

NACC Proposals 2007-C

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Change English name of the *Colibri* from Violet-ear to Violetear

Proposal: This proposal, if approved, would result in the name "Violet-ear" for the *Colibri* hummingbirds becoming "Violetear".

Discussion: Warning: this proposal is likely to induce sleep, but bears consideration. It is brought to seek approval or rejection of an English name, the usage of which appears to show considerable geographical variation. Having worked principally in Colombia, I was surprised to need to insert a hyphen for these bird names when recently conforming a checklist to SACC standard. There are two issues here: (i) extent of usage, and (ii) consistency with other bird names.

(i) Extent of usage.-- The term "Violet-ear" is used for the *Colibri* by most North American texts (e.g. AOU list), many international texts (e.g. Sibley & Monroe 1990; Schuchmann 1999; Clements 2000; Dickinson 2003) and in Central America's leading bird guide (Howell & Webb 1995). However, in some parts of the Neotropics, particularly northern South America, the term "Violetear" (non hyphenated) is arguably the more commonly used name (see e.g. Hilty & Brown 1986; Rodner et al. 2000; Salaman et al. 2001; Stiles et al. 2000; Ridgley & Greenfield 2001 and Hilty 2003; though e.g. Meyer de Schauensee 1970 & 1974 and Dunning 1987 each use "violet-ear").

A GOOGLE search suggests this pattern of usage when one considers the geographical range of each species:

Brown Violetear 308 - 663 Brown Violet-ear (*Colibri delphinae*)
 Green Violetear 860 - 26,200 Green Violet-ear (*Colibri thalassinus*)
 Sparkling Violetear 600 - 594 Sparkling Violet-ear (*Colibri coruscans*)
 White-vented Violetear 196 - 1,030 White-vented Violet-ear (*Colibri serrirostris*).

Overall, it would seem that the Violet-ears have it, although the predominantly northern Andean species *C. coruscans* is majority used without a hyphen.

(ii) Consistency with other bird names.-- Most Neotropical birds with a compound name and a hyphen include a modifier and a name of a type of bird: e.g. Wood-Quail, Yellow-Finch, Wood-Rail, Water-Tyrant (and other tyrant modifiers: see proposal 187), Tit-Spinetail, Tiger-Heron. However, almost all Neotropical birds with a compound name relating to a bird body part do not include a hyphen: e.g. Avocetbill, Bentbill, Blossomcrown, Conebill, Goldentail, Goldenthrout, Greytail, Helmetcrest, Hookbill, Jewelfront, Lancebill, Metaltail, Pintail, Plushcrown, Redstart (Whitestart), Sabrewing, Sapphirewing, Scythebill, Softtail, Starthroat, Spadebill, Thornbill, Thorntail, Tuftedcheek and Yellowthroat. An exception to this is where the compound word would be confusing, obviously "Bare-eye" and

"Fire-eye" being better than "Bareeye" and "Fireeye" respectively and "Racket-tail" used rather than "Rackettail" presumably for the same reason (though n.b., somewhat inconsistently, "Softtail"). In each of these cases, the second word after the hyphen is lower case, so that the bird is not indexed e.g. as an "eye" but as a "bare-eye". I would suggest that "Violetear" is not as confusing as any of the other hyphenated body part bird names mentioned (and "Starthroat" is arguably much worse), but if one looks at "Violetear" with squinted eyes, it could perhaps be pronounced in a different way.

References not on SACC list:

Salaman P., Cuadros T., Jaramillo J. G. & Weber W. H. 2001. Lista de Chequeo de las Aves de Colombia. Sociedad Antioqueña de Ornitología, Medellín, Colombia, 116 pp.

Stiles, F. G., Bohórquez, C. I., Cadena, C. D., de la Zerda, S., Hernández, M., Rosselli, L., Kelsey, M., Valencia, I.D. & Knapp, D. Aves de la Sabana de Bogotá, Guía de Campo. Asociación Bogotana de Ornitología, Bogotá, 276 pages.

Conclusion: I have no strong views on this (admittedly) not very exciting proposal. Thank you for reading this far if you have! A "Yes" vote would be to change to "Violetear". A "No" vote would endorse "Violet-ear".

Thomas Donegan

Comments from Stiles: "YES. Actually, the best point is that body-part names be unhyphenated on principle unless a double-vowel or other combination seems likely to produce confusion or unpronounceable monsters."

Comments from Zimmer: "YES, and echo the comments by Gary concerning the desirability of formalizing a non-hyphenated construction except in cases such as "Bare-eye", when there is obvious potential for confusion in pronunciation when the hyphen is removed."

Comments from Nores: "YES. Me parece bien uniformar los nombres compuestos, especialmente en los casos como este en donde las excepciones están justificadas por problemas de pronunciación."

Comments from Pacheco: "YES. Concordo com a proposta e com as opiniões de aprovação."

Comments from Remsen: "YES. One less annoying hyphen to worry about, and as Donegan notes, removal of hyphen is more consistent with other similar

English names. I think the danger of mispronunciation as "Viole Tear" is minimal, certainly not worth the hyphen."

Comments from Jaramillo: "YES - Attempting to be consistent on this issue of not hyphenating names that refer to body parts is a good idea."

Note from Remsen (8/21/07): This proposal passed SACC 7-2, and is submitted with T. Donegan's permission and with the goal of making SACC-NACC lists more uniform. Gill-Wright list also removes the hyphen. If anyone needs Spanish or Portuguese translation of SACC comments, let me know.

Submitted by Remsen from SACC, 21 Aug. 2007

Resurrect the monotypic genus *Helicolestes* for the Slender-billed Kite

Effect on South American CL: Slender-billed Kite and Snail Kite are currently treated as congeners in *Rostrhamus*. This proposal is for splitting them into two monotypic genera, *Helicolestes* for Slender-billed Kite and *Rostrhamus* for Snail Kite.

Background: The Slender-billed Kite was first described by Temminck in 1821 as *Falco hamatus*. In one early treatment, Sharpe (1874) included it in *Rostrhamus*, along with the Snail Kite *R. sociabilis*, and a third species based on an immature plumage of Slender-billed Kite. Bangs & Penard (1918) cleared up the mess by describing adult and immature plumages of the Slender-billed Kite and erecting a new monotypic genus for the species (*Helicolestes*), a treatment followed by many authors (e.g., (Peters 1931, Hellmayr & Conover 1949, Meyer de Schauensee 1970). The merger of *Helicolestes* and *Rostrhamus* (with the latter taking priority) was proposed just over 40 years ago by Amadon (1964), and followed by most subsequent authors (e.g., Stresemann & Amadon 1979, Thiollay 1994, Ferguson-Lees & Christie 2001), although usually with misgivings.

Analysis: Amadon's taxonomic revision (found here: <http://digitallibrary.amnh.org/dspace/handle/2246/3352>) rests on three short paragraphs containing a qualitative analysis of specimens. After describing a few similarities, chiefly the bill design and the adaptation to the middle toe, Amadon pronounced that "After careful consideration, my opinion is that the clear-cut affinity of these two species is of more importance than their differences." In the following sections I use points of similarity and difference between *hamatus* and *sociabilis* to arrive at the opposite conclusion.

Morphology: At a glance, the heads of the two species are very similar in shape and color pattern. The most impressive similarity is the bill, but even this feature is not the same: in *hamatus* "the upper mandible is even thinner and more decurved" (Amadon 1964). Aside from their similar coloration, Amadon (1964) also noted that they overlapped even in such a minor adaptation as "a pectinate flange on the inner edge of the middle claw", which they presumably use to remove slime from plumage.

Given their overlapping taste for freshwater snails, it need hardly be pointed out that bill and toe morphology may have resulted as much from convergence as from shared evolutionary ancestry. The same pectinate flange is found in unrelated fish-eating birds, e.g. Ardeidae.

Balanced against these overlaps, there are striking differences in the proportions of wing and tail: "When seen in life, *Helicolestes* shows no similarity to *Rostrhamus*, for its short tail and broad wings make it look like a buzzard (*Buteo*),

while the long tail and wings of *Rostrhamus* suggest a harrier (*Circus*)" (Haverschmidt 1959). These differences tally with distinct flight actions. In normal flight *hamatus* flaps with a rapid series of stiff-winged beats between glides, quite like a Roadside Hawk (*Buteo magnirostris*). This could scarcely be more different from the familiar rangy flight of Snail Kites, with their loose, maneuverable wings, and tilting tails. In addition, *hamatus* regularly soars on wide-open wings, whereas *sociabilis* soars much less readily, and then with wings bowed.

The distinctive, harrier-like plumage of female and juvenile Snail Kites is well known. Unlike adult males they are mostly brown, with streaked or blotched underparts. By contrast, female Slender-billed Kites are similar to males, and juveniles are similar to adults, differing only in the slight buff barring or tipping to scapulars and banding to the tail (Ferguson-Lees & Christie 2001).

Voice: The main call of the Snail Kite is an odd clicking "krikik-ik-ik, ik, ik, ik" (Ferguson-Lees & Christie 2001), versions of which are used in social interactions and alarm. Males apparently also give a squawked "koreea; ker-wuck ker-wuck" in a variety of contexts: at roosts, in aerial displays, and during social interactions (Ferguson-Lees & Christie 2001). The Slender-billed Kite seems to have one main call, which is a very different, kazoo-like mewing (Beissinger et al. 1988, Ferguson-Lees & Christie 2001), much more readily overlooked as a Roadside Hawk (*B. magnirostris*) than a Snail Kite.

Behaviour: Courtship display of the Slender-billed Kite consists of "a series of repeated swooping dives in which the bird would fold its wings and plummet about 5 m before opening them again and rising" (Beissinger et al. 1988). This buteonine display differs from the display of the Snail Kite, which involves butterfly-flight elements (Ferguson-Lees & Christie 2001).

The nest of the Snail Kite is made of twigs, reeds etc., and usually placed less than 2 m up, non-arborescally, and always close to water (Ferguson-Lees & Christie 2001). That of the Slender-billed Kite is arboreal, usually placed in the canopy of swamp forest or forest near wetlands (Beissinger et al. 1988, Ferguson-Lees & Christie 2001, Greeney et al. 2004). Haverschmidt (1959) found one in the crown of a large *Ceiba pentandra*, describing a bulky *Buteo*-like nest made of dead sticks. Unlike *sociabilis*, nests of *hamatus* are "well dispersed" (Beissinger et al. 1988).

Although both species eat large freshwater snails, their hunting methods are different, as would be expected from their morphology. *Rostrhamus* quarters like a harrier, catching snails in flight, whereas *Helicolestes* sits motionless and pounces on snails from low branches.

The Snail Kite often travels, feeds and roosts in groups, sometimes as large as 1,000 individuals, and it regularly nests in scattered colonies of up to 100 pairs;

the Slender-billed Kite is essentially solitary or pair-living, especially when nesting (Ferguson-Lees & Christie 2001).

In conclusion, it appears that (1) the differences between the two species are many and varied, and (2) their main similarities are associated with specialist prey. Given the fair chance that the bill and middle toe are homoplasies, an overall similarity in plumage and bare parts colors is not sufficient evidence for Amadon's merger. It is not impossible that the rarer Slender-billed Kite is mimetic, achieving some advantage via its apparent similarity to the Snail Kite. Moreover, I can think of no other raptor genus, worldwide, that embraces the scale of differentiation found in *Rostrhamus* as currently composed. There are so many differences, including in that powerful evolutionary clue, the voice, it would not be surprising if genetic studies revealed that they were not each other's closest relative.

Recommendation: The key point is that the original merger was poorly substantiated. I advocate a return to an earlier, long-established treatment, which better reflects the unique characters of these two species. I recommend a YES vote to this proposal, which would return the Slender-billed Kite to the monotypic genus *Helicolestes*.

References

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Thiollay, J. (1994) Family Accipitridae (hawks and eagles). Pp. 52-105 in J. del Hoyo, A. Elliott, and J. Sargatal editors), Handbook of the Birds of the World. Vol. 2. New World Vultures to Guineafowl. Lynx Edicions, Barcelona.

Joe Tobias, January 2006

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Comments from Remsen: "YES. Although no new data per se have been published, the Amadon rationale would fall far short of sufficient for a merger of two genera today. Regardless of whether these two birds are really sister taxa, I would rather return to the previous classification than follow Amadon's weakly supported opinion. After becoming familiar with *Helicolestes* near Leticia, I was mystified by the post-Meyer de Schauensee merger into *Rostrhamus*. If genetic data corroborate their sister relationship, I would be tempted to vote to retain both in monotypic genera for the reasons outlined by Tobias unless the genetic distance between them is short."

Comments from Stiles: "YES. Birds as a group are replete with examples of impressive convergences related to similar feeding habits - and the more socialized the habits, often the more impressive the convergences. Hence, I am not surprised that a careful analysis of other, non-feeding adaptations of the species point to separating them (again). Certainly my own experience with both species convinces me that they are very different critters!"

Comments from Zimmer: "YES. Despite the lack of any new published analysis, I vote YES because it overturns a poorly supported revision by Amadon. Structural, vocal and ecological differences (along with lack of sexual dimorphism in plumage characters in Slender-billed Kite) all support the conclusion that these are two very different beasts, and that convergence of plumage characters and bill morphology are exactly that."

Comments from Robbins: "YES. Although no new published data are presented in this proposal the original rationale for generically merging these two is weak and the reinterpretation is enough to persuade me to vote "yes". Rob Fleischer (pers. comm.) has a preliminary molecular data set that has both of these represented. Hopefully, we see this in print soon."

Comments from Silva: "YES, let us be back to the original taxonomic situation before Amadon. However, we should take in consideration Van's viewpoint about molecular data. If these species are monophyletic, I will vote for combining them into a single genus."

Comments from Pacheco: "YES. Em minha experiência, com ambos os táxons, também considero bastante plausível a sugestão de convergência morfológica em lugar de genuíno parentesco."

Comments from Nores: "NO. Aunque los fundamentos dados por Tobias son bien fundamentados, pienso que no son suficientes cuando las diferencias son a nivel de género. Existen muchos géneros en los cuales las especies tienen diferentes comportamiento, canto, silueta, dimorfismo sexual, etc. Así por ejemplo, *Crotophaga*, *Caprimulgus*, *Melanerpes*, *Upucerthia*, *Thamnophilus*, *Saltator* tienen diferentes tipo de cantos entre las especies. También hay muchos géneros de aves con diferente proporción de alas y cola entre las especies. Por ejemplo *Buteo* (compare *leucorrhous* con *brachyurus*), *Zenaida*, *Tyrannus*, etc. Yo no veo que *Rostrhamus* se parezca mucho a *Circus*; compare en la guía de aves argentinas de Narosky e Yzurieta la silueta de *Rostrhamus* con la de *Circus buffoni* (ambas son tomadas de fotografías). También en muchos géneros hay diferencias entre las especies en relación al color de macho y hembra. En *Molothrus bonariensis* por ejemplo el macho es negro violáceo y la hembra gris y el joven es similar a la hembra, mientras que en *M. rufoaxillaris* ambos sexos son negros y el joven es similar a *M. (Agelioides) badius*. Otros ejemplos serían *Anas*, *Knipolegus*, *Progne*, etc. Con más razón en comportamiento, tipo de nido y forma de alimentarse. Por citar sólo un ejemplo de cada cosa, menciono el caso de *Mimus saturninus* que durante el display hace círculos completos en el aire, cosa que no hacen las otras especies; *Anas flavirostris* nidifica en huecos de rocas o en nidos de cotorra (*Myiopsitta monachus*) veces a gran altura mientras que la mayoría de los otros patos lo hacen en el suelo y *Falco peregrinus* captura aves en vuelo, mientras que *Falco sparverius* captura grandes insectos, roedores y lagartos en el suelo."

Comments from Jaramillo: "YES - If genetic data clarifies that these are sister taxa, I would still feel comfortable retaining two genera for these very different species."

Note from Remsen (8/21/07): This proposal passed SACC 8-1, and is submitted with J. Tobias's permission and with the goal of making SACC-NACC lists more uniform. If anyone needs Spanish or Portuguese translation of SACC comments, let me know.

Split *Gypopsitta* from *Pionopsitta*

Effect on NACC: This proposal would remove two species (*haemototis* and *pyrilia*) from current *Pionopsitta* and place these in a separate genus, *Gypopsitta*.

Background: Traditional classifications (e.g., Peters 1937, Pinto 1937, Phelps & Phelps 1958a, Meyer de Schauensee 1970, Forshaw 1973) treated *Pionopsitta vulturina* in a separate monotypic genus, *Gypopsitta*, but Haffer (1974), Cracraft and Prum (1988), and others noted that *vulturina* was clearly a member of *Pionopsitta* on the basis of biogeography and plumage pattern. SACC classification treats *vulturina* as a member of *Pionopsitta* on this basis.

New information: Ribas et al. (2005) found, using mtDNA sequence data (2181 bp), that *Pionopsitta* as currently constituted is not monophyletic. The type species for the genus, extralimital *pileata*, is only distantly related to the others, which in fact are more closely related to, for example, *Hapalopsittaca*, *Amazona*, *Graydidascalus*, *Pionus*, and even *Triclaria* than to *Pionopsitta pileata*. Bootstrap and Bayesian posterior probabilities for the nodes in their tree are generally high. Rather than expand *Pionopsitta* to include a broad array of parrot genera, Ribas et al. (2005) resurrected *Gypopsitta* for the other species.

Analysis: Haffer (1974) and others long ago recognized that *P. pileata* was the "oddball" in the genus, and so the genetic data are not surprising in corroborating this. What is surprising is how distant *P. pileata* is from the *Gypopsitta* group, and how far one would have to expand *Pionopsitta* to retain it as a monophyletic group, e.g., minimally all the genera mentioned above. Although the genetic analysis covered only two mtDNA genes, it is difficult to imagine that mtDNA would be this misleading at this level (intergeneric) of analysis. Comforting is that formal analysis of plumage characters (Cracraft and Prum 1988) shows that *pileata* is the outgroup to the rest; in fact, with the benefit of hindsight, if you look at specimens, you wonder why *pileata* was ever associated with the *Gypopsitta* group. [Perhaps someone could check Bret et al.'s parrot CD for vocal information.]

Recommendation: YES. There are no data of which I am aware that support the continued association of *Pionopsitta pileata* with the *Gypopsitta* group, and so to continue to treat them as congeners would be highly misleading.

Literature Cited (see SACC Biblio for the rest):

CRACRAFT, J., AND R. O. PRUM. 1988. Patterns and processes of diversification: speciation and historical congruence in some Neotropical birds. *Evolution* 42: 603-620.

RIBAS, C. C., R. GABAN-LIMA, C. Y. MIYAKI, AND J. CRACRAFT. 2005. Historical biogeography and diversification within the Neotropical parrot genus *Pionopsitta* (Aves: Psittacidae). *Journal Biogeography* 32:1409-1427. □

V. Remsen, August 2007

Note from Remsen: This proposal is modified from SACC proposal #212, which passed SACC 9-0. If anyone needs Spanish or Portuguese translation of SACC comments, let me know. To conform with SACC linear sequence (explained in proposal #213, we should reverse *pyrilia* and *haematotis* in our sequence.

Comments from Stiles: "YES. The genetic (as well as morphological) data clearly mandate this change, supported by two different studies and several genes (pity that *Gypopsitta* is a bit misleading as a name for most species, but priority rules!)."

Comments from Robbins: "YES. The Ribas et al. (2005) genetic data and their taxonomic recommendations of resurrecting *Gypopsitta* seem solid. I vote 'yes'."

Comments from Nores: "YES. Es muy claro en el análisis de Ribas et al. (2005) que *Pionopsitta pileata* está muy poco emparentada con el resto de las especies que proponen poner en el género *Gypopsitta*. Esto también resulta evidente cuando se comparan las especies desde el punto de vista morfológico, principalmente la coloración."

Comments from Zimmer: "YES. The evidence presented is convincing. As Gary notes, it is unfortunate that priority dictates the name *Gypopsitta*, which is great for two of the species but doesn't hold for the others. In response to Van's query about vocalizations of *pileata* versus the other species currently in *Pionopsitta*: *pileata* is quite different from all of the others with which I'm familiar, whereas *vulturina*, *aurantiocephala*, *barrabandi* and *caica* all have some similarities with one another."

Comments from Jaramillo: "YES - Change clear based on genetic data and morphological data, also unpublished vocal data fide Zimmer."

Comments from Pacheco: "YES. Os dados disponibilizados por Ribas et al. (2005) são plenamente convincentes para a adoção do arranjo proposto."

"É oportuno mencionar que um trabalho independente chegou a conclusões semelhantes:

"Eberhard, J. E. & Bermingham, E. 2005. Phylogeny and comparative biogeography of *Pionopsitta* parrots and *Pteroglossus* toucans. *Mol. Phyl. & Evol.* 36: 288304."

Change English name of *Brotogeris versicolurus* from White-winged to Canary-winged Parakeet

This one is confusing. SACC started with Canary-winged because it used Dickinson (2003) as its starting point. So, I submitted a proposal (pasted in below) to change this to "White-winged", as in AOU (1998). Somewhat to my surprise, the proposal narrowly failed, 4-3 (5-2 would have been needed to pass). So, this creates a difference between NACC and SACC. Therefore, I submit a proposal to change the AOU (1998) name for introduced *B. versicolurus* from "White-winged" to "Canary-winged" to conform to SACC.

Proposal (#224) to South American Classification Committee

Change English name of *Brotogeris versicolurus*

Effect on SACC list: If accepted, this would change the English name of *Brotogeris versicolurus* to the one used in AOU (1998).

Background: The English name of *Brotogeris versicolurus* has traditionally been "Canary-winged Parakeet." With the taxon *chiriri* now generally ranked (but see Juniper & Parr 1998) as a species ("Yellow-chevroned Parakeet") rather than as a subspecies of *B. versicolurus*, some authors (e.g., AOU 1998, Ridgely & Greenfield 2001, Forshaw 2006) have changed the English name of the latter to "White-winged Parakeet", whereas others retained "Canary-winged" for narrowly defined *B. versicolurus* (e.g., Sibley & Monroe 1990, Sick 1993, Collar 1997, Rodner et al. 2000, Dickinson 2003).

Analysis:

Reasons for changing SACC list to "White-winged" are as follows:

1. It is typically a wise move to create new English names for all the taxa newly ranked as species from a more broadly defined previous "concept" rather than use the name formerly applied to the more broadly defined species; this prevents confusion by emphasizing the new species limits. [The exceptions have usually been in those cases in which the split refers only to a marginal, small population (such as Red-shouldered Blackbird split from widespread Red-winged Blackbird)]. This was the rationale for the AOU's (1998) use of White-winged for *B. versicolurus* once *chiriri* split from it.
2. "White-winged" does describe the most obvious plumage difference between *versicolurus*, which has conspicuous white inner primaries and secondaries, and *chiriri*, which has green and blue remiges.

3. It is reasonable to expect North American and South American AOU lists to match, particularly in subjective matters.

Reasons for NOT changing to "White-winged" are as follows:

1. Several widely used references, including HBW and Dickinson (2003), stuck with "Canary-winged", presumably because it is a long-established name (since at least Meyer de Schauensee 1966) with some "personality."

2. "Canary-winged" is still accurate, because the secondary coverts of *versicolorus* are conspicuously bright canary yellow, slightly more extensively than those of *chiriri*. [In fact, in my opinion, the mix of white and yellow enhances the appeal of the name, given the way cagebird canaries often have this same mix.]. Further, note that earlier in the century, when *chiriri* was considered a separate species, Cory (1918) used the English name "Yellow-winged Paroquet" for *B. virescens* (= *versicolorus*), so the emphasis on yellow in the English name can't be grossly inaccurate.

3. If SACC stays with "Canary-winged", I strongly suspect North American AOU would follow SACC, given that this is strictly an exotic species in North America, and that "White-winged" is a recent invention with little historical momentum.

Recommendation: I have none. Either one is fine with me. I'll vote depending on comments received.

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- SIBLEY, C. G., AND B. L. MONROE, JR. 1990. Distribution and taxonomy of birds of the World. Yale University Press, New Haven, Connecticut.

SICK, H. 1993. Birds in Brazil. Princeton Univ. Press, Princeton, New Jersey.

Van Remsen, June 2006□

[Note: Thanks to Johan Ingels and Ian Paulsen for help with references.]

Addendum from Remsen: Gill and Wright (2006) use "White-winged" and "Yellow-chevroned."

Comments from Steve Hilty: "As an interested but non-voting observer I offer the following perspective on English names for the parakeet *Brotogeris versicolurus* and allied *B. chiriri*.

"There's no doubt the name "Canary-winged" has more cachet than the name "White-winged." On the other hand, both names have been used for *versicolurus* with White-winged gaining acceptance since *chiriri* was split off and called Yellow-chevroned. In the field, when this bird is flying, it's the "white" that is obvious, much more than the narrow yellow wing covert band. Secondly, calling one species Canary-winged and the other Yellow-chevroned invites confusion because both names mean "yellow" and that doesn't help to distinguish these two birds. *Versicolurus* is mainly white-winged, *chiriri* is the one that's yellow (or golden) and this is easy to remember, a distinction that is genuinely helpful to English-language users of these names. I think the AOU, and Ridgely and others who have been using White-winged, even if it's less imaginative and with less history, have got it right.

"On the other hand, if the two populations *versicolurus* and *chiriri* are merged, then Canary-winged is the name with the greatest longevity and probably the most appropriate one to use. I might also add that it's a shame, perhaps, that the name Canary-winged couldn't have been transferred to the other species (*chiriri*) because the English name, Yellow-chevroned is awkward and, to me, sounds contrived. Nevertheless, I wouldn't change this one either."

Comments from Zimmer: "NO". Van makes a good point regarding the combination of yellow and white on the wing of *versicolurus* being reminiscent of canaries. This, combined with the distinctiveness of the name (not just another combination of a color and a body part), the fact that it predates "White-winged" and the fact that some major references, including HBW, have stuck with "Canary-winged", in my opinion, trumps the argument of the need to concoct a new name for both of the split forms (a practice that I am generally in favor of)."

Comments from Jaramillo: "YES - change to White-winged Parakeet, I think if at all possible giving both members of a split a new name from the inclusive species is a good move (adjusting for the issues mentioned by Van in the proposal). So I am in favour of this, and in addition White-winged has quickly gained favor in the

last few years so it does not seem to be causing much confusion out there. In fact retention of Canary-winged for *versicolorus* would be more confusing for the average user of English names at this point. I don't see any reason to stand in the way of this change, name stability and priority of names are concepts generally applied to decrease confusion. In this case, the new name White-winged is the one that decreases confusion for users, so in the "spirit but not the letter" of the concepts I think it is a good move to change the name of *versicolorus*."

Comments from Stiles: "YES. The name "White-winged" is much more descriptive and accurate; it's what one immediately notices in the field and it is a feature unique to *versicolorus* among all the parakeet-types I know. Leave "Canary-winged" for the more inclusive species if one prefers not to split off *chiriri*."

Comments from Robbins: "YES. On the whole White-winged Parakeet seems like a better name for *Brotogeris versicolorus*."

Comments from Stotz: "NO. I guess I have always thought of the Amazonian population as the Canary-winged Parakeet, even when *chiriri* was part of it. Yes, it has white in the wing, but it also has yellow, and as Van notes this really does give the impression of a Canary. I can't see that any confusion is engendered by maintaining the historical name Canary-winged."

Comments from Remsen: "NO. Just can't see dumping one of the better English names."

Submitted by Remsen
21 Aug. 2007

Sequence of genera and subfamilies in Furnariidae

Effect on AOU CL: This would change slightly the sequence of genera in the Furnariidae and divide the family into three subfamilies.

Background: In a previous proposal, we merged the family Dendrocolaptidae into the Furnariidae but placed the dendrocolaptids last in the sequence without any subfamily designations. The main reason for doing this was that independent data sets (Irestedt et al. 2002, 2006, Chesser 2004) indicated that the old Furnariidae was a paraphyletic family with respect to Dendrocolaptidae because *Geositta* (extralimital) and *Sclerurus* are basal to all other ovenbirds + woodcreepers.

Analysis and Proposal: The data sets above, although limited in their taxon sampling, yielded strong support for three major lineages within a broadly defined Furnariidae: (1) *Sclerurus* + *Geositta*; (2) remaining furnariids, and (3) traditional dendrocolaptids. Chesser (2004) analyzed beta fibrinogen (a nuclear gene) sequences and found strong support in both maximum parsimony and maximum likelihood analyses for the groupings above. Fjeldså et al. (2005) and Irestedt et al. (2006) analyzed both mitochondrial and nuclear gene (myoglobin, not b-fib) sequences, with similar strong support for the three groups, except that the furnariid genus *Xenops* fell within the dendrocolaptids in two of their three analyses. However, an analysis of a combined dataset of ca. 6000 bp indicates that *Xenops* is basal in the Furnariinae and is not a dendrocolaptine (R. Brumfield, pers. comm.).

For our classification to reflect these phylogenetic data, I propose that we recognize the three major groups as subfamilies: Sclerurinae (*Sclerurus* was described before *Geositta*), Furnariinae, and Dendrocolaptinae. The latter two names are used by some earlier classifications that considered the two dendrocolaptids are members of the Furnariidae.

Our linear sequence can stay intact except that *Sclerurus* should be moved to the front. Because of the uncertainty of the position of *Xenops*, I suggest that we leave it in Furnariinae until additional analyses are published, but place it last ... thus next to Dendrocolaptinae in the linear sequence. Thus the structure would look like this:

Furnariidae

 Sclerurinae

Sclerurus

 Furnariinae

 (all other furnariid genera as is but with *Xenops* moved to the end)

Dendrocolaptinae
(all dendrocolaptid genera)

Recommendation: Although additional taxon and gene sampling may alter the position of some genera, especially *Xenops*, this sequence reflects data from two independent analyses and seems unlikely to change substantively. So, I recommend a YES on this one.

References:

CHESSER, R. T. 2004b. Molecular systematics of New World suboscine birds. *Molecular Phylogenetics and Evolution* 32: 11-24.

FJELDSÅ, J., M. IRESTEDT, AND P. G. P. ERICSON. 2005. Molecular data reveal some major adaptational shifts in the early evolution of the most diverse avian family, the Furnariidae. *J. Ornithology* 146: 1-13.

IRESTEDT, M., J. FJELDSÅ, AND P. G. P. ERICSON. 2006. Evolution of the ovenbird-woodcreeper assemblage (Aves: Furnariidae) - major shifts in nest architecture and adaptive radiation. *J. Avian Biology* 37: 260-272.

IRESTEDT, M., J. FJELDSÅ, U. S. JOHANSSON, AND P. G. P. ERICSON. 2002. Systematic relationships and biogeography of the tracheophone suboscines (Aves : Passeriformes). *Molecular Phylogenetics & Evolution* 23: 499-512.

Van Remsen, August 2007

Note: a version of this proposal passed SACC 7-0, with following comments:

Comments from Stiles: "YES. I am not unhappy with Sclerurinae - if you paint a *Geositta* brown you have a leaf-tosser. The differences in color and habits of these two genera seem like obvious adaptations to different habitats that the birds occupied on opposite sides of the Andes as these were uplifted. I rather suspect that the "original" color scheme was *Sclerurus*-like and these birds simply stuck to their lowland forest floors and did not climb far into the nascent Andes, while the miners did and wound up on the other side (such speculations aside, I see no problems with this proposal)."

Comments from Robbins: "YES. Based on the published data this proposal seems quite logical and is well supported."

Comments from Zimmer: "YES. Additional taxon sampling may lead to refinements, but on the currently available data, this arrangement seems to make the most sense."

Comments from Nores: "SI, aunque yo no estoy tan convencido como Gary de que pintar una *Geositta* de marrón uno tiene un *Sclerurus*. A mi, como a la mayoría de los autores que han visto el tema, no se me hubiera ocurrido nunca

que *Geositta* y *Sclerurus* estuvieran emparentados. Tanto es así, que en todos los trabajos previos, *Geositta* está al comienzo de Furnaridae y *Sclerurus* al último. No obstante, ante la evidencia molecular (si está bien hecha) no queda otra posibilidad que aceptar la relación entre ambos géneros."

Comments from Jaramillo: "YES - I agree with Manuel on *Geositta* being a bit more than pale *Sclerurus*, I would have bet \$\$ that they were sister to *Upucerthia*! But then again, I have been wrong sooo many times before. But to the point of this proposal, I do think the new arrangement is a good one, consistent with the best available data."

Submitted by Remsen
21 Aug. 2007

Recognize the genus *Epinecrophylla* (Thamnophilidae)

Effect on NACC: This would recognize a newly described genus for several species currently classified in *Myrmotherula*.

Background & New information: Tiny, short-tailed thamnophilids have all been placed in a single genus, *Myrmotherula*, due to overall similarities in size and shape. Hackett & Rosenberg (1990), using protein electrophoresis, found the first evidence that the genus was not monophyletic. One of the groups that they identified as distinct from true *Myrmotherula* was the stipple-throated group, which share plumage similarities (Hackett & Rosenberg 1990), foraging similarities (dead-leaf specialists; Remsen & Parker 1984), and vocal similarities (Zimmer & Isler 2003). The genetic data of Irestedt et al. (2004), although very limited in its taxon sampling, also indicated that *Myrmotherula* was not a monophyletic group. Unpublished data from Robb Brumfield's ongoing phylogenetic analysis of the family, including 6 species from the stipple-throated group plus 12 other *Myrmotherula*, found that the two groups were not sisters (Isler et al. 2006).

Isler et al. (2006) named a new genus, *Epinecrophylla*, for the stipple-throated species, to include the following species:

fulviventris

and 7 endemic to South America:

gutturalis
leucophthalma
haematonota
fjeldsaai
spodionota
ornata
erythrura

Analysis and Recommendation: I vote YES because at this point there are no data other than vague morphological similarities to support the monophyly of *Myrmotherula*. Thus, with two independent genetic data sets that show that *Myrmotherula* is not monophyletic, and three phenotypic data sets that are consistent with the stipple-throated group as being a separate, monophyletic group, recognizing *Epinecrophylla* as a genus is the appropriate solution in terms of classification.

References:

HACKETT, S. J., AND K. V. ROSENBERG. 1990. Comparison of phenotypic and genetic differentiation in South American antwrens. *Auk* 107: 473-489.

IRESTEDT, M., J. FJELDSÅ, J. A. A. NYLANDER, AND P. G. P. ERICSON. 2004. Phylogenetic relationships of typical antbirds (Thamnophilidae) and test of incongruence based on Bayes factors. *Bio Med Central Evolutionary Biology* 4: 23.

ISLER, M. I., D. R. LACERDA, P. R. ISLER, S. J. HACKETT, K. V. ROSENBERG, AND R. T. BRUMFIELD. 2006. *Epinecrophylla*, a new genus of antwrens (Aves: Passeriformes: Thamnophilidae). *Proceedings of the Biological Society of Washington* 119:522-527.

REMSEN-PARKER. 1984. *Condor* 86: 36-41.

ZIMMER-ISLER. HBW chapter on Thamnophilidae.

Van Remsen, August 2007

This proposal passed SACC, 7-0, with following comments:

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Comments from Stiles: "YES. Again, multiple data sets support this. I'll feel a bit better when the Brumfield et al. study is published, but current evidence is certainly consistent in this respect."

Comments from Cadena: "YES. The only thing that bothers me a little bit is that phylogenetic studies based on DNA sequences have only sampled six of the eight species that Isler et al. suggested should be transferred to *Epinecrophylla* (*M. fjeldsaai* and *M. ornata* are missing, although the latter was included in the allozyme study by Hackett and Rosenberg). But I guess we can take the behavioral, morphological and vocal data discussed by Isler et al. as evidence that these taxa belong in the clade."

Comments from Robbins: "YES, but like Gary I wish the Brumfield et al. data were published."

Comments from Nores: "YES. Hay una buena coincidencia entre los estudios moleculares entre si y con los estudios de vocalizaciones y comportamiento de alimentación. Pienso que es un caso bien claro de que se trata de un género distinto, para el cual no había nombre disponible, ya que todas las especies fueron descritas como *Myrmotherula* o *Formicivora*."

Change linear sequence of forest Tinamidae genera

This proposal is to change the current linear sequence of the forest Tinamidae genera (*Tinamus*, *Nothocercus* and *Crypturellus*) to make it consistent with recently published molecular and morphological phylogenies. Currently, the sequence is:

Tinamus
Nothocercus
Crypturellus.

A "YES" vote to this proposal would change to:

Nothocercus
Tinamus
Crypturellus

Discussion of generic sequence in Tinamidae: A published phylogeny based on morphological data held *Nothocercus* to be sister to all other Tinamidae; with *Tinamus* sister to all genera other than *Nothocercus*; and *Crypturellus* sister to the remaining (aridland) genera (Bertelli et al. 2002). A phylogeny based on molecular (mtDNA) data held *Nothocercus* to be sister to (*Tinamus* + *Crypturellus*), these three forest genera together forming one of two broad Tinamidae lineages: "forest" and "aridland" (Porzecanski 2003). A strict consensus phylogeny combining both data sets found *Tinamus* + *Crypturellus* (1) to be sister, with *Nothocercus* (2) and aridland Tinamidae (3) forming three Tinamidae lineages (Bertelli & Porzecanski 2004). A single most optimal phylogeny using the combined data produced similar results to the molecular tree as regards the relationship between these three genera (Bertelli & Porzecanski 2004). The current linear sequence is not consistent with any of these hypotheses. However, moving *Nothocercus* to the start of the linear sequence would be consistent with all of them.

No other elements of the current generic sequence on SACC appear materially inconsistent with the Bertelli & Porzecanski (2004) phylogenies.

References:

Bertelli S. & Porzecanski A.L. 2004. Tinamou (Tinamidae) systematics: a preliminary combined analysis of morphology and molecules. *Ornitologia Neotropical* (Suppl.) 2004. (available at http://www.arches.uqa.edu/~perdiz/pdf/bertelli_&_porzecanski_2004.pdf)

Bertelli S., Giannini, N.P. & Goloboff, P.A. 2002. A phylogeny of the tinamous (Aves: Palaeognathiformes) based on integumentary characters. Syst. Biol. 51:959-979.

*Porzecanski, A.L. 2003. Historical biogeography of the South American aridlands: a molecular study of endemic avian taxa. Doctoral thesis. Columbia University, New York.

(*Note: I have not seen this but have relied in the above discussion on trees reproduced in Bertelli & Porzecanski (2004).)

Thomas Donegan

Note from Remsen (8/21/07): This proposal passed SACC 8-0, and is submitted with T. Donegan's permission and with the goal of making SACC-NACC lists more uniform. If anyone needs Spanish or Portuguese translation of SACC comments, let me know.

Comments from Stiles: "Morphological and genetic data both support the change, so YES."

Comments from Stotz: "YES. Fits biogeographically as well as *Crypturellus* and *Tinamus* are basically lowland taxa, while *Nothocercus* is Andean."

Comments from Jaramillo: "YES - Data look solid, and nothing is obviously "off" with this new arrangement. The Bertelli and Porzecanski data suggest that perhaps lumping *Tinamotis* with *Eudromia* is reasonable, but this may be a question of taste."

Comments from Pacheco: "YES. Os resultados parecem bem comportar a posição de *Nothocercus* no início da sequência."

Change English names of several *Turdus* from "Robin" to "Thrush"

Below is a copy of Thomas Donegan's proposal to SACC to change the English names of several *Turdus* from Robin to Thrush, which passed 6-0. It is submitted with T. Donegan's permission and with the goal of making SACC-NACC lists more uniform. Species in NACC list to which this proposal applies would be:

Turdus nigrescens Sooty Robin
T. infuscatus Black Robin
T. plebejus Mountain Robin
Turdus grayi Clay-colored Robin
Turdus nudigenis Bare-eyed Robin
Turdus assimilis White-throated Robin

They are interwoven in our list with species already called "Thrush." NOT affected are *T. rufopalliatu*s or *T. rufitorques*, which form a group with our beloved American Robin.

SACC is currently processing a proposal to change English name of *T. nudigenis* to avoid duplication with name of Old World *T. tephronotus*, so we can take action on that subsequently, if this one passes. Van Remsen (8/21/07)

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Proposal (#260) to South American Classification Committee

Proposals 260 a to d: Change English names of *Turdus nudigenis*, *T. grayi*, *T. assimilis*, and *T. albicollis* to "Thrush"

There are two general issues involved here. Further, two of the possible species names here are also names given to old world species.

1. Principle: are *T. nudigenis* etc. "Thrushes" or "Robins"?

Turdus migratorius is the "American Robin". The early European colonisers of North America, presumably on seeing a bird with a red breast similar to the European Robin *Erithacus rubecula*, adopted the same name for this species. Some other small Eurasian chats (which are generally colorful, reddish, red-brown or otherwise similar morphologically to the original robin *E. rubecula*) also use "Robin" as part of the vernacular name: e.g. some *Irania*, *Luscinia* and *Cercotrichas* species.

Turdus nudigenis, *T. assimilis* and *T. albicollis* are all bog standard brown *Turdus* thrushes. *T. grayi* has a reddish (terracotta) breast, but is otherwise a fairly

typical thrush in plumage. Clement & Hathaway (2000) elucidated some vocal and wing morphology similarities between the taxa known as "Robins" which may explain the alternative version of their vernacular names. However, such similarities (if real) for the three species in question here are likely to be independently derived or ancestral. The "Robins" occurring in South America do not even come close to forming a monophyletic group (Voelker et al. 2007). *T. migratorius* is not part of the New World *Turdus* clade. *T. albicollis* and *T. assimilis* are sisters. *T. nudigenis* and *T. grayi* are also closely related to one another, but not certainly sisters. However, the *T. albicollis/assimilis* clade is rather distant from the one containing *T. nudigenis* and *T. grayi*. In my view, the English name for *T. migratorius* should be restricted to its peculiar facts and, possibly, close relatives. However, this is not a debate about the other "Robins" on the AOU list: just about these four.

2. Usage

AOU and various North American and Central American publications use "Robin" for various non-*migratorius* *Turdus* species, including, on occasion, *T. assimilis* and *T. grayi*. However, very few texts relating to South America since the 1960s have used "Robin" for *Turdus nudigenis*, *T. assimilis* or *T. albicollis*.

My understanding is that Peters and other earlier authors used "Robin" for these species. A change to "Thrush" was introduced by Meyer de Schauensee in his 1964 Colombia guide. He then used the name again in Birds of South America (Meyer de Schauensee 1970). Ridgely & Tudor (1989) in Birds of South America, Hilty & Brown (1986) Birds of Colombia, Hilty (2003) Birds of Venezuela, Clement & Hathaway's Thrushes, various checklists dealing with the region (Rodner et al. 2000; Salaman et al. 2001) and many international texts (e.g. Clement 2000; IOC 2006 and all BirdLife publications) all use "Thrush" for *Turdus nudigenis*, *T. assimilis* and *T. albicollis* and, where found in the relevant region, *T. grayi*. Howell & Webb (1995), possibly the leading Central American field guide, also call all species subject to this proposal and found in that area "Thrushes". I would argue that the SACC position does not reflect either the status quo nomenclature for these birds or prevailing modern usage.

T. grayi and *T. assimilis* each occur in the USA, the latter as a vagrant. NACC has adopted "Robin" for the name for these species, and such names are used in some (but not all) of the field guide literature dealing with Northern and Central America.

Among all available names, a Google straw poll shows a majority use of: White-necked Thrush, Bare-eyed Thrush (which is a synonym: see below), Clay-colored Robin and White-throated Robin (which is also a synonym: see below). The two predominantly South American species are predominantly called "Thrushes". The two species occurring in southern USA are terms "Robins" generally there but such usage is not prevalent in recent South American texts.

3. English name conflicts

T. nudigenis: As pointed out in a previous version of this proposal by Dan Lane, changing "Bare-eyed Robin" to "Bare-eyed Thrush" would conflict with the widely used English name of the African *Turdus tephronotus*. Recent authors who have come up against this same problem (Sibley and Monroe 1990, Clement 2000); have changed the name to "Yellow-eyed Thrush" and Collar (2005 [HBW]) to "Spectacled Thrush". On principle, "Spectacled" is arguably better in that the more distinguishing morphological feature that is yellow is in the orbital region whilst many thrushes have yellow irises. However, "Yellow-eyed" is more widely used of the two, given "Spectacled"'s novelty. "Bare-eyed" is probably the better name descriptively. It could be argued that retaining "Robin" for *T. nudigenis* would be preferable to the alternative of adopting a relatively novel English name for the species. I will raise a series of proposals to adopt one of the three available names if "Thrush" is approved for this species, such that questions as to whether "Bare-eyed", "Spectacled" or "Yellow-eyed" are better can be taken off the table for present purposes.

T. assimilis: The current SACC name for *T. assimilis*, "White-throated Robin", is the same as that used for the Eurasian species *Irania gutturalis*, long thought to be a Turdidae, now considered possibly in the Muscicapidae. No alternative vernacular name currently exists for that species, of which I am aware. Even BOU, who are more than content to change long-established names for international reasons (e.g. Monk Vulture, Hedge Accentor, Pied Avocet), have no alternative name for this species. Changing *T. assimilis* from "Robin" to "Thrush" would seem a simple way of doing away with English name conflicts. I realise that there is certain international posturing involved in these sorts of decisions, and SACC could assert that *Irania gutturalis*' name should better be changed to something else - as it is not a true robin either. However, the simplest approach would be to adopt the alternative name for the *Turdus* species, given that one exists and is widely used.

Recommendations:

260a: Change *T. nudigenis* from "Bare-eyed Robin" to "Bare-eyed Thrush". On balance, YES. A reasonable approach could be to leave this as a "Robin" due to the pre-occupation of this name by another species and the uninformative (Yellow-eyed) or very novel (Spectacled) alternatives. This and *T. grayi* are closely related to one another and could be subject to the same conservative treatment. However, this species is called a "Thrush" in almost all South American literature. The SACC approach is the exception, not the status quo, on the Thrush/Robin point for this species.

260b: Change *T. grayi* from "Clay-colored Robin" to "Clay-colored Thrush". I am ambivalent about this one. It is an NACC area bird. NACC chose "Robin" for it.

Unlike for *T. assimilis*, there is no synonymy involved. "Thrush" would be a better name on principle but "Robin" is rather entrenched and this bird is terracotta (almost red) breasted.

260c: Change *T. assimilis* from "White-throated Robin" to "White-throated Thrush". A strong YES. Notwithstanding the NACC position, this seems like the most sensible way of removing English name conflicts and is a better description of the bird in question. This approach would also be consistent with 260d as *T. albicollis* and *T. assimilis* are sisters.

260d: Change *T. albicollis* from "White-necked Robin" to "White-necked Thrush". A strong YES. To reflect the status quo nomenclature, prevailing usage and a proper description of the bird in question.

Thomas Donegan, January 2007
(updated, *T. grayi* added, and split into 4 sub-proposals: March 2007)

Comments from Stiles: "YES to all. There has been little rhyme or reason to the use of "Robin" for various brown thrushes, except that what would seem to be a desire to call them "robins" because they remind North Americans of the bird they see hopping around on their lawns. Given that many (all?) are not particularly closely related to *T. migratorius*, there is little use in implying that they are sister groups or species by using "robin" for any of them. As for *T. grayi*, the "terra-cotta" bit doesn't wash - the widespread race of *grayi* in Colombia is in fact much "grayer" than that in Central America.. here, I think, is a justifiable case for parting company with the NACC, however heinous that may sound! Robins have red breasts, let it go at that!"

Comments from Zimmer: "YES. Let's just call everything other than American Robin a thrush and be done with the confusion!"

Comments from Jaramillo: "YES - Undue confusion has been caused by the use of the Robin name for these various Neotropical thrushes. I see no good reason to keep Robin for them, it just causes confusion, and that overrides any issue of stability of names for me in this case."

Comments from Robbins: "YES, refer to all the Neotropical Turdus as thrushes, instead of robins. Long overdue."

Split *Icterus icterus* into three species

CLC: The proposal below was submitted by Rosendo Fraga to SACC and passed 7-0. With Rosendo's permission, I hereby also submit it to NACC. This "species" is introduced only in our area (Puerto Rico), evidently only 1 of the 3 taxa (nominate). Thus, if it passes, it would affect the English name of that species as well as the elevation of our "groups" in the Distribution statement to species rank. Van Remsen, 21 Aug. 2007

Proposal (#288) to South American Classification Committee

Split *Icterus icterus* into three species

There are just three viewpoints on troupial taxonomy: there are either one, two or three species (Hellmayr 1937, Short 1975, Jaramillo and Burke 1999). In this proposal, I summarize the arguments for the recognition of three species. Their respective names would be Venezuelan Troupial (*Icterus icterus*), Campo Troupial (*Icterus jamaicaii*) and Orange-backed Troupial (*I. croconotus*). The first species is often called just "Troupial". Many important books and field guides have already recognized the three species (Jaramillo and Burke 1999, Ridgely and Greenfield 2001, Hilty 2002). Authors that recognize two species lump Campo and Orange-backed Troupials (e.g. Hellmayr 1937, Omland et al. 1999). Venezuelan Troupial is divisible into three subspecies, the Campo Troupial is monotypic, and the Orange-backed Troupial consists of two subspecies (Jaramillo and Burke 1999).

Troupials are not rainforest birds, and often occur in areas with less than 500 mm of yearly rainfall. Their main habitat is thorny scrub, open woodland and savannas, in arid and semiarid climates, or climates with a strong dry season. The main exception to the rule concerns Orange-backed Troupials of the nominate *croconotus* subspecies, which occurs in the várzea and other semiopen riparian habitats in Amazonia.

The alleged intermediate subspecies *metae*.

One argument for lumping the three troupials is the existence of a population (*Icterus icterus metae*) apparently intermediate in plumage between the Venezuelan and the Orange-backed Troupials. The pattern of black and orange colors in the plumage of *metae* is basically that of the Venezuelan Troupial, but the nape is orange rather than black, and some individuals show an orange patch in the back. This partial resemblance of *metae* to Orange-backed troupials may be a case of convergence, as other characters of *metae*, particularly the divided white wing patch, are unique to this population, or are the same as in

Venezuelan Troupials. The molecular phylogeny of orioles has revealed many cases of color convergence in plumage (e.g. Omland et al. 1999, Hoffman et al. 2006).

Subspecies *metae* has a limited range in eastern Colombia (Arauca) and western Venezuela (Apure). Populations of *metae* are separated by a huge distribution gap from the nearest populations of Orange-backed Troupials (Hilty and Brown 1986, Jaramillo and Burke 1999), an unusual fact for forms that supposedly hybridize or intergrade. Short (1975) suggested that *metae* is a product of a past hybridization event between *icterus* and *croconotus*, but his idea requires the existence of a former dry corridor across the humid forests of Eastern Colombia. I'm not expert in the biogeography of Amazonian Colombia, but if such corridor existed, it should have affected other animals and plants, not just the troupials.

Behavioral evidence

Troupials are well-known by their rich melodious songs. Vocal characteristics shared by all troupials include the existence of female song (Skutch 1969, Sick 1990, pers. obs.), pair duetting (pers. obs.), and the ability to mimic other birds (at least in captivity) (Sick 1990, pers. obs.). A recent study mentions a few differences in song between two troupial species (Price et al. 2007). I have myself recorded songs from a few individuals of the three species (in the Paraguayan Chaco, Minas Gerais in Brazil, Cojedes in Venezuela) and noticed some differences. However, only a detailed analysis of many songs, for both sexes of (at least) all described subspecies, would be needed to clarify troupial song evolution.

There is almost no information on other troupial vocalizations.

Allopatry and sympatry

Other than the apparent intermediacy of *metae*, the main argument for lumping all troupials was basically their allopatric distribution. Their disjunct distribution mostly reflects basic disjunctions in climate and vegetation within South America. Under the biological species concept (BSC) all good species eventually evolve differences in behavior, size, bill morphology, etc. that would permit coexistence. Troupials have one extra behavioral character that would make coexistence difficult. Most troupial populations (excepting *I. i. ridgwayi*) seem to breed only, or mostly, in other birds' nests (Pinto 1967, Skutch 1969, Pearson 1974, Lindell and Bosque 1999, Maugeri and Drozd 2006, and pers. obs. for *jamacaii* and *croconotus*). Aggressive nest piracy, with egg or chick removal, has been reported for the three species (sources above), and would probably decrease the nesting success of troupials breeding in sympatry.

Recently an area of sympatry between Campo and Orange-backed Troupials has been reported by Pacheco and Olmos (2006). It is located in the Brazilian state

of Tocantins, and apparently the forms in contact behave as good biological species (at least no hybrids or mixed pairs were reported). Less relevantly, Campo and Orange-backed troupials had breeding feral populations around the city of Belem, Para, Brazil (Cardoso da Silva and Oren 1990), and likewise, no mixed pairs or intermediate specimens were reported.

I think that available evidence favors the recognition of three troupial species as good biological species, even if *metae* reflects a past episode of hybridization, because it would then involve a very restricted area. The three troupials were described as species as early as 1766, 1788 and 1829, at a time when avian taxonomy was based mostly on plumage characteristics of museum specimens. The forms are clearly diagnosable, as advocates of the phylogenetic species concept (PSC) should recognize. Lastly, the three forms were initially described as species on their own, and maintained as separate species well into the 1950s and 1960s (e.g. De Schauensee 1952, Pinto 1967). The official Brazilian checklist (CBRO 2006) recognizes the three species.

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Rosendo M. Fraga, June 2007

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Comments from Stiles: "YES. I have long felt that *croconotus* and *icterus* were different birds from fairly extensive experience with both. I don't have any experience with *jamacaii*, but the fact that where it is sympatric in artificial situations (where one might expect interbreeding if small numbers of released/escaped birds were to found a feral population) very strongly suggests that reproductive isolation is alive and well. My limited experience with *metae* suggests that it is a bird of the borders of gallery forest (ecotone with the open llanos) and in general is much more like *icterus* in voice and behavior."

Comments from Cadena: "YES. The turning point here is the documented sympatry of *jamacaii* and *croconotus* with no apparent hybridization. The allopatric distribution of *icterus* implies that the case for splitting this taxon is not as strong, but I guess one could say that it differs from the two taxa that are

reproductively isolated to a similar extent than these two differ from each other. Also, considering these taxa were described as good species and lumped without a strong rationale, and that Gary's experience indicates *croconotus* and *icterus* are different, I'll go along with the three-way split."

Comments from Remsen: "YES. The report of sympatry between *jamacaii* and *croconotus* refutes any previous treatment as conspecific, and for consistency, *I. icterus* should be ranked at the species level until further evidence indicates otherwise."

Comments from Robbins: "Without a molecular assessment, a tentative YES."

Comments from Zimmer: "YES. Sympatry of *jamacaii* and *croconotus* without apparent hybridization is the clincher. Morphological, vocal and ecological differences between each of those two forms and nominate *icterus* are on a par with consistent differences between *croconotus* and *jamacaii*, so treating *icterus* as a third species makes sense."

Comments from Nores: "YES. Como ha sido indicado en varias de las respuestas, la simpatria de *jamacaii* y *croconotus* sin hibidización es el mejor fundamento para separar las especies. Además están las vocalizaciones, que aparentemente son algo diferentes. Sin embargo Ridgely y Tudor mencionan que las voces son esencialmente similares. Como en el caso anterior sería muy importante un análisis molecular."

Change English name of *Chlorothraupis carmioli* from “Olive Tanager” to “Carmioli’s Tanager”

This species is called "Olive Tanager" in AOU (1983, 1998), Isler & Isler (1987), Ridgely & Tudor (1989), Ridgely & Greenfield (2001), and elsewhere, but this creates unnecessary confusion with *C. olivacea* (Lemon-spectacled Tanager). As noted by Meyer de Schauensee (1966), "Olive" is best used for those classifications that consider the two conspecific, in which *olivacea* has priority. For that reason, Dickinson (2003) and SACC use “Carmioli’s Tanager,” a name with at least $\frac{3}{4}$ of a century of use (e.g., Ridgway 1902, Meyer de Schauensee 1970).

Evidently, the trouble started with Eisenmann’s Great Patronym Purge (Eisenmann 1955). Although Hellmayr used “Carmioli’s Olive Tanager” for nominate *C. carmioli*, Eisenmann then shortened it to “Olive Tanager,” thus generating chronic confusion with *C. olivacea*.

I recommend that we return to “Carmioli’s Tanager” for *C. carmioli* to restore a name that has more than 100 years of history and to avoid confusion with a congener. [On the slim chance you didn’t know ... Carmioli was an ornithologist and bird collector who worked in Cost Rica in the 19th C.]

Van “English Names are My Life” Remsen, August 2007

Note from Banks: Gill and Wright use Carmioli’s T. for *carmioli*, but use Olive T. for *C. frenata*, a subspecies of *carmioli* in Hellmayr but a “group” within *carmioli* in AOU 1998.

Change English name of *Microbates cinereiventris* from “Tawny-faced Gnatwren” to “Half-collared Gnatwren”

This proposal was generated by Marshall Iliff's list of discrepancies between NACC and SACC, and I agreed to do it, for reasons that remain obscure to me now. Anyway ... hang on for an intellectually stimulating review.

Microbates cinereiventris occurs in our area from Nicaragua through Panama, but the larger part of its range is in South America, in the Chocó region of western Colombia and northwestern Ecuador and then western Amazonia in eastern Colombia, Ecuador, and eastern Peru.

For most of the century, it was known as “Half-collared Gnatwren”, e.g.,:

- 1911 Ridgway
- 1924 Cory (Birds of the Americas, widely known as “Hellmayr”)
- 1955 Eisenmann (Middle America list)
- 1964 Meyer de Schauensee (Birds of Colombia)
- 1966 Meyer de Schauensee (Birds of South America, orange)
- 1970 Meyer de Schauensee (Birds of South America, blue)

The earliest usage of “Tawny-faced” that I can find is:

- 1976 (Ridgely, Birds of Panama)

Followed by:

- 1983 AOUC
- 1989 Ridgely & Gwynne (Panama)
- 1989 Stiles & Skutch (Costa Rica)
- 1990 Sibley & Monroe
- 1998 AOUC
- 2001 Ridgely & Greenfield (Ecuador)
- 2006 Gill-Wright
- 2007 Restall (Birds of Northern South America)

But NOT by:

- 1982 Parker et al. (Peru)
- 1986 Hilty & Brown (Colombia)
- 1999-2007 SACC
- 2003 Dickinson (World checklist)

Reasons for staying with “Tawny-faced”:

1. Slightly better name. Although the bird's ragged collar generated "Half-collared" (and one species is named *semitorquatus*), a more striking feature, as well as a more obvious difference between this species and its sole congener, *M. collaris*, is the tawny vs. gray face.
2. Prevalence of current usage in our area. In addition to AOUCL, most recent Central American texts use "Tawny-faced."

Reasons for changing to "Half-collared":

1. No reason to have meddled with name in first place. Yes, Tawny-faced might be better, but Half-collared is not inaccurate, had at least 65 years of historical use, and nicely mirrors the English name of its congener *M. collaris* ("Collared Gnatwren").
2. Prevalence of current usage in South America: The species is arguably more South American than North, and our sister committee there uses "Half-collared", as does a majority of South American literature.

Recommendation: Tough call. With a minimum of conviction, I favor Half-collared. As far as I can tell, there was no good reason to change in the first place, and I favor historical continuity over name "improvement." As you've heard me say before, if we can live with, say, Hairy Woodpecker and Evening Grosbeak, then we can survive continuing use of Half-collared.

Van Remsen, August 2007

Treat *Nonnula frontalis* as a separate species from *N. ruficapilla*

Effect on AOU CL: By recognizing an extralimital taxon as a separate species, this proposal would change only the species name of the form in NACC area.

Background: Two allopatric groups of *Nonnula* nunlets have been treated historically as one or two separate species. They are:

frontalis: of e. Panama and N. Colombia (Gray-cheeked Nunlet)

ruficapilla: of southern Amazonia (Rufous-capped Nunlet)

They share plumage features (gray facial region contrasting with rufous crown) that are not found in the other 3 *Nonnula* taxa ranked as species. However, all *Nonnula* are fairly similar to one another, and the two groups in question are the two most-distant taxa geographically. There are no published studies of vocalizations or any other data that might provide insight into species limits or relationships within the group.

Without tallying up their treatment as 1 or 2 species through the years (because all such treatments are arbitrary), I note that SACC treats them as two species, as does Handbook of the Birds of the World (Rasmussen & Collar 2002), Dickinson (2003), and Hilty & Brown (Birds of Colombia). Ridgely & Gwynne (1989, Panama) treat them as conspecific but cited AOU (1983) for the change, and there is no justification provided in AOU (1983). So, I suspect that we are in the minority for a treatment for which no rationale has provided (at least in any recent literature).

Further, although it feels like “insider trading,” I know that Chris Witt found that a broadly defined Gray-cheeked Nunlet was paraphyletic with respect to two other species. Chris’s unpublished dissertation can be accessed at:

<http://etd.lsu.edu/docs/available/etd-11122004-054504/>

In his Puffbird tree (p. 69), *Nonnula brunnea* and *N. sclateri* are more closely related to *N. frontalis* than to *N. ruficapilla* (mitochondrial and nuclear genes, 100% Bayesian support for nodes). Thus, the plumage similarity between distant *frontalis* and *ruficapilla* does not reflect sister relationships – perhaps a leapfrog pattern in coloration.

Analysis: (Prevalence of current treatment) + (absence of published data to contrary) + (unpublished genetic data that strongly indicate that our *N. frontalis* is paraphyletic with respect to two other species taxa) = best to change to two species. Even if we do not factor in the unpublished data, I would say that the first two variables in the equation are sufficient to bring NACC into alignment with

SACC. As for waiting for the Witt phylogeny to be published, I would advise against this because I have not seen any evidence that Chris is going to publish this.

Recommendation: Yes, split the two, with the NACC taxon being *N. frontalis* and retaining the English name Gray-cheeked Nunlet.

Van Remsen, August 2007

Recognize sister relationship between Podicipediformes and Phoenicopteriformes

Effect on NACC: This would move the two orders next to each other in the linear sequence to recognize their sister relationship.

Background and New Information: The relationships of these two orders to other bird groups has always been controversial -- see synopses in the SACC Notes sections under each order. Perhaps the single most-surprising result from analyses of DNA sequence data so far has been the association of grebes with flamingoes, first found by van Tuinen (2001) and subsequently also by three independent genetic data sets (Chubb 2004a, Cracraft et al. 2004, Ericson et al. 2006). One analysis of morphological data also supports this relationship (Mayr 2004), but another (Livezey & Zusi 2007) does not. Recently, DNA sequence data (Johnson et al. 2006) [let me know if you want a pdf] also showed that the ischnoceran lice of flamingos and grebes were sisters. Johnson et al. (2001) also cited a Bob Storer monograph on grebe parasites to the effect that they shared parasites with flamingos.

Analysis and Recommendation: I vote YES because consistency between multiple independent data sets is convincing to me that this relationship is solid and needs to be recognized in our classification.

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Van Remsen, August 2007

Note: this proposal passed SACC

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Comments from Stiles: "YES. Multiple data sets support this. Place them together in the sequence, I'd suggest grebes-then-flamingoes as representing the least drastic change (but this is clearly a minor point)."

Comments from Cadena: "YES. The sister relationship between these two clades appears to be strongly supported. Perhaps we could add an additional citation to the proposal or to the notes on the SACC site: Sangster, G. 2005. A name for the flamingo-grebe clade. *Ibis* 147:612-615. This author presented a review of the evidence, listed synapomorphies for the group, and proposed to name it "Mirandornithes". Although Sangster's naming of the clade was based on the principles of "phylogenetic taxonomy" (i.e. PhyloCode), he suggested that under the Linnean system, this clade could be ranked at the superorder level (I'm not sure if the SACC will deal with categories like superorders)."

Comments from Robbins: "YES, given the independent data sets that support this change."

Comments from Pacheco: "YES. Diversos estudos independentes dão um suporte contundente a esta proposição. Aproveitando da questão colocada por Cadena, é oportuno lembrar que o sufixo de "Mirandornithes" Sangster, 2005 parece sugerir Subclass (vide A.O.U 1999)."

Comments from Nores: "NO. Y no es porque esté en total desacuerdo con la propuesta, sino porque veo una marcada tendencia en el "SACC Committee" a considerar todo lo molecular como infalible e irrefutable. Después de leer el paper de Livezey & Zusi (2007), en particular los árboles de las Figs. 1-9, y especialmente el estudio molecular de Mindell et al. (1997) (Fig. 4) pienso que la veracidad de los estudios moleculares es relativa. Por ejemplo, Van Tuinen et al. (2000), ponen los Podicipedidae junto con Cuculidae y muy cerca de

Charadriidae y en el 2001 (sólo un año después) los ponen junto con Phoenicopteridae y muy lejos de Charadriidae (Fig. 6A,B). Yo pienso que hay que tener en cuenta el paper de Livezey & Zusi antes de seguir con las propuestas y repuestas. A continuación he puesto algunos párrafos que aparecen en Livezey & Zusi que me parecen importantes.":

"In the published record of phylogenetics, it has become virtually customary simply to generate phylogenetic hypotheses of varying consonance with little or no consideration of factors underlying divergent inferences. This tradition has led to a false sense of congruence among studies, especially among molecular systematists". There was considerable disagreement among recent molecular studies alone (e.g. Espinosa de los Monteros, 2000; Johansson et al., 2001; Poe & Chubb, 2004), regardless of data analysed (Philippe et al., 1996; Graur & Li, 2000), which reveals contrasts only between morphological and molecular inferences to be over simplifications of modern study (e.g. Braun & Brumfield, 1998; Van Tuinen, 2002). Studies based both on molecular and morphological phylogenetics (Figs 1-9) manifest substantial disagreement both within and between schools. We do not intend an assault on molecular methodology, but seek to refute persistent prejudices that afflict morphological phylogenetics. At present, molecular systematics is characterized both by the coexistence of general (if not unbridled) optimism (Van Tuinen, 2002) and by profound doubts regarding resolution of substantial segments of neornithine phylogeny (Poe & Chubb, 2004). Perhaps the deficiency attributed most widely to morphological phylogenetics stems from suspicions of morphological convergence, concerns seldom empirically substantiated and to which molecular methods are widely assumed to be immune. Without a consensus regarding a relationship between the Podicipedidae and Gaviidae, the former have been the subject of several extraordinary proposals, based on relatively weak evidence or mere speculation. See also Bourdon et. al (2005)."

**Separate *Pionopsitta* (= *Gypopsitta*) *coccinickollaris* from *P.* (= *G.*)
*haemototis***

This proposal follows on from 2007-C-03, above, and relies on the same paper by Ribas et al. (2005).

Assuming 2007-C-03 passes, the generic name becomes *Gypopsitta*, which I will use here on. There are two populations of *G. haemototis* in Central America, the nominate one which extends from Veracruz, Mexico, to western Panama, and *coccinickollaris* which occurs in eastern Panama and adjacent Colombia. In their analysis, Ribas et al. (2005) considered these different species.

Wetmore (1968) described the species as follows: “. . . lower foreneck and upper breast olive-yellow; (in the subspecies *coccinickollaris* the lower foreneck marked with red” That is the only morphological difference noted. He goes on to say: “Two geographic races are recognized, separated by the presence or absence of red across the base of the foreneck. Other characters that have been suggested . . . are variable in the series now available and so have no validity.” In the account of the nominate race, he says “Apparently intergrades with the race *coccinickollaris* on the Caribbean slope in northern Coclé and western Colón.” I find one USNM bird from Coclé that AW labeled “intermediate” but I think it could as well be *coccinickollaris*. There is very little difference between these two taxa, in my view.

Ribas et al. (2005) note that Cracraft and Prum (1998) considered the two taxa as separate species in their analysis of the genus; they used the PSC. Ribas et al. say: “These species occur in southern and northern Central America and there seems to exist a region of sympatry in Panama. In the present study, one [of 3] of the sampled *G. coccinickollaris* individuals . . . is from a locality that is closer to those of the two *G. haemototis* specimens from Veraguas than it is to the other two *G. coccinickollaris* collected in Darien Province.” Despite the geographical proximity and phenotypic similarity between these two species, **interspecific distances were markedly higher (2.6% in ND2, 4.6% in cyt b) than the intraspecific differences (0.1-1.2%)**, indicating the existence of an effective barrier separating these two species in the past.” Remsen notes that Ribas et al. treated any diagnosable units as species; therefore, their species” are not directly equivalent to our species rank.

Perhaps one of our molecular members can comment on the numbers highlighted in the previous paragraph. Unless we are told that this is a valid specific difference, I suggest a NO vote (indicating that we do not accept this split).

Richard C. Banks, 7 Sept. 2007