

Additional comments from Thomas Donegan: I'd like to thank Steve Hilty for taking the time to read through our Bull BOC paper and to make these comments on this proposal. The general tenor of Hilty's argument is that further evidence is required before this split is sanctioned. As mentioned in the proposal, that is a reasonable point of view as an abstract general principle. However, various of the points Hilty has made are frankly baffling.

A principal thrust of Hilty's argument seems to be that the back colour of *A. l. nigrifrons* is not significantly different from northern *A. latinuchus*. This is simply not the case. He also states that the back of *A. l. nigrifrons* is both "tinged olive" and "very dark" grey. These are all both entirely false statements. A series of photographs is provided below, with commentary, to demonstrate this.

I hope that committee members and others find the below photographs, which include the "Perija bird", to be of interest. The *A. sp.* from Perija is probably either an *A. albofrenatus*/*A. l. nigrifrons* hybrid (if *A. albofrenatus* extends this far north), a colour morph of *A. l. nigrifrons* or an undescribed taxon. My suspicion is that it is the latter, though molecular results are not conclusive. Klicka, Cadena et al. will explore this further soon in their paper. Hilty also asks who is going to Perija. I plan to do so with others to resolve this issue and we have already applied for a permit.



Above: *A. l. yariguierum* holotype (left) and paratype (right). Note the lack of contrast between the back and facial mask in this species, the lack of black on the crown and the lack of black in the chin and moustachial region.



Above: left to right: *A. melanocephalus*, *A. l. nigrifrons* and *A. sp.* ("Perijá bird"). Note the similarity in facial patterns of these three taxa. Contrast this to *A. l. yariguierum* above.



Above: Left: series of *A. l. nigrifrons* from Phelps collection in Venezuela (provided to me by R. Restall in 2004); note lack of olive in back and rather light/neutral grey back contrasting with black mask (cf. *A. l. yariguierum* photograph). Right: dorsal views of (left to right) nominate Moustached Brush-Finch *A. albofrenatus*; Unknown Brush-Finch *A. sp.* = "Perija bird"; and the taxon in question here, *A. l. nigrifrons*. Taken at ICN. Again, note the lack of olive in the *A. l. nigrifrons* back and the rather light shade of grey (compared to other *A. latinuchus* taxa, some of which are illustrated below).



Above: dorsal views of (left to right): *A. fuscolivaceus*, *A. melanocephalus*, *A. l. nigrifrons*, Perija bird, nominate *A. albofrenatus*, *A. l. yariguierum* immature, *A. l. yariguierum* holotype, *A. l. elaeoporus*, *A. l. cauae* and *A. l. spodionotus* (ICN). Note the difference between light(er) grey backed *A. l. nigrifrons* (third from left) and the similarity of this taxon to *A. melanocephalus* (second from left). All *A. latinuchus* taxa (five birds on right) have much darker backs.



Above: ventral views of (left to right): *A. fuscolivaceus*, *A. melanocephalus*, *A. l. nigrifrons*, Perija bird, nominate *A. albobrenatus*, *A. l. yariguierum* immature, *A. l. yariguierum* holotype, *A. l. elaeoprorus*, *A. l. cauae* and *A. l. spodionotus* (ICN). Note that *A. melanocephalus*, *A. l. nigrifrons* and *A. sp.* (Perija) (second to fourth from left) have identical moustachial markings, a feature not shared with any other of the taxa illustrated.

The back colour analysis for *A. latinuchus* taxa was not explored in great detail in our paper. However, for completeness, the following patterns are observed. The current taxa in *A. latinuchus* (*A. l. nigrifrons* aside) essentially get progressively greyer in their backs going south, with some individual exceptions. The northernmost forms, *A. l. yariguierum* and *A. l. elaeoprorus* (Antioquia, northern Central Andes) approach black in adult form; *A. l. cauae* (southern Cauca valley), *A. l. spodionotus*, *A. l. comptus* and the nominate are very dark (slate) grey. *A. l. baroni*, the southernmost taxon in this complex from Peru, has a (light/neutral) grey back like *A. l. nigrifrons* (and also a light nape.) *A. l. chugurensis* (Peru, north of *baroni*'s range) is supposedly a darker grey than *A. l. baroni* (though I have not seen skins of this taxon). I hope that Hilty is not suggesting that *A. l. nigrifrons* is related to *A. l. baroni*. This is, however, the only *A. latinuchus* taxon that resembles *A. l. nigrifrons* closely in its back colour. I suspect strongly that *A. l. baroni* is not closely related to the northern taxa though have not seen any molecular analysis of this.

The above photographs should be sufficient evidence to support the claim in our article that the back of *A. l. nigrifrons* is substantially lighter than that of all other geographically close 'races' of *A. latinuchus*. I have Munsell codes of back colours of all *Atlapetes latinuchus* except *A. l. chugurensis*. If anyone would like these data please let me know. In the interests of not boring people further, it is not included herein. No *A. l. nigrifrons* skins in COP have any olive in the back. The mystery *A. sp.* does have olive in the back (indeed, very strong olive), but there are no skins of this at COP.

Back colour (beyond presence/absence of olive pigmentation) was not a character in our phylogenetic analysis due to the non-discrete nature of its variation in the genus. Shifts in pigmentation can further be somewhat plastic in this genus, so this whole back colour debate may be a bit of a red herring. I will not therefore labour the point more.

I stand fully by the criticism above of Gwynne's plate in Hilty's book. *Birds of Venezuela* is a truly fantastic book generally and I refer to it frequently. BUT - Fact: the *A. l. nigrifrons* plate in *Birds of Venezuela* is simply not accurate. The back colour is inaccurate and Hilty has himself noted that the extent of black on the face could be better illustrated. I am very sorry if any offence is or was caused to Steve Hilty (or John Gwynne) by these comments as they are persons for whom I have a great deal of respect. However, let's not deceive ourselves here.

Although back colour patterns give some clues, what really sets *A. l. nigrifrons* aside from other taxa is the facial pattern. Indeed, in the description of *A. l. nigrifrons*, Phelps and Gilliard noted that this form is substantially different in patterning from other (then) *A. rufinucha* taxa, referring particularly to *A. (r.) melanolaemus* as that which probably most closely resembles it (now considered to be only very distantly related to *A. latinuchus* taxa per Garcia Moreno & Fjeldsa). No other *A. latinuchus* have a wide black mark across the forehead, extensive black on the moustachial region and chin or such a strongly marked grey cheek. This represents a substantial shift in morphological pattern, not just in pigmentation. *A. melanocephalus* of Santa Marta is the only other Atlapetes taxon in the northern Andes which shares exactly this moustachial and facial pattern; the close relation between these taxa was borne out in our phylogenetic analysis. If you look at some of the now split southern taxa, shifts in patterning between adjacent populations are typically associated with species limits, per the Garcia-Moreno & Fjeldsa paper.

I take serious objection to the following statement of Hilty's: "At this point one might just as easily align the Perijá birds with the new Atlapetes *latinuchus yariguierum*" and the reasoning that follows it. Back colour is explored above. Presence of absence of wing speculum, referred to by Hilty as a common character, is a somewhat plastic character, as described in the article, so I would not put great faith in that. In terms of geographical barriers, which Hilty claims are not substantial between *A. l. yariguierum* and *A. l. nigrifrons*, it is noteworthy that the high elevation taxa of the East Andes are separated by the lowest section of the Andes in the Catatumbo region in Norte de Santander and Cesar departments, where the ridge attains only elevations of around 1500-1700m for several tens of kilometers. Turning to morphological evidence, the differences between the new taxon *A. l. yariguierum* (E Andes) vs. each of *A. l. elaeoprurus* (C Andes) and *A. l. spodionotus* (southern main Andes) are rather subtle (explained and discussed in detail the article: essentially presence/absence of wing specula, extent of yellow in supraloral and apparently some small vocal differences from *A. l. spodionotus*). These taxa would seem most likely to be *yariguierum*'s closest relatives. See also the photographs above. Provisional molecular analysis noted in the article also supports this proposition. However, *A. l. nigrifrons* shows a considerable shift in patterning (and pigmentation) from *A. l. yariguierum*. The proposition that, on a basis of its morphology, the new taxon might be more closely related to *A. l. nigrifrons* (compared to the other taxa the differences with which we focused on in the article) is, frankly, implausible.

A recording of the new taxon is on [www.xeno-canto.org](http://www.xeno-canto.org) if Hilty would like to consider it further in the context of any wider study. We have recently got some new recordings and hope to post these soon. It would have been good to have been able to include data on voices of other taxa and probably would have been necessary were we proposing the new taxon *yariguierum* for species rank and advocating a wholesale splitting of the taxonomic grouping currently known as *A. latinuchus*. However, we did not. Recordings of various of the

Colombian taxa (e.g. *caucae*, *elaeoprorus*) simply do not exist (as far as I am aware), never mind the Perija populations.

Hilty comments on our work on voice of the new taxon are quite unfair and also inaccurate. We indeed go beyond saying that the voice is “typical of the genus”. Sonograms are presented together with some of *A. l. spodionotus* and we note small differences between these calls. We also note that further additional research is needed into voice in the genus in Colombia that may have taxonomic implications, which seems to be exactly what Hilty is saying.

There are some good reasons that could be made in favour of rejection of this proposal (as mentioned in it). In particular, Hilty reiterated the point made in the proposal concerning a lack of supporting vocal data. Whether possible paraphyly as demonstrated by morphological considerations is enough to sanction a split is a matter of degree of evidence that committee members should consider. Let’s debate that if there’s a debate to be had. The morphological analysis presented is thoroughly researched.

Incidentally, the article is now available online:

[http://www.proaves.org/IMG/pdf/Donegan\\_Huertas\\_Atlapetes\\_latinuchus\\_yariguierum-2.pdf](http://www.proaves.org/IMG/pdf/Donegan_Huertas_Atlapetes_latinuchus_yariguierum-2.pdf)