

## NACC Proposals 2007-D

No.	Page	Title
01	2	Move Sapayoa aenigma to Eurylaimidae
02	4	Split <i>Icterus spurius</i> into two species
03	6	Resurrect the genus <i>Rupornis</i> for the Roadside Hawk
04	8	Separate Caribbean and European Flamingos
05.	10	Treat <i>Buteogallus subtilis</i> as a subspecies of <i>B. anthracinus</i>
06.	13	Change English name of <i>Cnipodectes</i> to Twistwing
07.	19	Lump the genera <i>Arremon</i> , <i>Buarremon</i> , and <i>Lysurus</i>
08	22	Change linear sequence in <i>Tangara</i>
09.	25	Change English name of <i>Gallinula chloropus</i> back from "Common Moorhen" to "Common Gallinule"
10.	27	Add Pallas's Warbler ( <i>Phylloscopus proregulus</i> )

### Move *Sapayoa aenigma* to Eurylaimidae

*Sapayoa aenigma*, the Sapayoa (AOU 1998), is a small passerine resident in a narrow zone of rainforest in Panama, Colombia, and Ecuador. Described by Hartert (1903), who found it "difficult to place in the system", *Sapayoa*, in keeping with its specific name, has long been a taxonomic puzzle. After much study, Hartert concluded that it was best placed in the New World suboscine family Pipridae (the manakins), part of the tyrannoid group of suboscines. This has been the generally prevailing view over the past century (e.g., Meyer de Schauensee 1970, Traylor 1979), although Prum (1990), based on study of morphological characters, suggested that *Sapayoa* may be more closely allied to the Tyrannidae (tyrant-flycatchers).

Lanyon (1985), in an electrophoretic study of the Tyrannoidea, found *Sapayoa* to be only distantly related to the other taxa studied, and concluded that the true affinities of *Sapayoa* may be outside the tyrannoids. Sibley and Ahlquist (1990) included *Sapayoa* in their DNA-DNA hybridization experiments, the melting curves of which indicated that it was close to the Old World suboscines (Eurylaimides: broadbills, asities, and pittas), but were evidently uncertain of their data and excluded it from their phylogenetic analyses and trees. Subsequently, Sibley and Monroe (1990), unable to place it with confidence among any group of suboscines, considered it a separate family *incertae sedis*. *Sapayoa* was also considered *incertae sedis* in the AOU Check-list 1998, and provisionally placed between the Tyrannidae and Cotingidae within the Tyrannoidea.

Using DNA sequence data, Fjeldsa et al. (2003) and Chesser (2004) clearly showed that *Sapayoa* is more closely related to the Old World suboscines than to any New World suboscine group. Irestedt et al. (2006) and Moyle et al. (2006) further showed that it is embedded within the Old World suboscine family Eurylaimidae (broadbills). Both studies found the Old World broadbill (and asity) species to form two sister clades, one consisting of *Calyptomena* and *Smithornis*, the other of *Eurylaimus*, *Cymbirhynchus*, *Serilophus*, *Corydon*, *Psarisomus*, *Pseudocalyptomena*, *Neodrepanis*, and *Philepitta*. Irestedt et al. (2006) found *Sapayoa* to be sister to the *Calyptomena-Smithornis* clade, whereas Moyle et al. (2006) found *Sapayoa* to be sister to the second clade.

**Proposal:** Change the taxonomic placement of *Sapayoa aenigma* to Eurylaimidae and insert this species and family before Furnariidae in the Check-list.

**Recommendation:** Vote YES. The only real alternative to this, given the uncertainty of *Sapayoa*'s relationships within the family, would be to create a separate family for *Sapayoa*, which would necessitate a split of the Eurylaimidae into three families: Eurylaimidae, Calyptomenidae, and Sapayoidae. The splitting of the Eurylaimidae is primarily an Old World issue and my view is that we should follow the traditional position, in which all broadbills constitute the single family Eurylaimidae, rather than making new policy. Dickinson (2003) placed *Sapayoa* in the separate family

Sapayoidae (*sic*) while maintaining the single broadbill family Eurylaimidae; this arrangement is not supported by the current data.

### Literature

- Chesser, R. T. 2004. Molecular systematics of New World suboscine birds. *Molecular Phylogenetics and Evolution* 32: 11-24.
- Fjeldsa, J., et al. 2003. *Sapayoa aenigma*: a New World representative of 'Old World suboscines'. *Proc. R. Soc. Lond. B (Suppl.)* 270: S238-S241.
- Hartert, E. 1903. On a remarkable new oligomyodian genus and species from Ecuador. *Novit. Zool.* 10: 117-118.
- Irestedt, M., et al. 2006. Nuclear DNA from old collections of avian study skins reveals the evolutionary history of the Old World suboscines (Aves, Passeriformes). *Zool. Scripta* 35: 567-580.
- Lanyon, S. M. 1985. Molecular perspective on higher-level relationships in the Tyrannoidea (Aves). *Syst. Zool.* 34: 404-418.
- Meyer de Schauensee, R. 1970. *A Guide to the Birds of South America*, Academy of Natural Sciences of Philadelphia, PA.
- Moyle, R. G., et al. 2006. Phylogeny and evolutionary history of Old World suboscine birds (Aves: Eurylaimides). *Amer. Mus. Novitates* 3544: 1-22.
- Prum, R. O. 1990. A test of the monophyly of the manakins (Pipridae) and of the cotingas (Cotingidae) based on morphology. *Occas. Pap. Mus. Zool. Univ. Michigan* 723: 1-44.
- Sibley, C. G., and J. E. Ahquist. 1990. *Phylogeny and Classification of Birds*, Yale Univ. Press, New Haven, CT.
- Sibley, C. G., and B. L. Monroe. 1990. *Distribution and Taxonomy of Birds of the World*, Yale Univ. Press, New Haven, CT.
- Traylor, M. A. (ed.) 1979. *Check-list of Birds of the World*, Vol. 8. Mus. Comp. Zool., Cambridge, MA.

R. T. Chesser  
21 Sept. 2007

**Split *Icterus spurius* into two species, *Icterus spurius* & *I. fuertesi***

[**Note:** This is an update of a proposal I originally submitted in 2003, prepared after reading Kiere et al. 2007.]

**Effect of AOU-CL:** Add *Icterus fuertesi* to the *Check-list*, presumably following *I. spurius*.

**History:** Although at various times treated as a separate species, *Icterus fuertesi*, the well-marked population of Orchard Orioles that breeds on the east coast Mexico, from central Tamaulipas south to central Veracruz, and winter in Guerrero, Morelos, and Chiapas. *I. fuertesi* has generally been considered as a subspecies of Orchard Orioles (*I. spurius fuertesi*) (e.g. current AOU-CL treatment).

**New information (2003):** Baker et al. (2003) have published information on the molecular relationships of these taxa based on 925 bp of mt cytochrome-b and 344 bp of mt control region, and 25 Orchard and 7 Fuertes' orioles. The Fuertes' Orioles came from 2 sites in Veracruz and the Orchard Orioles came from 7 sites in the US and 1 in central Mexico. There is a significant difference between the taxa, but little structure to the trees, and, on the basis of their data, Fuertes' Orioles are paraphyletic. Omland et al. (1999) found that these two taxa were very closely related (0.6% in cytochrome-b & ND2 sequences). Baker et al. (2003:855) conclude that "In our view, Orchard and Fuertes' orioles are evolutionarily distinct taxa and should be treated as separate species."

**2007 Update:** Kiere et al. (2007) used colorimetric analyses (based on a USB spectrometer) to clarify trends in geographic variation in these orioles. They used 5 specimens from the northeastern part of the range of *I. spurius*, 5 specimens from the northwestern part of their range, and 5 specimens from the southwestern part (including one from southern Texas, 2 from Chihuahua, one from Michoacan, and one from Jalisco). These specimens were collected from the 1890 through 1972. They also examined 10 specimens of *I. spurius fuertesi* from Mexico (5 from Veracruz and 5 from Tamaulipas), collected 1949-1970.

They found no evidence of clinal variation, which it has been suggested might exist; specifically, *I. spurius* are not paler (i.e. more *I. spurius fuertesi*-like) in the southern part of their range. There appears to be very little geographic variation in color in either taxon. The two are clearly separable in coloration (as I think was generally already accepted). It is not clear to me why they used such old material (i.e. could foxing be an issue?) or didn't use larger sample sizes or more samples, *but* the results are so clear-cut and straight-forward that I think that there is virtually no possibility that examining different samples would have affected their conclusions.

They argued that because (1) of these differences in coloration, (2) slight genetic differences (see Baker et al. above) and (3) differences in migration behavior (*I. spurius* are long-distance migrants and *I. spurius fuertesi* short distance ones [or sometimes resident?]), elevation of *I. spurius fuertesi* to species status might be warranted (a suggestion that seems to have been partially motivated to facilitate moves to conserve these birds, especially *I. s. spurius*). The color differences are clear. The breeding ranges of the two are completely allopatric, although there may be some geographic overlap between them in migration and winter.

**Recommendation:** There is no doubt that these two orioles differ in appearance, and there seems to be no overlap in plumage features, and clinal variation within *I. s. spurius* seems to be slight at most. Size has been said to differ, but if so, it isn't by much, and that is debated and of little relevance, in my opinion. The molecular differences are slight, and many species show variation in migratory behavior (sometimes within a population). At present there is no doubt evolutionary independent lineages, but I do not favour this split: not much difference in anything but plumage, and oriole plumage does not appear to be at all conservative (e.g. Omland and Lanyon, 2000). This seems to me to be a good place to go with the subspecies concept.

#### **References:**

- Baker, J. M., E. López-Medrano, A. G. Navarro-Sigüenza, O. R. Rojas-Soto, and K. E. Omland. 2003. Recent speciation in the Orchard Oriole group: Divergence of *Icterus spurius fuertesi*. *Auk* 120: 848-859.
- Jaramillo, A. and P. Burke. 1999. *New World Blackbirds: The Icterids*. Princeton Univ. Press, Princeton, NJ.
- Kiere, L. M., C. M. Hofmann, I. E. Tracy, T. W. Cronin, J. Leips, and K. E. Omland. 2007. Using color to define species boundaries: Quantitative analysis in the Orchard Oriole Complex supports the recognition of two species. *Condor* 109:692-697.
- Omland, K. and S. M. Lanyon. 2000. Reconstructing plumage evolution in orioles (*Icterus*): Repeated convergence and reversal in patterns. *Evolution* 54:2119-2133.
- Omland, K., S. M. Lanyon and S. J. Fritz. 1999. A molecular phylogeny of the New World orioles (*Icterus*): The importance of dense taxon sampling. *M. P. & E.* 12:234-239.

Jim Rising  
26 Sept. 2007

### Resurrect the genus *Rupornis* for the Roadside Hawk

From Proposal 2005-A-10:

The Roadside Hawk species *magnirostris* has been in the genus *Buteo* in both editions of the Check-list in which it has appeared, i.e., 6th and 7th. It has been associated with the “woodland Buteos” on the basis of plumage and morphology. At one point it was treated in its own genus, *Rupornis*, by Kaup (1844) and others (**who and til when?**)

Now, Riesing et al. (2003) have done a mitochondrial DNA study of 61 taxa of *Buteos* and sub-buteonine hawks. In that study, the species *magnirostris* falls outside the limits of the genus *Buteo* and in the sub-buteonine category, not coupled with any other taxon. To avoid having a paraphyletic *Buteo*, Riesing et al. (2003) recommend reviving the genus *Rupornis* Kaup, 1844 for the species *magnirostris*. They note that this action is supported by the “aberrant external morphology as well as plumage characters.”

NEW:

A new study of Accipitrid phylogeny by Griffiths et al. (2007) has again shown that the species *magnirostris* does not come out with species of *Buteo*. This study was based on the nuclear RAG-1 exon. These authors recognized *Rupornis* in their study on the basis of Riesing et al. (2003). *Rupornis* came out as a sister of the genus *Parabuteo*, well away from *Buteo*. They state that “although the two species of *Buteo* [in their study] were sister to each other, the monophyly of *Buteo* was only obtained because we had tentatively placed the roadside hawk in the genus *Rupornis*.” They further stated that “the genus *Buteo* would be polyphyletic unless the genus *Rupornis* is resurrected for *Buteo magnirostris* and related species.” They do not say what related species. This may open a large can of worms.

Thus, studies of two different sets of DNA give the same results.

The problem is that Griffiths et al. also kept *Asturina* out of *Buteo* for their analysis, whereas Riesing et al. had used their study to merge *Asturina* into *Buteo* (which we followed) in 2005.

#### Literature Cited

Griffiths, C. S., G. F. Barrowclough, J. G. Groth, and L. A. Mertz. 2007. Phylogeny, diversity, and classification of the Accipitridae based on DNA sequences of the RAG-1 exon. *Journal of Avian Biology* 38:587-602.

Riesing, M. J., L Kruckenhauser, A. Gamauf, and E. Haring. 2003. Molecular phylogeny of the genus *Buteo* (Aves: Accipitridae) based on mitochondrial marker sequences. *Molecular Phylogenetics and Evolution* 27:328-342.

Richard C. Banks  
3 October 2007

### Separate Caribbean and European Flamingos

The fifth edition (1957) AOU Check-list treated the American Flamingo, *Phoenicopterus ruber*, as a distinct species. In the next two editions, 1983 and 1998, the European *Ph. roseus* was merged with it. A Feb. 1978 memo in my files from Les Short provides the reason. He had checked with Phil Kahl, as apparently the committee has asked him to do. Kahl strongly supported the merger of those two forms (but not *chilensis*, as some have done). Kahl said “at the 1976 “Flamingo Group” [of ICBP ?] meeting it was consensus that *roseus* is conspecific with *ruber*.” Also the “new” Birds of the Western Palearctic merged them, as did the revised volume 1 of Peters (for which Kahl wrote that part). No biological evidence was cited. The BOU (Knox et al. 2002) and other European groups have more recently recognized Greater *roseus* and Caribbean *ruber* as distinct—again. This is based on several papers that were summarized by Sangster (1997), as follows:

Plumage color is distinct; *roseus* is predominately white, often with a pale pinkish hue on neck; *ruber* is mainly pinkish orange, incl. head, neck, and underparts. Intensity of color varies with diet and season.

In *roseus*, the basal part of the bill, as the bare skin bordering it, is relatively dark pink; the black on the bill is less extensive—on the upper mandible black does not extend beyond the ventral curvature. The bill of *ruber* has a pale orange-red base and an extensive black tip which does not extend below the nostril.

Studies of group displays in captivity by Studer-Thiersch indicate that at several stages of the display the taxa attain different postures of head, neck, body, and wings. Calls of *roseus* are short and bi-syllabic; in *ruber* they consist of 3 syllables and are drawn out. However, Studer-Thiersch did not separate these forms on the basis of this behavior, considering them closer to one another than to *chilensis* (not discussed here).

Whether the color and behavioral differences act as reproductive barriers is not really known, because of the wide geographic separation of the taxa. In a colony of *roseus* in France, mixed pairs of *roseus* and both (escaped) *ruber* and *chilensis* have been observed. Sangster mentions several observations of hybrids of *roseus* and *chilensis*, but there is no further discussion of hybrids with *ruber*. At any rate, hybridization or lack thereof in such a situation would not necessarily be meaningful.

Perhaps most important here is the fact that the forms were merged with no analysis or reasons given. We often reverse such arbitrary decisions. In this case, we might catch up with the rest of the world by recognizing two species, and I so recommend.



I recommend returning to the English name American Flamingo, following Gill and Wright (2006) rather than using Caribbean, as done by Sangster (1997) and Knox et al. (2002). There is a population in the Galapagos, which is American but not Caribbean.

#### Literature

Gill and Wright. 2006. You know.

Knox, A. G., et al. 2002. Taxonomic recommendations for British birds. *Ibis* 144:707-710.

Sangster, G. 1997. Species limits in flamingos, with comments on lack of consensus in taxonomy. *Dutch Birding* 19:193-198.

Richard C. Banks  
2 Nov. 2007

N.B. Every time I typed *ruber*, MSWord automatically “corrected” it to rubber. I hope I caught them all and re-corrected them.

### **Treat *Buteogallus subtilis* as a subspecies of *B. anthracinus***

This proposal would effectively remove a species from the current NACC list. The status of *Buteogallus "subtilis"* has long been debated. A detailed historical summary of the varying treatments (as a species vs. a subspecies of *B. anthracinus*) as well as a thorough morphological and distributional analysis has recently been published by Clark (2007), who concludes that *subtilis* is best considered a subspecies of *anthracinus*. I will summarize the main points below.

Thayer and Bangs described *subtilis* (as a species, following conventions of the time) in 1905 from two specimens from I. Gorgona off the Pacific coast of Colombia (interestingly, an island with only a few very tiny patches of mangroves and no mangrove specialists, although there are abundant mangroves on the adjacent mainland coast). Its characters were differences in size and wing coloration from *B. anthracinus*. The taxon was first considered a subspecies of *anthracinus* by Chapman in 1926 and then by Swann in 1930, correcting his earlier erroneous assignment as a subspecies of *B. urubitinga*; he also described the race *bangsi* from the Pacific coast of NW Colombia and E Panama (original citations of these and subsequent authors are given in detail by Clark 2007). Peters (1931), on the other hand, treated *subtilis* as a distinct species, giving its range as the Pacific coast from El Salvador to S Ecuador but without explanation or justification (including for extending its range so far northward). Most subsequent authors disagreed, including Amadon (1961) and Brown and Amadon (1958), until Monroe (1963) resurrected species status for *subtilis* and described a new race thereof from El Salvador and Honduras, based mainly on discontinuities in habitat and size, as well as the apparent propensity of coastal birds to feed mainly upon crabs (although crabs have often been reported in the diet of inland birds as well). Subsequently Amadon in 1979 reversed his opinion and considered *subtilis* a species, apparently following Monroe, although he presented no detailed analysis: he gave its range as N to S Mexico, apparently based upon a mislabeled specimen. The AOU treated *subtilis* as a species in 1983 and 1998, also surely following Monroe, who chaired the checklist committee. By contrast, virtually every author with field experience of these birds including (from south to north) Howell & Webb in Mexico, Land in Guatemala, Stiles & Skutch and Slud in Costa Rica, Wetmore and Ridgely in Panama, Hilty & Brown in Colombia and Ridgely & Greenfield in Ecuador, have failed to find convincing evidence for species status of *subtilis*. In spite of this, Thiollay treated *subtilis* as a species in HBW, albeit with reservations, and this was followed by Ferguson-Lees and Christie in a recent book on raptors of the world (reference at SACC Bibilo page). Finally, Clark (2007) presented a detailed analysis based upon examination of ca. 140 adult specimens including both inland and coastal birds, from Mexico to extreme N Peru, as well as field experience from Mexico to Ecuador and Venezuela. He found that birds with the characters of *subtilis* occur on the Pacific slope from N Peru to E Panama; intergradation with nominate *anthracinus* occurs in E Panama and perhaps in NW Colombia, (*bangsi*, considered barely distinguishable from *subtilis* by Wetmore,

presumably represents such an intergrade). He failed to find a clear-cut difference in size, coloration or vocalizations between coastal and inland birds northward, although noting a tendency towards smaller size in mangrove birds, and recommended treating *subtilis* as a subspecies of *anthracinus*. I note that on the Pacific slope of at least Colombia, only *subtilis* is recorded: there is no "coastal-inland" dichotomy. I might mention that in Costa Rica, Manuel Marín and I collected several birds in mangroves and inland and found no differences in size or vocalizations (some mangrove birds were as large as inland ones, other were smaller). The small size of the mangrove-inhabiting race *utilensis* off the Caribbean coast of Honduras can best be taken as an indication that a total diet of salt-water crabs stunts one's growth in Black-Hawks, and the larger size of some coastal birds of the Pacific likely indicates that some of their food was small vertebrates, or came from less saline areas.

In sum, considering the detailed analysis of Clark (2007) in particular (which I will admit I was gratified to find was in complete agreement with my own experience), I strongly recommend a YES on this proposal.

Reference:

Clark, W. S. 2007. Taxonomic status and distribution of Mangrove Black Hawk *Buteogallus (anthracinus) subtilis*. Bulletin of the British Ornithologists' Club 127:110-117.

(Other references are cited in full by Clark (2007); a pdf of this paper will be available at NACC website.

F. Gary Stiles, Nov. 2007

---

---

This proposal passed SACC unanimously, with the following comments submitted:

Comments from Remsen: "YES. Burden of proof now falls on those continuing to rank *subtilis* as a species. Mark Robbins told me that some genetic data are forthcoming, but I think that Clark's overview of the evidence is sufficient at this point to sink *subtilis* as a species-level taxon based on what is in print to-date."

Comments from Stotz: "YES. Sorry to see *subtilis* go, but it seems pretty clear that it is now the thing to do."

Comments from Jaramillo: "YES - This seems similar to the *Buteo polyosoma* situation, although my guess is that a paper to counter this new arrangement will not be produced."

Comments from Robbins: "YES. Based on the Clark's morphological data *subtilis* appears not to deserve species status. I have collected or facilitated obtaining genetic material of "*subtilis*" from mangroves in Ecuador, Panama, and most recently El

Salvador. Those data soon (hopefully) will be presented in a molecular based phylogeny of *Buteogallus* (Fleischer, Olson, Nyari and I) that corroborates that *subtilis* does not merit species status."

Comments from Nores: "YES. Los datos de Clark, los agregados por Stiles y los datos moleculares mencionados por Robbins, no dejan dudas de que se trata de una subespecie. Por distribución (ambas en Central América) parecía poco probable que fuera una subespecie, pero si *subtilis* está restringida a la zona pacífica donde no existe *anthracinus*, no existen problemas."

Comments from Pacheco: "YES. Diante das informações disponíveis – incluindo o testemunho de Robbins acerca dos dados moleculares – eu concordo com a subordinação sugerida."

Comments from Zimmer: "YES. Like Gary, I'm happy that the detailed analysis by Clark (and, apparently, forthcoming molecular analysis) was consistent with my own field experience. Vocalizations of *subtilis* from mangroves in Pacific Costa Rica are identical (to my ear) to those of North American "Common Black-Hawks" from Arizona/New Mexico/Texas, to the extent that tape playback of mangrove birds elicits an immediate response from our birds. Good to clean this one up."

### Change English name of *Cnipodectes* to Twistwing

*[Note from Remsen: this is nearly verbatim from Thomas Donegan's proposal to SACC. And for those of you who just can't get enough Twistwing discussion, I have also pasted in below SACC proposal 184 so you can see the original arguments.]*

In SACC Proposal no. 184 (which I posted in October 2005), a majority of SACC members voted in favor of changing the name of *Cnipodectes subbrunneus* from Brownish Flycatcher to Brownish Twistwing (6-4). However, due to SACC voting rules, the proposal was rejected as not achieving 7 votes. Interestingly, following committee member changes and the fact that native Spanish or Portuguese speaking members no longer vote on English names, if everyone who voted on the previous proposal and who still has a vote maintains their vote on proposal 184, this one will now pass.

Committee membership changes and gripes about the SACC voting system are not good reasons for a new proposal to be considered. There are however important reasons to reconsider this issue. First, a new *Cnipodectes* has been described from Peru (Proposal 297). It would be sensible to unite these two congeners under the English name "Twistwing", given that the two species share similarly modified primaries. Secondly, despite SACC's approach and a willingness on the part of the ornithological community generally to follow its recommendations, several authors of key publications have made a point of not following SACC on the English name for *Cnipodectes*: (i) Restall et al. 2006 Birds of Northern South America purported to use SACC recommendations for English names throughout but, without drawing attention to the point, used "Twistwing" for *Cnipodectes*; (ii) Salaman et al. 2007 Checklist of Birds of Colombia followed SACC taxonomy and nomenclature on all but a short list of specified issues, including usage of "Twistwing"; and (iii) the *Auk* allowed Lane et al. (2007) to use "Twistwing" in assigning an English name for the new *Cnipodectes* species, despite the AOU's own checklist committee having rejected the name for the genus.

In summary, SACC's decision in Proposal 184 was supported only by a minority of committee members; and other persons have gone out of their way to avoid SACC's recommendation. Reasons of principle and usage for changing the name for *Cnipodectes* were set out in Proposal 184. The SACC use of "Flycatcher" is destabilizing in the face of widespread use of the name "Twistwing" and would create a novel name for a recently described species, another avoidable negative outcome. I recommend a yes vote, again.

Thomas Donegan, November 2007

=====

Comments from Remsen: "YES. In spite of the numerous brilliant and cogent reasons I presented previously for retaining the cherished and charismatic name Brownish Flycatcher for *C. subbrunneus*, I recognize that the cutesy name Twistwing is here to stay. Add to the list of traitorous capitulators: J. Fitzpatrick et al. in HBW, as well as the devious and cunning Dan Lane, whose preferred name Rufous Twistwing for *C. superrufus* leaves Rufous Flycatcher 'preoccupied' by *Myiarchus semirufus*. Wounded and sulking, I remain coiled like spring ready to retaliate with a barrage of NO votes on subsequent proposals."

Comments from Stiles: "YES. I liked "Twistwing" before and I like it now!"

Comments from Stotz: "YES. I hate to vote for Twistwing, having voted against it in the past, but I guess I don't see a clear alternative. The other option would be to create a new name for the new species of *Cnipodectes* (the Lane et al Flycatcher or the Status Quo Flycatcher?), which doesn't do much to maintain stability."

Comments from Jaramillo: "YES - I think Twistwing is an absolutely fantastic name. The issue of Rufous Flycatcher being preoccupied also clarifies this choice."

Comments from Robbins: "YES. I favored "twistwing" the first go around. Perhaps we should now revisit the English name for *Neopipo*, and get it right this time by calling it Cinnamon Tyrant!"

Comments from Zimmer: "YES. I liked (and voted for) "Twistwing" the first time around for *subbrunneus*, and nothing since has changed my mind. The preoccupation of "Rufous Flycatcher" by *Myiarchus semirufus* provides even further ammunition for this change. I congratulate "the devious and cunning Dan Lane" for engineering this coup, although the image of a "wounded and sulking" Van Remsen "coiled like a spring ready to retaliate" makes for a sufficiently scary scenario for future English name proposals."

---

The original prop 184:

Proposal (#184) to South American Classification Committee: Change English name of *Cnipodectes subbrunneus*

*Cnipodectes subbrunneus* has been given the English name "Brownish Twistwing" in most recent leading textbooks (e.g. Ridgely & Tudor, 1994; Ridgely & Greenfield, 2001; Fitzpatrick, 2004) and some other publications (e.g. Lopez-Lanus, 2000). However, older works (e.g. Hilty & Brown 1986), the checklist of the birds of the world (Dickinson, 2003) and regional checklists (Rodner et al., 2000; Salaman et al., 2001) have used the name "Brownish Flycatcher". This species has also been referred to as "Brown Flycatcher" (e.g. Wetmore, 1972). The current draft of the SACC checklist states that a proposal is needed for the English name of this species.

Those who have observed *Cnipodectes* in the hand or in a museum will have noted its unusual primaries, which really do appear superficially as if they have been twisted around the mid-shaft. Although various other tyrannids, cotingids and piprids have modified primaries (e.g. in *Mionectes*, *Xenopsaris*, *Lipaugus* and *Machaeropterus*), no other genus of which I am aware shows this bizarre form of primary modification. Interestingly, there are reports of an undescribed *Cnipodectes* from Peru: see <http://www.granperu.com/discoveries.htm> and photograph at [http://www.ornithomedia.com/infos/breves/breves\\_art1\\_5.htm](http://www.ornithomedia.com/infos/breves/breves_art1_5.htm) which is being proposed to be described as a "twistwing", suggesting that it too may have similarly modified primaries. The names "Brown Flycatcher" and "Brownish Flycatcher" do little to distinguish *Cnipodectes* from other tyrannids, a very large number of which are brown or brownish. Zimmer (1939) further noted that the toe structure of *Cnipodectes* suggests a relationship to the Pipridae, not the Tyrannidae. The name "Twistwing" would work in either family, unlike "Flycatcher".

The case for majority usage of "Brownish Flycatcher" vs. "Brownish Twistwing" in the published scientific literature appears to be a tie. Turning to less formal publications, a Google search showed 172 hits for Brownish Twistwing and 423 for Brownish Flycatcher, suggesting that the latter may be more widely used. The HBW treatment may, however present a sea change towards the use of Twistwing, given the impact of that book and the fact that the relevant chapter was not authored by Robert Ridgely, the main proponent of this name to date. "Brownish Twistwing" also has the advantage of being family-neutral and is a more informative and original name in a family plagued with English name homogeneity. A "yes" vote would be for "Brownish Twistwing". A "no" vote would be to retain "Brownish Flycatcher".

#### References:

DICKINSON, E. C. (ed.). 2003. The Howard and Moore complete checklist of the birds of the World, Revised and enlarged 3rd Edition. Christopher Helm, London, 1040 pp.

FITZPATRICK, J. W. 2004. Family Tyrannidae (tyrant-flycatchers). Pp. 170-462 in "Handbook of the Birds of the World, Vol. 9. Cotingas to pipits and wagtails." (J. del Hoyo et al., eds.). Lynx Edicions, Barcelona.

LOPEZ-LANUS, B. 2000. An unusual altitudinal record of the Brownish Twistwing *Cnipodectes subbrunneus*. *Cotinga* 12 (2000): 74.

RIDGELY, R. S., AND G. TUDOR. 1994. The birds of South America, vol. 2. Univ. Texas Press, Austin.

RIDGELY, R. S., AND P. J. GREENFIELD. 2001. The birds of Ecuador. Vol. I. Status, distribution, and taxonomy. Cornell University Press, Ithaca, New York.

RODNER, C., M. LENTINO, AND R. RESTALL. 2000. Checklist of the birds of northern South America. Yale University Press.

SALAMAN, P., T. CUADROS, J. G. JARAMILLO & W. H. WEBER. 2001. Lista de Chequeo de las Aves de Colombia. Sociedad Antioqueña de Ornitología, Medellín, Colombia.

WETMORE, A. 1972. The birds of the Republic of Panamá, part 3. Smithsonian Misc. Collect., vol. 150.

ZIMMER, J. 1939. Studies of Peruvian birds, No. 31. Notes on the genera *Myiotriccus*, *Pyrrhomyias*, *Myiophobus*, *Onychorhynchus*, *Platyrinchus*, *Cnipodectes*, *Sayornis*, and *Nuttallornis*. American Museum Novitates 1043: 1-15.

Thomas Donegan, October 2005

=====  
Comments from Remsen: "NO (barely). I like the clever name Twistwing and could be talked out of a NO vote on this after HBW adopted it. For now, I'll stick with Flycatcher for the sake of historical stability. One disagreement I have is that "Brownish Flycatcher" does not distinguish this bird from other tyrannids. Ridgway (1907), who first used the name (or actually "Brown Flycatcher", but that name "preoccupied" by *Muscicapa dauurica*), knew what he was doing. Although a myriad of small flycatchers are various shades of greenish, olive, olive brown, rufous, and reddish brown, none is really as truly solid BROWN as *Cnipodectes*. A browse (or is it brownse?) through our synoptic series produced only *Elaenia pelzelni*, *Hemitriccus obsoletus*, and *Contopus pertinax* as rivals in their overwhelmingly stunning brownosity, brownacity, and brownaceousness, and these three do not overlap with *Cnipodectes* in either range or habitat. So, given how miserably similar most small tyrannids are, and how difficult it is to come up with a distinctive descriptive name, "Brownish Flycatcher" isn't so bad, and actually pretty good -- after-all, I haven't heard any rumblings about "Greenish Elaenia," "Yellow-green Tyrannulet," or "Yellow-olive Flycatcher," all of which must make the name-improvers lose sleep.

"Additional older literature that used Brownish Flycatcher:

"1955 (Eisenmann, Middle America) = "Brownish Flycatcher"  
1964 (Meyer de Schauensee) = "Brownish Flycatcher"  
1966 (Meyer de Schauensee) = "Brownish Flycatcher"  
1970 (Meyer de Schauensee) = "Brownish Flycatcher"  
1976 (Ridgely, Birds of Panama) = "Brownish Flycatcher"  
1982 (Parker et al., Peru) = "Brownish Flycatcher"



Additional comments from Donegan: "In the proposal, I said that the names "Brown Flycatcher" and "Brownish Flycatcher" do little to distinguish *Cnipodectes* from other tyrannids, a very large number of which are brown or brownish. I agree with Van's point on the relative brown-ness of the bird and perhaps did not express this point as well as I could have. The point here revolves around Flycatcher vs Twistwing, as "Brownish" would remain. The term "Flycatcher" does not distinguish this unusual genus from other "Flycatchers", a term which straddles many genera (and indeed two families). In the Neotropics, "Flycatchers" are big (e.g. *Megarynchus*), medium-sized (e.g. *Myiozetes*, *Empidonax*) and small (e.g. *Mionectes*, *Leptopogon*, *Tolmomyias*, *Myiophobus*), brightly marked (*Pyrocephalus*) and dull (*Contopus*), but none has a "twisted wing" like this. (I am of course not proposing new English names be given to all those other genera!) Although discussions as to appropriateness of name are interesting and of relevance, the main argument here is one of stability and of which name has become more accepted, which, as stated is a finely balanced issue."

Comments from Stiles: "YES, in part out of cussedness and in part because I like the name, which does have the advantages of unique applicability and much recent usage. I don't know if anyone is seriously advocating placing *Cnipodectes* in Pipridae, but if by some stretch of the imagination such were to occur, "brown (ish) manakin" could be a bit confusing as *Schiffornis turdinus* was for long considered a manakin and is certainly pretty "brown"!"

Comments from Jaramillo: "YES - One day, the votes on English names will end correct? At some point we will have all the good names that are stable and everyone agrees are not misleading, right? In this case, to have such an odd and unique structure on a bird not be reflected in the name, and leave it as Brownish Flycatcher seems like a lost opportunity to me. It is memorable."

Comments from Zimmer: "YES". As the proposal's author points out, the appropriate "Brownish" modifier is not in play; only the uninspired "Flycatcher". "Twistwing" is not only clever, it calls attention to a unique character of the bird, which, although not something you would notice in the field, does produce the dramatic mechanical wing whir that you hear when the bird is displaying or roaring back-and-forth past you in response to playback. Furthermore, with the recent discovery of what appears to be a new species of *Cnipodectes* in SE Peru, we actually have the opportunity to cement a name change that could convey even more information regarding the relationships of the bird to another species (and hopefully, the describers of the new species will suggest "Twistwing" as part of their English name construction!)."

Comments from Robbins: "YES. . I fully endorse using the very appropriate English name "twistwing" for *Cnipodectes*."

Comments from Nores: "YES. Brownish Flycatcher no significa nada, hay muchos flycatchers con esas características. En todo caso podría ser Brown Flycatcher, pero pienso que Brownish Twistwing es mucho más apropiado."

Comments from Pacheco: "NO. Alinho-me, neste caso, a defesa de Remsen à estabilidade histórica proporcionada pela manutenção de "Brownish Flycatcher . Este é o nome em Remsen & Parker (1989) e A.O.U (1998)."

**Lump the genera *Arremon*, *Buarremon*, and *Lysurus*  
in an expanded genus *Arremon***

*[Note from Remsen: this is a SACC proposal forwarded to NACC with Daniel Cadena's permission; it passed unanimously, and SACC members' comments appended]*

Effect on NACC list: Three currently recognized genera would be lumped into a single genus.

Background and New information: Relationships among genera of Neotropical Emberizine finches and sparrows have traditionally been inferred based on superficial analyses of phenotypic variation, but are starting to be assessed in a more rigorous fashion as molecular phylogenetic studies start to accumulate. In particular, the affinities of birds in the genus *Buarremon* have been somewhat uncertain. The three species currently recognized in the genus were merged for quite a while in the genus *Atlapetes*, whose members are now known not be particularly closely related to them. Hackett (1992) indicated that species of *Buarremon* are more closely allied to members of the genus *Lysurus* than to *Atlapetes*, a result corroborated recently by an ongoing study on the phylogeny of all the nine-primaried oscines, whose preliminary analyses clearly show that *Buarremon* belongs in a strongly supported clade with *Lysurus* and another similar genus, *Arremon* (J. Klicka et al., unpublished data).

A recently completed molecular phylogenetic study (Cadena et al. 2007; pdf available at: [http://evolvert.uniandes.edu.co/Site/Publicaciones\\_files/mpe2007-1.pdf](http://evolvert.uniandes.edu.co/Site/Publicaciones_files/mpe2007-1.pdf)) analyzed the relationships among taxa in these three genera based on sequences of four mitochondrial (ND2, cytb, ATPase 6, ATPase 8) and two nuclear (MUSK, ACO1) genes. Mitochondrial data provided strong support for a clade formed by the *Buarremon torquatus* complex and the genus *Arremon*, indicating that *Buarremon* as currently defined is not monophyletic. A clade formed by *B. brunneinucha* and *B. virenticeps* consistently appeared to be sister to the genus *Lysurus*, but this was not strongly supported. Variation in one of the nuclear genes was entirely consistent with these results, but analyses of sequences of the other nuclear gene placed *Arremon* outside a clade formed by *Buarremon* and *Lysurus*, albeit with relatively weak support.

When mitochondrial and nuclear data were analyzed together, strong support (1.0 Bayesian posterior probability and 80% maximum-likelihood bootstrap) was obtained for the *B. torquatus* - *Arremon* clade. In these combined analyses, the *B. brunneinucha*-*B. virenticeps* clade was recovered as sister to *Lysurus*, but support for this relationship remained relatively low (although it closely approached significance in a Bayesian analysis, with a posterior probability of 0.94). It is noteworthy that a clade formed by all taxa placed in *Buarremon* in the current classification was not recovered in any single tree in this study, even considering the 120,000 trees that were sampled in Bayesian

MCMC searches. From a Bayesian perspective, this means that given the data and the models employed in analyses, the probability that *Buarremon* is monophyletic is zero.

Analysis: Molecular data indicate strongly that as currently defined, *Buarremon* is not a monophyletic genus because *B. torquatus* is the sister taxon to the genus *Arremon*, which in retrospect is not entirely surprising considering that some taxa currently placed in *Arremon* (e.g. some *A. taciturnus*) seem like "miniature" versions of *B. torquatus* given their high similarity in plumage. In turn, *B. brunneinucha* and *B. virenticeps* seem to form a clade with *Lysurus*, but support for this relationship is not very compelling. In any event, the problem of the non-monophyly of *Buarremon* needs to be addressed. I believe that the best alternative is to merge the three genera into a single genus for two reasons. First, birds currently placed in *Buarremon*, *Arremon*, and *Lysurus* are rather similar in overall plumage, behavior, microhabitat, and vocalizations, so lumping them does not result in a highly heterogeneous genus. Second, because the sister relationship between *Lysurus* and *B. brunneinucha* - *B. virenticeps* is not very strongly supported, it seems that recognizing two genera (one for each of these clades) would not be the best option because support for the monophyly of one of the clades is weak. Because it has priority, the name for the expanded genus would be *Arremon*. For a more detailed discussion of the issues involved, please refer to Cadena et al. (2007).

Recommendation: I would suggest voting YES to lump *Buarremon*, *Arremon* and *Lysurus*.

#### References

Cadena, C. D., J. Klicka & R. E. Ricklefs. Evolutionary differentiation in the Neotropical montane region: molecular phylogenetics and phylogeography of *Buarremon* brush-finches (Aves, Emberizidae). *Molecular Phylogenetics and Evolution*, in press.

Hackett (1992) - See SACC Literature Cited.

C. Daniel Cadena, June 2007

=====  
Comments from Remsen: "YES. As Daniel noted above, an expanded *Arremon* is the only classification that is consistent with current phylogenetic data. Whether current *Buarremon* is paraphyletic may not be 100% certain from Cadena et al., but the monophyly of *Buarremon* + *Arremon* + *Lysurus* seems indisputable."

Comments from Nores: "YES. Al mostrar que *Buarremon* no es monofilético resulta aceptable que la mejor manera de resolver la cuestion es unificar los géneros en *Arremon* que es el más antiguo. Queda por resolver si *A. virenticeps* y *A. crassirostris* deben ser incluidos en la SACC list."

Comments from Stotz: "YES. Makes sense in terms of morphology. Once broad *Atlapetes* bit the dust it seemed clear that there would be further refinements of the taxonomy of the Brush-finches and allies."

Comments from Jaramillo: "YES - Logic and data tell me it is so, although my heart is fighting it. I guess it is fighting the issue of having what seems like such a nice and neat genus, *Arremon*, become somewhat more heterogeneous. But don't let my sentiments be misunderstood, the data appears solid, and as mentioned in the proposal it makes sense for various reasons (voice for example, habitat etc.)."

**Change linear sequence in *Tangara***

Effect on NACLIC: All that this would do is change the linear order of species in *Tangara* to reflect published data.

Background and New information: The current linear sequence is essentially unchanged from Storer's sequence in the Peters checklist and is maintained largely by historical momentum rather than explicit rationale:

Current NACC sequence =

*Tangara inornata*

*Tangara cabanisi*

*Tangara palmeri*

*Tangara florida*

*Tangara icterocephala*

*Tangara guttata*

*Tangara gyrola*

*Tangara lavinia*

*Tangara cucullata*

*Tangara larvata*

*Tangara dowii*

*Tangara fucosa*

Burns & Naoki (2004) sequenced about 1500 bps of two mitochondrial genes, cytb and ND2, for 43 of 49 species of *Tangara*. Their hypothesis of relationships within *Tangara* (their Fig. 2) contains about 23 nodes with 97-100% Bayesian support that involved more than 1 species. To incorporate that information on sister relationships into our classification, we have produced the following sequence, using the conventions of basal taxa listed first and allotaxa listed NW to SE. Otherwise, we then incorporated the remaining topology of their Fig. 2, but maintained historical stability wherever possible.

*Tangara palmeri*

*Tangara cabanisi*

*Tangara cucullata*

*Tangara larvata*

*Tangara guttata*

*Tangara fucosa*

*Tangara dowii*

*Tangara inornata*

*Tangara lavinia*

*Tangara gyrola*

*Tangara florida*

*Tangara icterocephala*

Some of this sequence will likely require tweaking with additional analyses, although Burns's unpublished data with more taxa and sequences strongly supports the suggested sequence. There are no sequence data for *cabanisi*, but if we consider *palmeri* a good predictor of the position of *cabanisi* (the Islers have them in the same species group), then they should go in the sequence right after *palmeri*.

Recommendation: Yes. Regardless of any future tweaking, the above sequence reflects published data, whereas the traditional one does not.

### References

BURNS, K. J., AND K. NAOKI. 2004. Molecular phylogenetics and biogeography of Neotropical tanagers in the genus *Tangara*. *Molecular Phylogenetics and Evolution* 32: 838-854.

Van Remsen & Kevin Burns, November 2007

---

---

*Here are SACC comments on the version of this proposal that passed there:*

Comments from Zimmer: "YES. Even if modifications are needed, this represents a distinct step forward."

Comments from Nores: "YES, aunque mirando el árbol de Burns y Naoki (2004) no me resulta tan claro como fue finalmente logrado el ordenamiento. De todos modos parece que es un buen avance comparado con la tradicional secuencia."

Comments from Stotz: "NO. Given that Kevin Burns was involved in creating this proposal, perhaps it is a bit hard to fathom that I am voting against it because of further data from Kevin. I feel this is similar to the *Hemispingus* case, where more data is indicating that there are broader issues that remain to be dealt with. In this case *Tangara* is not monophyletic, and given the limited information that is contained within the order of taxa in a large genus like *Tangara*, I would prefer to wait until the more complete dataset is available."

Comments from Stiles: "YES. The proposal best reflects the phylogenetic evidence available. I may be myopic, but I fail to understand Doug's objection here. The branch to *Tangara* in the Burns & Naoki paper has 100% Bayesian support in both trees, and it leads only to *Tangara* (no putative *Tangara* species on other branches, no non-*Tangara* on the *Tangara* branch; there are evidently several major clades within *Tangara*, but this does not affect the monophyly of the genus)."

Comments from Jaramillo: "YES - Even if refinements are needed in the future, the new order will be easier to work with than our present order. This applies to events such as the need to divide *Tangara* to maintain monophyly, or the addition of other species into

*Tangara* ... whatever may arise with further sampling of tanagers (reading between the lines of Doug Stotz's comments)."

Comments from Kevin Burns: "Doug is correct that our new data show that six species of *Thraupis* are imbedded within *Tangara*. Thus, *Tangara* is not monophyletic. These data are strongly supported with nuclear and mtDNA data. So, when our results are published, these species will need to be moved within the sequence. But note that Al Jaramillo is correct in his comments in that this wouldn't result in rearrangement of all the *Tangara*. These *Thraupis* species would simply be inserted as a group within the sequence. (most likely between *larvata* and *guttata* in the above arrangement).

"*Hemispingus* is a different story. In that case, the nonmonophyly of *Hemispingus* is the result of multiple species being inserted at multiple places within *Hemispingus*."



Proposal to AOU Check-list Committee:

**Change English name of *Gallinula chloropus* back from "Common Moorhen" to "Common Gallinule"**

Effect on AOU CL: This proposal would change the English name of a species on our list from a "globalized" name back to a "New World" name that was in use in this hemisphere for many decades.

Background: *Gallinula chloropus* was known in the W. Hemisphere as "Common Gallinule" from the 1957 AOU checklist but was changed to "Common Moorhen" in a Supplement sometime in advance of the 1983 AOU checklist. For more than a century prior to the 1983 list, it had been known as either Florida Gallinule or Common Gallinule, but always Gallinule. The change was a concession to the BOU to keep the "Moorhen" in the name; the species there had been known "forever" as the Moorhen.

"Analysis": From before I joined AOUCCL in 1984 through the present, if I were to pick one name change that angered people more than any other, this is it. As an AOUCCL member, I have limply tried to defend the name in that it emphasizes that Purple Gallinule is not in the same genus. The response I always get (besides "so what!") is that how could AOU have changed the name away from Gallinule when the genus is *Gallinula*? How can a *Porphyryula* be called a Gallinule but a *Gallinula* called a Moorhen??? The taxonomically oriented further point out that this is the type species for the genus, and so if any species in the world was to be called Gallinule, this is it. Those who don't care about the taxonomy think that the name Moorhen itself is totally absurd. The species has nothing to do with moors, per se, and even if it did, we don't have any gosh-darned moors in this hemisphere. And then there's the "hen" part. What is that all about, they ask? This is the point when I mumble something about how quaint the British name is and try to change the subject after making it clear that the change happened, of course, before my tenure on the Committee. I cannot tell you how many times I've had this conversation when the dreaded topic of changing English names comes up. I am reasonably certain that if we were to poll ornithologists and birders, this name change would get by far the most votes for the most unpopular change "we" ever made. Did I mention that this happened before I was on the Committee?

The breaking point for me came when, at the Neotropical Ornithology Congress in Venezuela this year, even the Spanish-first speakers were ridiculing it and using it as an example of an absurd common name. To make matters worse, the endemic Neotropical *Gallinula* still retains the name Gallinule (Spot-flanked Gallinule, *G. melanops*). Our credibility as a body capable of governing English name usage was questioned. Yes, I mentioned to them that this change happened before I was on the Committee.

Although most Old World *Gallinula* are now called Something Moorhen, two Australian species are called Native-hen, so the genus itself already does not go by a single English name.

The globalizers will go ballistic if we backtrack on this one, and there will be some who say that, heck, we've lived with Moorhen for 25 years and to backtrack now looks bad. I am reasonably certain, however, that the vast majority of our clientele, professional and amateur, will welcome a return to a better and historically traditional name. In fact, many of you may have noticed that many people refuse to use Moorhen in the field anyway except to fill out official checklists, and that many state game agencies retain *Gallinule*.

Recommendation: YES (my self-esteem in public circles is at stake).

Van Remsen, November 2007

Addendum by Remsen in email dated 19 Dec 2007: I forgot to point out in original version that at least four major books in South America have essentially blown off the AOU change and now use "Common Gallinule": Hilty & Brown (1986, Birds of Colombia), Fjeldså & Krabbe (1990, Birds of the High Andes), Haverschmidt & Mees (1994, Birds of Suriname), and Ridgely et al. (2001, Birds of Ecuador)."

**Add Pallas's Warbler (*Phylloscopus proregulus*)**

**Background:** On 25 September 2006, a Pallas's Warbler was found and photographed at Gambell, St. Lawrence Island, Alaska. The record has subsequently been accepted by the Alaska Committee and by the ABA Checklist Committee (Pranty et al. 2007) and an article with published photos was published in *North American Birds* (Lehman and Rosenberg 2007). A published photo in color is also in Pranty et al. (2007). The record is non controversial and is extremely well documented.

**Taxonomy:** The species is now generally considered monotypic. The Himalayan subspecies (*P. p. chloronotus*) was split as a full species on the basis of distinct differences in vocalizations and lack of response to playback to calls of the other taxon. There are minor morphological differences as well (Alstrom and Olsson 1990). The English name of Lemon-rumped Warbler is used now for *P. chloronotus* (Dickinson 2003 and Inskipp et al. 1996). Another taxon, *P. p. kansuensis*, is treated by Inskipp et al. (1996) as a subspecies of *P. proregulus*, but they note that Alstrom et al. in prep. have determined that the calls and songs are markedly different and that playback experiments strongly suggest it be treated too as a separate species. It is treated as separate by Dickinson (2003).

**I recommend** that this species be added to the Check-list given the excellent documentation, including numerous photos.

**English name-** The English name of Pallas's Warbler is fairly widely used, but since there is a Pallas's Grasshopper Warbler (also called Rusty-rumped Warbler) for *Locustella certhiola*, the name of Pallas's Leaf Warbler (Dickinson 2003) is an alternative, or would it be Pallas's Leaf-Warbler? I prefer the latter name, but that is an additional issue to be decided.

**Position on Check-List-** From both Dickinson (2003) and Inskipp et al. (1996) the linear placement within *Phylloscopus* would have it coming after Dusky Warbler (*P. fuscatus*) and before Yellow-browed Warbler (*P. inornatus*).

Literature cited for draft account:

Alstrom, P and U. Olsson. 1990. Taxonomy of the *Phylloscopus proregulus* complex. *Bull. Brit. Orn. Club* 110:38-43.

Dickinson, E.C. (Editor) 2003. *The Howard & Moore Complete Checklist of the Birds of the World*. 3<sup>rd</sup> Edition. Princeton University Press, Princeton, New Jersey.

Inskipp, T., N. Lindsey, and W. Duckworth. 1996. *An Annotated Checklist of the Birds of the Oriental Region*. Oriental Bird Club.

Lehman, P.E., and G. H. Rosenberg. 2007. First North American record of Pallas's Warbler (*Phylloscopus proregulus*) at Gambell, Alaska. *North American Birds* 61:4-8.

Pranty, B., J. L. Dunn, S.C. Heintz, A. W. Kratter, P.E. Lehman, M.W. Lockwood, B. Mactavish, and K.J. Zimmer. Annual Report of the ABA Checklist Committee: 2007.

Jon L. Dunn  
3 December 2007

Note by Banks—English name is Pallas's Leaf Warbler in Gill and Wright. No hyphen.