it is a polymorphic lineage, which must be recognized as a single species with several subspecies, several analyses show a single lineage with different phenotypes.

YES (a and b). Post-addendum: With the added input from the European committee members and the details from the assortative mating studies, I think I have a clearer picture of the current state of knowledge. The claims of assortative mating are the main thing that gave me pause in the initial proposal, and after reading the summaries provided in the addendum, I must say that I'm not terribly impressed with the evidence. Many thanks for digging those up. Most of these studies were based on very small sample sizes, multiple studies reported intermediates or mixed pairs, and a few did not confidently identify the females of the pairs so don't actually provide any evidence of assortative mating. The Molau (1985) and the Lifjeld and Bjerke (1996) papers seem to be the best evidence of multiple species. However, Lifjeld and Bjerke (1996) was between *flammea* and *cabaret*, and Lifjeld later reported finding intermediates. Also, a few of the studies appear to be based on captures at fixed banding stations, so it's not clear to me how those provide evidence on assortative mating. I agree that the Molau (1985) study did find good evidence, but if only 1-2 out of the seven studies found assortative mating, that seems like marginal support at best. I do find the part of Molau (1985) where they kept some young birds in cages while they molted into adult plumage to be very interesting. If I'm interpreting this correctly, they placed birds of various phenotypes into cages but after they molted most looked phenotypically like *hornemanni*. So, either there is more phenotypic variation in young *hornemanni*, or some of the genetic expression findings of Mason and Funk are playing a role.

This has already been mentioned by some other committee members, but I do suspect that the degree of assortative mating varies spatiotemporally. If assortative mating barriers break down in certain areas that would certainly provide ample opportunity for gene flow. I'm left wondering if, given the available evidence and a starting point of one species, if I would vote to split into multiple species. I strongly suspect I would not. Given, also, that one of the authors of the paper that originally found evidence of assortative mating (Lifjeld), is now in favor of one species, and that multiple global taxonomic authorities are treating these all as one species, I will change my vote to a YES for both a and b. Although we're not voting on subspecies, given the local sympatry, *exilipes* should be considered a synonym of *flammea.* As for common names, the simple "Redpoll" without any modifiers would be my vote. It would now be a monotypic genus, and is known by that name by at least some authorities. It looks like some authorities also use the single-word name for *A. flammea s.s.* but that seems to be based on a Eurocentric tendency to use single-word names even when there are multiple species in a group, so I don't think it should play a role in our naming decision.

Pre-addendum: NO. This is an excellent proposal based on an excellent paper, and there is a lot of interesting stuff going on with the genetics, but I'm not convinced that it fully answers the question of species limits in this complex. I went back and read proposal 2017-B-7 and all the committee member's comments, and I think there are a lot of relevant issues brought up there that haven't been fully answered. I also read Knox (1988), which contains a lot of good information on morphology, breeding behavior, and vocalizations that are relevant to species limits in this complex.

Regarding genetics. There is clearly a lot of gene flow going on, including across large swaths of the genome. That is reflected in the widespread genetic homogeneity in the Admixture plots (although I find it interesting that *hornemanni s.s.* is the most genetically distinct of the three taxa in the group). I am not bothered by most of the genetic differences being solely within the inversion. With the rapid advances in genome-wide sequencing, we've been finding these inversions cropping up everywhere, often associated with different color morphs or behaviors, and sometimes (but not always) with morphological differences that correspond to species limits. It seems like they provide a way for selection to act on specific genes that may or may not be relevant to gene flow / reproductive isolation, while the rest of the genome is swamped out by widespread gene flow. So, it's more of a mechanism that can lead to genetic differences fixing between or within populations, but is not, on its own, indicative of species limits.

The really interesting part to me, though, is the gene expression data. I find it hard to fully justify these being different species if the basis of the plumage differences is controlled by gene expression levels rather than an underlying fixed genetic difference. Both the Mason & Taylor and the Funk et al. studies quite clearly demonstrate that plumage coloration is controlled at least to some degree by gene expression levels of loci within the inversion.

I asked the authors of the paper to provide some clarification on the genetic patterns mentioned in the proposal. I was most intrigued by a PCA plot in the supplemental materials that showed genetic patterns for loci outside the inversion and quite clearly sorts individuals into three clusters. One cluster is *cabaret*. The other two clusters are each comprised of both *hornemanni* and *flammea*, so would seem to contradict the story of widespread genetic homogeneity but also don't sort into current taxonomy. The authors were gracious enough to provide a nice map of the samples from these two clusters along with their morphological identifications, and pointed out that these two *hornemanni/flammea* clusters correspond to males and females, a pattern seen in some PCA-based genomic datasets. That *cabaret* separates out at this fine genetic scale is interesting to me, and could be interpreted as marginal evidence for genetic separation.

I do think there is an issue regarding subspecies if we are to merge all taxa into a single species of redpoll. If pale and dark birds are breeding side by side across their range, that would be incompatible with the subspecies concept. One solution could be to consider *exilipes* as a synonym of *flammea*, with the expanded *flammea* being a variable taxon containing both dark and pale individuals, while maintaining the largely allopatric/parapatric *rostrata, hornemanni*, and *cabaret* as separate subspecies.

In reading Knox (1988), I was struck by how many different papers he cites that looked for and <u>found</u> strong assortative mating between pale and dark redpolls. He cites six papers (Taverner & Sutton 1934, Lundevall 1952, Hildén 1969, Jehl & Smith 1970, Lobkov 1979, Nyström & Nyström 1987) that found assortative mating in Scandinavia, Alaska, and Manitoba, so this pattern is shared across the circumboreal contact zone. I haven't had time to dig into all of these, but they should be included in a future NACC proposal. More recently, Lifjeld & Bjerke (1996) found almost perfect assortative mating between *cabaret* and *flammea* in Norway after these taxa came into secondary contact during an invasion year. So, I would argue that the evidence that we do have on assortative mating points to these being biological species <u>despite</u> the genetic similarities.

The overall picture that is forming in my mind is this: we seem to have pale and dark forms repeatedly evolving across the distribution of redpolls, and these differences are largely (but not entirely?) controlled by gene expression levels rather than the fixed underlying genetic differences that are found in basically all other bird species. However, individuals are, to some degree, mating assortatively based on those morphological differences. So, do we call these biological species? It seems like a unique situation among birds, and given its uniqueness, I think we need to make changes based on solid data from all relevant datasets, including morphology, behavior, genetics, etc. I think this paper and proposal puts us 95% of the way there on the genetics front, but I really want to see more data from other relevant datasets before making changes.

Regarding *cabaret*, I really think we should leave that to other committees, as it is largely outside our purview. It's also the one taxon that forms a unique genetic cluster in the supplemental PCA figure. Let's wait on that one until other committees consider its taxonomic placement.

Hildén, O. 1969. Uber Vorkommen und Brutbiologie des Birkenzeisigs (*Carduelis flammea*) in Finnisch-Lappland im Sommer 1968. Ornis Fenn. 46: 93-112.
Knox, A. G. 1988. The taxonomy of redpolls. Ardea 76: 1-26.
Jehl, J. R. & B. A. Smith. 1970. Birds of the Churchill Region, Manitoba. Man. Mus. Man and Nature. Spec. Publ. No. 1.
Lifjeld, J. T., & Bjerke, B. A. 1996. Evidence for assortative pairing by the *cabaret* and *flammea* subspecies of the Common Redpoll *Carduelis flammea* in SE Norway. Fauna norv. Ser. C, Cinclus 19: 1-8.
Lobkov, E. G. 1979. On biology and interrelations of the Common (*Acanthis flammea*) and the Tundra (*Acanthis hornemanni*) Redpolls in Kamchatka. Biol. Nauki 11: 64-68.
Lundevall, C. F. 1952. The bird fauna in the Abisko National Park and its surroundings. K. Svenska Vetensk Akad. Avh. Naturskydd. 7: 1-73.
Nyström, B. & H. Nyström. 1987. Biotopval och hackning hos grasiskor *Carduelis flammea* och snosiskor *C. hornemanni* i Ammarnasomradet, sodra Lappland. Var Fagelvarld 46: 119-128.
Taverner, P. A. & G. M. Sutton. 1934. The birds of Churchill, Manitoba. Ann. Carneg. Mus. 23: 1-83.

YES (a and b). Post-Addendum and Pre-Addendum: Really neat proposal about a very cool system. Seems like it could prove productive for several dissertations of study. I agree that the mechanisms of phenotypic and genetic variation and degree of assortative mating are yet to be completely worked out, but I feel that the results point to redpolls as being a single species without enough assortative mating and too much gene flow to be considered separate species. Because these patterns seem to occur across a huge area, it may not be surprising that there may be pockets (and periods) where some assortative mating may occur, but looking at this at a continental scale, I can't look past the extensive gene flow and near-continuous phenotypic variation. I voted Yes to lump these 2 species in 2017, and the additional data provided here only reinforce that recommendation. We now have whole genomes and a pretty good explanation for why phenotypic differences are maintained in the population. The genetic data clearly show that genetic variation does not match the named species. Although there may be times and places where mating is assortative, that doesn't appear to be significant enough to cause or maintain genetic differentiation. To me, these are behaving like one biological species, and I don't see strong evidence to support their continued separation other than to maintain the status quo until further study. It's always nice to have more data, but at this point we have enough to merge these.

YES (a and b). Post-addendum: Finally, I'm compelled to vote YES to the three-way lump, given the non-definitive and conflicting nature of the evidence for, and the strong genomic evidence against, species status. One issue that really struck me was the statement in 1985 by Molau that Greenland "*hornemanni*" most likely evolved independently from *rostrata* rather than from *flammea*, which if true would mean *hornemanni* is not even a monophyletic species, let alone reproductively isolated from *flammea*. It seems remarkable to me that despite the huge amount of research that has gone into this complex, we still don't know very much about many aspects, and although I hate to suggest that even more time and effort should be invested in redpoll taxonomy, I can't see keeping them split on the basis of present evidence. **Pre-addendum:** Tough decision and I applaud the amazing paper that we are discussing. That said, I voted for species status for Great White Heron despite a fair bit of gene flow because they usually mate assortatively, and I think this case is somewhat analogous. I just reread Shirihai and Svensson's taxonomic notes which state, e.g.: "Difficult or not, the two redpolls can nearly always be identified from a careful morphological analysis…" "Despite widespread sympatry there is no proof of actual interbreeding; birds with intermediate appearance are more likely extreme variations… When breeding Arctic Redpolls are encountered, both parent birds are invariably white-rumped. On direct comparison, song and calls are subtly finer and higher-pitched than Common Redpoll…" Remember, Svensson lives in Sweden. If we as a North American committee were to make the far-reaching decision to lump I think we should make sure we involve other such experts first. I can't square this situation with subspecies status. How would we even write a range statement for two subspecies that occur broadly sympatrically? So, reluctantly, I vote NO to the lump.

YES (a and b). Post-addendum: I am still very much on the fence on this, and my vote to lump the redpolls is a very weak yes vote. Even after reading the addendum, there is still some doubt in my mind regarding the presence and potential strength of assortative mating. However, given that not all studies found evidence for assortative mating (although many of these were based on very small sample sizes, and so assortative mating could still have been occurring), I now have to reconsider my position. The hypothetical question posed by another committee member has had a strong impact on my assessment, "if we were voting to *split* these redpolls based on the current evidence, how would I vote?" If the situation was flipped, I would definitely *not* vote to split the redpolls based on the current evidence. As for a name for the newly lumped taxon, I concur that simply "Redpoll" is the best option. **Pre-addendum:** NO. I am very much on the fence on this one, and I see the arguments on both sides. I certainly came into this proposal assuming I was going to lump them, but upon reviewing some of the other literature and the observations of assortative mating, I am a bit more swayed by that. True, assortative mating can occur within species, but it can also be the first thing to evolve between species in the absence of any real genetic divergence. Knowing which side of this divide the redpolls lie on is challenging to figure out, but I think until there are additional studies on this topic, I'd prefer to keep the status quo.

YES (a and b). Post-addendum and Pre-addendum: Reasons are stated in the proposal and addendum.

YES (a and b). Post-addendum: YES. Thanks to the authors for providing the addendum. Although this system remains difficult, I reconsider my previous vote and support the lump (weakly), in agreement with other taxonomic committees. It remains imperative to understand phenotypic and genetic variation as well as assortative mating across the entire geographic range of redpoll (at the geographic scale that the study of assortative mating requires). I hope that the lump of species does not discourage further research on the diversification processes happening in this system, since this study system provides the perfect setting to delve deeper into the diversification processes. I support Redpoll as the English name of the species.

Pre-addendum: NO. I have been reading about redpoll trying to understand the system and evaluating the evidence from both viewpoints, to lump the species or to keep them separated. Redpolls represent a species complex of recent diversification, with substantial gene flow, and large population effect sizes. The current three species of redpoll are a complex system that has motivated discussion of species limits for decades throughout their geographic distribution in the Holarctic. The phenotypic variation they present is extensive, discrete in some geographic regions and in other regions, continuous. Furthermore, their distribution areas show high overlap, both during the reproductive season and during winter, although *A. flammea* extends its distribution further south than *A. hornemanni* during winter. What a challenge to disentangle species limits in such a system!

Having the explanation of the genetic basis of the phenotype definitely helps to understand the sympatry of individuals with different phenotypes, which makes a great contribution to the understanding of phenotypic variation in this genus. However, the extensive gene flow mentioned in the proposal does not reflect, neither explains, the previously reported assortative mating between *A. flammea* and *A. hornemanni*. Funk et al. (2021) indicate that 99% of SNPs significantly associated with redpoll phenotype were located on chromosome 1, within or close to the inversion; however, 167 SNPs were located elsewhere in the genome, which suggests that the rest of the genome is not completely homogenized. There are closely related species with fewer differences than 167 SNPs across the genome and still show assortative mating (e.g., *Vermivora chrysoptera* – *V. cyanoptera*).

I agree with the authors of the proposal that the genetic evidence can indicate what the birds are doing, however, I think that the geographical scale at which the genetic analyses were performed is not allowing us to examine the local scale of gene flow in the areas of contact or sympatry. The evidence reported about pair formation indicates that there is a certain degree of mate selection and genetic data on the regional/global scale do not allow us to understand this selective process at the scale at which said process is occurring. The species *A. cabaret* has been considered a separate species based on assortative mating at the local geographic scale despite being genetically admixed with the other species of redpoll. Funk et al. (2021) report that heterozygous redpolls for the inversion appear to occur in fewer number than homozygotes (7/73 samples in the study); local scale studies might allow to approach and better understand this finding.

What is going to move us forward in understanding redpolls? Lump them all into a single species or keep them separated and try to understand mate selection processes? We are looking for a classification that reflects biological species in the best way possible, at least in the best way that current knowledge allows. Therefore, we should keep the redpoll species as they are until we get a better understanding of assortative mating.

Otherwise, if the final decision of the NACC is to lump the species, in my opinion, that would require a revision and redefinition of the redpoll subspecies, in a way that subspecies reflect phenotype and genetic basis of that phenotype but also their geographic distribution. We could have subspecies that show polymorphisms (as in continental North America) and subspecies that replace each other (as in Greenland). I do not agree with the acceptance of extensively sympatric subspecies.

NO (a), YES (b). Post-addendum: (a) NO. I am not wedded to the concept that these morphs, subspecies, or species, should be recognized as separate species. What I am wedded to is that there should be a comprehensive study *on* the breeding grounds. This should not be difficult to do as Hoary and Common Redpolls (*sensu lato*) are sympatric in a circumpolar fashion around the globe. There are many places this study could be done. Nome on the Seward Peninsula in western Alaska comes to mind with multiple daily flights (three), lots of places to stay and both types being numerous and nesting in a seemingly sympatric fashion. I agree that none of the studies, either way, are that detailed, but if they do seem to assortatively mate with their own type, why? Perhaps it is because the pale types might migrate earlier in the spring and have established territories when *flammea* arrives, much as with *Plectrophenax hyperboreus* on St. Matthew and Hall Islands where migratory *P. nivalis* arrives after *P. hyperboreus* has established breeding territories. I believe they are genetically close, but the motion to lump failed unanimously as I recall. One issue that I think has cropped up again is the notion that based on phenotype, hybridization is assumed but in one publication it was stated that at least some of these birds were SY Hoary Redpolls. Ideally, studies should be done with birds of known age. I am resigned at this stage to the lump but am troubled that nearly all aren't troubled enough to await an actual field study on the breeding grounds. As Joseph R. Jehl, Jr. stated in his most recent (2004) Churchill book in regards to this issue: "What remains to be studied in detail is how the birds respond to each other with respect to mate choice. Such data should be easy to amass, because males feed females at the nest. This would allow both members of the pair to be captured in a mist net for examination." I find it sad that no graduate student has been inspired to do such a study. Until such a study is done I think maintaining the status quo is best. As for the English name for the combined birds, simply Redpoll is just fine, even though some might find it too non poetic or vanilla. It is certainly the name that will be used in the Old World and well-describes the most obvious field mark that is found in all birds (post juvenal plumage).

(b) YES. This is more clear cut and in fact it should not have been split in view that it wasn't universally split in Europe. Its presence on the North American list is based on one Greenland specimen. Besides, maintaining Lesser Redpoll as a species while lumping Hoary hardly makes any sense.

Pre-addendum: For now I can't support this proposal, which basically doesn't ally with my definition of a species. Nick says that evidence of assortative mating from zones of sympatry doesn't indicate that they are separate species. I'm not sure we have definitive evidence of that, but there are studies, plus anecdotal evidence to suggest that assortative mating is taking place. What I asked for in 2017 and repeat now is that actual studies take place on the breeding grounds in the zone of sympatry, Jehl (2004) even suggested a methodology on how such a study take place (following around the males until they feed the females on the nest and then mist net them for further study). Jehl (2004) and others have suggested why these two might be separate species and these include the timing of the arrival on the breeding grounds (possibly earlier in Hoary), vocalizations and habitat.

My no vote doesn't mean that I'm convinced they are separate species, but I prefer to maintain the status quo, especially with such a limited time frame for a vote. This lump has global ramifications and I'm curious to know what the European ornithologists think now. As I indicated, Svensson (2023) continues to recognize Hoary Redpoll, while not recognizing Lesser Redpoll (*cabaret*). Getting their input on these issues would be useful. And, I agree that changing my thoughts on what is a species (by this lump) would also change my notion on what is a subspecies, assuming we follow the recommendations in the motion as recognizing these sympatric types as valid subspecies. Some of the WGAC members live close to the "problem," so getting their thoughts on this issue might be just as valuable as hearing from us. To my knowledge, no European authorities have yet lumped Hoary and Common Redpolls as a single species. If we lump those two, but not Lesser Redpoll, leaving it to the Europeans, I wonder what the English name would be for the two lumped species, I realize that this is the least of the problems.

I would agree that the species limits, as we have defined them, might well be inaccurate, but until there is more comprehensive study (like on the nesting grounds), maintaining the status quo is an option that NACC has often embraced in difficult cases. Again, the fact that this is a worldwide issue is all the more reason to proceed cautiously.

Jehl, J., Jr. 2004. Birdlife of the Churchill Region.: Status, History, Biology. Sponsored by the Manitoba Special Conservation Fund and Churchill Northern Studies Center.

Svensson, L. 2023. Birds of Europe, Third Edition. Princeton University Press.

2024-B-4
Treat *Anas crecca* as two species: Green-winged Teal *A. carolinensis* and Common (or Eurasian) Teal *A. crecca*

NO. Retain the current one-species treatment. I have no idea why the plumage score for the vertical white bar was suddenly changed from 2 to 3, thus putting these over the line for species status using the Tobias criteria, but the lack of differentiation in female plumage, vocalizations, or pair-bonding behavior all contradict the relatively high plumage differentiation. Regardless, the marginal score indicates that this is clearly a borderline case and is a situation where the scoring criteria are particularly vulnerable to over-simplifying a complex case.

This is one of the few recent cases where we do have good estimates of the number of migrants between populations based on good genetic data, and that number is very high, especially considering the relatively small population sizes on the islands where these taxa are in contact (compared to the larger continental populations). Also, a "broad" hybrid zone is also a point against species status, but that statement is another over-simplification of the situation in this case, with the unequal gene flow and the resident *nimia* between the two migratory taxa. The highly migratory nature of the two continental taxa, the frequency of vagrants on both continents, and the fact that these ducks form pair bonds on the winter grounds, all provide a clear mechanism for ongoing high levels of nuclear gene flow despite mitochondrial genetic differences. The almost total lack of differentiation in the nuclear UCE data shows the results of this hybridization quite clearly.

Clearly, the relatively strong plumage differences are not especially relevant to the birds themselves.

NO. We voted on this a good long while ago and I didn't support a split. Nothing new has developed to cause me to reconsider. I am often asked about it and folks counter that if we don't split these teal, we should lump American with Eurasian Wigeon. On that issue I spent a good long while looking at a loose assemblage of well over a thousand wigeon in Sonoma County. I counted over a dozen Eurasians. The males were easier to locate, of course, but four of the males were clearly paired with female Eurasian Wigeons. Assortative mating on the winter grounds, the norm in waterfowl. Given the close appearance of females of *crecca* and *carolinensis* (perhaps the secondary pattern differs, in particular the width of the color and width of the borders to the speculum, particularly the forward bar) it is hard or currently impossible (based on knowledge) to visually determine mixed pairings. Thus what can be done with the wigeon can't be done with these two teal, something would be particularly useful on the eastern Aleutians, but also in the central Aleutians and other islands in the Bering Sea where both taxa appear with regularity (e.g. on the Pribilofs and St. Lawrence Island). From all of these areas, intergrade males are not the least bit unusual and are also seen from elsewhere on the West Coast. Gibson & Byrd (2007) point out that in the eastern Aleutians, intergrades are "much more numerous" than the central and western Aleutians where *crecca* predominates. When the earlier motion to split these two taxa was presented there was commentary by a number of members that the male calls were identical to one another. Compare the situation to male wigeon calls where Eurasian and American give easily distinguishable calls. Spend any amount of time in late winter when wintering ducks are still numerous and watch the courtship and pair bonding going on and listen to the cacophony of duck calls. If the calls are identical and there is such a close match in morphology, well…..

As I recall the evidence for the difference in display cited in the earlier motion some 15 years ago, involved a quantitative, not a qualitative difference in male head bobbing from a study done in captivity in Europe. I don't recall other reasons other than the genetic comparisons. But the genetic studies were done between European birds in comparison to birds from eastern North America. It is less clear cut in western Alaska where the two taxa come much closer and indeed do overlap. These are perfectly good subspecies.

Leave them at that. I also want to point out that the calls of male Green-winged Teal are very similar to the calls of Northern Pintail which it is next to in the linear sequence of *Anas.* In fact, I can't hear the difference but Kimball Garrett and others have said there are subtle differences. I have never seen a hybrid between those two species.

NO. I agree with the proposal's recommendation to retain these as a single species in view of the apparently high levels of hybridization and thus lack of reproductive isolation.

NO. Hybridization and documentation of hybrids between the two forms in the areas where they come into contact suggest that both *crecca* and *carolinensis* mate successfully and the hybrids reach adulthood. A large number of hybrid records in North America (west and east) and Europe (west) provide evidence against reproductive isolation between *crecca* and *carolinensis*.

NO. This is a borderline case especially given that other waterfowl species we accept (especially the Mallard complex) also experience high levels of hybridization. But the rampant hybridization and similar if not indistinguishable male calls just push it to the subspecies side of the equation for me, while the at least-distinguishable male calls of wigeon (which of course we are not voting on) keep them on the species side, although both are close calls.

NO. The molecular data contradict and do not support the split. I agree with the proposal that these taxa are not biological species, and they have substantial levels of gene flow.

NO. Same vote as way back when, and nothing new really alters my viewpoint that the genetic, morphological, and natural history evidence actually indicate a lack of reproductive isolation. Great case study of why monophyly is not a reliable indicator of species status, especially with mtDNA based studies.

NO. Following the Biological Species Concept, these need to be considered as a single species, given the degree of hybridization happening between the two forms. Genetic evidence from the nuclear genome (and estimated number of migrants) shows lack of significant reproductive isolation. Even eBird records show the hybrids are common and widespread.

2024-B-5
Treat *Colaptes mexicanoides* as a separate species from Northern Flicker *C. auratus*

YES. I'm swayed mostly by the vocal differences. Yes, there is some overlap in call pace between *mexicanoides* and other flickers, but if we're recognizing *chrysoides* as a separate species when that one shows no vocal differentiation from other flickers, then by yardstick extension the combination of vocal *and* plumage differences of *mexicanoides* point to reproductive isolation. The lack of a "*kleer*" call in *mexicanoides* is especially convincing. I know that much has been written about how plumage differences are of limited utility in reproductive isolation of Northern Flickers, given the extremely broad hybrid zone in the central United States, and that species-level differences in flickers should be associated with vocal differences. It seems to me that's what we see here with *mexicanoides*. The alternative to recognizing *mexicanoides* as a species would be to consider *chrysoides* as a subspecies of *auratus* (not a voting option), but the relatively narrow (at least compared to Yellow-shafted vs. Red-shafted) hybrid zone suggests that *chrysoides* is a good species, and by extension so is *mexicanoides*.

I am a bit concerned about the purported genetic differences, though. The Manthey et al. (2017) study did not sample *any* Red-shafted Flickers from Mexico, so the reported genetic distinctiveness of *mexicanoides* could easily just be due to the unsampled intermediate birds between their southernmost Red-shafted sample (Arizona) and their two *mexicanoides* from El Salvador. However, those two *mexicanoides* were sister to all remaining "northern" flickers (including *chrysoides*!) and are separated by a major biogeographic barrier.

Guatemalan Flicker has been used for this taxon in the past, and I think should be used here if the proposal is adopted.

YES. The advertising song certainly sounds different to me while the other species in *Colaptes auratus* plus *Colaptes chrysoides* sound the same and this includes the year-round *clear* note which is apparently lacking from *mexicanoides.* I agree that this is significant, and if we don't split here, it's hard to justify continuing to maintain Gilded Flicker as a separate species, although habitat selection (and altitude) largely separate these two out. I'm fine with the English name of Guatemalan Flicker for *mexicanoides* if the motion passes.

YES. The vocal differences, combined with the molecular and phenotypic data and the low likelihood of gene flow across the Isthmus of Tehuantepec, support this split. Guatemalan Flicker seems like a good name based on the species' distribution and its use in the literature.

YES. This split seemed a long time coming, and it's nice to see the vocal analyses that support the split. Based on the importance in these vocalizations in courtship, the differences observed are consistent with species status for *mexicanoides*. The vocal differences, combined with the genetic and morphological differences, all lend support to recognizing *mexicanoides* as distinct from the rest of the Northern Flicker complex. I agree with the name Guatemalan Flicker for this species should it be split.

YES. This is what Wetmore had to say about the differences in vocalizations in 1941, and he has now been proven right. Given this level of vocal differentiation, now quantified, I think there would have to be strong evidence that these vocal differences don't matter to the birds themselves (which we already know to be the case for certain plumage differences) in order to justify not splitting them.

Family PICIDAE

COLAPTES MEXICANOIDES MEXICANOIDES Lafresnaye

Colaptes mexicanoides LAFRESNAYE, Rev. Zool., 1844, p. 42 (Mexico).

Near Sierra Santa Elena these handsome flickers were common along the borders of woods and trails, ranging from Chichivac at 8,600 feet upward. I saw them also at Canderas and at Patzicia and found them common above 10,200 feet at Desconsuelo. They are typical flickers in general appearance as they fly away with bounding flight, displaying their white rump patches. But most of their high-pitched, chattering, laughing calls are quite different from the notes of the northern species, and only occasionally did my ear catch a sound from them that indicated their flicker relationship. They were found often in pairs. Three were taken at Sierra Santa Elena on November 17 and 18, and two more at Desconsuelo on November 24. One of the latter was prepared as a skeleton.

YES. I agree with the proposal, there is enough evidence in the molecular data, plumage and highly differential vocal behavior that indicate deep divergence between *C. mexicanoides* and other *C. auratus* groups. And, I agree with the English name Guatemala Flicker.

YES. Although I had similar questions as another committee member regarding overlap in vocalizations and genetic results I reached a different conclusion. To me the vocalizations sound pretty different (*mexicanoides* longer, quicker, higher pitched with a different tonal quality) . The lack of "clear" note is interesting as this is such a common vocalization in most populations.

YES. The argument of non-monophyly of *C. auratus* presented in the proposal is not that compelling to me since Biological Species can be non-monophyletic. But the genetic and vocal divergence and lack of hybridization opportunities are compelling. My vote on this in 2017 was a tentative No, but the vocal analyses described here are enough for me to agree with the recommendation to split. I also agree with the name Guatemalan Flicker.

NO. The committee evaluated a proposal on this issue in 2017 (NACC proposal 2017-C-4), which already included the genetic information from Manthey et al. (2017). Although quantitative vocal analyses were not included in the 2017 proposal, *mexicanoides* was mentioned as having unique vocalizations, along with unique plumage and habitat requirements. The 2017 proposal did not pass (3 YES – 7 NO); genetic analysis with a small sample size (two *mexicanoides* individuals), plumage coloration differences, and apparent vocal differences were not considered sufficient evidence to support *mexicanoides* as a separate species. In the committee's comments, it was indicated that habitat requirements are shared between *mexicanoides* and *cafer* on the west side of the Isthmus of Tehuantepec.

The current proposal adds vocal quantitative analyses from the doctoral dissertation work of Lausch (2020). Vocalizations of *mexicanoides* show some differentiation but still overlap with the vocalizations of the other four *Colaptes auratus* taxa (including *C. chrysoides*). As assessed by Lausch (2020), post-hoc classification correctly assigns 70% of *mexicanoides* calls. Interestingly, Lausch discusses that given the amount of overlap in the long calls of Northern Flicker (Gilded, Red-shafted, Yellow-shafted, and Caribbean) across the geographic range “it is doubtful that variation in the long call could contribute to species-specific recognition among these groups”. That statement raises the question: why would the long call be a species-specific recognition trait in allopatric populations east of the Isthmus of Tehuantepec (*mexicanoides*) and not just a differentiated trait given allopatry?

Genetic analyses from Manthey et al. (2017) are also included in the current proposal. The differentiation across the Isthmus of Tehuantepec is consistent with the pattern that that barrier imposes on multiple mountain taxa in the region (that is the rule rather than an exception, but we do not consider separate species each taxa with 15 or more ND2 differences across the Isthmus of Tehuantepec). Therefore, genetic differences are consistent with allopatric subspecies rank. There are no recent genetic studies that have increased the sample size.

Long call (Lausch 2020) and phylogenetic analyses (Manthey et al. 2017) suggest that the species status of *C. chrysoides* should be reevaluated. Moreover, playback experiments show no differences in responses to conspecific versus heterospecific vocalizations in Gilded and Red-shafted Flickers in Arizona (Lausch 2020). Similar playback experiments would help to understand *mexicanoides* taxonomic rank.

If the proposal passes, I agree with the English name Guatemalan Flicker, since it is the name that has previously been used for *mexicanoides*.

2024-B-6
Treat *Buteo elegans* as a separate species from Red-shouldered Hawk *B. lineatus*

YES. A weak vote in favor. I’ve thought about this one for decades. My main reservation is that the calls are really very similar to my ear, although I note Bryce’s spectrograms detailing the slight differences. The two strongest points in my mind are the distinctly different juvenal plumage of eastern birds in comparison to *elegans.* To put it simply, you see a juvenile *lineatus* and it looks very much like a juvenile Broad-winged Hawk. In fact once with a group in West Virginia I saw what I initially called a juvenile Broad-winged Hawk on a telephone line. I quickly thought that’s really weird to have a fledged Broad-winged on such an early summer date. It then called and it was Red-shouldered. I am used to juvenile *elegans* which appears much like an adult and one shouldn’t ever confuse it with a young Broad-winged. The 2nd compelling reason for splitting for me has gone largely unnoticed, but I think it results from the much shorter wing of *elegans.* When you watch an *elegans* in flight and watch it fly it invariably flies with shallow and quick flaps, almost Sharp-shinned Hawk like. While nominate Red-shoulders don’t flap exactly like Red-tails, their wing flaps are slower, more relaxed in comparison to *elegans.*

Nominate *lineatus* is partly migratory and it is an early spring and late fall migrant. Given that it is migratory, I'm surprised that very few stray west. Shorter winged *elegans* is more of a resident, but birds, particularly juveniles, regularly disperse into the deserts and I've seen one as far east as the Rio Grande in New Mexico. Its breeding range has expanded north and east. I also point out that juvenile *elegans* disperse well away from breeding areas by early August, places like Corn Creek in southern Nevada or from desert oases in the Mojave or Colorado deserts of eastern California, locations where no *elegans* breed. I haven't researched this, but wonder if there are any records of *lineatus* west of breeding areas in the East, say from the central or eastern Great Plains? Bryce discusses some aspects of the life history between these taxa, but I'm left thinking that the life cycle of *elegans* is pretty different from eastern birds.

I suspect there is lots of intergradation within the eastern subspecies, particularly *alleni* with nominate *lineatus* and *alleni* with the questionably valid *texanus*, the latter being not recognized by some (need to check on this). I don't believe that any Red-shouldered Hawks breed in southmost Texas or northeast Mexico, although they are regular in winter, and these likely represent migratory nominate *lineatus.*

Red-bellied Hawk is the well-established English name for *elegans* and was used since the first edition of the AOU Check-list (1886).

In summary I weakly support a split based on the distinct morphology and morphometrics which in regards to the wing length reflects a different flight style in *elegans.* My hesitation is caused by the similar morphology in adults and particularly by the close similarity in vocalizations.

I note that the genetic differences were first described in 2008. I still remember sitting in a talk at an AOU conference in Cornell in 1999 where the speaker said there was no molecular difference in *elegans* and therefore it did not even deserve recognition as a subspecies, this despite the distinct morphological differences. Perhaps she didn't publish the results?

YES. However, as others have stated, this is on the fence. I'm especially swayed by the juvenile plumage differences and genetic differentiation. A proper analysis of vocalizations is clearly needed and would likely not be too complicated to perform given the nature of the territorial song.

YES. There is genetic differentiation between *Buteo elegans* and *B. lineatus*, plus the morphometric, plumage and vocalizations exhibit some differences between them too.

NO. The available data are suggestive but do not definitely support a split in my opinion. I would like to see a quantitative analysis of putative vocal differences which, as noted by the proposal author, deserve "a close and thorough look in the future." Seems like a good project for a Master's student.

NO. This is an extremely tentative no, and I could easily be swayed to vote yes after reading comments from other committee members, given that it's a very borderline case. I fully agree that *elegans* warrants species status under some species concepts, but I'm not sure that the BSC is one of them.

One minor quibble about this otherwise excellent proposal is the purported lack of other species pairs of *Buteo* with which to compare levels of genetic differentiation. The Old World buzzard radiations (especially *augur* and *buteo* and their relatives) provide plenty of potential comparisons, as do *swainsoni* vs *galapagoensis* and *jamaicensis* vs. *ventralis* in the New World. Surely some of those have sequence data with which to compare the genetic results from *elegans* vs. *lineatus*. However, I do agree that both the microsatellite and mitochondrial differentiation of *elegans* vs. *lineatus* is relatively high and suggestive of species-level differences.

I think the plumage differences are the strongest evidence of species status for *elegans*. The combination of the solidly rufous chest, more dorsal/wing contrast, and lack of dark streaking below (as seen in some populations of *lineatus*) in the adults are all quite distinctive. The additional plumage differentiation in the juveniles likely has little relevance to reproductive barriers, but it does point to high overall levels of differentiation. An interesting comparison that I wish was made in the proposal is how different *elegans* is in comparison to *ridgwayi*, the other taxon in this complex. As far as I know, *ridgwayi* is unambiguously considered a species, but is clearly part of this complex based on plumage and vocal similarities, and is the sister species of *lineatus*+*elegans*, with a roughly 2 Ma divergence time (do Amaral et al. 2009). *Buteo ridgwayi* is much more different in terms of plumage than is *elegans*, but in somewhat the same ways. The adult plumage of *ridgwayi* is much grayer (vs somewhat more rufous in *elegans*) and the juvenile plumage differs mostly in the head and underpart pattern.

What gives me pause is the low differentiation in vocalizations between *lineatus* and *elegans*. The proposal mentions differences in pace and note length, but also states that these have not been quantified. However, I'm not hearing much in the way of consistent differences in listening to recordings, and I haven't noticed any differences between them in the field. That said, the differences that are mentioned in the proposal do differ in the same way that *ridgwayi* differs. The vocalizations of *ridgwayi* are largely similar to *lineatus*+*elegans* in quality, but the notes are more widely spaced, and it gives fewer notes per phrase. Playback trials between *lineatus* and *elegans* would be straightforward to do and would be very informative.

If the proposal passes, I think we should have a separate proposal on the common name. Red-bellied Hawk is misleading. It's the chest that's more red in *elegans*, not the belly. Elegant Hawk could be an evocative choice that parallels the scientific name. Or we could go with compound names like Eastern and California/Western Red-shouldered Hawks.

do Amaral, F. R., Sheldon, F. H., Gamauf, A., Haring, E., Riesing, M., Silveira, L. F., & Wajntal, A. (2009). Patterns and processes of diversification in a widespread and ecologically diverse avian group, the buteonine hawks (Aves, Accipitridae). *Molecular Phylogenetics and Evolution*, 53(3), 703-715.

NO. I am very on the fence about this one, as the genetic and morphological differences are certainly suggestive of species-level divergence. The differences in vocalizations are also very intriguing, although I think a formal analysis is warranted to clarify the differences that I hear and see on the spectrograms.

NO. Although there is evidence (morphology, plumage coloration, and genetics) to suggest that they may be different species, a formal analysis of vocalizations and playback experiments could provide the necessary evidence to tip the balance towards splitting *elegans* from *lineatus*.

NO. The proposal's chief weakness is that it bases its recommendation on genetic distance. Although important in allopatric situations, genetic distance cannot be the chief evidence of a proposal to split. Underpart color and extent of barring is extremely variable within many *Buteo* hawk populations. In Florida, Red-shouldered Hawks have been vocally active and displaying for a few weeks already, and it is only 23 January.

NO. Although I'm voting "no", I wouldn't be surprised if additional data provides a compelling case for splitting. To me, the strongest evidence is plumage. I would like to see a more formal vocal analysis and additional genetic data. The genetic data are not convincing to me. Although no mtDNA haplotypes are shared, the mtDNA haplotype of *elegans* is only one substitution away from another haplotype found elsewhere. Bottom line, these are good phylogenetic species, but don't meet the threshold of our criterion for a biological species.

NO. A close call, as others have mentioned, a detailed analysis of vocalizations would be helpful. More genetic data would be useful too.

2024-B-7
Reconsider the generic treatment of *Calocitta*, *Psilorhinus*, and *Cyanocorax*

YES. Option 2, but with Option 3 as a close second. *Calocitta* especially is such a morphologically distinctive clade that merging it in with the rest of *Cyanocorax* vastly increases the morphological variation within the expanded genus, but I don't like the idea of separating out four species in *Uroleuca* just so that we can maintain *Calocitta* and a monotypic *Psilorhinus* (option 1). Ignoring the two *Calocitta* for a moment, the rest of group 1, including *morio,* are rather cohesive in terms of plumage. Take away the brown coloration and replace it with blue, and *morio* looks a lot like *cyanomelas* and its relatives. Plus, some pale individuals of *morio* actually show a "ghost" of the face pattern of *Calocitta*, perhaps bridging the gap between the magpie-jays and the rest of the clade.

I think the main question is whether to separate group 1 as an expanded *Psilorhinus* (Option 2). I rather like this option. The seven species in group 1 are all allopatric/parapatric lowland replacements from northern Mexico through South America. That said, I don't know of any plumage autapomorphies for the clade, and nothing is really jumping out from the specimen photos. Maybe someone else can come up with some good plumage characters to diagnose clade 1.

However, vocalizations may provide an autapomorphy. Although corvid vocalizations are of course quite variable, clade 1 has a rather conserved call type shared by all species, a loud hawk-like series of harsh rising-falling notes. The species in groups 2 and 3 give more clipped notes in rapid series, often with a whistled/sweet descending quality. *C. beecheii* throws a bit of wrench in this but is clearly part of clade 2 based on plumage. This, combined with the relatively deep node separating groups 1 from 2&3 point to genus-level differences to me. It is of course subjective, and I'm happy to go along with option 3 if that is the consensus.

I do wonder about the placement of *mystacalis*. The short internode distance suggests long-branch attraction could be playing a role, especially if there is poor node support here or higher missing data for that sample. It seems like it should go with group 3 based on morphology. If that is the case, then group 3 becomes a set of South American allotaxa that are rather cohesive, while group 2 is the Middle American radiation. *C. yncas* is the odd one out here of course. Either way, a combined group 2&3 becomes rather cohesive in my mind, and I would be in favor of considering this group as *Cyanocorax*.

No matter which option we adopt, we'll need to change the linear sequence in the checklist. By my estimation it should be: *colliei*, *formosa, morio, yncas, melanocyaneus, yucatanicus, beecheii, sanblasianus, dickeyi, affinis*.

YES. Option 3. As noted in the proposal, Option 3 seems like the most prudent approach and would put NACC in agreement with the WGAC. I also vote YES to changing the linear sequence per the proposal's recommendation.

YES. Option 3.

YES. Option 3. This largely aligns with my philosophy regarding higher order taxonomy as well, recognizing fewer larger groups rather than increasingly small and uninformative smaller groups. In this case, I agree with the proposal that splitting this group into a total of 4 genera would create very young genera with very short branches relative to other members of Corvidae, while also breaking up the phenotypically very similar birds that were traditionally united in *Cyanocorax*. And in my opinion, Option 2 would create a genus no less phenotypically heterogenous than Option 3, but it would just break up the similar members of the traditional *Cyanocorax*. For part C, YES, as the change in linear sequence is necessary based on the phylogenetic studies.

YES. Option 2. Here is my rationale: To me despite the fundamental plumage similarity of *violaceus*, *cyanomelas* and *coeruleus* with the Mesoamerican *Cyanocorax* branch in plumage, the former group are generally bulkier and dark-eyed, vs. the mostly yellow-eyed (except Yucatan Jay), more petite Mesoamerican group, and so their plumage similarity is likely convergent. Same with *cristatellus*---it resembles the white-bellied group of *Cyanocorax* but for its unique crest type, except in having dark irides and lacking the facial markings of all members of the white-bellied group. Also both in the Bonaccorso and Peterson and McCullough et al. phylogenies, the split between these groups is pretty deep, almost as deep as for the *Gymnorhina/Cyanocitta/Aphelocoma* split, for example. I also hate to lose *Calocitta*, with their flamboyant tail plumes and crests, but this seems inevitable. I'm ambivalent on *Psilorhinus*.

YES. Option 3. I agree with transferring the species currently placed in *Calocitta* and *Psilorhinus* to *Cyanocorax*. The broad genus *Cyanocorax*, what an interesting group to study phenotypic evolution! And yes, change the linear sequence as specified in the proposal.

YES. Option 3. I agree to transfer the species currently placed in *Calocitta* and *Psilorhinus* to *Cyanocorax*.

YES. Option 3. In this case, I think drawing generic boundaries based on similarities in plumage, soft part color, behavior, or structure is fraught with uncertainty, because of convergence (or reversals?). I think we have to trust the DNA sequences and the resulting analyses, and go to a hypervariable single genus. Yes to the proposed linear sequence.

YES. Option 3 (slight preference). This seems to be the simplest option and, most importantly, requires fewer changes in taxonomy than the other two options. I also don't mind Option 1, which would keep *Cyanocitta* for the long-tailed jays as well as *Psilorhinus*. Option 2 is probably the most informative representation of the phylogeny, so I would be ok with this as well. And I vote yes to the required change in linear sequence based on the new phylogeny.

YES. Option 3. I am sad to lose *Calocitta*, but I think this is the best option given the data with the fewest taxonomic changes.

2024-B-8
Treat Isthmian Wren *Cantorchilus elutus* as a subspecies of Cabanis's Wren *C. modestus*

YES. Reasons are given in the proposal. In particular, the lack of vocal discrimination strongly suggests conspecificity.

YES. Multiple reasons are outlined in the proposal.

YES. I agree with the proposal that a formal analysis of vocal differences is needed, but for now the putative vocal similarity, apparent lack of discrimination in playback experiments (although more reciprocal trials would strengthen those data), and plumage similarity all suggest that merging these taxa is justified despite the deep mtDNA divergence.

NO. While the evidence presented in the proposal is certainly suggestive of *elutus* being a subspecies of *modestus*, I am not ready to make the change in the light of the deep mtDNA divergence and resulting paraphyly. While I certainly accept that, as is the case in many taxa, mtDNA paraphyly can exist within species, and that we are relying on a gene tree to assess species level relationships, given the very deep divergence, and the lack of formal vocal analyses, I think it is best to keep the status quo on these taxa for the time being, with the acknowledgement that *elutus* is likely best treated as conspecific with *modestus*.

NO. I agree that in order to overturn the previous decision based on deep mtDNA divergence and (now disputed) vocal differences, at least a formal analysis of the vocalizations will be needed and preferably further genetic work or at least some explanation of how the deep divergence is maintained if song discrimination is absent.

NO. The available evidence continues to be practically the same (genetics, quantitative analysis of song, morphology, and plumage coloration) that the committee used in 2016 to unanimously separate the species. Genetic and vocal analyses in the contact zone are required to understand the deep divergence in mitochondrial DNA despite the apparent lack of song discrimination. With respect to plumage, it is not unusual for Neotropical birds to maintain their plumage coloration even after long periods of divergence.

NO. I think we need a formal analysis of the vocalizations and additional genetic work, especially in the contact zone.

NO. First, I have a high bar for overturning previous decisions, especially recent ones. Overturning recent decisions undermines one of our key philosophies, that stability is the foundation of classification, and that we should only make changes when evidence is overwhelming. Although Boesman is likely correct in his analysis of the vocalizations in this group, this work was not presented in a peer-reviewed publication. There still is a major genetic shift.

NO. While yes it is a single locus, the mtDNA divide is substantial, and there seems to be quite different interpretations of the vocal duets at hand. These displays are complex and difficult to quantify. Similarity in vocal displays and responsiveness to playback may suggest song is not a strong pre-mating barrier to gene flow, but there may be other pre-mating or post-mating barriers contributing to what appears to be a strong divide in a lineage with modest phenotypic variation overall. I’d rather see a more comprehensive genetic analysis before we overturn the current status quo.

NO. There is strong genetic evidence to treat these three as separate species, yet no strong vocal evidence has been described to explain that data. Nevertheless, that doesn’t mean we should ignore the existing data that shows a deep divergence among these taxa. Furthermore, the 2016 committee voted on to split these species based on that data and thus I’m reluctant to overturn that vote. If the mtDNA divergence was weaker, I might have voted Yes.

2024-B-9
Treat Intermediate Egret *Ardea* (or *Casmerodius*) *intermedia* as two or three species

YES. There seems to be a pretty widespread agreement to split off *intermedia* from *brachyrhyncha* and *plumifera* as its own species. The differences in alternate plumage are pretty striking. The differences between *brachyrhyncha* and *plumifera* are underwhelming, but I appreciate the geographical distance between the two taxa. Without genetic studies, I'm uneasy about endorsing a split between the African and Australian subspecies. If these two occurred more proximally to each other, would there be support for a species split? I would prefer genetic evidence which as noted in the motion is lacking. So, why not split off *intermedia* as its own species and then cite the authorities that also split *plumifera* and *brachyrhyncha* as their own separate species? From our viewpoint the important thing is making range revisions for what would now be a monotypic species. So delete Africa and Australia. Indonesia and Wallacea is more confusing, but *intermedia* is mapped as breeding for Java and east of the Wallacea line, Intermediate Egrets are said to breed in north Sulawesi. I assume these breeders are *intermedia.* I don't think *plumifera* is known to breed away from Australia, but I'm not certain of this. It does disperse north to New Guinea. There is uncertainty over which taxon is involved over much of Wallacea, but since *intermedia* (some populations) is so migratory, it seems likely to me that this would be the taxon involved. Eaton *et al.* (2016) say about Indonesia that the "geographic distribution of taxa [*intermedia* or *plumifera*] in C [central] Indonesia still insufficiently known; non-breeders may overlap." And (*ibid*) they continue "widespread migrant and local breeder in W [west]. Uncommon migrant in E [east].

The records for North America of *intermedia* are both from the western Aleutians and substantiated as specimens at UAM: One from Buldir Island (30 May 2006) and one from Shemya Island on 28 September 2010. The record (with published photos) from Midway Atoll on 25 June 1997 (Richardson 1999) was not endorsed by the NACC (not sure what the Hawaii rarities committee did), but another evaluation would perhaps be in order, in part to see if it is acceptable as a *coromandus* Cattle Egret. More recently a record, submitted as a Great Egret, in eBird from Kure Atoll well out the Hawaiian chain, appears to be an Intermediate Egret based on the gape line (only to eye) and the shorter bill (but bill tip not blackish). The report was passed on to Oscar (then to me) by Marshall Iliff. The eBird report was on 15 Nov 2017, but notes indicate that the "same individual was reported on 4 November 2017.

As for the English name, Intermediate Egret seems well-established and I favor using it for (if motion passes) the monotypic Asian taxon.

As a final comment, I found it puzzling that we were determining taxonomic issues with both this species and with Cattle Egrets, but not with Great Egret (*Adea alba*). The soft part color differences in alternate plumage between New World *egretta* and Old World *alba, modesta* and *melanorhynchos* seem to be comparable. I wrote to Terry as to why

WGAC hadn't considered this issue and the reason was that the checklists weren't in conflict. Terry included commentary from Birdlife-HBW: "Distinctive characters in subspecies *modesta* score highly against nominate *alba* and would result in treatment as a full species, but subspecies *melanorhynchos* is intermediate; all four taxa score fairly highly against each other, and work is needed in order to establish differences in display repertoire during breeding season." There seems to be little consideration of New World *egretta* here. There are two specimens of presumed *modesta* from the Aleutians and a photographed Great Egret with a black bill from the east coast in summer (seemingly precluding *egretta* so perhaps *modesta* or more proximal *alba*, even *melanorhynchos*.

Eaton, J.A., B.V. Balen, N.W. Brickle and F. E. Rheindt. 2016. Birds of the Indonesian Archipelago (Greater Sundas and Wallacea). Lynx Edicions.

Richardson, S. 1999. Intermediate Egret on Midway Atoll. North American Birds 53:441-443.

YES. I'm not sure if I would have voted for this split, but this is mainly an Old World issue and thus I am voting yes for global conformity as recommended in the proposal.

YES. I vote in favor, but with great reluctance. I'm not really seeing any quantitative evidence that these are actually separate species. If these were taxa that primarily occurred in our region, it would be a strong no vote from me until we see better (or any) data on species limits. I would especially want to see comparisons to species limits in other herons (of which there are many comparisons to be made), data from the zone of secondary contact (if any) between *plumifera* and *intermedia*, or genetic data of any kind. However, given that it's outside our area and only one taxon has occurred in North America, I suppose we should go along with other authorities. However, I hope that someone dedicates the time to a thorough study on this complex so that the taxonomic issues can be addressed with a foundation in data.

Medium Egret is kind of a silly name, but not more so than "intermediate", and I rather like it. I see no reason to select another name.

As noted by another committee member, there are two records of *intermedia* from the western Aleutians. The 1997 Midway Island record was re-identified as *Bubulcus ibis coromandus*. Photos are here: http://hbs.bishopmuseum.org/birds/rlp-monograph/HRBP-pages/03-PHAE-GRUI/ACEG-HRBP.htm There is, however, a more recent (2013) Midway Island record of what looks to me to quite clearly be an *intermedia,* that was accepted by Pyle and Pyle 2017: http://hbs.bishopmuseum.org/birds/rlp-monograph/HRBP-pages/03-PHAE-GRUI/INEG-HRBP.htm . I don't believe that the 2017 Kure Atoll bird has been reviewed by the Hawaii records committee, and it is still entered in eBird as a Great Egret. It does also look to me like *intermedia*, however.

If we're adopting this split, I agree that we should reconsider the species status of *Ardea alba egretta.*

Pyle, R.L., and P. Pyle. 2017. The Birds of the Hawaiian Islands: Occurrence, History,

Distribution, and Status. B.P. Bishop Museum, Honolulu, HI, U.S.A. Version 2 (1 January 2017) http://hbs.bishopmuseum.org/birds/rlp-monograph

YES. I agree with following the global consensus on these taxa which largely fall outside of the NACC's purview. I also agree with the WGAC members who voted in favor of the split, that the drastic differences in soft part colors during the courtship and breeding period likely act as isolating mechanisms (or at least contribute to isolation). I vote to adopt the name Medium Egret for *intermedia*.

YES. This has been obviously needed for a long time, although largely obscured by the similarity of birds in non-breeding soft-part colors (e.g., most of the time). (In fact Mees considered them all consubspecific, based on museum specimens!) The similarity of breeding soft-parts of African and Australian birds (widely separated by the very different Asian species) might lead to doubts but they differ in proportions and Australian *plumifera* has more developed plumes.

The English name Medium Egret, while not lovable, was chosen by Clements and IOC list-makers, rather than continuing to use the now-confusing name Intermediate Egret, as it really does fall in the middle size-wise between Little and Great, with which it everywhere co-occurs and is frequently seen. The name also parallels Large, Medium, and Small Ground and Tree-finches of the Galapagos, which no one seems to mind. The name Median Egret had long been used for *intermedia* sensu stricto, but it did not find favor among those queried about it.

AND, by all means, let's do something about American Great Egret! It differs strikingly in vocalizations from Old World birds, and according to a recent BirdForum discussion there are sequence data on GenBank showing a deeper divergence than between Old World taxa. Long overdue, but the reason we're not doing it now is that no one has published the split, and it's not an incongruence that WGAC has had to deal with yet.

YES. Like the others, I think we should follow the standard taxonomy used in the Old World. Not sure if the Tobias score paragraph in the proposal is based on published evidence. Although Medium Egret just sounds off to me, if this is what they are calling it in the parts of the World where it occurs, then I would prefer that.

YES. Adopt the new global taxonomy recognizing three species and the English name Medium Egret for *intermedia*.

YES. I agree to adopt the new global taxonomy for this complex. And retain the English name Intermediate Egret for *A. intermedia*.

2024-B-10
Treat Cattle Egret *Bubulcus* (or *Ardea*) *ibis* as two species

YES. I support the split of these two taxa. When I look at *coromandus* nearly every winter in Thailand I'm always reminded that they differ in a number of ways, notably that they seem longer necked and fly in a more relaxed manner (slower, less fluttery). In short, watching *coromandus* makes me more comfortable that it might belong in *Ardea* as opposed to nominate *ibis.*

The extent and color difference of the alternate plumage is certainly distinct as all have commented. It's pretty strikingly different. I would be interested to reconcile whether there is a difference in vocalizations, or not. This split seems more convincing to me than splitting the Intermediate Egrets. I'm not particularly troubled by *seychellarum* from the western Indian Ocean and suspect it is very close to nominate *ibis.*

As for English names, I'm not thrilled by Western Cattle-Egret and Eastern Cattle-Egret. I believe that African Cattle-Egret is a better name for the western taxon and sort of think of this species as "out of Africa." For *coromandus* I think Asian Cattle-Egret reflects their distribution. On the other hand it seems inappropriate for us to come up with different English names for birds that primarily are Old World species.

The subspecies *coromandus* has been definitely recorded once in North America, once, one found dead (female in alternate plumage) on Aggatu Island in the western Aleutians on Agattu, AK, on 19 June 1988 (Gibson and Kessel 1992, Gibson and Byrd 2007). Some on the committee might remember reviewing a published record of an Intermediate Egret (*Ardea intermedia*) from Midway Atoll on 25 June 1997. Some of us postulated that it might be a Cattle Egret instead, perhaps *coromandus.* The article (Richardson 1999) includes two color photos, one with a Laysan Albatross for size comparison. Perhaps these photos should be reviewed again to see if they can be identified to species.

Gibson, D.D. and B. Kessel. 1992. Seventy-four new avian taxa documented in Alaska 1976-1991. Condor 94:454-467.

Gibson, D.D. and G. V. Byrd. 2007. Birds of the Aleutian Islands, Alaska. Nuttall Ornithological Club and the AOU.

Richardson, S. 1999. Intermediate Egret at Midway Atoll. North American Birds 53:441-443.

YES. I would likely have voted no to split these taxa, but this is mainly an Old World issue and thus I am voting yes for global conformity as recommended in the proposal.

YES. I am again in favor of adopting the global consensus on these taxa. Even though *ibis* does occur throughout the NACC region, this is still largely an Old World issue, and I defer to those authorities.

YES. The two differ in so many ways—the lankier non-breeding Asian *coromandus* is very easy to confuse (and many have confused them in published photos) with Asian Intermediate, which would be much less likely with the compact African *ibis*. As for the breeding plumage differences, they are much greater than simply distribution of carotenoids, as the color and texture are both different. That said, there is poorly documented but seemingly likely evidence of hybridization in Seychelles and Chagos, but that can be expected when mateless birds end up on the same tiny islands.

I agree that the names Eastern and Western aren't great, but they have been in wide usage for a long time, and unless much better names are coined I'd stick with them. And anyway most people probably see them outside of Africa and maybe even Asia now.

YES. The split is primarily an Old World issue. Adopt the global taxonomy and recognize *B. coromandus* as a separate species from *B. ibis*. Use the English name Western Cattle Egret to agree with IOC and ebird/Clements lists.

YES. I agree to adopt the new global taxonomy for this complex. And use Western Cattle-Egret for *B. ibis* and Eastern Cattle Egret for *B. coromandus*.

YES. Like the others, I think we should follow the standard taxonomy used in the Old World. The overlap shown in morphological analysis is surprising given the quite different looks of the two taxa. Sometimes standard measures do not capture what can be very apparent differences.

YES. I am in favor of following the lead of the Eastern Hemisphere taxonomists as this is largely an extralimital issue for us.

YES. A somewhat reluctant Yes to this split. My decision is largely based on other comments regarding the description of differences and also to follow global lists for a species mostly outside our purview. However, the evidence here is not as strong as we would like to see when we make decisions like this.

NO. I have the same concerns as with the Intermediate Egret proposal, except here we do have both taxa occurring in North America, even if *ibis* is not native and *coromandus* is accidental. So, I think we should have a say in this instead of simply going along with global authorities. However, I acknowledge that this is primarily an issue outside of our area, that the split should probably be adopted.

The best evidence in favor of the split seems to be the extent of orange/buff coloration on the head and neck in breeding plumage, plus some minor differences in structure and possibly vocalizations. I am not aware of any other heron species that is recognized almost solely on the basis of the extent of a carotenoid-based color. The closest example I can think of are some species of *Ardeola*, but here the carotenoid differences are accompanied by multiple other characters that all point to species status.

There are photos online of breeding *Bubulcus* from the Seychelles that to me look like typical *ibis*, so if we consider those Indian Ocean populations to be *ibis,* then the overlap in the morphometrics is even greater (based on the PCA shown in the proposal). I see, however, that the Birds of the World account suggests that some of these Seychelles birds might be hybrids. If I'm not mistaken, these two taxa are also in secondary contact in the UAE and Iran. Any data on hybridization from this region seems critical to determining species limits. The conflicting evidence on morphometric and vocalization differences is also concerning, and clearly needs to be quantified. The one point that seems to me to point towards species status is the relative constancy of the head/neck color across the range of each taxon (i.e., not very clinal), but I'm not convinced that this one character is sufficient for species status.

If the proposal passes, I am in favor of the hyphenated names to indicate monophyly of the "cattle egrets", so Eastern Cattle-Egret and Western Cattle-Egret. However, African Cattle-Egret and Asian Cattle-Egret would I think be better names.

The Pyle monograph (Pyle and Pyle 2017) lists two records of *coromandus* for the northwestern Hawaiian Islands (Midway and Tern Island).

Pyle, R.L., and P. Pyle. 2017. The Birds of the Hawaiian Islands: Occurrence, History, Distribution, and Status. B.P. Bishop Museum, Honolulu, HI, U.S.A. Version 2 (1 January 2017) http://hbs.bishopmuseum.org/birds/rlp-monograph

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2024-B-11
Adjust the placement of the monotypic genus *Ectopistes* (Columbidae) in the linear sequence

YES. Reasons are given in the proposal. The available evidence, although not based on great genetic data, do suggest that *Ectopistes* is closer to *Patagioenas* than its current placement in the linear sequence. However, I don't think that this is the final word on the matter, and I suspect that better species-level and genomic sampling may change this arrangement at some point in the future. For now, we should place it in the position based on the best available data.

YES. This seems to make the most sense based on what is now known.

YES. Multiple lines of evidence suggest that *Ectopistes* is more closely related to *Patagioenas* than is reflected in our current sequence. Additional changes in sequence may be forthcoming, but this adjustment makes sense for now based on reasons given in the proposal.

YES. Given the recent genetic studies that consistently show *Ectopistes* as sister to *Patagioenas*, moving *Ectopistes* in the linear sequence to before *Patagioenas* is required by our guidelines.

YES. Great concordance between multiple studies bolstering this required change.

YES. Multiple phylogenetic studies suggest that *Ectopistes* is closely related to *Patagioenas* and therefore the linear order of the checklist should be rearranged to reflect phylogenetic relationships.

YES. Multiple phylogenetic studies support this adjust of the linear sequence of monotypic genus *Ectopistes* (Columbidae) close to *Patagioenas.*

YES. Evidence is quite strong that this move is required under our sequencing rules.

YES. Position needs to change based on multiple phylogenetic studies. Seems long overdue.

2024-B-12
Transfer *Burhinus bistriatus* (Double-striped Thick-knee) to new genus *Hesperoburhinus*

YES. I vote to recognize *Hesperoburhinus* for all the reasons given in the proposal. The very old node depth, much deeper than that dividing some bird *families*, almost necessitates a change. This is supported by there being at least a few autapomorphies despite the largely similar bauplan. It's outside our purview, but it does seem like *Esacus* should be merged into *Burhinus*. The homonym issues are unfortunate but can be sorted out.

YES. Hardly surprising given the fact that different continents are occupied.

YES. Reasons are given in the proposal, especially the depth of the split. This change also conforms to SACC and WGAC treatments.

YES. I am not a proponent of making higher level classification decisions on the basis of clade age (although the age of these groups is indeed remarkable given the relatively little morphological divergence, all things considered), but this split is also required on the basis of paraphyly with respect to *Esacus*.

YES. Given this surprisingly deep split, congeneric status cannot be sustained.

YES. Strong phylogenetic evidence support the New World thick-knees as a separate genus, *Hesperoburhinus*, both based on estimated divergence times and paraphyly of the genus *Burhinus*.

YES. I agree with the proposal.

YES. Deep genetic split and paraphyly with regard to *Esacus* require this move.

YES. For reasons stated in the proposal.

YES. I am voting yes due to paraphyly, age of the split and also this will conform with SACC.

2024-B-13
Revise the taxonomy of the Sharp-shinned Hawk complex: Split mainland *Accipiter velox* from Caribbean *A. striatus*

YES. In both the mtDNA and UCE data sets, there are plenty of genetic differences to indicate a long history of isolation. To me, that is enough to regard at least the Caribbean taxa as a whole as a separate species, despite the lack of vocal data. The data indicate that there has not been gene flow occurring between the islands and mainland for some time.

NO. Not at this time although I have often thought that the three Greater Antillean island isolates might be more distinct from mainland subspecies than mainland subspecies are from one another.

For me what is lacking in the proposal are vocalizations (perhaps because there aren't recordings of the West Indian subspecies archived?) of the three West Indian subspecies that can be compared to mainland subspecies. Within members of the genus *Accipiter* calls are important. For instance, calls of mainland Sharp-shinned Hawks sound very different from Cooper's Hawk. I must add that I heard a Sharp-shinned for the first time when I heard a circling adult Sharp-shinned calling over presumed breeding habitat in west-central Idaho. Surprising in that the species is basically common and widespread, at least in migration and winter. On the other hand, Gundlach's Hawk (*A. gundlachi*) from Cuba sounds nearly identical to Cooper's Hawk in my experience and I'll add the limited play-back I've found that Gundlach's Hawk responds better to Cooper's Hawk than with calls of Gundlach's. Given the apparent close similarities of Gundlach's and Cooper's, I've wondered if those two are better treated as subspecies of one another. This brings me back to wondering about whether one has recorded calls for any of the three Greater Antilles subspecies when they are in display flights. Calls of juveniles on or near the nest might also be useful to record. Recall too that vocal differences were one of the reasons for the split recently in Northern Goshawks (Old and New World).

Regarding Cuba where *fringilloides* is a rare resident, *velox* is a migrant and perhaps a winter visitor too. It has been recorded in some numbers at favored raptor migrant spots in the western part of the country. For instance, some 695 were tallied over Cabo de San Antonio in the fall of 2007 (Kirkconnell *et al.* 2020). I doubt if any of these migrants would pay much attention to resident birds. Other raptors (e.g. Mississippi and Swallow-tailed Kites) regularly migrate through Cuba, so migrant Sharp-shinned Hawks being there is not surprising. They are numerous enough on the Dry Tortugas off Key West, FL. I suspect that all three island isolated subspecies are threatened, if not endangered. In glancing through appropriate references, it is termed rare (Cuba) to uncommon and local in Hispaniola and Puerto Rico.

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

NO. As noted by another committee member, the proposal does not mention any data on vocal differences which would be important for understanding species limits in this complex. The plumage and morphological variation is interesting but not sufficient for species-level status. The UCE dataset is based on a small number of samples with none from Cuba and only one representative each of *ventralis*, *velox*, and *chionogaster*. More study is needed before adopting this change.

NO. The genetic differences are borderline for species status. I don't share the concerns of other committee members regarding insufficient genetic sampling. There are two Cuban samples that were sequenced for UCEs, although the proposal doesn't make it clear that these are from toe pads. Based on the available data, it does appear that the Caribbean clade is sister to the remainder of the Sharp-shinned Hawks, but the degree of genetic difference does not, to me, immediately indicate a species-level difference.

However, what tips the scales for me is the very similar breeding biology and courtship flights between Caribbean and mainland birds mentioned in the proposal. The proposal correctly states that there are few or no opportunities for interbreeding, which is of course the case for most allopatric populations of any species, so does not automatically imply species status. The occasional wintering *velox* in the Caribbean I don't think qualifies as range overlap, as (as far as I know) hawks do not initiate courtship in the winter. However, perhaps the lack of recent gene flow found by Catanach et al. should be given more consideration than I am allowing.

Another major concern for me is the lack of discussion of vocalizations, something that came up in the last Sharp-shinned Hawk proposal but hasn't yet been addressed. One of the issues then was a lack of recordings of Caribbean birds. Although an extremely low sample size, there are now three recordings of Sharp-shinned Hawks on the Macaulay Library from Puerto Rico, representing both the single call and the rapid "kek-kek-kek" call. According to Birds of the World these calls are used during courtship (the former) or by paired birds (the latter), so both should have a role in reproductive isolation. Although a very low sample size, these recordings to me sound identical to analogous recordings from the United States, Canada, and Brazil, which suggests conspecificity not just for the Caribbean birds but for the entire complex. I would like to see a formal analysis of these recordings, or even playback trials, both of which should be straightforward to do.

The plumage differences do seem to me to be the strongest evidence of species status for the Caribbean birds. The uniform orange throat and (depending on the taxon) fine gray barring below are quite different from that of *velox*. However, as with the recent proposal to split *chionogaster* from *velox*/*striatus*, it's not clear how relevant plumage differences are to species limits in this group given the high degree of plumage variation / polymorphism in the complex, and in other species of *Accipiter*.

NO. I vote no for now, but I am fairly sure that further research will lead to the recognition of multiple species in the *Accipiter striatus* complex, including the resident Caribbean group as one species. The situation is simply too poorly resolved at present.

NO. Vocal analyses are necessary to understand species limits in the *Accipiter striatus* complex, as well as phylogenetic analysis with greater representation of continental populations.

NO. Very complicated group due to color variation, and I think a formal study of vocalizations is needed.

NO. A difficult decision given what is known and not known. In favor of species status, the Caribbean birds are morphologically variable and definitely diagnosable from *velox*. Structural differences appear minimal, though the proposal does state that Caribbean birds are smaller. The Caribbean taxa form a monophyletic clade basal to other taxa in this complex and the genetic distance indicates a moderate level of genetic isolation. What is needed, however, is a mechanism that would create reproductive isolation (aside from no geographic overlap). The small amount of data on vocalizations, stated by Oscar above, shows extreme similarity between Caribbean taxa and *velox*. Courtship behavior also seems similar. My feeling is that we should not change the status quo until more data are available, especially on courtship and vocalizations.

NO. This is a complicated issue that needs more data with better sampling and a detailed, quantitative analysis of phenotypic variation before we can render a verdict.