Response from Watson

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Source: BioScience, 55(5) : 389

Published By: American Institute of Biological Sciences

URL: https://doi.org/10.1641/0006-3568(2005)055[0389:RFW]2.0.CO;2

Species Limits in Birds: A Response to Watson

David M. Watson (2005) purports to propose a whole new set of methods for avian taxonomic studies, but his arguments actually amount to a misguided attempt to bolster the phylogenetic species concept (PSC). His first claim, that bird taxonomy depends on field marks, misses the point that subtle but crucial distinctions in plumage and morphometrics are often revealed by handheld material. His second, that use of field marks represents a consistent bias, is contradicted by his own graphs, which show distributional, behavioral, and habitat data being used more in new avian descriptions than for other taxa. The only evidence that bird studies miss, *fide* Watson's charts, is internal morphology, but this is also rare in amphibian and reptile studies. He confidently presumes that rectifying this omission will improve avian taxonomy, but produces no evidence that internal morphology might indeed be of value: syringeal studies are not new (and should anyway be reflected in vocal characters), while skeletal characters perform weakly at the species level.

Watson then suggests that molecular information is also overlooked by avian studies, but his data reveal genetic characters in use just as often in bird descriptions as elsewhere. No light is shed by his example of a bird showing "deep genetic division" between two morphologically similar populations, and his implication that earlier taxonomists were biased in their treatment of this bird is baffling, considering the novelty of molecular techniques. In any case, genetic studies, like skeletal characters, are far from unequivocal in species-level assessments.

Nevertheless, Watson suggests that birdwatchers and checklist committees inhibit "the widespread acceptance of valid species," describing the latter's decisions as "merely opinions." He complains this is "at the expense of...new taxonomic concepts" and resents the dominance in ornithology of the biological species concept (BSC), yet later he asserts this "is not a 'splitting vs. lumping' issue, nor is it a question of...choice of species concepts"! He criticizes Collar's view that the number of PSC species would be "prohibitively large" as (a) fundamentally incorrect

and (b) irrelevant. But how can he be sure of (a)? Any geographically isolated yet morphologically ill-distinguished population might prove in some degree genetically distinct. How many such populations are there in the world? Thousands? Millions? And if the question is irrelevant because what matters is "to discern the total number of evolutionarily distinct units as accurately as possible," then what is Watson's objection to *subspecies,* which can also represent evolutionarily distinct units?

Meanwhile, Watson ignores the PSC's fundamental problem with character triviality. How minor can a character be before (in what will essentially be "merely opinion") it gets discounted as defining an evolutionarily distinct unit? Watson accuses the BSC of fueling "unreliable and unstable taxonomies," but these are more likely to result from PSC-type approaches. When Cracraft (1992) assessed the birds of paradise (Paradisaeidae) on PSC principles, the number of species changed from 40–42 to 80–120, a jump in uncertainty over species limits from 5% to 50%. This kind of problem will emerge repeatedly with PSC-type applications to avifaunas, and it will not be resolved by methods imported from other taxonomic disciplines.

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Response from Watson

Collar and Spottiswoode clearly missed the point. I did not "propose a whole new set of methods," advocate a particular species concept, or recommend any technique as definitive. Rather, I evaluated current practice in bird taxonomy to learn how species-limit decisions are made in ornithology.

My central tenet is that birds are treated differently than other vertebrates; birds are described and diagnosed primarily using traits we can see and hear under field conditions. This is not an unqualified opinion nor a position statement, but an objective conclusion based on quantitative analyses of species descriptions during the last decade. Using detailed case studies, I demonstrate that field marks do not necessarily represent overall variation—some birds vary in ways we cannot detect in the field. Instead of dismissing this issue or ignoring the associated implications, I proposed several solutions. Using morphology, anatomy, molecular differences, or any other objective attributes, we can complement these visible characteristics and improve our understanding of how many species we are really dealing with. And yes, this will result in more species under any species concept. This is neither good nor bad; it's merely a better representation of actual diversity.

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