

Species concepts and conservation: a response to Hazevoet

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Whether they are “mere subspecies” or “phylogenetic species”, the omission of such forms from the ICBP (now BirdLife International) Red Data Book (RDB) was my personal decision, and I have long been sensitive to the type of charge contained in Hazevoet’s paper (preceding pages). This decision, taken back in 1982, was based on the time, space and cost that would have been involved in the systematic review and inclusion of subspecies (RDBs being much fuller treatments than those in *Birds to watch 2*, the work criticized for the omission by Hazevoet), and on the fact that the future of the many “full” species needing urgent documentation and action consequently stood to be compromised.

However, in announcing this move I made the exhortation that each nation should itself identify globally threatened subspecies within its borders, since the reduced range sizes of subspecies commonly means they are single-country endemics, and I later developed this line of thought into an outline model for a national bird conservation strategy which went before the entire ICBP membership (Collar 1987), although it was apparently only utilized once (Fanshawe and Bennun 1991). The decision was further tempered by a general commitment to splitting over lumping, and by the perception that many threatened “subspecies” are sympatric with threatened species, and stand to benefit from site-oriented actions on behalf of the latter (Collar and Stuart 1985: xvi–xviii). Moreover, the exclusion of subspecies from the RDB did not mean that BirdLife remained indifferent to their plight; the Azores Bullfinch *Pyrrhula pyrrhula murina* is a case in point, since the RSPB intervention for this bird, on which the profile by Bibby *et al.* (as cited by Hazevoet) was based, actually represented an extension of work I myself initiated with the local government of the Azores in 1982.

I make these points simply to emphasize that there has been no blind neglect of “subspecies” by BirdLife or its RDB programme. It might also be fair to point out that in continuing to press for global as against national standards of evaluation, even at national levels (see, e.g., Collar and Stuart 1986), and in developing the first-ever global analysis of unique assemblages of species (ICBP 1992, Stattersfield *et al.* in prep.), BirdLife has not shirked its obligations to endemic birds. Nevertheless, sympathetic as I am to Hazevoet’s concern for the fate of small island forms, and little as I like being accused of promoting extinction through my choice of taxonomy, I remain sceptical that the adoption in ornithology of the phylogenetic species concept (PSC) offers a practical solution at the global scale. These doubts remain even if the time- and fund-consuming business of major RDB documentation were to be abandoned in favour of annot-

ated listings represented by the *Birds to watch* formula (a move which would in any case be a false economy: Collar 1996). There are several reasons (further developed in Collar in press) for taking this line.

First, the sheer number of new threatened taxa would tend to be unmanageable (the political and legal ramifications are hard to gauge; I refer here just to the operational problems): the adoption of what seems a modest and uncomplicated set of principles at the level of a small island nation like the Cape Verdes becomes a very different proposition at the global scale. The new number would *not* simply be proportional to the total number of species resulting from PSC adoption (which in one view might only double the BSC's current 10,000: Zink and McKittrick 1995), since the subdivision of biological species inevitably produces forms with smaller ranges and lower populations, and hence with much greater proximity to the thresholds in the new IUCN criteria of endangerment (see *Birds to watch* 2: 14–21). Thus Schodde (1978) identified 100 species plus no fewer than 400 subspecies (probably involving few clinal forms of the sort the PSC rejects) for inclusion on Papuaia's avian red list. My own current work in preparation for *Threatened birds of the Philippines* suggests that, beyond the 90-odd species scheduled for treatment, some 160 subspecies appear likely to fulfil the new criteria (so that, if one then also splits the polytypic threatened species, the total number of phylogenetic species at risk in the islands rises to around 280). Clearly, then, red listing of such species risks creating a jam of priorities in which very well-marked (= BSC) species would have to vie for (and doubtless often fail to obtain) attention in a wide sea of morphologically very similar taxa. I am not sure that this would represent an advance for the conservation of biological diversity (I can already see the title looming: "Taxonomic fastidiousness promotes the extinction of distinctive birds"). Nor am I certain that a small archipelago like the Cape Verdes, with a handful of PSC species, would attract any more attention than it does now, given the great number of new areas such as mountain ranges and archipelagoes, richer in threatened PSC species, that would seem likely to emerge and lay prior claim to conservation resources.

Second, there are the problems of the time it would take for a PSC evaluation of the global avifauna to be completed (generally family by family), and of the imbalance in priority setting that would meanwhile exist. Hazevoet's point about special pleading on behalf of *Pterodroma* petrels in the eastern Atlantic is well taken: the inherent unfairness of some species or regions having their advocates was eventually what determined my decision *not* to follow Hazevoet's own taxonomy (at that stage unpublished) in 1994. How much more invidious will the situation be when whole families, or major regional avifaunas, have been subject to PSC re-evaluation while others have not. To adopt Hazevoet's taxonomy for the Cape Verdes (which itself required the review of "a large number of specimens in major museum collections [plus] lengthy field studies" – i.e. years of painstaking labour devoted to a fraction of a percent of the global avifauna) would immediately demand of the even-handed conservationist an adoption of PSC principles for the rest of the world, otherwise the taxa elevated to species level would shift at a stroke from seeming disadvantage to manifest overprivilege; by the same token, one cannot – or at least should not – simply split *Pyrrhula murina* from *P. pyrrhula* without examining the con-

sequences for the other subspecies in the current arrangement. I see no easy solution here, only various types of compromise (Collar in press) in what otherwise will be a very long wait.

Third, there is the issue of the limits of diagnosability. Hazevoet does not regard this as a problem, but the fact that he copes with it in two potentially contradictory ways suggests that it is one of genuine intractability. Thus on the one hand he indicates that he has himself rejected 11 Cape Verdean subspecies on the basis of their morphological indistinctness, thereby refusing their claims to any unique independent lineage; but on the other he argues for further research on *Pterodroma (feae) deserta* which, although “usually considered morphologically indistinguishable from *feae*. . . [is] not unlikely to be distinct in other – e.g. molecular and/or ethological – characters”. Later, having admitted that there is no theoretical limit on the number of species, he asserts that “nothing is gained” by obscuring different lineages in morphologically identical forms, if molecular evidence shows them to exist.

Clearly there are some daunting consequences for the conservationist here. Most obviously, the view that molecular and ethological differences can be used to diagnose species reveals the PSC as a labyrinthine construct – the misprint in Savage (1995), which estimates the true number of bird species at 950,000, momentarily looks uncannily apposite, as there must be thousands, perhaps hundreds of thousands, of morphologically inseparable isolates to consider – in which the cause of conservation could be lost for decades and *still* not emerge with renewed clarity or certainty. In other words, the PSC is just as provisional and arbitrary in assigning taxonomic status to morphologically identical and near-identical forms as the BSC is to trivially differentiated ones. Then indeed there is the challenge of deciding the degree of triviality (if there is one) of a character by which a phylogenetic species might *not* be recognized. On the basis of what Hazevoet writes here, it can be very trivial indeed, just so long as it is not determined on a quantitative basis; yet clearly some information about that lineage *can* be conveyed by statistical evidence (scraps to be retrieved from Hazevoet’s “almost meaningless wastebasket”), and those interested in the preservation of biological diversity might well wish to continue to retain the services of a system that provides, however unsatisfactorily in some respects, for the recognition of quantitatively measured differences between populations.

Nothing I say here is to pretend that the BSC is without significant problems, and it is certainly not to underplay the crucial importance of Hazevoet’s personal long-term endeavours, for which I have the deepest admiration, in support of the Cape Verdean avifauna. Nevertheless, each of the three points above carries some weight (the first the least), and their cumulative effect is to indicate that more has to be done to demonstrate the capacity of the PSC to contribute fully and fairly, steadfastly and *soon*, to the cause of global bird conservation.

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