'When I use a word,' Humpty Dumpty said, in a rather scornful tone, 'it means just what I choose it to mean, neither more nor less.'
'The question is,' said Alice, 'whether you can make words mean so many different things.'
'The question is,' said Humpty Dumpty, 'which is to be master – that’s all.'

And here is another question: why were so many avian taxonomists – Berlepsch, Bonaparte, Buffon, Cuvier, Gyldenstolpe, Hachisuka, Kittlitz, Kuroda, Lacépède, Lafresnaye, Meyer de Schauensee, Ogilvie-Grant, Rothschild, Salvadori, Taczanowski, Temminck, Tschudi, Tschusi, Walden, Wied, Yamashina, Zedlitz – members or descendants of the nobility? Partly, perhaps, because aristocrats have more time on their hands than the rest of us, and can devote themselves more readily to such heady things as classifying the natural world. Partly, perhaps, because laying down the law relating to hierarchy, lineage and identity comes most naturally to people of high birth. Even so, the arrivistes have predictably similar vested interests: Vigors was a Conservative Member of Parliament, P. L. Sclater the brother of a Conservative peer, Bocage a government minister. And even if, by contrast, Linnaeus was ennobled because of his achievements in taxonomy rather than vice versa, he evidently sensed no hubris in declaring ‘God creates, Linnaeus arranges’ or in describing his best students as his ‘apostles’.

Democracy, at any rate, came slowly to taxonomy. Its seeds were sown in the early 1840s, when a group of English naturalists, Darwin and Owen among them, sought to establish the principles of nomenclature, notably using a political metaphor to explain the new order to which they aspired:

The world of science is no longer a monarchy, obedient to the ordinances, however just, of an Aristotle or a Linnaeus. She has now assumed the form of a republic, and although this revolution may have increased the vigour and zeal of her followers, yet it has destroyed much of her former order and regularity of government. The latter can only be restored by framing such laws as shall be based in reason and sanctioned by the approval of men of science. (Strickland et al. 1843)

The first of the laws to be enshrined in what came to be known as the ‘Strickland Code’ was, famously, the ‘law of priority’, whereby the scientific name first applied to a taxon (genus, species or subspecies) was fixed as the name forever to be borne by it. People who established such names became not merely authors but authorities; and their own names would live on forever in association with the names they bestowed.

In reality, of course, the nineteenth-century republic of science was still very far from being a democracy, as the phrase ‘men of science’ so tellingly indicates. Although diagnostic descriptions of taxa were now requisite, comparative material was an essential component of the process of validation, so systematics and taxonomy inevitably remained the province of small numbers of museum-based experts, not all of them bearing inherited titles; and after Strickland this situation persisted for another 150 years. In the middle of this long period, and two years before the UK gave equal voting rights to women, Tate Regan (1926), rising to a challenge that has long vexed students of
natural history, showed the temerity and self-knowledge to define a species as:

a community or a number of related communities whose distinctive morphological characteristics are, in the opinion of a competent systematist, sufficiently definite to entitle it, or them, to a specific name.

This somewhat convoluted formulation, widely paraphrased as *a species is whatever a competent systematist says it is*, explicitly locates the authority for original descriptions in the museum scientist, and it may well be that this view – to which Bernard Tucker (Tucker 1949: 132) himself evidently subscribed – reflected as much an incapacity to imagine an alternative as it represented an act of defiant gate-keeping. It certainly reflected a tradition of practice and belief that was far from spent. While preparing the ground for a guide to the birds of the Philippines, Delacour & Mayr (1945) produced a taxonomic review of the country’s avifauna which included peremptory assertions of the following type (here relating to the endemic broadbills *Eurylaimus* or *Sarcophanops*), backed up by not a shred of evidence or analysis:

> Although differing clearly in size and color, the two Philippine forms *steerei* and *samarensis* are in our opinion subspecies of one species.

The world took their word for it (some still do), perhaps simply trusting in the view that they were, by any standard of the time, competent systematists. As the century moved on, however, and species descriptions, at least in ornithology, began to seep into the literature from a wider constituency, gate-keeping of sorts may have become a stronger motive behind the albeit invaluable reviews of new species (see reference list to table 1 in Collar 1999) produced by Ernst Mayr and staff at the American Museum of Natural History (AMNH) in the 50 years 1943–1992.

Ironically, as if closing an era, 1992 – the centenary of the foundation of the *Bulletin of the British Ornithologists’ Club* and its German rival *Ornitológische Monatsberichte*, both established for the rapid publication of new species descriptions – was the year in which Joel Cracraft moved to AMNH and published his review of the birds of paradise (Cracraft 1992) based on a revolutionary proposition for the identification of species that he himself had brought to ornithology (Cracraft 1983) and which pitted monophyly (i.e. common descent from a single ancestor), the principle invoked under the phylogenetic species concept (PSC), against reproductive incompatibility (the inability of two taxa to produce viable offspring), the key criterion of Mayr’s biological species concept (BSC). This proposition came at a time when molecular genetic research, buoyed on the broad wake of Sibley & Monroe (1990), was finally coming to be recognised as the most compelling force in modern systematics.

Looking back now, the consequences were dramatic, profound and liberating: over the next two decades avian taxonomy came alive with many new voices, the number rising as in a dawn chorus. PSC converts, molecular researchers and compilers of field guides, avifaunas, monographs and handbooks, abetted by increasingly sophisticated equipment both inside and outside the lab, all took the opportunity to import into their classifications new insights from field observations, sound recordings, photographs and even simply a fuller consideration of pre-existing evidence. Front-line birdwatchers published their views. Magazine journalists speculated. National committees took their positions. Museum scientists – if not moving smartly into molecular systematics – withered and drooped; democratic taxonomy burst into leaf and flower. We were all competent systematists now.

Or were we? How well has the redistribution of powers worked, and what are the implications and prospects for the future? Is it democracy or anarchy? Euphony or Babel? As in any revolution, the running was made by the more radical elements. Taxa that are ‘diagnosably distinct’, representing a single line of descent, are species under the PSC. The PSC was applied to the Cape Verde avifauna (Hazevoet 1995) and to the Dutch List (Sangster *et al.* 1999). The latter had perhaps the greater effect, since nothing on the Dutch List is a national endemic and the Netherlands has international borders; so would
other national lists follow the lead? The pressure was strong – so strong, indeed, that Svensson (1997) detected:

an almost arrogant and depreciatory tone when describing the... shortcomings... of the BSC, an inability to acknowledge the problems inherent in the proposed alternative... and an apparently insatiable aspiration for taxonomic hegemony.

The pressure extended to conservationists. Having responsibility to the International Union for Conservation of Nature (IUCN) for the listing of birds on the IUCN Red List, BirdLife International, a global partnership of national NGOs, was an early target. ‘Taxonomic neglect promotes the extinction of endemic birds’ was part of the title of a critique by Hazevoet (1996) of BirdLife’s dependence on a BSC-based world list (Sibley & Monroe 1990), and the point was repeated in the pages of Conservation Biology by Sangster (2000) with particular reference to the Cape Verde Red Kite Milvus (milvus) fasciicauda (for which see more below). Six years later conservationists were again bidden to ‘pay attention... pay close attention’ to taxonomic matters if they wanted their endeavours on behalf of biodiversity to reflect that diversity properly (Peterson 2006).

Some of this was entirely justified. Systematics is a living science, and new insights compel new conclusions – in this world, nothing can be said to be certain except death and taxa – and the taxonomic establishment is perhaps by definition behind the curve. For example, Nazca Booby Sula granti was ‘to me, unnecessary splitting’ (Nelson 2005), and both Ardea purpurea bournei and Passer motitensis hemileucus were denied even the status of subspecies (Hancock & Kushlan 1984; Summers-Smith 1998); yet the booby is now widely accepted, while the heron (Hazevoet 1995) and sparrow (Kirwan 2008) have been claimed, with varying degrees of success, as full species.

Ultimately and inevitably, however, for conservationists in particular there has to be a point of common reference from which to develop and against which to react. In the 1980s, the world list of birds that BirdLife used was Morony et al. (1975), which, being

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88. Nazca Boobies Sula granti, pair at nest-site, Genovesa Island, Galapagos, October 1998. Systematics is a living science, and the taxonomic establishment is perhaps by definition behind the curve. In 2005, Bryan Nelson, in his family monograph Pelicans, Cormorants and Their Relatives, felt that the Nazca Booby Sula granti was an example of ‘unnecessary splitting’, yet this is now widely accepted as a separate species.
largely a species-level synthesis of Peters (1931–1987), was regarded as possessing more authority than any other (Parkes 1975). In the 1990s, BirdLife switched to Sibley & Monroe (1990) as its cornerstone reference for species limits (but not family sequence). But with the proliferation of insights and ideas that followed the PSC challenge to established species limits, plus a requirement from IUCN in 2001 to assess all species—not just likely-looking candidates—against its Red List criteria, BirdLife was compelled to evaluate proposed taxonomic revisions on a case-by-case basis. The maintenance of a clear, well-grounded and defensible list has been an obligatory component of BirdLife’s scientific remit since the start of this century, with two key criteria bearing on this work.

The first is consistency: it is obviously inappropriate to use one species concept—one set of rules—in one part of the world and another in another. This instantly places the PSC at a disadvantage, since current world lists were broadly formulated in an era of general acceptance of the BSC; therefore the BSC is the default setting for this criterion, although it is fair to say that most species-level revisions proposed in the past 10–15 years have invoked a broad BSC, which accepts that a degree of hybridisation between taxa is not fatal to their status as species.

The second is robustness: the evidence needs to be exhaustively assembled and dispassionately compelling. Here again the PSC is at a disadvantage, this time relating to its inherent difficulty with diagnosability: when is a character, however consistent, however diagnostic, too trivial to merit acceptance? Even so, ‘robustness’ does not refer just to strength of character: it refers also to strength of argument, clarity of presentation, and adequacy of the appraisal of uncertainty, things which the peer-review process ought to ensure but commonly—as conservationists find out to the cost of their own time and trouble—does not. Illustrations of these and other difficulties follow.

The Fuerteventura Blue Tit *Parus* (now *Cyanistes*) *degener* was promoted as a distinct species—on what was considered BSC evidence (playback tests) alongside PSC-level plumage diagnoses—in a review that pointed out that as few as 200 individuals might survive (Sangster 1996). A survey happily increased that number by an order of magnitude (Garcia-del-Rey & Cresswell 2006), but in any case molecular work concluded that *degener* was indistinguishable from North African populations of Blue Tit *C. caeruleus* and should not be recognised even as a subspecies (Dietzen et al. 2008).

The Cape Verde Red Kite *Milvus fasciacauda* was elevated to (phylogenetic) species level by Hazewoet (1995). Sangster (2000), hailing the PSC’s potential to deliver ‘more efficient use of resources’, called it a ‘valid species’ (only) ‘superficially similar’ to the Red Kite *M. milvus*. In response, the Peregrine Fund, a US-based raptor conservation organisation, went to some lengths to obtain the last few individuals for captive breeding, something they mistakenly thought they had achieved in 2002 (see Hille & Collar 2009); but then molecular analysis of museum skins placed *fasciacauda* within the range of variation exhibited by the Red Kite clade (Johnson et al. 2005), arguably thereby rendering it ineligible even for subspecific status.

A review of the Bean/Pink-footed Goose *Anser fabalis/brachyrhynchus* complex (Sangster & Oreel 1996) concluded that it consists of three morphologically and reproductively distinct populations, *A. brachyrhynchus*, *A. fabalis* and *A. serrirostris*, thereby rejecting, among others, the putative subspecies *middendorfi* (Siberian taiga populations allied with *fabalis*). By contrast, a phylogenetic analysis (Ruokonen et al. 2008) set aside reported behavioural, ecological, vocal and morphological distinctions and instead recognised *A. brachyrhynchus*, *A. fabalis* (heavily rearranged) and *A. middendorfi*.

Almost universal acceptance followed the split of Great Grey Shrike into Northern *Lanius excubitor* and Southern Grey Shrike *L. meridionalis* (Isenmann & Bouchet 1993), even though claims of parapatry, unambiguous diagnosability and differences ‘in behaviour and habitat, with *excubitor* nesting in grassland with scattered trees and *meridionalis* in dry grassy scrub’ (!) (Sangster et al. 2002) were less than compelling. Now Olsson et al. (2010) report that such a division ‘is not
compatible with the mitochondrial tree; the Great Grey Shrike is either one species or as many as six.

Lesser Redpoll *Carduelis cabaret* was split from Common Redpoll *C. flammaea* on morphological, genetic and vocal grounds, plus a single-year record of assortative mating in a newly sympatric population (Knox *et al.* 2001). However, molecular differences prove not to exist and 10% of migrant redpolls at Falsterbo, in southern Sweden, could not be assigned to either *cabaret* or *flammaea* in the hand, so ‘the two taxa should be treated as subspecies’ (Ottvall *et al.* 2002; also – with bad news too for fans of Arctic Redpoll *C. hornemanni* – Marthinsen *et al.* 2008).

Molecular results such as these have a seemingly impregnable authority that trumps other modes of species-limits analysis, but this assumption requires major qualification. At the most basic level, mistakes happen – for example, Zou *et al.* (2008), with implications in Pilgrim *et al.* (2009) – or seemingly happen, as when Jønsson *et al.* (2010) reported White-arsed Oriole *Oriolus albioris* to be so close genetically to Isabela Oriole *O. isabellae* that they should ‘perhaps… be treated as a single taxonomic unit’ (in life the two taxa are sympatric and have highly divergent bill morphology and certain other features: Collar 2011). Moreover, different researchers, perhaps using different methods, obtain different results and make different interpretations. Contemplating the white-capped form *stricklandii* of White-rumped Shama *Copsychus malabaricus*, Sangster *et al.* (2010) wrote:

> Our cyt b sequences of *stricklandii* and *C. malabaricus* differ by 2.2%– similar to or exceeding that in several species pairs… [This and] previously reported differences in plumage and sympatric breeding indicates that the inclusion of *stricklandii* as a subspecies of *C. malabaricus* may be premature and that further research is warranted.

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Yet simultaneously Lim et al. (2010), despite finding a slightly higher percentage difference, took the opposite line:

Although stricklandii is distinguished from western Bornean suavis by its white cap, this distinction is muted in the hybrid zone... Moreover, their ND2 divergence of 2.7% is within the commonly detected range of conspecific passerine taxa... Thus, the taxon stricklandii should be maintained as a subspecies of C. malabaricus.

Closer to home are the recent molecular analyses of the Iberian Green Woodpecker Picus viridis sharpei by Pons et al. (2011), who tentatively retain it as a subspecies, and by Perktas et al. (2011), who emphatically consider it a species.

But there are other issues to negotiate in these taxonomic reviews, molecular or otherwise, than the incongruence of their findings. Making the appropriate comparisons is perhaps the next most salient. When Kirwan & Shirihai (2007) argued for the species-level separation of northwest African populations (sahari) of the House Bunting Emberiza striolata, they puzzlingly disregarded the form jebelmarraine, which is geographically intermediate between sahari and nominotypical striolata and, as they acknowledged, intermediate in certain plumage features and, from their data, bill measurements. When Saetre et al. (2001) claimed species status for Moroccan speculigera from Middle European Pied Flycatchers Ficedula hypoleuca on genetic and morphological grounds, they omitted full and fair analysis (no treatment in their morphological table; no reference to its genetic identity) of the geographically interposed form iberiae, which is, as noted by Taylor (2006), ‘an intergrade between [speculigera] and nominate’ (a point already evident from Cramp 1993: 86). And when Küpper et al. (2009) split Snowy Plover Charadrius nivosus from Kentish Plover C. alexandrinus on (almost entirely) genetic grounds (despite their title’s claimed ‘phenotypic… analyses’, some of which had to be supplied by Donegan et al. 2011), they missed out South Asian seebohmi despite the fact that – for all its biogeographical disjunction – it is closer to nivosus in coloration and
measurements than it is to *alexandrinus*.

Clarity of evidence is a third need. Shirihai *et al.* (2001: 167) split Orphean Warbler into Western *Sylvia hortensis* and Eastern *S. crassirostris* on the grounds that the taxa are ‘genetically well differentiated... [and] as divergent as either is from [Arabian Warbler] *S. leucomelaena* (Helbig *et al.* submitted, Shirihai *et al.* submitted)’, adding that ‘conclusive biometrics’ (this is a heading in bold type) show *hortensis* to have a shorter, less attenuated bill (‘15.3–16.9 mm’) than *crassirostris* (‘18.0–20.2 mm’). Acceptance of this split seems now to be widespread; yet more than a decade later, even allowing for the sad loss of one of the authors, neither of those apparently clinching citations has been published, and elsewhere in Shirihai *et al.* (2001: 188) there are far from ‘conclusive biometrics’, taken by C. S. Roselaar, in which (male) bills of 25 *hortensis* fell in the range 17.0–18.8 mm and of eight *crassirostris* in the range 17.7–20.0 mm.

Studying the *Larus michahellis*/*armenicus* interface in central Turkey, Liebers & Helbig (1999) reported that three haplotypes of *michahellis* were present in 62% of birds at Beysehir and 14% of birds at Tuz Gölü, so ‘hybridisation does occur, hybrids are fertile and backcross at least with *armenicus* in central Anatolia’, but ‘whether gene flow in the other direction... also occurs requires further study’. In recognising *L. armenicus* as a species, however, Collinson *et al.* (2008) condensed this account as disclosing ‘limited maternal gene flow from *michahellis* into *armenicus* populations (although not in the opposite direction)’, thereby unwittingly causing absence of evidence to morph into evidence of absence.

Problems such as these – and there are many more examples, which suggests rather more Babel than euphony – could perhaps have been avoided if some common standards had been set and met for reporting and gauging comparisons of taxa near the species boundary; and in fact a decade ago some such guidelines were provided (Helbig *et al.* 2002). These were footnoted as ‘an official document of the British Ornithologists’
Union’ and hailed by one of its authors as ‘a written constitution for species-level taxonomy’ (Collinson 2002). Even so, the formulations possess a degree of ambiguity that has made them hard to interpret or apply with confidence. The consensus on their most fundamental points appears to be that (1) taxa to be split should differ on multiple characters and (2) these differences should be greater than those between pairs of species that are close(st) relatives of the taxa under review. Drawbacks here are that (a) ‘multiple’ can mean two; (b) characters can be extremely small (including molecular); (c) choice of species pairs for comparison is rarely straightforward and, when no close relatives exist, seemingly optional rather than obligatory; and (d) only a single character is needed to trigger ‘allospecies’ status, an ambiguous taxonomic category that embraces the key PSC criterion even while exhibiting an impractically porous border (Appendix S1 in Tobias et al. 2010; also table S1 for an attempted tabulation of the BOU criteria).

Thus when the BOU criteria were applied to 23 pairs of taxa widely treated as subspecies in the Western Palearctic avifauna, eight (35%) converted to species and nine (39%) to probable species, producing a total potential change of 17 (74%) (Tobias et al. 2010). This tends to suggest that the BOU criteria, although claimed to be more stringent than the PSC (Helbig et al. 2002: 524), yield results not dissimilar to it. Moreover, the uncertainty in these evaluations, which were made in the Natural History Museum, Tring, using specimens and literature, reflects not only the ambiguities in the criteria but also the fineness of the distinctions being evaluated (Lincoln Fishpool pers. comm., and pers. obs.). As Snow (1997) elegantly indicated, when differences between taxa are subtle and slight, the potential for instability at species boundaries rapidly rises, under the influence of different interpretations of the criteria and subjective assessment of character strength and significance. For example, the seminal PSC review of the birds of paradise (Cracraft 1992) elevated the number of species in the family from 40–42 under the BSC (5% uncertainty) to 80–120 (33%...
uncertainty) (Collar 1997). Hazevoet’s (1996) argument that:

Admitting that some situations are not entirely clear should stimulate further research and seems highly preferable to pretending that all problems have been solved when taxa are simply allocated as either ‘species’ or ‘subspecies’ needlessly characterises established taxonomies as fixed and closed, and impractically suggests that leaving species limits entirely undefined in some taxa is better than allowing them to remain determined by poor (but still the best available) evidence.

There are, of course, many frustratingly difficult cases to confront; indeed, it is important to acknowledge that the delimitation of species is inherently problematic. Bernard Tucker (Tucker 1949: 162) himself made the familiar but necessary point that:

the provision of a hard and fast definition by which species can always be distinguished from subspecies has defied all

the efforts of taxonomists for the excellent reason that in nature no hard and fast line of separation exists.

Even so, it is clearly undesirable to have species defined on relatively minor characters winking in and out of taxonomic existence on the basis of shifts in opinion and evidence such as those surrounding Parus degener or Milvus fasciculata above. Conservationists cannot build programmes and relationships with donors, governments, international agreements and conventions, as well as crucial local interests and communities, on the back of tenuous and debatable claims and cases, even when formulated in the best peer-reviewed outlets of science. They need species-limits criteria that deliver more precise, stable, transparent, consistent, defensible and reasonably rapid results but which at the same time do not simply endorse and extend an unsatisfactory status quo.

One solution is to give some kind of value to characters in order to rank and sum their strength. A system in which distinctions in

93. Azure-winged Magpie Cyanopica cyanus, Portugal, April 2007. The genetic differences between the disjunct Iberian and East Asian populations of this eye-catching corvid look convincingly large, yet there is still a lack of compelling evidence that, other than the white tips to the tails of Asian forms, the plumage, mensural, vocal, behavioural and ecological differences between the two populations are anything but minor or even non-existent.
plumage, size, voice and – with restrictions – behaviour and ecology are scored 1 for minor, 2 for medium, 3 for major and 4 for exceptional (with the number of plumage and vocal characters capped at three, and morphometric at two) was trialled on 58 pairs of closely related sympatric or parapatric bird species, and in 95% of cases species status was reflected in a total score of at least 7 (Tobias et al. 2010). When the system was applied to those same 23 pairs of Western Palearctic subspecies which the BOU criteria reclassified as 8–17 species, 21 (91%) remained subspecies while two (both Moroccan) became species. Applied to the global avifauna, the number of species may then increase by very approximately 1,000, although given the likelihood of greater taxonomic inaccuracy in the less completely resolved avifaunas of the tropics this may well be a minimum.

The Tobias criteria ‘firmly place a degree of consistency and transparency upon taxonomic decisions’ (Winker 2010). The weighting and explicitness of the scoring system ought, in theory at least, to make these criteria hard to apply without a rigorous and accurate evaluation of the evidence, and consequently they should establish a greater degree of long-term taxonomic stability, notwithstanding the collection and analysis of new data. Moreover, they include a technical method for quantifying levels of vocal divergence and proposing thresholds for the significance of such differences, an issue that the BOU guidelines left unaddressed.

However, they also take the uncomfortable but unavoidable step of excluding molecular evaluation from the criteria. This is primarily because there is no fixed threshold of genetic divergence which can be used to determine whether two taxa are species or not, but also because different techniques, measurements and interpretations all affect the equivalence of the results, so that (for the present, at any rate) the whole process of comparison between taxa is fraught with uncertainty. This exclusion applies even in cases where greater genetic distances between taxa are reported from within a recognised species than between that species and another, as in the case of the Yellow Wagtail $M. citreola$ and Citrine Wagtails $M. citreola$ (Pavlova et al. 2003) and Palearctic Sandwich $S. sandvicensis$ and Nearctic Cabot’s Terns $S. acuflavida$ (Efe et al. 2009). While such molecular paraphyly is unacceptable to pure cladists, it need not dictate terms to broader taxonomies based on other evidence; and in any case such counterintuitive results need painstaking validation and evaluation.

The split of the Azure-winged Magpie $Cyanopica cyanus$ is another intriguing case. A ‘molecular genetic distance of 6.06%’ between Iberian and East Asian populations (Fok et al. 2002) looks clinchingly large, but as yet no compelling evidence (pace Kryukov et al. 2004) has been adduced to suggest that, other than the white tips to the tails of Asian forms, the plumage, mensural, vocal, behavioural and ecological differences between the two populations are anything but minor or indeed non-existent. Meanwhile, recent research has shown that two clades of Common Raven $Corvus corax$ that are >4% divergent in mtDNA coding genes interbreed successfully where they meet (Webb et al. 2011). Evolutionary lineages disclosed by molecular analysis may well be better represented under the PSC; but under the BSC deep clades alone do not a species make.

Conversely, an absence of genetic differentiation need not override claims based on strong phenotypic characters. The molecular analyses that appear to sink the Fuerteventura Blue Tit, Cape Verde Red Kite and Lesser Redpoll as species simply underscore what the morphological evidence already indicates – that the distinctiveness of the forms is sufficiently low to place them somewhere on the spectrum between synonymy and subspecies. On the other hand, even if the White-faced Plover $Charadrius dealbatus$ cannot be told from Kentish Plover at the molecular level, its morphological distinctiveness maintains its species status under the Tobias criteria (Rheindt et al. 2011 and pers. obs.).

The redrawing of species limits in birds is an enterprise that is likely to take many more decades of research and analysis, and cannot be obstructed or appropriated for long by any one faction in ornithology, conservative or radical, committee-based or birdwatcher-driven. Ultimately, however, what Sangster
(2009) appealingly calls ‘taxonomic progress’ depends on consensus, consensus on conviction, and conviction on evidence. A good case for splitting will marshal the relevant facts, cover the literature and reflect it accurately, make the best use of museum and other material, endeavour to achieve adequate sample sizes, ensure that all relevant taxa are properly considered, and present the findings clearly, fully and untendentiously. There will, inevitably, be fudges when the evidence is conflicting or partial and the practicalities of the decision-making process require assumptions to be made; but so long as these things are honestly acknowledged, convergence of opinion is still a realistic expectation.

However, it is worth just noting that the current extinction crisis has quietly been creating a new vested interest in taxonomy, consisting of a small group of people with unusual, unanticipated and for some perhaps unwelcome powers. Conservationists have been bidden to pay close attention to taxonomy, and indeed they must: for if it is they to whom the rest of the world turns to bear the burden of bringing as much biological diversity as possible through this extinction crisis, and if it is they who must seek to inscribe this diversity in national and international law for its greater security, then it is also they who, more than any others, have both the right and the responsibility to scrutinise what it is they are being bidden to save. This is the most compelling reason for the new democracy of taxonomists to ensure that their decision-making aspires to the highest standards. Conservationists have plenty of other work to be getting on with, and taking up their time with incomplete evidence and underdeveloped arguments whose final import is a species is whatever I say it is, in the style of Humpty Dumpty, seems hardly the most durable of foundations for ‘taxonomic progress’.

Acknowledgments

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References


The BB/BTO Best Bird Book of the Year 2012

British Birds and the British Trust for Ornithology announce the winner of the Award for Best Bird Book of the Year. All books reviewed in BB or the BTO publications BTO News and Bird Study (and on the BTO website www.bto.org) during the year 2012 were eligible for consideration for this Award.

It is in the nature of an annual competition that no two years are ever quite the same. In deciding which are the best bird books, our six judges must form views and reach conclusions that are based on the books qualifying for consideration in the year in question which, by decree, are different every time. There are, however, predictable genres into which many of the books will fall. Most years, for example, will bring forth a crop of new field guides, site guides and monographs for species and genera, or revisions of or spin-offs from previous ones, for which the expected standards are well established. Nonetheless, there are always books that stand out, attracting the judges’ admiration because they in some way push boundaries and break new ground in established genres, or are more or less unique. The 2012 competition was no exception in these respects. We are pleased now to announce the winner and other top titles.

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