

broad and moderately rounded. Its upright crest, however, was initially overlooked owing to the light conditions. Bird surveys in southern Bhutan have not been exhaustive and one might expect that other species typical of the plains and lower foothills will eventually be added to the Bhutan list.

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An appraisal of recent taxonomic reappraisals based on character scoring systems

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Three recent papers (Collar 2006a, b, 2007b) present taxonomic reappraisals of a significant swath of Asian bird diversity, recommending elevation of numerous populations and subspecies to species rank (Collar and Pilgrim 2007). Certainly, attention to patterns of variation and differentiation and their implications for species limits in Asian birds is both welcome and badly needed, but several concerns arise from the methodology employed. This note aims to clarify the nature of this system in the context of modern species concepts, and to examine what actually it achieves and what it leaves unassessed. Although the methodology has yet to be formally described (listed as in preparation in 2006), the large number of taxonomic changes recommended (Collar 2006a, b, 2007b) and now codified in a standard ornithological reference (del Hoyo *et al.* 2007) begs careful discussion by the ornithological community, as wrong decisions can make for bad taxonomy and bad conservation action.

The scoring system (Collar 2006a) involves tallying differences between populations as major (3 points), medium (2 points), or minor (1 point), summing these scores, and using 7 as a criterion for species status. One publication (Collar 2006a) carries the caveat that species status cannot be achieved based solely on minor or mensural characters, and that ‘all mensural characters, no matter how highly statistically significant, are scored as minor characters.’ However, a more recent publication

states ‘In Collar (2006a) I only allowed morphometric differences to count as minor characters, for reasons of complexity of material and inadequately developed criteria, but here I regard this restraint as unnecessary’ (Collar 2007b). Curiously, in this more recent paper, which treats *Loriculus* hanging parrots in the Moluccas and Sulawesi, the split that is recommended depends on the size difference between *L. sclateri* and *L. amabilis* counting as a major difference (3 points)—were Collar to have followed his previous caveats, this pair of forms would differ only by 7–8 points, and would be more ambiguous as to whether it merits splitting.

GEOGRAPHY

A first question is the species concept on which this method is founded, as species concepts are the critical basis for these decisions (Zink and McKittrick 1995, Remsen 2005, Peterson and Navarro-Sigüenza 2006). Collar has on numerous occasions (Collar 1996, Collar and Spottiswoode 2005, Collar 2007a) expressed his dislike for the Phylogenetic Species Concept (PSC, which defines species based on diagnosability and monophyly). Although he has not (to our knowledge) referred explicitly to the Biological Species Concept (BSC) as the basis for the scoring system, it appears that the BSC forms the

basis of his taxonomic opinion. Mayr (1963) offered this BSC definition: 'species are groups of [actually or potentially] interbreeding natural populations that are reproductively isolated from other such groups.' Importantly, though, for consideration of Collar's work, Mayr commented on the same page, 'The more distant two populations are in space and time, the more difficult it becomes to test their species status in relation to each other ...' As such, careful consideration of the geographic relationships among forms under consideration becomes central to BSC decisions regarding species limits.

Nonetheless, at least in print, Collar apparently pays only occasional attention to the geography of the forms that he considers, and spatial position certainly does not enter into the quantitative scoring system. It is stated in the abstract (but only very indirectly in the text) of the most detailed explanation of the method (Collar 2006a) that the method is designed for application to allopatric taxa. Although some species accounts in the three papers mention allopatry, sympatry, or spatial arrangement, most focus solely on the history of taxonomic opinion and the character scores achieved. Most of the taxa treated, we concede, are insular or genuinely disjunct and allopatric, but several of the forms under consideration approach one another closely in range and could be considered parapatric (e.g., *Garrulax merulinus* group, *Spelaeornis chocolatinus* group, *Napothera rabori*), yet spatial trends are not discussed. Collar's clearest reference to geographic situation of the forms under consideration is in the discussion of *Yuhina castaniceps*, in which he states 'where the morphologically closest subspecies is separated geographically by one or more other taxa in the subspecies, there is a case for adding a point, which would be conveniently reassuring in this case' (Collar 2006a), which seems nothing short of an *ad hoc* manipulation. Without this extra point, the form *everetti* remains on the cusp of species-level distinction, and Collar admits that 'the differences, other than the crown, are very minor and debatable.'

Collar (2006a) dismissed the British Ornithologists' Union guidelines for assigning species rank (Helbig *et al.* 2002) as setting the bar too low, in essence allowing too many species to be recognised, but seems to have missed the useful, clear, and operational ideas set out in that publication. Helbig *et al.* (2002) not only reviewed conceptual ideas, but also laid out a necessarily complex, but clear and reasoned, operational plan for decisions regarding species limits. They correctly pointed out that decisions based on the same degree of phenotypic difference will often differ given distinct geographic relationships between the forms in question—Collar nonetheless relies in his discussions chiefly or exclusively on his scorings.

SUBJECTIVITY

A second major concern is the subjectivity involved in the scoring system (Collar 2006a, b, 2007b). To give a few examples, in the description of the method (Collar 2006a), he uses the phrase 'what I judge to be a major character' and in the species accounts we see wording such as 'it is a leap of faith' (p. 93), 'is probably variable' (p. 91), and 'should, for the moment, allow it species status' (p. 96), among other examples. Collar's scoring system is indeed

subjective—does a red crown *versus* a green crown merit the same score in a barbet as in a *Phylloscopus* warbler? The variable treatment of mensural characters described above is an additional concrete example. These decisions will inevitably end up being a simple matter of opinion and *a priori* biases—assigning a number to a subjective decision does not impart objectivity to the process. Thus, the scoring system, as currently described and implemented, adds a veneer of objectivity, yet remains almost entirely subjective, and this is not even mentioning the difficulties of deciding which differences in vocalisations are major or minor. Surely, this sort of opinion-driven taxonomy is not repeatable, and research generally is required to be repeatable for it to be considered science.

The operational characteristics of this quantitative character scoring method distill to the following: (1) focus is on establishment of multiple-character phenotypic difference; (2) geographic situation and potential for interchange of genes is not emphasised heavily; and (3) characters are not weighed based on their likely involvement in establishment of reproductive isolation. This system, then, is not particularly allied to recent BSC methodologies and does not address key issues necessary for BSC-based decisions. Rather, in spite of Collar's oft-expressed dislike for the PSC (Collar 1996, Collar and Spottiswoode 2005, Collar 2007a), his scoring system distills down to a simple PSC criterion—diagnosability. The method's strengths are in dealing with allopatric taxa, as this quandary has long complicated BSC applications for lack of a test of sympatry, so the multiple-character basis of achieving the 'critical' score of 7 avoids micro-division based on trivial characters that concerns him regarding the PSC (≥ 3 characters would be necessary) (Collar 2007a). The method has so far been used to evaluate certain groups of barbets and babblers, groups that generally display fairly apparent differences between species and many allopatric populations. We envision that the same criteria will be difficult to apply to groups with more subtle differences like swiftlets, *Phylloscopus* and *Seicercus* warblers, cuckoo-shrikes, and many bulbuls. However, even in this best-case situation, the method is seriously vulnerable to subjective opinion entering into decisions, as illustrated above.

In sum, the scoring system is based chiefly on diagnosability as in the PSC, yet does not take into account the lineage concepts inherent in the PSC (De Queiroz and Donoghue 1988, Davis and Nixon 1992). Put simply, this approach appears to disregard the strengths of prominent species concepts, and brings little or nothing new to the debate. The *ideal* approach to these questions under the BSC would evaluate whether significant gene flow occurs when populations are in sympatry or parapatry; in allopatry, although the BSC encounters problems of applicability (Mayr 1963), other modern species concepts would marshal diverse data sets to evaluate whether populations are following independent evolutionary trajectories (Wiley 1978). While we agree that Asian birds are probably seriously overlumped, and thereby would probably agree with many of the splits that have been identified using this method, it is not founded in any modern species concept and is probably best regarded as an inadequate attempt to make simple and operational an issue that is quite complex and challenging.

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Subjectivity and space in evaluating species limits: a response to Peterson and Moyle

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Peterson and Moyle (2008) are at a disadvantage when the detailed paper setting forth my system of assessing species status in allopatric taxa remains unpublished (largely because, as Peterson and Moyle conclude, the issue is indeed ‘quite complex and challenging’). I hope this situation will soon change.

Even so, in highlighting subjectivity as a flaw in the system I use, I think they miss a couple of fairly obvious points. (1) All taxonomy involving allopatric forms is to some degree opinion-driven. Even Helbig *et al.* (2002), whom Peterson and Moyle praise, begin with this acknowledgement, and a key difference between their system and mine is simply that they give greater weight to far smaller characters. They write that ‘taxa that differ only slightly (e.g. in size or darkness of plumage)... are best treated as subspecies’ and that ‘a single base substitution in a DNA sequence, or a single barb on a single feather’, even if consistently different between two populations, are insufficient ‘to base a taxonomic rank’. In other words, opinion and subjectivity have just shifted to different areas along the scale. (2) Collar (2006) indicated that the system sets limits on the subjective assessment of degree (‘strength’) of difference, and makes that assessment transparent, so that others can see the process, judge it for themselves and, if in disagreement, challenge it on the basis of the detail provided. Although assigning a numeric

value to a subjective decision may not impart objectivity to the process, it does allow an explicit, quantitative measure of a necessarily qualitative assessment, and therefore brings in a degree of standardisation and repeatability. It is encouraging to see that Kirwan (2008) has found the system helpful.

Peterson and Moyle follow an intriguing but difficult line of argument on the issue of distance between populations. My scoring system will in fact make allowance for various types of spatial arrangement: so taxa in parapatric arrangements and narrow hybrid zones earn one score, and those in broader hybrid zones another. However, I find it difficult to see how allopatric populations, whatever the distance between them, can be treated in more ways than one. Taxa spatially very far apart might well be judged more reproductively isolated than closer taxa; but taxa very close together (yet not parapatric) could also be said to merit special recognition for having retained their distinctiveness. So making allowances for degree of geographic disjunction would, I think, be unworkable, and far more open to the opposition of opinions than a simple concentration on morphological and acoustic characters.

I am happy to confirm my allegiance to the Biological Species Concept (BSC). Judgements about the reproductive incompatibility of allopatric taxa have, under