

**AERC TAC's
TAXONOMIC RECOMMENDATIONS
1st December 2003**



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Introduction

This is the 14th draft of the AERC TAC's Taxonomic Recommendations. This is not a 'final' draft. It is a working document reflecting the current situation. The major merit of this document is that it presents a commented list by consensus or majority decision, the common denominator based on two years' work. By a rather technical 'voting process' it is established which decisions are supported by most of the European Taxonomic Committees (TCs). This gives an idea which decisions are most widely supported by a majority of the TCs at present under various species concepts. Inevitably, some of the proposals will not correspond to the *Guidelines for assigning species rank* (Helbig et al. 2002) and may need to be corrected in the future (particularly in borderline cases).

The AERC TAC (*Taxonomic Advisory Committee of the Association of European Records and Rarities Committees*) was formed during the 4th Conference for European Rarities Committees at Blahová, Slovakia, in July 1997 (online version of minutes at <http://www.aerc.be/Blahova1997.htm>). Its structure and functioning currently include all five national European taxonomic committees, following a joint proposal by the BOURC and Germany, at the 6th Conference of the AERC at Hel Peninsula, Poland, in September 2001 (http://www.aerc.be/Hel_2001.htm). The following **Terms of Reference** for the TAC were agreed at the 6th Conference of the AERC, to take effect immediately:

- Membership to comprise one member from each country which currently has its own taxonomic committee (Britain, France, Germany, the Netherlands and Sweden)
- Individual's appointments to be for two years, renewable by a majority agreement at the next AERC Conference
- An independent chairman to be elected by AERC, ideally from a different country, and preferably not a taxonomist, also to serve for a two year period, renewable by a majority agreement at the next AERC Conference
- The chairman to co-ordinate activities by the member countries, and to request recommendations from all at six-monthly intervals (1 January and 1 July each year)
- TAC chairman to be non-voting on taxonomic recommendations
- Taxonomic recommendations best to be made unanimously by the TAC; if two members do not agree, the final recommendation will be postponed, if one member does not agree, the chairman can make a decision
- Announcements, usually in the form of press releases, to include a brief summary of the reasons for any split, with an indication of where and when a full paper is likely to be published; and where appropriate, to include statements about species where insufficient evidence exists for a split at the current time
- The chairman to circulate draft announcements of recommendations to all AERC members for comment before publication, allowing a period of one month for responses and presentation of new or unpublished scientific evidence

The following points were also agreed:

- TAC chairman to obtain a copy of the *Guidelines for assigning species rank* when agreed by the existing four TAC members and circulate these to each taxonomic committee
- TAC chairman to prepare a new *List of species for consideration*, based [among other sources] on the German list by Andreas Helbig (i.e. single page list of splits proposed to the AERC by A.J. Helbig and P. Barthel, based on their taxonomy in the German translation of Svensson et al. 2000)

The AERC TAC consists of a chairman (**Gunter De Smet**) and five TAC representatives appointed by their national RC:

- **Pierre-André Crochet** (France) for the **Commission de l'Avifaune Française (CAF)** including Pierre Le Maréchal (chairman), Philippe J. Dubois (secretary), Pierre-André Crochet, Roger Cruon, Frédéric Jiguet, Jean-Marc Pons and Pierre Yésou (n=7)
- **Andreas Helbig** (Germany) – a German taxonomic committee has not yet been formed
- **C.S. (Kees) Roselaar** (The Netherlands) for the **Commissie Systematiek Nederlandse Avifauna (CSNA)** including C.S. (Kees) Roselaar, George Sangster (secretary), Ronald Sluys (former member, left CSNA per 1 January 2003), Arnoud B. van den Berg and André J. van Loon (n=5)

- **Björn Anderson** (Sweden) for the **Swedish Taxonomic Committee – Taxonomikommittén (STC – Tk)** including Per Alström, Björn Anderson (secretary), Lars Larsson, Urban Olsson and Lars Svensson (n=5)
- **Martin Collinson** for the **British Ornithologists Union Records Committee’s Taxonomic Subcommittee (BOURC TSC)** (United Kingdom) including Martin Collinson (secretary), Andreas Helbig, Alan Knox, David Parkin, Tony Prater (former chairman), Eric Meek (chairman of BOURC) and George Sangster (n=7)

Excluding dual membership, 24 people contributed to the AERC TAC.

The work of the AERC TAC was the major topic during the 7th AERC Meeting in the Danube Delta, Romania, 26–30 August 2003 (see online version of minutes at: http://www.aerc.be/romania_2003.htm). The official sponsor of the AERC is Carl Zeiss Sports Optics http://www.zeiss.de/DE/bino/home_e.nsf. We would particularly like to thank Walter Mergen of Carl Zeiss Sports Optics for the financial support of the AERC during many years.

During the voting process, TAC representatives are supposed to state the opinion of their TC, which may not always reflect their personal opinion. As any taxonomist, they can, however, be consulted as individual experts as well. A clear distinction should be made, however, between the opinion of the TCs and the personal opinion of the TAC representatives/individual taxonomists.

The AERC TAC uses the *List of Recent Holarctic Bird Species*, originally published in three parts (Voous 1973, 1977a, 1977b), and reprinted integrally (Voous 1977c), as a starting point for its Taxonomic Recommendations. Sangster et al. (2002a) published a first series of *Taxonomic recommendations for European birds* on behalf of the AERC, in accordance with AERC protocol at the time, and representing the first agreed taxonomic changes in this ongoing procedure. A brief rationale of these decisions is reproduced here in appendix 1. Next, it seemed desirable to verify if any taxonomic decisions published by national TCs since Voous (1977c) were in compliance with the *Guidelines for assigning species rank* (Helbig et al. 2002) in an effort to define the most widely supported consensus. The *Taxonomic Recommendations of the AERC TAC* also include some widely supported novel treatments.

A digital version of the Voous list was provided by C.S. Roselaar, including all subspecies as mentioned in the headings of *The Birds of the Western Palearctic* – it should be noted that the Voous list (1977c) did not include any subspecies. This list was converted into an Excel file by Marnix Vandegehuchte and regularly updated by Gunter De Smet according to the TAC’s decisions and including all bird species new to the Western Palearctic up to and including 2003. The AERC TAC adopted this document for several reasons as a working list: (1) it already included most widely accepted taxonomic decisions since Voous (1977c) – which were carefully compared with Voous (1977c) and justified in the Recommendations; (2) it includes subspecies whereas the Voous list does not; and (3) it is digitally available. The Voous list remains as such, unless changes are mentioned in the recommendations below.

The geographic scope of this document is the Western Palearctic. For practical reasons, the geographical limits of the Western Palearctic are those defined by the editors of BWP; see, however, Martins & Hirschfeld (1998) for a view that better reflects the eastern limits of this zoogeographical region. Martins (fide G. Kirwan) also considers that the Cape Verdes should not be included in the WP. In fact, as has been noted several times, in recent literature, the Cape Verdes show a striking degree of biogeographical congruence with Socotra (which is not included in the WP either). Although the remit of the AERC and the TAC were primarily European, it should be noted that the Rarities Committee of Cyprus is also a member. Although the RC of Morocco is on the AERC homepage (as a service for travelling birders), the CHM has not yet been invited to an AERC meeting as it is not a European RC. The Greek part of Cyprus will become a member of the European Union on 1 May 2004. The AERC extends beyond Europe, and the zoogeographical region of the Western Palearctic better fits its requirements than the political entity of Europe. At the moment, the AERC TAC is primarily concerned with AERC countries and the report may therefore cite taxonomic issues, which are of greater relevance to the core area and omit for the time being other taxonomic issues, which are equally important on the fringes. In some cases, we have not examined the data in sufficient detail, or there may not be adequate data for such an assessment to be worthwhile. Such cases are listed in the pending category.

The *Guidelines for assigning species rank* specify that taxonomic recommendations should be based on a study of peer-reviewed papers, the results of which should preferably be confirmed independently. This does not need to be in print, and may be in a refereed electronic publication. It may be acceptable to refer to unpublished work in a supporting role, but it should not be amongst the primary evidence for change. For the

sake of completeness, some additional sources are cited which do not meet this standard (particularly when referring to identification papers); it should be stressed, however, that papers, which are not refereed, have no impact on the Recommendations proper. Such papers should be judged on their own merits. We do not consider it wise, however, to neglect other sources of information and consider it important to build bridges between birders and scientists. In the reference list, hyperlinks are mentioned whenever cited publications or abstracts are freely available on the Internet.

The following taxonomic issues are addressed by the AERC TAC in the Recommendations:

(A) The list addresses those taxonomic issues, which differ from the Voous position. It should be noted that A.J. Helbig and P. Barthel proposed a *List of species for consideration*, a single page document with German splits, to the AERC TAC during the 6th AERC meeting. Four of these proposed splits, however, were already accepted by Voous (1977c), the starting point for the considerations, and will therefore not be treated in the Recommendations: *Butorides striata* (note spelling change!) – *B. virescens*, *Pernis apivorus* – *P. ptilorhynchus*, *Falco vespertinus* – *F. amurensis* and *Picus viridis* – *P. vaillantii*. The Helbig & Barthel list was published in full in Svensson, L., Grant, P. J., Mullarney, K. & Zetterström, D. (2000) *Vögel Europas, Nordafrikas und Vorderasiens*. Stuttgart. In this German translation of the large size edition of a field guide, taxonomy was adapted by the translators without the consent of the first author. Whenever reference is made to ‘Helbig & Barthel in Svensson et al. (2000)’, these taxonomic decisions are meant.

(B) The list presents a full review of all decisions since Voous (excluding full details of those, which have already been reported by Sangster et al. 2002a on behalf of the AERC); this choice is based on three reasons:

(1) taxonomic decisions are now scattered in many reports, by TCs using different species concepts or different interpretations of the same species concept (and therefore publishing contradictory opinions at times). It would be useful to summarise these in a single report as a courtesy to the readers, rather than ask them ‘to look it up for themselves’. It is practical to have all information available in a single publication.

(2) in some cases, new evidence is available since the publication of a particular taxonomic decision in a report. In these cases, it would be useful and scientifically correct to present and discuss the new evidence.

(3) the wide acceptance of the *Guidelines for assigning species rank* is a valid reason in itself to recapitulate all decisions since Voous: (a) do previous decisions not based on the *Guidelines for assigning species rank* still stand? (b) how are the *Guidelines for assigning species rank* applied in practice in individual cases? (including cross-references to show that we are really applying the Guidelines correctly as they have been intended by the authors).

There are three kinds of decisions in the Recommendations: (1) spelling corrections, (2) decisions whether taxa should be considered species or subspecies and (3) decisions on generic level. All these change bird names and are therefore included in the same paper.

(1) The AERC TAC accepted the changes in gender agreement of species names proposed by David & Gosselin (2002a, b) applying the rules of the *International Code of Zoological Nomenclature* (ICZN).

(2) The AERC TAC accepted the *Guidelines for assigning species rank* (Helbig et al. 2002). The necessity to create the Guidelines was expressed at the 4th AERC Conference, at Blahová, Slovak Republic, 18–22 July 1997, and this major task was successfully completed by the four pioneering AERC TAC members (Andreas Helbig, Alan Knox, David Parkin, George Sangster) and Martin Collinson, together forming the combined membership of the BOURC TSC. So far, there is little evidence of the application of the *Guidelines for assigning species rank* (Helbig et al. 2002) in the species texts. In practice, most TCs/individual taxonomists have stated their opinion based on the species concept of their choice. Although the AERC TAC has formally accepted the Guidelines, much more time is needed to re-evaluate all decisions prior to the publication of the Guidelines seriously. Conscientious taxonomists may wish to examine all relevant publications personally, before stating a ‘final’ opinion. First proposals should be written (with reference to the Guidelines), next they should be discussed (in comparison to the Guidelines), then a decision should be taken and the results should be published. There is no shortcut. This may take a long time and implies willingness to debate.

Species concepts

There are a number of different species concepts and various interpretations of these e.g. the **Biological Species Concept** (BSC; Mayr 1963), the **Recognition Species Concept** (RSC; Paterson 1985), the **Phylogenetic Species Concept** (PSC; Cracraft 1983) and the **Evolutionary Species Concept** (ESC; Simpson 1951); see e.g. Ereshefsky (1992), Klaridge et al. (1997), Wilson (1999), Helbig (2000 a,b,c) and Wheeler & Meier (2000) for further reading. In all species concepts, taxonomists are attempting to partition a continuous process (evolution) into artificially distinct units (species). This is difficult, and judgement is required in borderline cases. ‘Opinions’ and ‘truth’ should not be confused. Waples (1991) highlighted the importance of the **Evolutionary Significant Unit** (ESU) in conservation.

Subspecies

If there is a disagreement on subspecific names among TCs, the decisions in BWP are adopted as status quo. The subspecies in the *Western Palearctic List* (Excel file) are taken over from the headings in BWP (unless changed in the Recommendations). Unfortunately, BWP uses an inconsistent mixture of at least three ‘subspecies concepts’ (**75% rule**, Amadon 1949, **90% rule**, Amadon & Short 1992, and **distinction of populations with statistically distinct means**, e.g. Engelmoer & Roselaar 1998). Additionally, some authors accept **very fine and clinal variations** (even within small samples!) to be sufficient for the recognition of subspecies (e.g. Eck 1975). Ball et al. (1992) examined the phylogeographic population structures revealed by restriction analyses of mtDNA (mitochondrial DNA) within six bird species with continent wide distributions in North America. Overall, most of the subspecies currently recognised within these species were genetically very close, and showed no obvious mtDNA differences.

Lars Svensson commented: ‘It is to be preferred if the adopted subspecies concept is a wider one rather than recognising very subtle variations as formally named subspecies. Finer variations which are clinal (and often the result of well-known causes due to climate, length of migration, habitat, etc.), and which will often not coincide with the clinal variation of other traits, frequently resulting in mosaic patterns of complex but subtle morphological variation, should not form the basis of recognised subspecies. For a subspecies to be valid I suggest that **(a) 90% of all individuals from the centre of its range (or from its type locality) can be separated morphologically from a neighbouring subspecies, also sampled from the centre of its range (or type locality). It is an advantage, but not a prerequisite, if there is (b) a step or unevenness in clinal variation between them, if variations are clinal, and, in the case all differences are subtle, (c) more than one trait differ, preferably two or more unrelated ones.**’

Subspecies Guidelines are needed as a tool to describe geographic variation accurately. The subspecies of pipits and wagtails are according to Alström & Mild (2003). A review of the subspecies on the *Western Palearctic List* still needs to be done.

(3) Generic changes are only included in the Recommendations if their current treatment differs from the original treatment by Voous (1977c). This implies that a number of changes, reverted in the national reports of the participating TCs to the original Voous names, e.g. *Stercorarius* – *Catharacta* – *Stercorarius* (see e.g. Cohen 1997; Braun & Brumfield 1998) and *Phalaropus* – *Steganopus* – *Phalaropus* (see incongruent results of Dittmann et al. 1989, Dittmann & Zink 1991 and Chu 1995) are omitted here.

Differences in genus names are commonly encountered when comparing decisions of national TCs. For several reasons, a ‘genus definition’ may not solve this problem. Actually, the genus category (= unit of classification) cannot be defined; there is no such a thing as a ‘genus concept’. Even though a genus taxon (= unit of evolution) is a monophyletic group of species, not all monophyletic groups of species are given genus rank. Although monophyletic groups can be reconstructed, it is arbitrary which ones we call a genus, a superspecies or a family, etc... The only ‘genus definition’ possible would be to have objective guidelines to assign genus rank to certain groups of species (*Guidelines for assigning generic status*). One such guideline could be the size of genera (the number of species within them). This would, however, lead to unnecessary instability and would not result in genera being ‘equivalent’ in terms of evolutionary changes: in groups with few species, genera would include very different species (that have diverged a long time ago and differ a lot from their common ancestor) whereas in species-rich groups, some closely related and similar species would need to be placed in different genera. A more sensible criterion could be the time of divergence: recently diverged species (e.g. less than 5 million years ago for birds) would remain in the same genus, while more divergent species would be put in different genera. Again, this would not allow for different lineages with different evolutionary rates: we would need to classify some ‘similar’ species in different genera or keep in the same genus species that we have always viewed as sufficiently distinct to form different genera. Genus guidelines should not be based only on a genus’ raw size, but rather on the comparative support found for the clades that could potentially be ranked as genera. As such, they could, and should, be devised in an attempt

to reach long-term stability. Since genus taxa are (as all taxa of rank above species) human creations aimed at resuming phylogenetic information in names to facilitate communication and classification, the main criteria to adopt generic classifications should be (1) that genera are monophyletic groups (they do not contradict the natural classification of species); (2) that genera help to understand and describe biodiversity. Genus names will have to be a compromise between precision (each group of equally closely related species could be a different genus) and applicability (would it be practical to have *Phylloscopus*, *Sylvia*, *Calidris*, *Tringa*, *Falco*, etc... split into as many genera as there are species groups?). This is by no means objective. When in doubt, it is probably better NOT to split. This would imply to refrain as much as possible to use a genus name, which would make a larger genus non-monophyletic (e.g. *Nyctea* for *Bubo scandiacus* or *Anthropoides* for *Grus virgo* makes *Bubo* and *Grus* paraphyletic). If we are in doubt whether *Limicola falcinellus* is really basal to all *Calidris*, or *Actitis* to all *Tringa*, we should probably not use these genera either. Quite a lot of current unnecessary instabilities are due to taxonomists disagreeing with what should be given generic rank. In the Recommendations, we have opted for the most widely supported solution in a case-by-case approach. We should, however, always seek for the most robust acceptable solution – that is, the solution that, while being compatible with what is known of the phylogeny, would minimise the risk that species names need to be changed again (or back) in a near future. *Guidelines for assigning generic status* are clearly needed.

English names follow Beaman & Madge (1998), in which the authors mainly adopted the names used by Beaman (1994). The latter source gives the rationale for the choice of many English bird names in the entire Palearctic region. In the English language a confusing array of vernacular bird names has been, and are still, used, but great advances have been made in standardising these. It should be noted that the International English Names Committee under the International Ornithological Committee (IOC) has a Palearctic subcommittee working on this problem (members: Chris Perrins, chair, Per Alström, Mark Beaman, and David Parkin). Some of the advantages of Beaman & Madge (1998) are (1) they list a number of alternative names in common usage; (2) many of their choices are explained; (3) a large region is covered. It should be noted that strictly, taxonomy is concerned with scientific names only. The AERC TAC does not claim any authority in the use of vernacular names in any language, and any new names proposed should be validated by the proper linguistic bodies, or whoever is officially responsible for this, in the respective countries.

There is one exception to the use of names proposed by Beaman & Madge (1998): these authors use ‘Madagascar Bee-eater’ for *Merops superciliosus* (monotypic), whereas del Hoyo et al. (2001) use ‘Olive Bee-eater’ for *M. superciliosus* (polytypic, including ssp. *superciliosus* and *alternans*). As the latter is not only breeding in Madagascar, it seems useful to make the distinction in vernacular names as well. Some other changes are suggested, but not applied: Sturm’s Bittern *Ixobrychus sturmii* may be a better name than Dwarf Bittern, and Eastern Stonechat *Saxicola torquatus maurus* seems a better choice than the misnomer Siberian Stonechat.

Changes in sequence: Galloanserae forward, changes within *Acrocephalus* and *Sylvia*

The AERC TAC unanimously accepted to move the Galloanserae forward (preceded by Ratitae, of which Ostrich *Struthio camelus* is the only representative in the WP). The CSNA decided to put the sister-group of Galliformes and Anseriformes in front in 1998 (Sangster et al. 1998) and the Galloanserae head the British list since 2002 (see Knox et al. 2002 for a rationale). Within Galloanserae, Anseriformes will precede Galliformes. CAF and STC both accepted this change on 6 October 2003. There is still some discussion, however, about the sequence within this parvclass.

For the sake of homogeneity, decisions on changes in sequence within *Acrocephalus* (see Knox et al. 2002) and *Sylvia* (DNA-DNA hybridisation distances by Blondel et al. 1996, mtDNA sequences and morphology by Shirihai et al. 2001) are also followed here. Both CAF and CSNA objected against the position of smaller *Hippolais* warblers (subgenus *Iduna*), which may be nested within *Acrocephalus*. This matter will need to be addressed later. The CAF also objected to changes in sequence within *Sylvia* as conclusions in Shirihai et al. (2001) are partially based on unpublished evidence.

Changes in sequence within *Acrocephalus* and *Hippolais* (Knox et al. 2002):

Acrocephalus aedon Thick-billed Warbler
Acrocephalus melanopogon Moustached Warbler
Acrocephalus paludicola Aquatic Warbler
Acrocephalus schoenobaenus Sedge Warbler
Acrocephalus scirpaceus Reed Warbler
Acrocephalus palustris Marsh Warbler

Acrocephalus dumetorum Blyth's Reed Warbler
Acrocephalus agricola Paddyfield Warbler
Acrocephalus griseldis Basra Reed Warbler
Acrocephalus arundinaceus Great Reed Warbler
Acrocephalus orientalis Oriental Reed Warbler
Acrocephalus stentoreus Clamorous Reed Warbler
Acrocephalus brevipennis Cape Verde Cane Warbler
Hippolais pallida Eastern Olivaceous Warbler
Hippolais opaca Western Olivaceous Warbler
Hippolais caligata Booted Warbler
Hippolais rama Sykes's Warbler
Hippolais languida Upcher's Warbler
Hippolais olivetorum Olive-tree Warbler
Hippolais icterina Icterine Warbler
Hippolais polyglotta Melodious Warbler

Changes in sequence within *Sylvia* (Shirihai et al. 2001):

Sylvia atricapilla Blackcap
Sylvia borin Garden Warbler
Sylvia nisoria Barred Warbler
Sylvia curruca Lesser Whitethroat
Sylvia hortensis Western Orphean Warbler
Sylvia crassirostris Eastern Orphean Warbler
Sylvia leucomelaena Arabian Warbler
Sylvia communis Common Whitethroat
Sylvia nana Asian Desert Warbler
Sylvia deserti African Desert Warbler
Sylvia conspicillata Spectacled Warbler
Sylvia deserticola Tristram's Warbler
Sylvia undata Dartford Warbler
Sylvia sarda Marmora's Warbler
Sylvia balearica Balearic Warbler
Sylvia cantillans Subalpine Warbler
Sylvia melanocephala Sardinian Warbler
Sylvia mystacea Ménétries's Warbler
Sylvia rueppelli Rüppell's Warbler
Sylvia melanothorax Cyprus Warbler

Helbig's Checklist of the Birds of Europe

At the 2001 AERC meeting P. Barthel announced that A.J. Helbig would produce a list of systematic changes for circulation within the AERC TAC. A first draft of this list has been received by the AERC TAC in May 2003. The *Checklist of the Birds of Europe* includes comments on a revised sequence based on phylogenetic information currently available. Please, notice that this list is not primarily concerned with justifying any splits and reflects entirely the personal opinions of the author. Nonetheless, it might be useful as a basis for discussion and as a source for literature references. The new Concise Edition *Kompendium der Vögel Mitteleuropas* to be published in 2004 will follow this list. Please, note that given our poor knowledge of avian phylogenetics, many aspects of this list may have to be modified in the near future as additional evidence is published. This is the nature of science, there is no such thing as 'stability' in systematics (unless taxonomists would be willing to agree upon it). A number of changes in sequence are well founded and the AERC TAC will discuss a preliminary list of these by A.J. Helbig.

'Allopecies', 'semispecies' and 'superspecies'

There are many references to 'allopecies', 'semispecies' and 'superspecies' in the text. This system is not supported or used by the BOURC TSC, CAF, CSNA and STC. These terms, and their use, are clarified in the *Guidelines for assigning species rank* (Helbig et al. 2002): 'A superspecies is a monophyletic group of allopecies (geographically separated) and/or semispecies (connected by a hybrid zone). Allo- and semispecies are terms that can be used to label qualitatively different categories of species whose evolutionary independence cannot be determined empirically.'

AERC TAC: objectives, methodology and consequences

Practical task and target group of the AERC TAC

First of all, the AERC TAC has a practical task. At the 7th AERC meeting (Danube Delta, Romania, 26–30 August 2003), it presented (1) a **digitally available updated list of the birds of the Western Palearctic** (including subspecies and range) for AERC purposes; (2) a working document showing the progress of its **Taxonomic Recommendations**; (3) a list with **pending decisions** and taxonomic problems that will be considered after the AERC meeting.

(1) The updated WP list is intended as an international yardstick for AERC use. It is believed to be particularly helpful for those countries lacking a TC and to improve international comparison of data among rarities committees. Contradictory opinions of different TCs are not satisfactory as a basis for up-to-date standardized national lists for countries which are lacking a TC and may in some cases reflect a lack of communication between TCs rather than genuine taxonomic differences. A careful comparison of publications of different TCs will satisfy most readers that strongly diverging national lists are of no practical use to other countries and if anything create confusion. The same scientific names may mean very different things in different countries, which is a problem when comparing biodiversity, in some conservation issues, legislation, etc. Co-operation between the current TCs could prevent the creation of even more TCs resulting in an even more confusing picture. The strong focus of some TCs on their national lists, limits the usefulness of these lists in other countries as taxa beyond their remit are not dealt with.

(2) The *Taxonomic Recommendations of the AERC TAC* will be published on the website of the AERC on 1st December 2003 and will be made widely available to birding magazines. The exact date of the following biannual press release of the AERC TAC should be defined at the next AERC meeting.

(3) All TCs can propose to circulate additional taxonomic or systematic questions to the AERC TAC. To facilitate the discussion, however, the taxonomist or TC requesting to circulate a proposal should always include the main reasons and relevant references in his proposal. Indeed, it would be interesting if anyone could provide a scientifically motivated taxonomic proposal or elements for discussion to the AERC TAC (including birders).

Methodology of arriving at recommendations

The following Term of Reference for the TAC was agreed at the 6th Conference of the AERC: ‘**Taxonomic recommendations best to be made unanimously by the TAC; if two members do not agree, the final recommendation will be postponed, if one member does not agree, the chairman can make a decision.**’

Collaboration between TCs and the AERC TAC

The Taxonomic Recommendations of the AERC TAC are intended as a **common currency for AERC purposes** and could be compared to the euro. It is important to distinguish two levels: the national and the international level. **Nationally**, a country may wish to accept the international Taxonomic Recommendations, or it may stick to its own. A taxonomic recommendation by the AERC TAC does not automatically imply acceptance by the participating TCs and should not be read as agreement to split. Some country lists have legal implications, some have none. Many lists are used for different purposes, e.g. the BOURC list in the Country and Wildlife Act, EURING for ringing, Sibley & Monroe for CITES, etc. The AERC TAC list is one more list, next to other lists. **Internationally**, it is desirable to use AERC TAC taxonomy for AERC purposes. The importance of the AERC TAC list, will depend on its acceptance at various levels, which will in turn depend on its scientific quality (and the input of all taxonomists involved).

The remit of the various TCs is different. The BOURC TSC has an interest in extralimital subspecies of species on the British List and whether or not they should be split but does not have a primary interest at the moment where none of the subspecies of a particular taxon are on the British List.

Some TCs may wish to agree on a **moratorium** not to publish any taxonomic decisions of their own that are not discussed and resolved by the AERC TAC. This is a free choice and certainly not an obligation. National TCs are free to decide whether they accept such a moratorium or not. It has been argued that a moratorium may hamper the taxonomic processes but it should be noticed that thought-provoking ideas could also be published in separate papers. Controversial decisions are not necessarily wrong, but they should not be included in national lists right away.

The participation of TCs in the AERC TAC is on an **independent and voluntary basis**. The degree of participation depends on the goodwill of the TCs and the taxonomists involved. The AERC TAC is tolerant towards a ‘couleur locale’ of a TC and is willing to work on an equal but different basis. Genuine collaboration is expected, however, and the degree of collaboration must suffice to guarantee the well-functioning of the AERC TAC. This may imply the circulation and discussion of taxonomic decisions at least four months before publication as to allow the AERC TAC to state an opinion on a decision by a TC in a national publication. Ideally, only approved decisions should be published. There may be a disagreement between taxonomists on species guidelines, but once a species concept has been agreed upon (such as the *Guidelines for assigning species rank*), TCs should theoretically reach the same conclusions based on the same information.

The AERC TAC should avoid the task of national TCs and duplication of work should be avoided at all

times. **The co-operation should reduce differences between national lists.**

The AERC TAC is a **consensual committee**. Decisions taken by national TCs are evaluated by the AERC TAC and are only accepted by the AERC TAC if there is a wide consensus, preferably by unanimous decision. It is dealing with taxa occurring in the WP (and taxa occurring outside the WP if these may have an influence on taxonomy/systematics of WP taxa). In the case of taxa figuring on the national list, an attempt should be made by the TCs to reduce international disagreement on their decisions, preferably by widening the debate and taking into consideration opinions of the other TCs on taxonomic decisions before publication. **Peer-reviewing** is a generally accepted scientific approach and ideally, this is the role the AERC TAC could play before publication of any decisions by TCs.

Prereleasing information

The AERC TAC should avoid prereleasing information if it has not reached a conclusive agreement. Only if there is a prior agreement accepted by all TCs, information can be prereleased. If so, it should be prereleased simultaneously in all AERC countries, including a full motivation of the splits.

Consequences

The rules of the AERC TAC should not be seen as a rigid framework, but as a **flexible set of rules** which can be redefined by the members and/or the AERC **to improve efficiency, the scientific value of the Recommendations and to achieve its practical task**. Any changes agreed upon among all five TCs can take effect immediately. Afterwards, any changes should be formally ratified by the AERC.

The purpose of the AERC TAC is to foster international co-operation.

Suggestions to improve the functioning of the AERC TAC as defined at the Hel Meeting

During the 7th AERC meeting, the work of the pioneering AERC TAC members (Andreas Helbig, Alan Knox, David Parkin and George Sangster) has been duly acknowledged. Their important contribution was treated unfairly in the minutes of the 2001 Hel meeting and a more balanced judgement was appropriate. The *Guidelines* are a fine example of their achievement. Some of the statements about the 'old' AERC TAC in the Hel Minutes do not reflect the views of the AERC as a whole.

Voting System

1. Introduction

Voting by consensus is not a commonly accepted principle among taxonomists. Notice, however, that the methodology of arriving at recommendations is not clarified by any of the national TCs. Therefore, there may be differing standards among TCs and this may affect scientific credibility. The AERC TAC is combining decisions, but has no insight how these decisions were reached within the various TCs. Widely accepted taxonomic decisions are not necessarily well founded. Voting by consensus can be painstakingly slow and it offers little possibility for innovative thoughts, merely allowing the confirmation of well-established decisions and chronically lagging behind decisions already published elsewhere. Within a TC most taxonomists state opinions in a fraternal way. Reasons for decisions are exchanged and commented upon, until a reasonable degree of agreement has been reached. In a sense, this final agreement may be a consensus, but it is **the fruit of reasoning, not of a voting process**. Such a decision by consensus can only be reached after careful consideration and this is the way it should be interpreted by the AERC TAC as well. The Terms of Reference for the AERC TAC are clear which principle should be used to reach taxonomic decisions (and is similar to decisions reached within RCs): **'Taxonomic recommendations best to be made unanimously by the TAC; if two members do not agree, the final recommendation will be postponed, if one member does not agree, the chairman can make a decision.'** Ideally, recommendations which have not received the positive support of all five committees (not by default) should be removed until they have

been debated and resolved. Unanimous decisions are accepted. The recommendations of the AERC TAC should reflect opinions of the AERC TAC members at the time of writing.

2. The following issues should be discussed:

- 2.1 Unanimous decisions: proposals supported unanimously by all 5 TCs**
- 2.2 Majority-less-one decisions: proposals supported by 4/5 of the TCs (and resolved)**
- 2.3 Proposals by 4/4 majority if beyond the remit of one of the TCs**
- 2.4 Proposals accepted by 3/4 (and resolved) if beyond the remit of one of the TCs**
- 2.5 Proposals accepted by 3/3 if beyond the remit of two TCs**
- 2.6 Proposals accepted by 2/3 (and resolved) if beyond the remit of two TCs**
- 2.7 If two or more TCs consider a decision to be pending**
- 2.8 If two or more TCs reject a decision**
- 2.9 Blanc votes**
- 2.10 Status quo**
- 2.11 Ad interim status quo**
- 2.12 Generic changes**
- 2.13 Stepwise splits**
- 2.14 Inclusion of formal votes in the Recommendations**
- 2.15 If important new information or an important review is awaited**

2.1 Unanimous Decisions

Undoubtedly, everyone agrees on the principle of unanimous decisions. It also seems possible to accept unanimous decisions if beyond the remit of one or two TCs (4/4 and 3/3-majority resp.).

2.2 Majority-less-one decisions

If one member disagrees with a particular decision or prefers to keep the decision pending, the chairman needs to take the difficult decision between a cautious approach (pending the decision of the AERC TAC because of a minority view) or a majority decision (adopting the 4/5 majority as the view of the AERC TAC). This needs to be done in a way that offers scientific credibility, which is otherwise lacking.

There seem to be two options to resolve majority-less-one decisions:

- 1) Voting on a cautious approach (pending) or a majority decision (accepted) within the AERC TAC
- OR
- 2) Systematically accepting minority-less-one decisions

The Terms of Reference are clear about 4/5 majority decisions: the chairman can decide.

We may want to discuss, however, if we also accept the principle of a minority-less-one decision in other cases:

3/4 majority decisions if beyond the remit of one of the TCs; 2/3 majority decisions if beyond the remit of two TCs.

2.2.1 majority-less-one; difference between pending and rejected dissenting vote?

It could be argued to make a distinction between:

A + A + A + A + P

and

A + A + A + A + R

(With A = accepted and R = rejected.)

and that acceptance is possible in the former case, but not in the latter (where a cautious approach would be preferred). It may be better, however, to avoid such a distinction, as the latter option could be abused as a veto to block the functioning of the TAC. Notice, however, that in the latter case, the arguments AGAINST a split should be clearly mentioned in the final recommendations.

Minority-less-one decisions could be resolved in various ways:

(a) Internal agreement (by voting on a cautious approach or a majority decision or as the fruit of discussion).

(b) Consulting or studying external authorities (e.g. independent taxonomists or researchers working on particular taxa) or other TCs (e.g. the AOU for matters concerning both the American and the Western Palearctic List) to resolve the matter.

(c) Comparing the proposed opinions with those in widely accepted and commonly used checklists and handbooks as well as the *Guidelines for assigning species rank* (Helbig et al. 2002); if the majority opinion of the TAC that needs to be resolved is also adopted in an overwhelming majority of references, it should be followed. This is difficult to assess, however. A practical solution might be to assess 'common usage' in an objective way by using a powerful search engine, e.g. <http://www.google.com> (this gave the following results for Great White Egret: *Casmerodius albus* 4370, *Egretta alba* 6340, *Ardea alba* 4430). If opinions are divided because of 'a matter of taste' this might be an objective solution; in this case, however, *Casmerodius* seems to be nearly as often used as *Ardea*.

(d) Offering a balanced view, including the cases for and against with arguments presented, reviewed or improved by the taxonomists. The proposals in the Recommendations are discussed between the TCs by adding constructive comments (based on scientific arguments) to the texts during the circulation of the consecutive drafts. The TCs aim to finish this work by a deadline within a reasonable delay, as to allow the AERC TAC to present a working document at the AERC meeting.

(e) This working document will be presented to the AERC and the meeting will be asked to cast a vote on accepting or rejecting the proposed Recommendations (with a final possibility to comment upon and to improve the draft to both the AERC and the AERC TAC before publication in the minutes of the AERC meeting).

(f) This list is not exhaustive.

2.3 Proposals by 4/4 majority if beyond the remit of one of the TCs

Should be treated like a unanimous decision.

It also seems possible to accept unanimous decisions if beyond the remit of one or two TCs (4/4 and 3/3-majority resp.).

2.4 Proposals accepted by 3/4 (and resolved) if beyond the remit of one of the TCs

In the case of beyond remit + majority-less-one, the question is again whether we should make a difference between a pending and a rejected dissenting vote?

It could be argued to make a distinction between:

$\emptyset + A + A + A + P$

and

$\emptyset + A + A + A + R$

or

$\emptyset + \emptyset + A + A + P$

and

$\emptyset + \emptyset + A + A + R$

(With \emptyset = beyond remit.)

As a minimum requirement at least three TCs should be in favour of a change.

2.5 Proposals accepted by 3/3 if beyond the remit of two TCs

Can be regarded as a unanimous decision.

2.6 Proposals accepted by 2/3 (and resolved) if beyond the remit of two TCs

At least three TCs should be in favour of a change.

2.7 If two or more TCs consider a decision to be pending, it is automatically regarded as 'pending' (unless two or more TCs have rejected the decision: in that case it is rejected).

E.g.
 A + A + A + P + P = pending
 or
 A + A + R + P + P = pending

2.8 If two or more TCs reject a decision, it is automatically rejected. ‘Rejected’ is dominant over ‘pending’.

E.g.
 A + R + R + P + P = rejected

2.9 Blanc votes are not accepted. It is of course possible, that during the voting procedure TCs may not have had enough time to cast a vote. If a single blanc vote is blocking the voting procedure, the chairman can decide. A + A + A + P + blanc vote can be regarded as accepted. If there are two blanc votes, the decision is regarded as pending.

2.10 Status quo

In the case of both rejected and pending proposals, the **status quo is Voous (1977c) for species and BWP for subspecies.**

2.11 Ad interim status quo

If, however, the status quo is definitely wrong, an **ad interim status quo** should be proposed:

E.g. *Larus fuscus*. Voous (1977c) does not mention any subspecies and it is therefore not clear which taxa he includes in this species. For subspecies, we turn to BWP. BWP, however, does not give a satisfactory solution (in BWP *barabensis* is included in the *cachinnans* group of *Larus argentatus*, whereas recent studies show that it should be placed in the *fuscus* group) (see Yésou 2002). In that case, the most widely supported view can be accepted as an ad interim status quo: *Larus fuscus*, including *L. f. graellsii*, *intermedius*, *fuscus*, *heuglini*, *taimyrensis* and *barabensis*).

2.12 Generic changes

In the case of generic changes, a 3/5 majority is sufficient for a change, particularly if the AOU is also agreeing with the change. This also applies when the dissenting TCs have voted R, P or blanc.

In order to revert a generic change, a 4/5 majority or unanimous decision is needed.

2.13 Stepwise splits

If more than two splits are proposed, the chairman can select the voting procedure: there can either be a vote on the recommendation as a whole or a separate vote on each component. E.g. Herring Gull *Larus argentatus*: there can either be a vote on the six fold split as a whole, or a separate vote on Herring Gull *L. argentatus* (polytypic: *L. a. argenteus*, *argentatus*), American Herring Gull *L. smithsonianus* (monotypic), East Siberian Gull *L. vegae* (polytypic; extralimital: *L. v. vegae* and *mongolicus*), Caspian Gull *L. cachinnans* (monotypic), Yellow-legged Gull *L. michahellis* (polytypic: *L. m. atlantis*, *michahellis*) and Armenian Gull *L. armenicus* (monotypic).

If – in the case of separate votes – a final decision on a four fold or more (five fold, six fold, etc.) split is blocked by a single pending vote on one of the components, the split as a whole can still be accepted.

2.14 Inclusion of formal votes in the Recommendations

The **formal votes should be included in the website version of the Recommendations** (as they are not votes of individuals, but of TCs, there is no objection to this). This would also avoid misrepresentations of formal votes by individual TCs and facilitate the application of clear rules in the voting process. In print, this is not a necessity, as space is generally limited in magazines.

This can be done in the following way:

BOURC TSC*	CAF	CSNA	A.J. Helbig	STC

With letters clarifying the official vote of the participating TCs (A = accepted; R = rejected; P = pending; Ø = beyond the remit). Remarks can be indicated by * and explained below the table.

(*) **IMPORTANT:** Decisions recorded within this document by the BOURC TSC do not affect the British List unless they have been, or until they are, published in the annual BOURC or TSC reports in *Ibis*.

2.15 If important new information or an important review is awaited (e.g. a paper in press or in prep.), the chairman can decide to move a provisionally accepted proposal to the pending category. In such cases, there should be a formal request by the taxonomist actively working on the issue to postpone acceptance. Such a decision was widely accepted by the participants of the 2003 AERC meeting in the Danube Delta for a paper on scoter *Melanitta* taxonomy by Martin Collinson. It should be noted that without this appeal, the scoter splits would have been accepted according to the regular voting procedure.

This is also the case **if important new information is available**. *Mergus cucullatus* and *Mergus albellus* have not yet been renamed *Lophodytes cucullatus* and *Mergellus albellus* as all published decisions by TCs on this matter predate an important new paper on this issue (Donne-Goussé et al. 2002). A proper evaluation of this paper is necessary to limit the risk of a need to change back in the near future. Another example is the Ottvall et al. (2002) paper on Redpolls *Carduelis flammea*.

When many new viewpoints are brought forward, the chairman can decide to move the case to pending (e.g. comments on the proposed split of Carrion Crow and Hooded Crow), particularly if this is requested by a third party.

3. The process of reaching decisions and writing recommendations should be improved:

Ideally, the **order** of reaching decisions should be:

3.1 writing proposals (including references to the Guidelines)

- The species accounts should be distributed by the chairman over the five participating TCs: it is their task and responsibility to write the species accounts.
- The species accounts should be written BEFORE the formal voting. In this way, the TCs do not only vote for the (a) the taxonomic change but also for (b) the foundations of the arguments and possibly already for the (c) formal text in the final publication.

3.2 discussing proposals (in comparison with the Guidelines)

Discussion could take place on a password-protected website, permanently saving all comments on each issue and the results of a formal 'voting' process. Then, it is easy to review opinions on a certain subject and also to know who has not yet reacted. Some websites automatically generate email which is sent to all people concerned. The BOURC TSC is working with such a website and it has become an essential tool.

3.3 deciding

3.4 publishing the reasons

- Full appraisals can be published separately by the TC who is the main author of the paper. CSNA and BOURC TSC are working in this way.
- Depending on the complexity of the taxonomic matter, this can be (1) an extensive literature review or (2) a concise conceptual text (1–2 paragraphs). Each decision should be summarised briefly in 1–2 paragraphs, indicating the main reasons of the decision and referring to relevant papers.
- In either case, the Guidelines should be applied. When species limits are changed, the criterion on which the change is based must be named (e.g. 4.1. Diagnosably distinct allopatric taxa). In this way, other TCs can easily evaluate the criterion.

The **proposal of splits** without accompanying discussion texts is unsatisfactory. New taxonomic recommendations can be proposed by a TC or individual taxonomists for circulation within the AERC TAC, but they should always be accompanied by a **discussion text**, containing relevant evidence regarding the proposal and essential references. A proposal will not be accepted if the details to make a decision are not given. Taxonomists/TCs proposing a split should always be willing to provide additional information on their proposals when requested to do so by other TCs.

Once a split has been accepted, the taxonomist/TCs who have proposed the split are **responsible for writing** the chapter on the decision by the AERC TAC in the minutes of the 2003 AERC meeting.

4. On what basis should the AERC TAC's Taxonomic Recommendations be judged by the AERC?

The working document was presented to the AERC and the meeting was asked to cast a vote on accepting or rejecting the proposed Recommendations (with a final possibility to comment upon and to improve the draft to both the AERC and the AERC TAC before publication in the minutes of the AERC meeting). It would be useful if TCs specified which species concept they are following [e.g. phylogenetic approach to species-level taxa Zink & McKittrick (1995) by CSNA, variants of Biological Species Concept by most other TCs (e.g. Mayr 1963, 1982, 1996),...]. Also, interpretations of the various species concepts seem to differ, not only among TCs, but also among individual taxonomists within TCs. The Guidelines are not totally supported by many, and – most surprisingly – not even by some of its authors. Should the Guidelines be taken as gospel, or are they encouraging a wider debate? Some concern has been expressed that the Guidelines tend to a phylogenetic viewpoint in some instances (e.g. *Motacilla alba* and *M. flava* complex).

It may seem awkward if TCs vote on a European list when using different species concepts to reach decisions. There are three possibilities:

(a) Everybody will agree on the Guidelines (or some other clearly published and practical species concept). This may be unrealistic.

(b) In the TAC's Recommendations, only the votes of those committees supporting the Guidelines are taken into account. This is against what was decided.

(c) There will be no explicit species concept behind the European list. There will be a consensus about the list (in practice, about the splits) but not about the reasoning behind them. Of course, it would be preferable if everybody agreed on basics but this seems to be impossible – and unfortunately seems to remain so.

5. Which species concept is used in the AERC TAC's Recommendations?

There will be no explicit species concept, but an implicit one that will be defined by the results. Should we talk about 'Consensual Species Concept'? The real species concept can be judged from the rationale behind each and every taxonomic decision. Even if we are lacking an explicit species concept or definition, the decision can be judged to be logical or illogical, well reasoned or not, in line with other decisions or not. This is far from a perfect solution, but what is the alternative? It could be compared to the choice in society between judgement by precedents or a written constitution. Decisions can be reconsidered whenever new scientific evidence is published that makes such reconsideration necessary.

6. The implications of the acceptance of the Guidelines should be discussed.

If the Guidelines are also accepted by the AERC, should they be applied literally, or should the AERC and the AERC TAC have the possibility to alter the Guidelines in line with a majority view?

7. 'Semispecies' – subspecies: more research is needed!

A weak point of the Guidelines seems to be that the decision if two taxa are 'semispecies' or subspecies needs a lot of information. There is hardly enough data on crows, one of the most familiar and well-studied birds. Alström & Mild (2003) noted that criteria for pure *Motacilla flava thunbergi* and pure *M. f. flava* are incorrect in several studies in the contact zone – which of course makes them almost worthless. The hybridisation zone of *flava* and *thunbergi* is quite well studied with several published works. But still we can not say much about its width, stability or presence of pure phenotypes – the criteria in the Guidelines (A. Lindholm in litt.). Obviously more research is welcomed. But what can we say about hybridisation zones, which run somewhere in the middle of Asian deserts, or in the tropics? It may be a logical guideline, but is it practical? One should practise on European cases of secondary intergradation, e.g.:

Phalacrocorax c. carbo \diamond *Phalacrocorax c. sinensis*

Anser f. fabalis \diamond *Anser f. rossicus*

Buteo b. vulpinus \diamond *Buteo b. buteo*

Calidris a. alpina \diamond *Calidris a. schinzii*

Larus f. heuglini \diamond *Larus f. fuscus*

Larus f. fuscus \diamond *Larus f. intermedius*

Larus f. intermedius \diamond *Larus f. graellsii*

All Yellow Wagtail *Motacilla flava* cases

Motacilla a. alba \diamond *Motacilla a. yarrellii*

Luscinia s. svecica \diamond *Luscinia s. cyanecula*

Aegithalos c. caudatus \diamond *Aegithalos c. europaeus*

Sitta c. europaea \diamond *Sitta c. caesia*

Nucifraga c. caryocatactes <> *Nucifraga c. macrorhynchos*

Which of these fulfil the requirements outlined in the Guidelines for 'semispecies' and which don't? Which are different from the crow or black-eared wheatear complex and why? Which are insufficiently studied?

8. 'Task forces' should be working on pending splits. Such task forces should consist of taxonomists with a special interest in a particular split. One taxonomist should co-ordinate the task force. He is also responsible for editing the account on this split in the Recommendations and/or the publication of a more detailed paper on this subject in a magazine of his choice (but quoted in the Zoological Record). A first draft of this text should be circulated among all TCs. A task force may also be useful to deal with WP species not covered by most of the national TCs (e.g. because the species is not on the national list).

9. The **deadline** to state an opinion should be **three months at the most** (one month may not always be possible, with the possible exception of more straightforward cases and depending on the quality of the submitted proposals). TCs are encouraged to **decide by email**, in order to speed up the process.

10. A report by the AERC TAC should be published **biannually in the minutes of the AERC meeting and proposed to interested birding magazines**.

11. There should be a contact person in the **AOU**, which should systematically be contacted when 'American' species are discussed. The main reason for this is an exchange of information. The AOU may have access to relevant information on North American taxa which is not (easily) available to the AERC TAC (e.g. an American PhD thesis may be scarcely available in European scientific libraries, there may be interesting information on ongoing research, etc.). The AOU's Committee on Classification and Nomenclature (J. V. Remsen, LSU) should be contacted.

12. There should be a **regular exchange of information** between all TCs represented within the AERC TAC. This information should be regarded as confidential until a final decision has been reached and all parties agree to release the information.

13. Ideally, there should also be a formal **co-operation between TCs and RCs** in the preparation of the Recommendations. It is useful to invite some RC members to act as referees during the preparation of the Recommendations (on condition that the contents of the Recommendations remain confidential between the referees and the TAC). In countries without a TC, often the RC is compiling the official national list. Such RCs are the main target group of the AERC TAC and the knowledge of such RCs should be consulted (e.g. advice on particular records – officially accepted or not – in the WP, advice on national ornithological literature, comments on ongoing research, assistance to collect samples for DNA study, study of specimens in national collections, etc.). **The AERC TAC is about building bridges.**

14. It is preferable if the **chairman** were a **taxonomist**.

15. The **task of the chairman** should be (a) to distribute the species accounts among the TCs, (b) to manage the data flow and the results of formal voting and (c) to compile the accounts in the report. He is a compiler and not a copywriter.

16. It may be worthwhile to reconsider the very **structure** of the AERC TAC. A core of motivated taxonomists, volunteering to co-operate, may be more productive than the current structure as defined at Hel Peninsula.

17. **Any changes to the AERC TAC should be agreed between the AERC and the taxonomists who are supposed to do the work.**

18. The procedure to accept the **final draft** of the Taxonomic Recommendations still needs to be agreed upon.

19. For category E species, a standard list is needed (e.g. Rolf de By's online version of Sibley & Monroe <http://www.ornitaxa.com/SM/SMOrg/sm.html> with the exception of species already treated in our WP list). The Sibley & Monroe list may be preferable for category E species, as this list is also used by CITES.

20. Some taxa occurring on the boundaries of the WP have received little attention by TCs. There were insufficient votes on some **widely accepted splits**: e.g. Audubon's and Persian Shearwater, Indian Spotted Eagle and Lesser Spotted Eagle, Least Tern and Little Tern. If these splits are accepted at the next AERC meeting, they could still be included in the AERC TAC's list. Similarly, generic changes of such taxa have received poor attention (e.g. Pale Rockfinch and Chestnut-shouldered Sparrow). In such cases, the AERC should decide.

21. Publication of the Recommendations of the AERC TAC. It is possible, but perhaps not crucial to publish the Recommendations of the AERC TAC in magazines. If magazines are willing to publish the Recommendations (or part of them), they are, however, free to do so. The Recommendations should, however, first be published on the website of the AERC.

22. The WP list

(a) We should decide who will be involved in the supplement, what each person's role will be, what review processes will be, and how we will arrive at a final agreed text for the supplement.

(b) We will need to agree the boundaries, which we will follow for the 'supplement'.

(c) Authorship team would need to see a current draft of the main list.

(d) A draft list of 'extra' species to be drawn up from knowledge of authors & a literature review.

(e) Appeal to be put out via the Internet and perhaps through the main journals, asking for people to review draft lists and make comments.

(f) Authorship team to review comments, prepares a second draft.

(g) Authorship team to perform a final check against the final version of the main list to eliminate duplication, and produce a final version of the supplement.

Other issues to think about:

(a) Will we want to cover infraspecific taxa? If so, how should we tackle this? One option would be to follow BWP, unless the AERC TAC has formally published a change.

(b) How to tackle species of unknown origin? We could include category A, B and C species. Should category D species be included or not?

(c) How to deal with verification of records from countries without a Rarities Committee? We may need a committee to deal with new taxa for the WP in countries without a RC. At the 7th AERC meeting, it was agreed upon that G. De Smet would make a list of these taxa and the countries involved and that AERC should offer to help in the verification of these records. The AERC also agreed that it is prepared to gather records of major rarities made in countries without Records Committees and to keep them on file until a national RC is in operation. Several members, however, noted the need for this work to be carried out sensitively and without offence to birders in the countries concerned.

(d) Where/when the supplement should be published?

23. Activity of the AERC TAC after the meeting

We should find a way to continue the functioning of the AERC TAC:

(a) the functioning of the AERC is discussed at the AERC meeting based on elements proposed by the TCs in this document

(b) a member of the AERC TAC or an expert who is not a member of the AERC TAC compiles the results of this discussion; he should draw up a proposal with changes to the Terms of Reference.

(c) the AERC TAC members give their advice on the Terms of Reference through the TAC representatives, allowing the possibility to make changes whenever the functioning can be improved.

Taxonomic Recommendations

Important: species texts have been attributed to the TCs by the chairman. TCs have not taken responsibility for the texts, unless the texts are followed by the name of an author. Texts, which are not followed by the name of an author, however, have been circulated up to fourteen times among all TCs, providing ample opportunity to react to those willing to contribute in a constructive way. The Recommendations have been compiled by Gunter De Smet.

Part one: non-passeriformes

Egyptian Goose *Alopochen aegyptiaca*

Notice the correct spelling (David & Gosselin 2000b).

Common Teal *Anas crecca* to be treated as two species (accepted by the AERC TAC in Sangster et al. 2002a):

- Common Teal *Anas crecca* (polytypic: *A. c. crecca*; extralimital: *A. c. nimia*)
- Green-winged Teal *Anas carolinensis* (monotypic)

Rationale: see Sangster et al. (2002a) and appendix 1.

Willow Ptarmigan *Lagopus lagopus*

Notice the correct spelling of *Lagopus lagopus scotica* (incl. *hibernica*), *Lagopus lagopus variegata* and *Lagopus lagopus rossica* (David & Gosselin 2002b).

Rock Ptarmigan *Lagopus muta*

Notice the correct spelling of *Lagopus muta pyrenaica*, *Lagopus muta helvetica*, *Lagopus muta muta* (incl. *alpina*, *scandinavica*), *Lagopus muta hyperborea* (incl. *hemileucura*) and *Lagopus muta pleskei* (incl. *transbaicalica*) (David & Gosselin 2002b).

Red-throated Diver *Gavia stellata* (monotypic, incl. *G. s. 'squamata'*) remains as is.

CSNA Red-throated Diver is treated as monotypic by De Korte (1972) because greyish edges to mantle feathers supposedly typical of *G. s. 'squamata'* in summer plumage (Svalbard and Franz Josefland) are variable and do not warrant subspecific recognition (see Cramp & Simmons 1977, Sangster et al. 1997). The BOURC TSC (e.g. Knox 1992) regards Red-throated Diver as polytypic. When TCs have different opinions on subspecies, the AERC TAC follows the choice of BWP as status quo.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
P	A	A		A

Black-throated Diver *Gavia arctica* to be treated as two species:

- Black-throated Diver *Gavia arctica* (polytypic: *G. a. arctica*; extralimital: *G. a. viridigularis*)
- Pacific Diver *Gavia pacifica* (monotypic)

CAF There are no records of *G. pacifica* in the Western Palearctic. All committees and individual taxonomists who stated an opinion are in favour of the split; mind that the BOURC TSC still keeps this decision pending. [Pacific Diver is already treated as a separate species in the Swedish Holarctic checklist (SOF 1995), which serves as a base for considerations by the STC.] On that basis, it is accepted by the AERC TAC. *G. pacifica* is widely sympatric with *G. a. viridigularis* in E Siberia, with sometimes pairs of both breeding on the same pond (Kistchinski 1978, pp 24–27, Portenko 1981, Stepanyan 1975, p 9, Il'icev & Flint 1985, pp 224–225). Both species have also been reported breeding sympatrically in W Alaska (Bailey 1943, Douglas & Sowl 1993, Gabrielson & Lincoln 1959). Bailey (1943) and Storer (1978) reported specimens suggesting hybridisation between the two species, but the occasional hybridisation between Black-throated Diver and Great Northern Diver *G. immer* is also documented (Hunter & Dennis 1972, Robertson & Fraker 1974). Both species differ in structural, postural and plumage characters. Differences in vocalisations between *G. arctica* and *G. pacifica* are mentioned by Il'icev & Flint (1985), p 224, and can be heard on sound recordings (e.g. Vepintsev 1982). We are not aware of any molecular studies on *G. pacifica*.

Treated as a full species by the AOU. We would like to thank O. van Rootselaar for updating and kindly allowing to use his summary of the taxonomic history of black-throated divers on the internet (<http://www.birder.com/science/taxonomychat/0434.html>) and to J. Van Impe for providing additional comments.

ID: Birch & Lee (1995), Evanich (1977/8), Jonsson (1996), Kaufman (1990), McCaskie et al. (1990), Roberson (1989), Schulenberg (1989) and Walsh (1988).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
P	A	A	A*	A

(*) Accepted as 'semi- or allospecies'.

Black-browed Albatross *Diomedea melanophris* to be renamed *Thalassarche melanophris* (polytypic; *T. m. melanophris*; extralimital Campbell Islands Albatross *T. m. impavida*)

BOURC TSC *Diomedea melanophris* becomes *Thalassarche melanophris* (Nunn et al. 1996, Sangster et al. 2002b). Cramp & Simmons (1977) treated Black-browed Albatross as a polytypic species (nominate and *T. m. impavida*) but (erroneously?) treated as monotypic by Knox (1992). These two taxa may be considered as 'allospecies'. Sympatric breeding, however, has been recorded on the Campbell Islands, where less than 30 pairs of *melanophris* breed among 26,000 pairs of *impavida*. 'Occasional interbreeding with Campbell Islands Albatross occurs, but with little success. This perhaps indicates that the "allospecies" may be a "biological species" and it is possible that further study will improve this assumption' (Russ & Shirihai 2000; see Shirihai & Jarrett 2002 for a considerably more detailed discussion). Phenotypically, adult Campbell Islands Albatross is well differentiated from nominate *melanophris* by (1) honey-coloured iris, (2) heavier black eyebrow (particularly in front of the eye), (3) usually more extensively dark underwing. Some seabirds have been split on account of less obvious phenotypical differences than these two taxa. As this issue has not yet been actively discussed within the AERC TAC, however, both taxa are provisionally retained in *T. melanophris* here. Additional comments on albatross genera are announced (Penhallurick & Wink in press). Relationships amongst the *melanophris* – *impavida* – *chrysostoma* complex have been studied by Burg & Croxall (2001) using mtDNA and microsatellites. Their genetic analyses support the classification of *T. impavida* as being distinct from *T. melanophris*, but would also suggest splitting *T. melanophris* into two groups: Falkland Islands, and Diego Ramirez/South Georgia/Kerguelen.

ID: For a recent treatment of the identification of Blackbrowed Albatross, cf. Jiguet (2000).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A*	A*	A*	A*	A*

(*) Systematic position of (sub)species remains to be studied.

Yellow-nosed Albatross *Diomedea chlororhynchos* to be renamed *Thalassarche chlororhynchos* (polytypic; *T. c. chlororhynchos*; extralimital: *T. c. carteri*)

BOURC TSC See Nunn et al. 1996 and Sangster et al. 2002b for arguments supporting this generic change. Robertson & Nunn (1998) proposed to recognise Atlantic Yellow-nosed *chlororhynchos* and Indian Yellow-nosed Albatross *carteri* as species. See Shirihai & Jarret (2002) for some discussion concerning the names *bassi* and *carteri*, which both appear to be available for the Indian Ocean form. According to Robertson (2002), who examined the type specimens of *carteri* Rothschild 1903 and *bassi* Mathews 1912, these two represent the same taxon. Hence *carteri* has priority and *bassi* should be dropped as a junior synonym. The AERC TAC has not yet discussed whether Yellow-nosed Albatross should remain a single species or be split into two species.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A*	A*	A*	A*	A*

(*) Systematic position of (sub)species remains to be studied.

Shy Albatross *Diomedea cauta* to be renamed *Thalassarche cauta* (polytypic; *T. c. cauta*; extralimital: *T. c. steadi* (?), *salvini*, *eremita*)

BOURC TSC See Nunn et al. (1996) and Sangster et al. (2002b) for the change in generic name. Robertson & Nunn (1998) proposed to split Shy Albatross into a number of species (without providing scientific data in support of their rearrangement). Russ & Shirihai (2000) regarded these as 'allospecies' and named these Shy Mollymawk *T. [c.] cauta*, White-capped Mollymawk *T. [c.] steadi*, Salvin's Mollymawk *T. [c.] salvini* and Chatham Islands Mollymawk *T. [c.] eremita*. S. Bartle, curator of birds in the Te Papa National Museum,

New Zealand, maintains that the Tasmanian form *steadi* cannot be reliably identified in museum collections and no convincing evidence on its separation has yet been published. According to a molecular study by Abbott & Double (2003a) the taxonomic separation of Shy (*cauta*) and White-capped (*steadi*) Albatrosses from Salvin's (*salvini*) and Chatham (*eremita*) Albatrosses is clearly justified, whereas the taxonomic classification within Shy and White-capped albatrosses remains unresolved. This implies that two species are justified within *T. cauta* s.l.: *T. cauta* (including *T. c. cauta* and *T. c. steadi* for the time being) and *T. salvini* (provisionally including *T. s. salvini* and *T. s. eremita*). Abbott & Double (2003b) further investigated genetic structure within the *cauta* / *steadi* complex using microsatellites. Although they argue for specific status of these forms, their data show low very level of divergence between *cauta* and *steadi* (typical of populations within species) and indicate that *cauta* was recently founded by colonisation from *steadi*. Their result are in fact consistent with a recent divergence of *cauta* and *steadi* and a lack/low-level of contemporary gene flow as a result of geographic isolation, a situation typical of intraspecific population structure rather than speciation. These taxa are thus better treated as conspecific. As the AERC TAC has not yet actively addressed this issue, all four taxa are provisionally combined in *T. cauta* here.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A*	A*	A*	A*	A*

(*) Systematic position of (sub)species remains to be studied.

Soft-plumaged Petrel *Pterodroma mollis* complex to be treated as three species (accepted by the AERC TAC in Sangster et al. 2002a):

- Soft-plumaged Petrel *Pterodroma mollis* (monotypic; extralimital)
- Fea's Petrel *Pterodroma feae* (monotypic, incl. 'deserta')
- Zino's Petrel *Pterodroma madeira* (monotypic)

Rationale: see Sangster et al. (2002a) and appendix 1.

Black-capped Petrel *Pterodroma hasitata* to be treated as two species:

- Bermuda Petrel *Pterodroma cahow* (monotypic)
- Black-capped Petrel *Pterodroma hasitata* (monotypic)

STC Voous (1973) treated Bermuda Petrel (locally known as Cahow) as a subspecies of Black-capped Petrel *P. hasitata*. Bermuda Petrel, however, appears to be more closely related to Fea's Petrel *P. feae* than to *P. hasitata* (e.g. Nunn & Stanley 1998) and is now widely recognised as a distinct species. In the Western Palearctic, Bermuda Petrel has been recorded once (*Birding World* 16: 22, 2003) and Black-capped Petrel four times (Howell 2002). The BOURC TSC has not considered this split.

ID: The identification of Bermuda Petrel is treated by Wingate et al. (1998).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
P	A		A	A

Manx Shearwater *Puffinus puffinus* to be treated as six species (accepted by the AERC TAC in Sangster et al. 2002a):

- Manx Shearwater *Puffinus puffinus* (monotypic)
- Yelkouan Shearwater *Puffinus yelkouan* (monotypic)
- Balearic Shearwater *Puffinus mauretanicus* (monotypic)
- Black-vented Shearwater *Puffinus opisthomelas* (monotypic; extralimital)
- Fluttering Shearwater *Puffinus gavia* (monotypic; extralimital)
- Hutton's Shearwater *Puffinus huttoni* (monotypic; extralimital)

Rationale: see Sangster et al. (2002a) and appendix 1.

Northern Gannet *Sula bassana* becomes *Morus bassanus* (monotypic)

Cape Gannet *Sula capensis* becomes *Morus capensis* (monotypic)

CAF Some arguments have been put forward recently to recognise several genera in the family Sulidae. From BOURC (1991): 'An osteological study of the boobies *Sula* and the gannets *Morus* has revealed differences between the two groups sufficient to warrant generic separation (van Tets et al. 1988; see also Olson & Warheit 1988)'. Sangster et al. (1997) presents additional reasons. HBW, on the other hand, advocates keeping one genus only for all Sulidae. A recent phylogenetic study by Friesen & Anderson

(1997), based on mtDNA sequencing, produced a strongly supported, and thus probably reliable, phylogenetic tree. Three main lineages were recovered: the gannets (*bassana*, *capensis*, and *serrator*), the boobies (*dactylatra*, *leucogaster*, *nebouxii*, *sula*, and *variegata*) and the Abbott's booby (*abbotti*), which is clearly more closely related to the gannets than the true boobies. These three lineages correspond to the three genera advocated by BOURC (1991). Approximate dating of the speciation events in Sulidae by Friesen & Anderson (1997) suggest that the current species within gannets and boobies originated within the last 3 million years, whereas the three main lineages diverged between 14 million years ago (*abbotti* from the gannets) and 23 million years ago (gannets + *abbotti* from boobies). These three lineages are thus well-supported monophyletic groups that diverged a long time ago. Three nomenclatural options are possible for the family Sulidae: (a) to retain all species in one genus (*Sula*), (b) to recognise three genera: *Sula* for the true boobies (including *granti*, recently elevated to species rank), *Morus* for the gannets and *Papasula* for *abbotti* or (c) a two-genera treatment (i.e., with *Morus abbotti*). All options are in agreement with the evolution of the group and are thus valid on scientific grounds. Based on the fact that the three main lineages within Sulidae diverged a long time ago, we recommend adopting the 'three genera' option. Keeping all Sulidae species in the genus *Sula* does not recognise the ecological, behavioural and morphological diversification of the Sulidae. Furthermore, the age of these lineages is more compatible with a treatment as different genera, since keeping in the same genus some species that diverged more than 20 million years ago would be unusual based on current treatment of other avian groups. Furthermore, since many authorities have accepted *Morus* as a full genus (including AOU) it might be best to follow for the sake of homogeneity. Notice that the AOU has accepted *Sula* (in 1886, 1895 and 1910), next *Morus* Leach 1816 (in 1931), then *Morus Vieillot* 1816 (in 1957), again *Sula* in 1983 and eventually *Morus* once more in 1998. (P.-A. Crochet)

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A	R	A

Pygmy Cormorant *Phalacrocorax pygmeus* (monotypic) remains as is

Shag *Phalacrocorax aristotelis* (polytypic: *P. a. aristotelis*, *desmarestii*, *riggenbacchi*) remains as is

No changes in cormorant genera.

CAF Siegel-Causey (1988) proposed to recognise nine genera of cormorants and shags based on a phylogenetic analysis of morphological characters. Kennedy et al. (2000), based on mtDNA sequencing, re-evaluated the evolutionary hypotheses of Siegel-Causey and found them to be mostly unsupported. They have a limited species sampling, however, and many of the basal relationships they found are not well supported. The evolutionary relationships among cormorants and shags are thus at present poorly understood, but the classification of Siegel-Causey is clearly unreliable. The genus *Stictocarbo* in particular is clearly polyphyletic as defined by Siegel-Causey. Furthermore, the level of sequence divergence among cormorants and shags is not unusual for intra-generic divergence in birds. Even if several genera can be recognised among cormorants and shags, it is recommended not to propose any generic splitting until a reliable picture of the evolution of the group is available. This is why Kennedy et al. state: 'Given the lack of resolution and the levels of sequence divergence we favour a conservative approach [...]. Until a more robust and complete phylogeny is available the use of the single genus *Phalacrocorax* appears sensible [...].' We recommend following this treatment for the time being. The AOU has always maintained all North American cormorant species in *Phalacrocorax*. (P.-A. Crochet)

Note: next to *Ph. pygmeus*, used by Voous (1973, 1977c), the spelling *Ph. pygmaeus* is often encountered. M. Gosselin commented: 'A difficult case, since *pygmeus* could be considered a lapsus calami for *pygmaeus* (but since *pygmaeus* is a Latin word, *pygmeus* is not an incorrect transliteration or latinisation) [...] but I don't think it would be a good idea. On the other hand, if *pygmeus* is not considered a lapsus calami, the only reason why the emended *pygmaeus* would be correct would be if it were in prevailing usage, which is probably not the case.' See also <http://www.zoonomen.net/>, where *pygmaeus* is considered an unjustified emendation. We thus retain the original spelling here, *Pelecanus pygmeus* (Pallas 1773).

(M. Gosselin; *this remark is not the responsibility of CAF or P.-A. Crochet, as none of us has the competence to comment on this purely nomenclatural problem*).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
	A	R	A	A

Dwarf Bittern *Ardeirallus sturmii* becomes *Ixobrychus sturmii*

CSNA Dwarf Bittern is currently placed in the monotypic genus *Ardeirallus* based on reduced sexual dimorphism compared to species placed in *Ixobrychus* and behavioural and ecological differences with species generally placed in *Ixobrychus* (Verheyen 1959, Curry-Lindahl 1971). However, the validity of these reasons has been questioned (Payne & Risley 1976). Cladistic analysis of osteological characters (Payne & Risley 1976), and re-analysis of this data set (McCracken & Sheldon 1998), indicate that Dwarf Bittern appears to be nested within the *Ixobrychus* clade. Therefore, Dwarf Bittern is placed in *Ixobrychus* and becomes *Ixobrychus sturmii*. (George Sangster; see full account in appendix 3)

Vernacular name: Sturm's Bittern may be a better name than Dwarf Bittern, as *I. sturmii* is not the smallest species of bittern (not even in Africa).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
Ø	A	A	A	A

Striated Heron *Butorides striata* to be treated as two species:

- Striated Heron *Butorides striata* (polytypic: *B. s. striata*, *brevipes*; extralimital – in alphabetical order: *B. s. actophila*, *albolimbata*, *amurensis*, *atricapilla*, *chloriceps*, *crawfordi*, *degens*, *idenburgi*, *javanica*, *macrorhyncha*, *moluccarum*, *papuensis*, *patruelis*, *rhizophorae*, *rutenbergi*, *solomonensis*, *spodiogaster*, *stagnatilis*)
- Green Heron *Butorides virescens* (polytypic: *B. v. virescens*; extralimital: *B. v. anthonyi*, *frazari*, *maculata*)

STC *Butorides* Blyth, 1852, was first established in combination with the feminine adjective *javanica*, and is thus feminine. In order to comply with the gender agreement rules to avian species-group names, the adjective *striatus*, *-a*, *-um* must have the feminine ending *-a* in this case (David & Gosselin 2002b). The split of *B. striata* and *B. virescens* was already accepted by Voous (1973) – the starting point of the AERC TAC list – and is accepted unanimously by the AERC TAC. Furthermore, it is already treated as a separate species in the Swedish Holarctic checklist (SOF 1995). Hartert (1920) took the view that *virescens* and *striata* should be regarded as conspecific. In North America, Monroe and Browning (1992) reanalysed the taxonomy of *Butorides* and concluded that *B. striata* and *B. virescens* were separate species; AOU (1993) adopted this change. Previously, Payne (1974) had lumped *striata* and *virescens*, and North American populations were regarded as Green-Backed Heron *B. striata*. Hayes (2002) reanalysed Payne's data and found an increased variability and intermediacy in the contact zone implying extensive hybridisation. However, the presence of apparently pure *B. virescens* and *B. striata* phenotypes within the contact zone suggests that assortative mating does occur, supporting the treatment of the two forms as distinct species. Lava Heron *B. sundevalli* is also considered a distinct species by Hayes (2002): the persistence of pure *B. sundevalli* in a potential hybrid zone on the Galápagos Islands supports its treatment as a distinct species. Dowsett & Dowsett-Lemaire (1980) pp 152–153 recognised *Butorides* as distinct from *Ardeola* but DNA-DNA hybridisation data are inconclusive (Sibley & Monroe 1990). A review of all the taxa is lacking. The number of recognised subspecies within the *Butorides* complex varies from source to source, e.g. 37 in Howard & Moore (1980), 30 according to Payne (1979), adopted by Hancock & Kushlan (1984), and 25 in Dickinson (2003) – which is the latest version of the *Howard & Moore Checklist* (the most recent source is followed here for convenience; mind, however, that the AERC TAC did not examine the subspecific limits within the entire complex). A record of *B. s. amurensis* in Norway was placed in category D and is therefore not included in the Western Palearctic list.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A	A*	A

(*) Accepted as 'semi- or allospecies'.

Little Blue Heron *Hydranassa caerulea* becomes *Egretta caerulea*

Black Heron *Hydranassa ardesiaca* becomes *Egretta ardesiaca*

Tricoloured Heron *Hydranassa tricolor* becomes *Egretta tricolor*

CSNA Throughout history, names of heron genera have often changed. In the checklists of the American Ornithologists' Union, Little Blue Heron was named *Ardea coerulea* in 1886, *Ardea caerulea* in 1895, *Florida caerulea* in 1910 and *Egretta caerulea* in 1983; Tricoloured Heron was named *Ardea tricolor* in 1886, *Hydranassa tricolor* in 1910 and *Egretta tricolor* in 1983. (Source: <http://members.aol.com/darwinpage/zoo/AOUa.htm>). Bock (1956) very tentatively placed Black Heron in *Hydranassa*, and Irwin (1975) supported this treatment. Dickerman & Parkes (1968) considered that the

characters used to separate *Hydranassa* (breeding plumes) were highly adaptive. Recent authors have merged *Hydranassa* in *Egretta* (e.g. Dowsett & Dowsett-Lemaire 1980). Although Black Heron was not covered in the phylogenetic study of Ardeidae by Sheldon (1987b), it may be preferable to merge *Hydranassa* with *Egretta* to reduce the number of genera but genus size is still largely a matter of taste. For heron systematics see Curry-Lindahl (1971) and Payne & Risley (1976). Sheldon (1987b) supplied DNA-DNA hybridisation data, which support the inclusion of the genus *Hydranassa* in *Egretta* (e.g. the close relationship between *E. caerulea* and *E. thula*) (see also Sheldon 1987a and Sheldon et al. 2000). Bolman et al. (submitted) may be particularly interesting, as they provide ‘a highly resolved and biologically sensible tree’ in which *thula* is sister to *caerulea*, and *tricolor* is basal to both ((*Egretta thula*, *E. caerulea*), *E. tricolor*). Hence, a taxon regrouping *caerulea* and *tricolor* but excluding *thula* would be paraphyletic, and this would be strong evidence against the validity of *Hydranassa* sensu Voous. For a hybrid between Little Blue Heron and Snowy Egret *E. thula*, see Sprunt (1982). Without gene flow, hybridisation has no influence on the specific status of birds under the BSC. Even when hybrids are fertile and when they mate with one or both of the parent species, they can be biological species if the fitness (reproductive success) is much less than in the parent species.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
∅	A		A	A

White Stork *Ciconia ciconia* to be treated as two species:

- White Stork *Ciconia ciconia* (polytypic; *C. c. ciconia*; extralimital: *C. c. asiatica*)
- Oriental White Stork *Ciconia boyciana* (monotypic; extralimital)

In the Palearctic, three forms of white storks are generally accepted, being

1. *ciconia* (Linnaeus) 1758 (Europe and N Africa)
2. *asiatica* Severtzov 1873 [Vaurie gives: 1872] (C Asia in Afghanistan and Turkestan)
3. *boyciana* Swinhoe 1873 (NE Asia and Japan)

The latter honours Robert Henry Boyce (1834–1909), civil servant in Shanghai, China. The taxonomic status of Oriental White Stork *C. boyciana* has been a matter of debate for a long time [see Vaurie (1965, p 85), Kahl 1972a, b, King 1981, Neufeldt & Wunderlich 1982, Creutz (1988, p 18), Hancock et al. (1992, pp 107–108)]. Howard & Moore (1980) included *boyciana* in *ciconia*, based on Peters (1931), Kahl (1971, 1972b) and Kahl & Schüz (1972, *Vogelwarte* 26). Walters (1981) also included *boyciana* in *ciconia*, but with the remark that ‘*Ciconia boyciana* Swinhoe, of Amurland, Korea and Japan, is sometimes separated’. Howard & Moore (1991, p 17) separated *boyciana* as Oriental White Stork, referring to note ‘23.2’ (p xvi): ‘Hancock, J. 1989. pers. comm.’. Subsequently, Beaman (1994, p 65: Taxonomic notes) only commented ‘Now frequently treated as specifically distinct [e.g. Sibley & Monroe (1990, p 317), Stepanyan (1990, pp 41–42)] due to significant morphological differences (including black bill colouration)’. White Stork and Oriental White Stork are allopatric, but so closely related that they could be considered a ‘superspecies’ (Amadon 1966). There are, however, significant morphological (e.g. body size, culmen and tarsus length, bare parts colouration – both in adults and nestlings – and plumage) and behavioural differences, e.g. up-down and threat up-down displays (King 1988, Archibald & Schmitt 1991). On account of these differences, the white storks are now widely treated as two species. (Text by O. van Rootselaar.)

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
P	A		A*	A

(*) Accepted as ‘semi- or allospecies’.

Greater Flamingo *Phoenicopterus ruber* to be treated as three species:

- Caribbean Flamingo *Phoenicopterus ruber* (monotypic)
- Greater Flamingo *Phoenicopterus roseus* (monotypic)
- Chilean Flamingo *Phoenicopterus chilensis* (monotypic)

BOURC TSC Greater Flamingo *Ph. roseus*, Caribbean Flamingo *Ph. ruber* and Chilean Flamingo *Ph. chilensis* are well-marked taxa, which are best treated as separate species (cf. Hazevoet 1995, Sangster 1997, Sangster et al. 1999) based on (1) distinct morphological differences, (2) qualitative differences in plumage and bill pattern, colouration of legs (e.g., van den Berg 1987b, Treep 1994, Sangster 1997a), (3) different displays and (4) vocalisations (Studer-Thiersch 1964, 1974 and 1975). This split is accepted unanimously by the AERC TAC.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC

A	A	A	A*	A
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(*) Accepted as ‘semi- or allospecies’.

Honey Buzzard *Pernis apivorus* to be treated as two species:

- European Honey Buzzard *Pernis apivorus* (monotypic)
- Crested Honey Buzzard *Pernis ptilorhyncus* (polytypic: *P. p. orientalis*; extralimital: *P. p. ruficollis, torquatus, ptilorhyncus, palawanensis, philippensis*)

A.J. Helbig The AERC TAC did not discuss this split as it is already accepted by Voous (1973). Crested Honey Buzzard is also treated as a separate species in the Swedish Holarctic checklist (SOF 1995). Although the two are alleged occasionally to interbreed west of the Yenisey, they differ constantly in wing shape and emargination, foot size, crest development, and wing and tail markings (Ferguson-Lees & Christie 2001). Reports of intermediate birds are few and could in many cases refer to individual variation rather than hybrids. The variation within *P. ptilorhyncus* requires further study. It is sometimes proposed that there should perhaps be a further division between the Japanese/Siberian *orientalis* and the variably crested *ptilorhyncus* forms of SE Asia. Incidentally, the variable crests are thought to be adaptations for mimicking the local hawk eagles *Spizaetus* (van Balen et al. 1999; Edelstam & King in Ferguson-Lees & Christie 2001). The variation among the mainly sedentary taxa is also insufficiently studied. The molecular phylogeny of *Pernis* is currently being investigated by Haring & Gamauf (in prep.) of the Research Group of Molecular Systematics, Museum of Natural History Vienna. Helbig (unpublished) also sequenced mtDNA of *P. ptilorhyncus orientalis* and found an important divergence from *P. apivorus*. Within the *P. ptilorhyncus* complex, genetic diversity is highest in the south-east (M. Riesing in litt.).

Note: besides *ptilorhyncus*, the emended *ptilorhynchus* is also regularly encountered. *Ptilorhynchus* is clearly not an ‘incorrect original spelling’, as incorrect transliterations or latinisations are not to be considered inadvertent errors (ICZN Art. 32.5.1). Therefore, the only reason why the emended ‘*ptilorhynchus*’ would be correct would be if it were in ‘prevailing usage’ [=substantial majority of the most recent authors, cf. ICZN Glossary + Art. 33.3.1], which is probably not the case (M. Gosselin in litt.).

ID: The identification of Crested Honey Buzzard is treated by e.g. Forsman (1994), Pøter et al. (1996) and Clark (1999); for Honey Buzzard, see e.g. Forsman & Shirihai (1997) and Forsman (1999).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
P	A	A	A	A

Steppe Eagle *Aquila rapax* to be treated as two species (accepted by the AERC TAC in Sangster et al. 2002a):

- Tawny Eagle *Aquila rapax* (polytypic: *A. r. belisarius*; extralimital: *A. r. vindhiana* and *rapax*)
- Steppe Eagle *Aquila nipalensis* (polytypic: *A. n. nipalensis* and *orientalis*)

Rationale: see Sangster et al. (2002a) and appendix 1.

Imperial Eagle *Aquila heliaca* to be treated as two species (accepted by the AERC TAC in Sangster et al. 2002a):

- Imperial Eagle *Aquila heliaca* (monotypic)
- Spanish Imperial Eagle *Aquila adalberti* (monotypic)

Rationale: see Sangster et al. (2002a) and appendix 1.

Red-footed Falcon *Falco vespertinus* to be treated as two species:

- Red-footed Falcon *Falco vespertinus* (monotypic)
- Amur Falcon *Falco amurensis* (monotypic)

STC This split was not discussed by the AERC TAC as it was already accepted by Voous (1973). Although Voous (1973) recognised *F. amurensis* as a species, he stated ‘sometimes treated as conspecific with *F. vespertinus* (author’s preference)’. Amur Falcon is treated as a separate species in the Swedish Holarctic checklist (SOF 1995). There is a wide consensus on the specific status of Amur Falcon among recent authors, e.g. Cramp et al. (1980), del Hoyo et al. (1994), Ferguson-Lees & Christie (2001). Amur Falcon differs significantly in plumage from Red-footed Falcon, while their breeding ranges are disjunct and their wintering ranges (in southern Africa) largely discrete. General papers on the phylogeny of Falconidae are Seibold et al. (1993) and Griffiths (1999).

ID: Identification of Red-footed Falcon is treated by Forsman (1995, 1999), Amur Falcon by Corso et al. (1998 2000) and Corso & Catley (in prep.). Amur Falcon was recently added to the Western Palearctic list (Corso & Dennis 1998).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
	A	A	A*	A

(*) Accepted as ‘semi- or allospecies’.

Small Buttonquail *Turnix sylvaticus sylvaticus*

Notice the correct spelling (David & Gosselin 2002b).

Allen’s Gallinule *Porphyryula alleni* suggested by Olson (1973) to become *Porphyrio alleni*

Purple Gallinule *Porphyryula martinica* suggested by Olson (1973) to become *Porphyrio martinica*

A.J. Helbig The generic change is based on Olson (1973) and is compatible with a molecular study (Trewick 1997; but note the limited taxon sampling), followed by Banks et al. (2002). Olson (1973), while advocating the lump, nevertheless stated that ‘because the three species of *Porphyryula* are more closely related to each other than to *Porphyrio* a case could be made for maintaining them as a subgenus’. He apparently accepted the validity of *Porphyryula* as a taxon, and an a priori notion of ‘what a genus should be’ is the only reason that led him to merge it with *Porphyrio*. Results in Trewick (1997) do not make this change mandatory (the only change to appear mandatory in their paper is that their ‘*Gallinula martinica*’ should be switched to another genus, but this genus could be *Porphyryula*, as well as *Porphyrio*). Incidentally, *Porphyryula* and *Porphyrio* are also both found to be reciprocally monophyletic by Livezey (1998), though some prefer to disregard this type of study. It seems that in this case, the most widely supported decision is based on a rather arbitrary decision (L. Raty in litt.).

Sibley and Monroe have accepted *Porphyrio martinicus* as a noun in apposition. N. David writes (on zoonomen.net) ‘that *Porphyrio* is masculine. The word martinicensis is definitely adjectival (masculine and feminine; neuter: martinicense) but *martinica* appears a noun in apposition (a place name), as the several African place names combined with *Cisticola*. *Porphyrio martinica* may be correct. The original name *martinica* and several others, e.g. *dominica*, *cajanea*, *guinea*, etc. are problematic. They must be studied together, and I plan to do that in the near future. Note, however, that the suffix -us, -a, -um is adjectival when added to a noun ending with a consonant (e.g., hainanus, -a, -um, from Hainan). Thus, *martinicus* could be viewed as a modified noun, not as an adjective. But I am not yet 100% sure.’ M. Gosselin commented that he believes that *Porphyrio martinica* is the correct name. ‘Where the author of a species-group name did not indicate whether he regarded it as a noun or as an adjective, and where it may be regarded as either and the evidence of usage is not decisive, it is to be treated as a noun in apposition to the name of its genus [ICZN 1999, Art. 31.2.2].’ There is no clear evidence of what Linnaeus intended when he created *Fulica martinica*, but he did use “Martinica” as a country name in his book. So, *martinica* can certainly be a noun [but also an adjective, i.e. “pertaining to Martin”]; the evidence of usage is not decisive [Ridgway quotes just as many *martinica* as *martinicus* in combination with *Porphyrio* and *Ionornis*], therefore it is to be treated as a noun in apposition.’ The AOU has accepted *Porphyrio martinica* (Banks et al. 2002). The AERC TAC accepts this generic change (accepted by at least three TCs and the AOU).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
	A		A	A

Demoiselle Crane *Anthropoides virgo* becomes *Grus virgo*

CAF Two studies using totally independent genetic data (cytochrome *b* sequences of mtDNA by Krajewski & Fetzner 1994, Krajewski & King 1996; DNA-DNA hybridisation of total nuclear DNA by Krajewski 1989, see also Ingold et al. 1989) address the question of phylogenetic relationships among cranes. Both studies fail to fully resolve the relationships among crane species groups, but in both studies the genus *Anthropoides* is included in the clade formed by species of cranes currently classified as *Grus*. In none of the recovered trees *Anthropoides* and *Grus* form reciprocally monophyletic clades. Given that these studies are based on independent data, this conclusion can be considered as very reliable. A classification as currently accepted, with *virgo* in one genus and *grus*, *canadensis*, and *leucogeranus* in another genus, thus does not reflect cranes evolution. The option favoured by the AERC TAC is thus to merge *Anthropoides* with *Grus* (as proposed by Krajewski 1989). The alternative choice to give genus rank to all species groups of cranes (a) would require to determine precisely the relationships of all species, (b) would require to find the genus

name available for every species group and (c) would result in many changes as only *Grus grus* would remain in the genus *Grus*. For the time being, this option is thus rejected.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
	A		A	A

Houbara Bustard *Chlamydotis undulata* to be treated as two species:

- Houbara Bustard *Chlamydotis undulata* (polytypic: *C. u. undulata* and *fuertaventurae*)
- Macqueen's Bustard *Chlamydotis macqueenii* (monotypic)

CSNA Gaucher et al. (1996) suggested splitting these taxa. Sangster (1996b) commented on the reasons supporting the split and Knox et al. (2002) summarised why the BOURC TSC accepted this split. General papers on bustard phylogeny include Pitra et al. (2002) and Broders et al. (2003). This split was accepted unanimously by the AERC TAC.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A	A*	A

(*) Accepted as 'semi- or allospecies'.

Lesser Golden Plover *Pluvialis fulva* to be treated as two species (accepted by the AERC TAC in Sangster et al. 2002a)

- American Golden Plover *Pluvialis dominica* (monotypic)
- Pacific Golden Plover *Pluvialis fulva* (monotypic)

Rationale: Sangster et al. (2002a) and appendix 1.

Sociable Lapwing *Chettusia gregaria* becomes *Vanellus gregarius*

CSNA cf. BOURC (1996) and Sangster et al. (1997).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A	A	A

White-tailed Lapwing *Chettusia leucura* becomes *Vanellus leucurus*

CSNA cf. BOURC (1996) and Sangster et al. (1997)

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A	A	A

Stilt Sandpiper *Micropalama himantopus* suggested by Jehl (1968) to become *Calidris himantopus*.

Andreas Helbig Based on morphological, behavioural and molecular studies, Stilt Sandpiper is better placed in *Calidris* (Jehl 1968 *San Diego Soc. Nat. Hist. Memoir* No. 3; Jehl 1973; Borowik & Mc Lennan 1999). This generic change is supported by at least three TCs and the AOU, hence accepted by the AERC TAC. A.J. Helbig, however, commented: 'Inclusion of *Micropalama* into *Calidris* (as advocated by AOU 1998) is insufficiently supported by molecular data (see low bootstrap values). Plumage of downy young shows similarities to *Calidris*, but does not prove *Micropalama* to be nested within *Calidris*. I therefore retain *Micropalama* until better evidence for relationships within this group becomes available'. The majority of the AERC TAC prefers the alternative not to use *Micropalama* until there is strong evidence that it is NOT in *Calidris*. The genus *Calidris* clearly needs more research. Stilt Sandpiper is provisionally placed after Curlew Sandpiper *C. ferruginea*.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A		R	A

Common Snipe *Gallinago gallinago* suggested to be treated as seven species:

- Common Snipe *Gallinago gallinago* (polytypic: *G. g. gallinago*, *faeroeensis*)
- Wilson's Snipe *Gallinago delicata* (monotypic)
- African Snipe *Gallinago nigripennis* (polytypic; extralimital *G. n. nigripennis*, *angolensis*)
- Madagascar Snipe *Gallinago macrodactyla* (monotypic; extralimital)

- Paraguayan Snipe *Gallinago paraguaiiae* (monotypic; extralimital)
- Magellan Snipe *Gallinago magellanica* (monotypic; extralimital)
- Puna Snipe *Gallinago andina* (monotypic; extralimital)

BOURC TSC Wilson's Snipe is best treated as a separate species based on slight differences in morphology (Miller 1996) and clear differences in vocalisations (Thönen 1969). Genetic differences between *gallinago* and *delicata* are discussed by Zink et al. (1995). They found a single DNA restriction site difference and a very low *p*-value between U.S. and Russian haplotypes ($p = 0.006$) Further sampling is required, however, to discern if there is a discrete mtDNA boundary between these taxa. The 'drumming' made by tail-feathers during display flight is clearly different between *gallinago* and *delicata*. The BOURC TSC is preparing a file on these taxa. Sangster et al. (1998) summarised the reasons for accepting this split: 'Common Snipe and Wilson's Snipe *G. delicata* are specifically distinct (cf. Olsson 1987, Gantlett et al. 1996) based on qualitative differences in morphology, vocalisations and drumming display (Thönen 1969, Cramp & Simmons 1983, Olsson 1987, Carey & Olsson 1995, Miller 1996a, 1996b, Gibson & Kessel 1997). Pending further analysis, *faeroeensis* and *gallinago* are provisionally retained as conspecific (cf. Miller 1996b). African Snipe *G. nigripennis*, Madagascar Snipe *G. macrodactyla*, Paraguayan Snipe *G. paraguaiiae*, Magellan Snipe *G. magellanica* and Puna Snipe *G. andina* are specifically distinct from Common Snipe based on qualitative differences in morphology, vocalisations, and drumming display (Tuck 1972, Sutton 1981, Hayman et al. 1986, Fjeldså & Krabbe 1990, del Hoyo et al. 1996). The AOU recognised Wilson's Snipe as a species in 2002 (Banks et al. 2002).

ID: The following papers or notes on field identification of Wilson's versus Common Snipe have been published in *Birding World*: Bland (1998), Bland (1999) and Leader (1999).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
P	A*	A	A**	A*

(*) Split of *gallinago* and *delicata* accepted, no opinion on the other taxa.

(**) Accepted as 'semi- or allospecies'.

Spotted Sandpiper *Actitis macularius*

Notice the correct spelling (David & Gosselin 2002b).

Red Phalarope *Phalaropus fulicarius*

Notice the correct spelling (David & Gosselin 2000; David & Gosselin 2002a; contra e.g. Parkes 1982, del Hoyo et al. 1996 and Sangster et al. 1997).

Herring Gull *Larus argentatus* proposed to be treated as six species by Yésou (2002):

- Herring Gull *Larus argentatus* (polytypic: *L. a. argenteus*, *argentatus*)
- American Herring Gull *Larus smithsonianus* (monotypic)
- East Siberian Gull *Larus vegae* (polytypic; extralimital: *L. v. vegae* and *mongolicus*)
- Caspian Gull *Larus cachinnans* (monotypic)
- Yellow-legged Gull *Larus michahellis* (polytypic: *L. m. atlantis*, *michahellis*)
- Armenian Gull *Larus armenicus* (monotypic)

CAF Yésou (2002) offers the most complete recent summary on the taxonomy of the *Larus argentatus* - *cachinnans* - *fuscus* complex. This complex has received a lot of attention by dedicated gull watchers and scientists alike; some of the unpublished results have been presented during lectures at the so-called International Gull Meetings (IGM). The AERC TAC follows P. Yésou's treatment of Caspian Gull *L. cachinnans*, Yellow-legged Gull *L. michahellis*, Armenian Gull *L. armenicus*, East Siberian Gull (*L. vegae*; extralimital; polytypic: *L. v. vegae* – incl. 'birulai' – and *L. v. mongolicus*) and American Herring Gull (*L. smithsonianus*). (Texts on *Larus argentatus* complex by P.-A. Crochet and P. Yésou.)

Herring Gull *Larus argentatus*

There is a lot of geographical variation within *Larus argentatus* s.s. (*L. a. argenteus*, *argentatus*). We have a valid name for one end of the variation (*argenteus*), but the variation within the rest of the Herring Gulls s.s. is at least as wide: *argentatus* from e.g. N Norway are more different from the Baltic birds than the latter are from *argenteus*. There are very few analyses covering the whole *argentatus* range. Nothing much has been added since Berth (1968). Such a variation leaves room for speculation, and as there is one poorly defined

name ('*omissus*'), there is a temptation to use it in different ways by different authors. Until more research has been done, '*omissus*' should better be considered as a synonym of *argentatus*.

American Herring Gull *L. smithsonianus*

American Herring Gull *smithsonianus* is clearly distinct in mtDNA from *argentatus* / *argenteus* (Crochet et al. 2002) and more closely related to *vegae* (A. Helbig in litt.). It is also distinct in all plumages, with differences being more pronounced in juvenile and immature plumages (e.g. Adriaens & Mactavish in press; Lonergan & Mullarney in press) and in vocalisations (Frings et al. 1958). The similarity of adult plumage between *smithsonianus* and *argentatus* / *argenteus* is probably a result of convergence and does not reflect true relationships. For identification of American Herring Gull, see e.g. Sibley (2000) and Jonsson & Mactavish (2001).

The geographical variation within *smithsonianus* still needs to be thoroughly described, but appears in some ways similar to the variation (*argentatus* / *argenteus*) in Europe (Jonsson & Mactavish 2001; Adriaens & Mactavish in press).

ID: Two important papers on the identification of American Herring Gull are submitted (Adriaens & Mactavish in press; Lonergan & Mullarney in press).

Is *Larus smithsonianus* a distinct species?

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
P	A	A	A	P*

(*) The STC believes that *smithsonianus* is separate from *argentatus* / *argenteus*, but is not clear about its relation to *vegae* and/or *mongolicus*, hence its hesitation to split further.

Caspian Gull *Larus cachinnans*

Yellow-legged Gull *Larus michahellis*

Armenian Gull *Larus armenicus*

See Liebers et al. (2001) and Yésou (2002) for reasons to treat these three taxa as non-conspecific. Reasons to treat *michahellis* as distinct from *argentatus* are summarised in Crochet et al. (2002) and Yésou (2002).

The key point in the context of the AERC TAC is whether the southeastern end of *argentatus* variation has something to do with *cachinnans* or not. Some authors believe in clinal variation between *argentatus* and *cachinnans* (e.g. Voipio 1954, Panov & Monzиков 1999), whereas others do not (e.g. Stegmann 1934). The latter opinion is part of some kind of consensus among western European gull-watchers and taxonomists.

Unpublished genetic results (Pons & Crochet, Liebers, Helbig, et al.) indicate that *argentatus* and *cachinnans* are not more closely related than other large gull species. On the basis of phylogenetic relationships, they should thus be treated as different species. The current uncertainties originate from reports of hybridisation between *argentatus* and *cachinnans* in E Europe. It is certain that hybridisation occurs quite extensively at least in some colonies, but the extent of intergradation remains unknown. The current situation (in Poland at least) corresponds to a recent secondary contact with mainly pure phenotypes of *argentatus* and *cachinnans* and a significant proportion of hybrids (Neubauer, Zagalska, Gay et al. in prep). Studies of the amount of pre- and post-zygotic isolation and the genetic consequences of hybridisation (in term of intergradation) are under way.

Panov & Monzиков (1999) claim that the pattern of variation from *argentatus* to *cachinnans* corresponds to a broad zone of intergradation. There is however little support in their data for this interpretation. Results based on morphometry and colouration are based on populations, not individuals: there is thus no way to interpret their results as intergradation (homogeneous populations of intermediate specimens) or sympatric occurrence of various proportion of *argentatus* and *cachinnans*. Results based on DNA (RAPD markers) are difficult to understand and, as for morphology, mix all individual within populations: they are thus unable to demonstrate intergradation. More convincing evidence of the existence of hybrid individuals comes from the study of vocalisations: a significant proportion of specimens from Rybinsk Reservoir are really intermediate in vocalisations between *argentatus* (Gulf of Finland, Barents, Sea, White Sea) and *cachinnans* (S Caspian Sea). Their data on vocalisations, however, clearly show two clusters of points (*cachinnans* and *argentatus*) with no overlap and a small number of intermediate birds. Their findings are thus in agreement with the observations on the Polish colonies that hybrids between *argentatus* and *cachinnans* occur frequently but that variation is not continuous. There is thus no indication of extensive intergradation between both taxa, and based on amount of divergence in behaviour, vocalisations, morphology, and on their phylogenetic relationships, *argentatus* and *cachinnans* are best treated as valid species.

Is *Larus cachinnans* a distinct species?

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
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P	A	A	A	A
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Atlantic Yellow-legged Gull *L. m. atlantis*

‘Cantábrican’ (or Galician) Yellow-legged Gull *L. m. ssp. (lusitanius?)*

Yésou (2002) postulated that ‘it is wise to restrict the name of *atlantis* to the birds breeding in the Azores, as Dwight originally did, pending the results of further research on the phenotypic and genetic variations within *michahellis*.’ Dubois (2001b) stated that ‘the form *atlantis* was first described from the Azores (Dwight 1922),...’ Thereby, both misquoted Dwight (1922): in the original description of *atlantis*, he included two specimens from the Canaries.

There seems to be some variation among gulls breeding in the Azores and the Canary Islands and Madeira (Dubois 2001). Pending further studies, it is best to keep all these populations in *atlantis*. The range of *atlantis* is sometimes extended to the Iberian Atlantic coasts (de Knijff et al. 2001, Liebers et al. 2001). Although birds from Atlantic Iberia are distinct from Mediterranean birds (Pons et al., submitted), they have not been compared to birds from the Atlantic Islands yet. It is possible that birds from the N Atlantic Iberian coasts constitute another subspecies. In that case, this subspecies should probably be named *lusitanius* Joriris, 1978. This name is based on a very poor description of birds seen in the harbour of Peniche (close to the Berlengas Islands). As the name is available, however, it may be recommended to designate a neotype collected on the Berlengas Islands (A. Dubois pers. comm.). If birds from the Berlengas belong to *atlantis* or to *michahellis*, *lusitanius* would become a synonym of one of these names. If not, the name *lusitanius* would be the valid name of the Atlantic Iberian subspecies.

Is *L. michahellis* (incl. *L. m. michahellis* and *atlantis*) a distinct species?

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
	A	A	A*	A

(*) Accepted as ‘semi- or allospecies’.

Armenian Gull *Larus armenicus*

See Liebers & Helbig (1999), Liebers et al. (2001) and Yésou (2002) for a review of the reasons to elevate Armenian Gull to species rank.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
	A	A	A*	A

(*) Accepted as ‘semi- or allospecies’.

When combining all votes on the six-fold split (see 2.13), the following result is obtained:

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
P	A	A	A	A

Gull-billed Tern *Gelochelidon nilotica* becomes *Sterna nilotica*

STC cf. BOURC (1996). See Sangster et al. (1999) for reasons for maintaining *Gelochelidon*. The majority of the AERC TAC believes, however, that it would not be logical to maintain *Gelochelidon* for *Sterna nilotica* while species as distinct as Little Tern *S. albifrons* and Caspian Tern *S. caspia* are all kept in *Sterna* (*Sterna* would then probably become paraphyletic). If *Gelochelidon* is recognised at genus level, it would then seem logical to elevate other subgenera to genus rank as well (e.g. *Thalasseus*, *Sternula*). Note, however, that keeping a distinct *Chlidonias* while merging all other terns in *Sterna* might not be a valid option either. A revision of all Sternini is urgently required. This generic change is accepted by four TCs and the AOU, and is therefore accepted by the AERC TAC.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	R	A	A

Whiskered Tern *Chlidonias hybrida hybrida*

Notice the correct spelling (David & Gosselin 2002a,b).

Marbled Murrelet *Brachyramphus marmoratus* to be treated as two species:

- Marbled Murrelet *Brachyramphus marmoratus* (monotypic; extralimital)
- Long-billed Murrelet *Brachyramphus perdix* (monotypic; one record in the Western Palearctic)

CSNA Molecular data from Zink et al. (1995) show that the degree of differentiation between *B. perdix* and *B. marmoratus* is comparable to that between well-differentiated species. Friesen et al. (1996) provided evidence from cytochrome *b* sequences and allozymes to recognise *B. perdix* as a distinct species, a conclusion fully supported by sequences of nuclear genes showing long-term reproductive isolation of both taxa (Friesen et al. 1997). Marbled and Long-billed Murrelets also differ in plumage and in size. Long-billed Murrelet occurs through the Sea of Okhotsk and the Kamchatka Peninsula. The split was accepted by the American Ornithologists' Union in 1998 (AOU 1998). Long-billed Murrelet has been recorded once in the Western Palearctic: a first-winter was found dead in a fishing net in Lake Zurich, Switzerland, between 15th and 18th December 1997 (Maumary & Knaus 2000). Its occurrence in North America is discussed by Sealy et al. (1982).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
Ø	A	A	A*	A

(*) Accepted as 'semi- or allospecies'.

Parakeet Auklet *Cyclorrhynchus psittacula* becomes *Aethia psittacula* (cf. Strauch 1985)

STC This treatment has been suggested on the base of morphological and ecological characters by Strauch (1985), and based on mtDNA sequences by Moum et al. (1994). Molecular evidence presented by Friesen et al. (1996) shows that Parakeet Auklet should be included in the genus *Aethia* (but the authors did not make this suggestion themselves).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
Ø	A		A	A

Pin-tailed Sandgrouse *Pterocles alchata caudacutus*

Notice the correct spelling. *Alchata* is a Latin transliteration of the Arabic name of the sandgrouse (cf. Jobling 1991), and is thus invariable. Del Hoyo (1997) has it right: *P. alchata caudacutus* (M. Gosselin in litt.).

Brown Fish Owl *Ketupa zeylonensis* becomes *Bubo zeylonensis* (Wink & Heidrich 1999)

Snowy Owl *Nyctea scandiaca* becomes *Bubo scandiacus* (Wink & Heidrich 1999)

A.J. Helbig Both Brown Fish Owl and Snowy Owl are derived within *Bubo* (Wink & Heidrich 1999; Wink & Heidrich 2000), thus subsumed in that genus. Furthermore, osteology does not support the separation of *Nyctea* from *Bubo* (Ford 1967). Some differences between *Nyctea* and *Bubo* are believed to be adaptations to the Arctic environment. The generic change of Snowy Owl is also supported by the AOU (Banks et al. *Auk* 120 (3): 922-931, 2003). Chewing lice (Phthiraptera) of the species *Strigiphilus ketupae* occur in Brown Fish Owl, Philippine Eagle-Owl *B. philippensis* and Barred Eagle-Owl *B. sumatranus*; furthermore, Brown Fish Owl and Barred Eagle-Owl are both hosts to *Colpocephalum turbinatum*, possibly supporting a close relationship between these owls (Dalglish 2003). The bare legs of fish owls are conspicuous, and this feature is usually correlated with their fish-catching habits; but certainly other owls that are not known to catch fish present much the same character.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A		A	P

Chimney Swift *Chaetura pelagica* treat as monotypic (BOURC 1991)

The only reason to include this species is that is not included in BWP IV, so a starting point is needed for the subspecific treatment. This is not a problem, as the species is universally regarded as monotypic (including the AOU).

Pied Kingfisher *Ceryle rudis syriacus*

Notice the correct spelling (David & Gosselin 2002b). Re. *syriacus*, Kasperek (1996) placed a good rationale not to accept this taxon. G. Kirwan will be considering *syriacus* as a synonym in his forthcoming book on Turkish birds.

Blue-cheeked Bee-eater *Merops superciliosus* to be treated as three species:

- Blue-cheeked Bee-eater *Merops persicus* (polytypic: *M. p. chrysocercus*, *persicus*; range: N Africa and Middle East to NW India)

- Olive Bee-eater *Merops superciliosus* (polytypic; extralimital: *M. s. superciliosus* and *alternans* – according to Fry in del Hoyo et al. 2001; different treatments have been proposed; range: southern Africa, E Africa, Madagascar and Comoro Islands)
- Blue-tailed Bee-eater *Merops philippinus* (polytypic; extralimital: *M. ph. philippinus*, *celebensis* and *salvadorii*; range: N Pakistan, SW Asia to New Guinea and New Britain)

CSNA Considered as distinct species, based on well-established plumage characters by e.g. Fry in Snow (1978), Glutz von Blotzheim & Bauer (1980), Fry (1984), van den Berg (1987a), CINFO (1993), Clements (4th and 5th edition), Sibley & Monroe (1993, 1996) and Sangster et al. (1997). Although Blue-cheeked Bee-eater (*M. persicus*; polytypic: *M. p. chrysocercus*, *persicus*) is widely accepted by recent authors, Olive (or Madagascar) Bee-eater *M. superciliosus* has been treated in various ways. Fry et al. (1992) combined Madagascar Bee-eater and Blue-tailed Bee-eater in a single species *M. superciliosus* (polytypic; extralimital; *M. s. philippinus*, *superciliosus* and *alternans*) and considered that Blue-tailed and Blue-cheeked Bee-eaters are distinct species, because they do not hybridise where they meet in breeding grounds in NW India. Voous (1977c), the starting point for the AERC TAC, did not mention *M. philippinus* in his Holarctic list and included *persicus* in *M. superciliosus*. Fry in del Hoyo et al. (2001), however, recognised a polytypic Olive Bee-eater *M. superciliosus* (no longer named Madagascar Bee-eater, because it is also breeding elsewhere; *M. s. superciliosus* and *alternans*) and a monotypic Blue-tailed Bee-eater (*M. philippinus*). This split is recognised by all TCs.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A*	A**	A

(*) Formally accepted by CSNA based on qualitative morphological differences by Sangster et al. (1997), p 24.

(**) Accepted as *Merops [superciliosus] persicus* under the superspecies concept.

Green Woodpecker *Picus viridis* complex to be treated as two species:

- European Green Woodpecker *Picus viridis* (polytypic: *P. v. viridis*, *karelini*, *sharpei*; extralimital: *P. v. innominatus*)
- Levaillant's Green Woodpecker *Picus vaillantii* (monotypic)

CAF The split of *viridis* and *vaillantii* was not discussed as it was already accepted by Voous (1973). Levaillant's Green Woodpecker is already treated as a separate species in the Swedish Holarctic checklist (SOF 1995) as well.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A	A	A

Remark: Recently (31 July 2003), a threefold split was proposed to the AERC TAC. The AERC TAC has, however, not yet examined this option: 'Iberian Green Woodpecker *P. sharpei*, is best treated as a separate species based on differences in plumage, morphology and vocalisation with limited intergradation (Winkler et al. 1995). A study of museum specimens in the contact area between *sharpei* and *viridis* in S France (Beuzart 1997) analysed 13 plumage characters in 21 male specimens from Spain, S France and C/N France. Specimens from Hérault department (n=2) do not differ from pure *viridis* from N and C France, whereas specimens from Aude department (next to the South) (n = 3) are not separable from pure *sharpei* from Barcelona area. Two specimens from E Pyrenees are outside the variation of *sharpei* and show characters of *viridis*: they are certainly hybrids or intergrades. This pattern of variation (more pure phenotypes than intergrades, abrupt passage from pure *sharpei* to pure *viridis* over a narrow zone without geographical barrier) is typical of (incomplete) reproductive isolation and indicates that the best treatment for *sharpei* is to split it from *viridis*.' Sound recordings of *vaillantii*, *sharpei* and *viridis* can all be found on Schulze (2003) (P.-A. Crochet).

Part two: Passeriformes

[Chestnut-headed Sparrow-Lark *Eremopterix signatus harrisoni*]

Notice the correct spelling (David & Gosselin 2002b). This species was considered as most likely erroneously recorded in the Western Palearctic by Beaman & Madge (1998), p 849. The single observer record in Israel is, however, still accepted by the IRDC (see Shirihai 1999; G. Kirwan in litt.).

Bar-tailed Lark *Ammomanes cinctura*

Notice the correct spelling (David & Gosselin 2002b).

Common House Martin *Delichon urbicum urbicum*

Notice the correct spelling (David & Gosselin 2002b).

Richard's Pipit *Anthus novaeseelandiae* to be treated as four species:

- **Richard's Pipit** *Anthus richardi* (monotypic – according to Alström & Mild 2003)
- **Grassland Pipit** *Anthus cinnamomeus* (polytypic; extralimital: *A. c. cameroonensis*, *lynesi*, *stabilis*, *cinnamomeus*, *annae*, *eximius*, *lacuum*, *spurium*, *itombwensis*, *lichenya* incl. 'katangae', *rufuloides*, *bocagei* and *grotei* – according to Clancey (1986); many different treatments have been proposed)
- **Paddyfield Pipit** *Anthus rufulus* (polytypic; extralimital: *A. r. rufulus*, *malayensis* – according to Alström & Mild 2003; see also Mayr & Greenway 1960)
- **Australian/New Zealand Pipit** *Anthus novaeseelandiae* (polytypic; extralimital: *australis* group: *A. n. exiguus*, *rogersi*, *subaustralis*, *bilbali*, *australis*, *bistriatus*; *novaeseelandiae* group: *A. n. reischeki*, *novaeseelandiae*, *chathamensis*, *aucklandicus*, *steindachneri*)

STC At least the above four taxa are specifically distinct, based on qualitative differences in plumage and vocalisations (cf. Devillers 1980, p 138, Glutz von Blotzheim & Bauer 1985 and references cited therein, Sangster et al. 1997, Schodde & Mason 1999). A number of taxa have been proposed to be split further, e.g. Grassland Pipit *A. cinnamomeus* and Cameroon Pipit *A. cameroonensis* (Clancey 1978), including four montane forms, one of which, Mountain Pipit *A. hoeschi* was later shown to be a distinct species. Initially, Jackson's Pipit *A. latistriatus* was also believed by some authors to be part of the Grassland Pipit complex, but was later accepted as a distinct species (Prigogine 1981, Clancey 1984). Clancey (1986) recognised no less than 13 subspecies of *A. cinnamomeus*; the taxonomic relationships within Grassland Pipit need more research. More research is also needed in Paddyfield Pipit *A. rufulus* (particularly the island populations) and in the Australasian taxa (*australis* / *novaeseelandiae*). Richard's Pipit *A. richardi* was considered as a distinct species by Dement'ev & Gladkov (1954), Stresemann (1959), Stresemann & Stresemann (1968), Kozlova (1975), Devillers (1980) and most subsequent authors. Voelker's phylogeny (1999) based on mtDNA sequence data indicated three independent origins for this complex. Alström & Mild (2003), however, believe that this complex is monophyletic, and that it consists of at least four separate lineages (the *cinnamomeus* group, the *richardi* group, the *rufulus* group and the *australis* / *novaeseelandiae* group) which they provisionally treat as four separate species (a treatment first proposed by Kozlova 1975). Alström and Mild (2003) describe geographic variation within *A. richardi*, which is, however, in their opinion insufficient to recognise several subspecies.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
P	A	A	A	A

Water Pipit *Anthus spinoletta* to be treated as three species (accepted by the AERC TAC in Sangster et al. 2002a)

- **Water Pipit** *Anthus spinoletta* (polytypic: *A. s. spinoletta*, *coutellii*; extralimital: *A. s. blakistoni*)
- **Rock Pipit** *Anthus petrosus* (polytypic: *A. p. petrosus* incl. 'meinertzhageni' and 'kleinschmidti' and *A. p. littoralis*)
- **Buff-bellied Pipit** *Anthus rubescens* (polytypic: *A. r. rubescens* and *japonicus*; extralimital: *A. r. geophilus*, *alticola*, *pacificus*)

Rationale: see Sangster et al. (2002a) and appendix 1.

Güldenstädt's Redstart *Phoenicurus erythrogastrus*

Notice the correct spelling (David & Gosselin 2002a).

Eversmann's Redstart *Phoenicurus erythronotus*
Notice the correct spelling (David & Gosselin 2002b).

Common Stonechat *Saxicola torquatus*

Notice the correct spelling of *torquatus*, *variegatus*, *armenicus* and *maurus* (David & Gosselin 2002b).

Black-eared Wheatear *Oenanthe hispanica* complex to be treated as three species:

- Black-eared Wheatear *Oenanthe hispanica* (polytypic: *Oe. h. hispanica* and *melanoleuca*)
- Pied Wheatear *Oenanthe pleschanka* (monotypic)
- Cyprus Wheatear *Oenanthe cypriaca* (monotypic)

Black-eared Wheatear *Oenanthe hispanica*

The STC pointed out that the forms *Oe. h. melanoleuca* and *Oe. h. hispanica* seem to be connected by a wide zone of intermediates in e.g. S Bulgaria and the N Balkans. The CSNA commented that the intergradation zone of *Oe. hispanica* and *Oe. pleschanka* is 1000 km wide (Haffer 1977) and there is no evidence of reproductive isolation (Panov et al. 1994). The existence of a wide zone of intermediates should not be cited as the basis for lumping *melanoleuca*, but ignored in the case of *Oe. hispanica* and *Oe. pleschanka* (G. Sangster in litt.). L. Svensson agrees, and specifies that the intergradation zones between *Oe. pleschanka* and *Oe. hispanica* have been extensively studied, whereas that of *Oe. h. hispanica* and *Oe. h. melanoleuca* not. Hence, he advocates pending due to this.

Pied Wheatear *Oenanthe pleschanka*

Pied Wheatear is already treated as separate species in the Swedish Holarctic checklist (SOF 1995). The BOURC commented on the taxonomic status of Pied and Black-eared Wheatears in its 13th report (BOURC 1988): Haffer (1977) presented detailed information on the hybridisation of these two species in Iran. Recent work by Panov (1986, 1999) on the W coast of the Caspian Sea has emphasised the extent to which these taxa interbreed and the variety of plumages found in the resulting hybrids. However, *Oe. pleschanka* and *Oe. hispanica* are largely parapatric with limited areas of contact.' G. Sangster added 'but where they meet, they show introgressive hybridisation without evidence for reproductive isolation.' Pied and Black-eared Wheatears are poorly differentiated acoustically; in playback experiments, they react to each other's songs. G. Sangster then asked: 'Why still consider them as species if this is the case?' whereupon L. Svensson replied: 'I agree that same or extremely similar song should make us very cautious before we split. However, taxonomy (or speciation) is complex, and in a few cases the song does not seem to be as important for upholding largely distinct species, as witnessed by Yellowhammer *Emberiza citrinella* and Pine Bunting *E. leucocephalos*, Red-headed *E. bruniceps* and Black-headed Buntings *E. melanocephala*, Willow *Parus montanus* and Songar Tits *P. songarus*, and others. Habitat choice, altitude, breeding season, mating behaviour etc. can sometimes be enough, while selection has apparently not (yet) developed distinct songs.'

Further reading:

Panov, N. & Ivanitzky, V.V. (1975) Evolutionary and taxonomic relations between *Oenanthe hispanica* and *Oe. pleschanka*. *Zool. Zhurn.* 54: 1860-1873.

Cyprus Wheatear *Oenanthe cypriaca*

Christensen (1974) and Sluys & van den Berg (1982), who examined plumages, behaviour, food, moult and biometry, split it from Pied Wheatear on these criteria, a view followed by Svensson (1992). Cyprus Wheatear is treated as separate species in the Swedish Holarctic checklist (SOF 1995). Cramp et al. (1988) and Keith et al. (1992), however, treated it as a subspecies, with the latter stating that the differences in biometrics, plumage and song between Pied Wheatear and Cyprus Wheatear are no greater than those sometimes shown by races of other species. The songs of Cyprus Wheatear and Pied Wheatear, however, are very different. Cyprus Wheatear has a very distinct cicada- or grasshopper-like, monotonous buzzing song, whereas the song of Pied Wheatear is a more varied, partly musical, more often dry twittering, often with mimicry interwoven, although it may rarely begin with a *cypriaca*-like 'bizz... bizz...' (Cramp 1988, Svensson et al. 1999).

ID: For identification of wheatears in general, see Clement & Harris (1987a, b). Following useful identification papers on the *Oenanthe hispanica* complex, listed by topic, were published in *Dutch Birding*: (1) *Oenanthe h. hispanica* and *Oe. h. melanoleuca* (Ullman 2003); (2) *Oenanthe pleschanka* (Small 1994; Ullman 1994) and (3) *Oenanthe cypriaca* (Small 1994; Flint 1995 *Brit. Birds* 88: 230-241).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A*	A**	A

(*) Both the CSNA and (**) A.J. Helbig split *Oenanthe hispanica* further into *melanoleuca* and *hispanica* (regarded as ‘allospecies’ by Helbig, however); Helbig (in litt.) includes the taxa *hispanica*, *melanoleuca*, *pleschanka* and *cypriaca* in the ‘superspecies’ *Oenanthe [hispanica]*.

Red-tailed Wheatear *Oenanthe xanthopyrmyna* to be treated as two species:

- Kurdish Wheatear *Oenanthe xanthopyrmyna* (monotypic)
- Red-tailed Wheatear *Oenanthe chrysopygia* (monotypic)

A.J. Helbig According to the STC, Red-tailed Wheatear is best treated as a separate species based on marked differences in morphology, incl. absence of sexual dimorphism. Kurdish Wheatear was found breeding in the 1980s in SE Turkey (Helbig 1984, Kumerlovee et al. 1984, Kasperek 1986, and Roselaar 1995); the distinctive male has a dark chestnut rump, white sides to the tail-base and a dark throat, whereas Red-tailed Wheatear from Transcaucasia lacks white on tail and dark throat. Some intergradation is said to occur and measurements and structure are similar, hence Vaurie (1949) *Amer. Mus. Novitat.* 1425 and C.S. Roselaar in Cramp et al. (1988) included these taxa in a single polytypic species. Dubois (2000) observed an ‘intermediate’ male on 5 March 2000 in the Hilleh area, Iran. This individual had the black throat of *Oe. xanthopyrmyna*, but the ‘red-cornered’ tail of *chrysopygia*. Such birds are sometimes separated as ‘*cummingsi*’ (Withaker 1899) and are stated to occur in the overlap zone, but a rufous tail may be normal for some pure *Oe. xanthopyrmyna* (Roselaar in Cramp et al. 1988), so the tail observed by Dubois has limited weight as an argument (L. Svensson pers. comm.). Although they are said to interbreed, the supposed characters of the hybrid are also found in some first year birds of *Oe. xanthopyrmyna* (Bates 1935, Helbig 1984, Roselaar 1995). Ivanov (1941) already advocated splitting the species. See also Stepanyan (1971). Panov (1999) considered them to form a superspecies. L. Svensson commented: ‘In my opinion, based on the examination of fairly long series of skins, and of seeing a few of both taxa in the field, these two are much more distinct than the various forms of *Oe. lugens*, and I maintain that such distinct taxa with so few positively known intermediates should easily qualify as separate species. Due to the fact that some *xanthopyrmyna*, notably females and some immature males, have reddish tail base, it is quite possible that the incidence of hybrids or intermediates has been exaggerated in the past. I think one needs now to take a critical new look at all claimed intergrades.’ As to the English names, alternatively, ‘Persian Wheatear’ could be used for *chrysopygia* (more in line with ‘Kurdish Wheatear’) (L. Svensson pers. comm.).

ID: Clement & Harris (1987a,b)

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
∅	A	P	A*	A*

(*) Accepted as ‘allospecies’.

Grey-cheeked Thrush *Catharus minimus* to be split in two species:

- Grey-cheeked Thrush *Catharus minimus* (polytypic: *C. m. minimus*, *aliciae*)
- Bicknell’s Thrush *Catharus bicknelli* (monotypic; extralimital)

CAF cf. BOURC (1996). Grey-cheeked Thrush is (erroneously) considered monotypic by Dubois et al. (2000), an official reference for the CAF.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A		A*	A

(*) Accepted as ‘semi- or allospecies’.

Zitting Cisticola *Cisticola juncidis neuroticus*

Notice the correct spelling (David & Gosselin 2002b).

Paddyfield Warbler *Acrocephalus agricola* to be treated as two species:

- Paddyfield Warbler *Acrocephalus agricola* (polytypic: *A. a. septimus*, *capistratus*; extralimital *A. a. agricola*)
- Manchurian Reed Warbler *Acrocephalus tangorum* (monotypic; extralimital)

STC Manchurian Reed Warbler *A. tangorum* is specifically distinct based on divergence in mtDNA (Leisler et al. 1997, Helbig & Seibold 1999) and qualitative differences in plumage (described by Alström et al. 1991, Lekagul & Round 1991 and Round 1994, but still treated as a subspecies of Paddyfield Warbler *A. agricola* in these publications). In fact DNA phylogeny not only shows that it is a good species, but that its closest relative is Blunt-winged Warbler *A. concinens*, not Paddyfield Warbler *A. agricola* as previously thought (Leisler et al. 1997). The genetic distance between *A. tangorum* and *A. agricola* (cytochrome *b*) is 7.7–7.9% (compare this to 7–8% between *A. palustris* and the *scirpaceus* group but only 1.6–2.5% between *fuscus* and *scirpaceus*) (Helbig & Seibold 1999). Note that Alström et al. (1991) did not actually propose a split, although all three authors are now in favour of this (Alström in litt.; Round 2000).

Sangster et al. (1997) accepted the split of *tangorum*, but did not consider *septimus* and *capistratus* as valid races of *A. agricola*. *A. a. agricola* from Kazakhstan and *septimus* from Crimea, Ukraine, are phenotypically highly similar, but their mtDNA sequences are divergent by as much as 4.5%; *septimus* might therefore be considered as a cryptic species but this needs independent confirmation (Leisler et al. 1997; Sangster 1997b). Pending further research, Paddyfield Warbler is therefore still considered a polytypic species here. Lars Svensson commented: ‘I have not been able to recognise more than one subspecies of *agricola* on morphology. Thus, I apparently agree with Sangster (1997).’

ID: Alström et al. (1991), Lekagul & Round (1991), Round (1994).

Should *A. agricola* be treated as monotypic?

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
		A		A

Does your TC accept the split of *A. agricola* and *A. tangorum*?

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
	A	A	A*	A

(*) Accepted as ‘semi- or allospecies’.

Great Reed Warbler *Acrocephalus arundinaceus* to be treated as three species:

- Basra Reed Warbler *Acrocephalus griseldis* (monotypic)
- Great Reed Warbler *Acrocephalus arundinaceus* (polytypic: *A. a. arundinaceus* and *zarudnyi*)
- Oriental Reed Warbler *Acrocephalus orientalis* (monotypic)

A.J. Helbig Basra Reed Warbler is already considered a separate species in the Swedish Holarctic checklist (SOF 1995). Pearson & Backhurst (1988) first clarified the characters and taxonomic position of Basra Reed Warbler. Leisler et al. (1997) discuss molecular data confirming the species status of *A. griseldis* and its basal position among the large reed warblers. The species Great Reed Warbler was suggested by Helbig & Seibold (1999) not to be monophyletic (see also Helbig 2000). *A. a. arundinaceus* and the poorly differentiated *A. a. zarudnyi* are more closely related to the different forms of Clamorous Reed Warbler *A. stentoreus* than to Basra Reed Warbler *A. griseldis*. Oriental Reed Warbler is best treated as a separate species based on differences in morphology, including juvenile plumage. Leisler et al. (1997) showed that it is genetically more closely related to Clamorous Reed Warbler *A. stentoreus* than to Great Reed Warbler *A. arundinaceus*. It is treated as a full species by e.g. Sibley & Monroe (1990), Howard & Moore (1991). For additional reading on the taxonomy of the complex of large reed warblers, see Salomonsen (1929), Stresemann & Arnold (1949), Cramp (1992) and Shirohai (1995). Ezaki (1984), Svensson (1992) and King (1996) described the moult of Oriental Reed Warbler.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A	A*	A

(*) Accepted as ‘semi- or allospecies’.

Olivaceous Warbler *Hippolais pallida* to be treated as two species:

- Eastern Olivaceous Warbler *Hippolais pallida* (polytypic: *H. p. reiseri*, *pallida*, *laeneni* and *elaeica*)
- Western Olivaceous Warbler *Hippolais opaca* (monotypic)

STC Olivaceous Warbler is best treated as two separate species based on clear differences in morphology, vocalisation, behaviour (Svensson 2001a) and genetic data (Helbig & Seibold 1999, Ottosson et al., in prep.). In spite of parapatric ranges in NW Africa, intermediates are unknown (Roselaar, in Cramp et al. 1992,

Svensson 2001). All taxa of the Eastern Olivaceous Warbler have a cyclic, scratchy-voiced song, and they all dip their tail down frequently. The Western Olivaceous Warbler has a more pleasing and varied song, like a slower version of *H. polyglotta*, and it does not dip its tail downwards. Bill-shape and some other biometric differences make it possible to distinguish all individuals in the hand. Recent DNA data (Ottosson et al., in prep.) show that all four subspecies of *pallida* have very similar genetic composition, whereas *opaca* differs markedly. – Sangster (1997b) proposed to include the small *Hippolais* warblers in the genus *Acrocephalus*. Helbig (2001) contradicted this as the classification based on cytochrome *b* sequences in Leisler et al. (1997) and Helbig & Seibold (1999) is only supported by poor bootstrapping (58%). This implies a possible error margin of over 40%. It is therefore not clear whether the small *Hippolais* are more closely related to *Acrocephalus* than to *Hippolais* but this may well be the case. [L. Svensson: ‘It seems unwise to move them to *Acrocephalus*, since this is not a perfect arrangement either. You gain some things but lose others. Square tail with whitish sides, a broad bill base, these are traits, which unite the *Hippolais*. For stability, the present order is better kept. Michael Walters (pers. comm.) has pointed at the egg patterns, which are similar for all the present *Hippolais*, but apparently slightly different for the *Acrocephalus*.’] (L. Svensson)

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A	A*	A

(*) Accepted as ‘semi- or allospecies’.

Booted Warbler *Hippolais caligata* to be treated as two species:

- Booted Warbler *Hippolais caligata* (monotypic)
- Sykes’s Warbler *Hippolais rama* (monotypic)

STC Sykes’s Warbler is best treated as a separate species based on differences in morphology, song, ecology, genetic data and partly sympatric breeding ranges (Svensson 2001a). Although both species are morphologically at times extremely similar (however separable in the hand to 99%), they differ in song and can invariably be separated on that. They are true cryptic species which are best told on vocalisation and habitat choice, *caligata* breeding in low scrub on steppe or steppe-like habitats, *rama* in sand or clay deserts with dense *Saxaul* or *Tamarix* vegetation. They have recently (Svensson 2001a) been found to apparently breed sympatrically in a part of the range (S Kazakhstan). Also in this year (May 2003) were *rama* found to sing north of Lake Balkhash, and *caligata* south of it. – The CSNA split this species in 1998 based on PSC (Sangster et al. 1998), and BOURC did so in 2002 (Knox et al. 2002). This split was accepted unanimously by the AERC TAC. Previously, Sykes’s Warbler was regarded as a separate species, mainly based on morphology, by Sykes (1832), Hartert (1910), Stepanyan (1978 & 1983), Haffer in Glutz & Bauer (1991), Sibley & Monroe (1993), and Clements (2000). (Lars Svensson)

ID: Svensson (2001a); Small (2002) commented on a difference in bill shape between *rama* and *caligata*.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A	A*	A

(*) Accepted as ‘semi- or allospecies’.

Marmora’s Warbler *Sylvia sarda*

suggested by Shirihai et al. (2001) to be treated as two species:

- Marmora’s Warbler *Sylvia sarda* (monotypic)
- Balearic Warbler *Sylvia balearica* (monotypic)

CAF *Sylvia sarda* and *S. balearica* have distinct songs and calls. Play back experiments suggest that *balearica* does not react to song of *sarda*. These two taxa are genetically distinct, and the level of genetic divergence is compatible with species status. This level of divergence in spite of the geographical proximity of *balearica* and *sarda* and the migratory behaviour of *sarda* is a further support for the existence of intrinsic mechanisms of reproductive isolation. Morphology is also diagnostically distinct (diagnosable taxa). The AERC TAC thus recommends treating *balearica* as a valid species. Unfortunately, there is no original data to assess sample sizes of genetic analyses or bioacoustical studies (unpublished information by G. Gargallo; see Shirihai et al. 2001 for details). Legrand & De Smet (2002) published additional sonograms of contact calls of *sarda*, *balearica* and *undata* as well as a summary of the occurrence as a vagrant of *sarda* in AERC countries. More accurate information on the Italian breeding and wintering ranges can be found in *British Birds* 95: 198–199 (N. Baccetti & G. Fracasso in litt.). (P.-A. Crochet)

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
P	A	A	A*	A

(*) Accepted as ‘semi- or allospecies’.

Desert Warbler *Sylvia nana* suggested to be treated as two species by Shirihai et al. (2001):

- Asian Desert Warbler *Sylvia nana* (monotypic)
- African Desert Warbler *Sylvia deserti* (monotypic)

BOURC TSC African Desert Warbler is best treated as a separate species based on differences in morphology and song; there are, however, no published genetic data (Shirihai et al. 2001). BWP VI recognises the extralimital *S. n. theresae*; this poorly differentiated taxon, however, is best treated as a synonym of *S. nana* (Shirihai et al. 2001). For recordings of the song of *nana*, readers are referred to Schubert (1982. *Stimmen der Vögel Zentralasiens*); recordings of *deserti* were published by Strömberg (no date. *Moroccan bird songs and calls*), Roché & Chevereau (1998. *Birds of North-West Africa*) and Chappuis (2000. *African bird sounds 1. West and Central Africa*). Sonograms of *nana* were published by Mauersberger et al. (1982. *Mitt. Zool. Mus. Berlin* 58: 11–74). Asian Desert Warbler *S. nana* occurs north and east of the Caspian Sea and from E Iran eastward through S/C Kazakhstan, N Afghanistan, Turkmenia, Uzbekistan to N and W China: Xinjiang, N Qinghai, N Gansu, W Inner Mongolia and SW Mongolia. African Desert Warbler *S. deserti* occurs in SE Morocco, E Western Sahara, N Mauritania, N Mali, Algeria south of the Atlas mountains, S Tunisia and W Libya. In winter it disperses into adjacent desert areas, incl. N Mali and N Niger.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A	A*	A

(*) Accepted as ‘semi- or allospecies’.

Orphean Warbler *Sylvia hortensis* suggested to be treated as two species by Shirihai et al. (2001):

- Western Orphean Warbler *Sylvia hortensis* (monotypic)
- Eastern Orphean Warbler *Sylvia crassirostris* (polytypic: *S. c. crassirostris*, and perhaps migrating through West Palearctic also *jerdoni*)

CSNA Eastern Orphean Warbler is best treated as a separate species based on slight but consistent differences in morphology, marked differences in song and genetic data (Shirihai et al. 2001). More research is needed on the potential contact zone of orphean warbler(s), especially in Italy, where the breeding birds have not been studied by Shirihai et al. (2001). N. Baccetti commented: ‘Both taxa might be breeding in Italy, *crassirostris* in the Trieste area (still reasonably common), and *hortensis* in the prealpine belt and peninsular Italy (very much decreasing). There is some gap between the two. Nobody, however, has recently checked any specimens as far as I know. Belonging of peninsular birds to *hortensis* is suggested by a recent (2003) recovery of an adult breeding in Marche (eastern C Italy), that was controlled in Mauritania (C. Sebastianelli and Italian Ringing Scheme, pers. comm.).’

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
P	A	A	A*	A

(*) Accepted as ‘semi- or allospecies’.

Greenish Warbler *Phylloscopus trochiloides*

proposed by BOURC TSC (Collinson et al. 2003) to be treated as one species:

- Greenish Warbler *Phylloscopus trochiloides* (polytypic: *Ph. t. viridanus*, ‘Green Warbler’ *Ph. t. nitidus*, ‘Two-barred Greenish Warbler’ *Ph. t. plumbeitarsus*; extralimital: *Ph. t. ludlowi*, *obscuratus* and *trochiloides*)

BOURC TSC The taxa within the Greenish Warbler complex are best treated as conspecific, based on application of the *Guidelines* (Collinson et al. 2003). Four of the five taxa form an interrupted ring *viridanus–ludlowi–trochiloides–obscuratus–plumbeitarsus*. There is distributional overlap between the two ends *viridanus* and *plumbeitarsus*. All published morphological and vocal characters vary clinally along the chain *viridanus–ludlowi–trochiloides–obscuratus–plumbeitarsus* and there is no evidence of species-level differences across the distributional gap between *obscuratus* and *plumbeitarsus*. Under the guidelines, taxa that are linked by a broad cline are retained within a single species. For *nitidus*, a treatment as a separate species could be defended considering its allopatric range, genetic differentiation (by 2.5–3.1% for cytochrome *b* mtDNA) and apparently distinct morphology, but it has not yet been shown that *nitidus* fulfils

diagnosability requirements under the guidelines. *Nitidus* is therefore, for now, retained within *Ph. trochiloides*. Based on sound-recordings, M. Schubert showed that some vocalisations of *Ph. t. plumbeitarsus* are very similar to those of *viridanus* [Schubert M. (1982) *Mitt. zool. Mus. Berlin* 58 (1): 109–128.]. Van der Vliet et al. (2003) showed that *plumbeitarsus* and *nitidus* have rather similar sparrow *Passer*-like contact calls with slightly different patterns; the contact calls of *viridanus* differ in showing a rising first part and a descending final part. For additional reading on the Greenish Warbler complex, see Ticehurst (1938), Vaurie (1959), Williamson (1967), Helbig et al. (1995), Irwin (2000, 2002), Irwin et al. (2001b) and Collinson (2001).

P.-A. Crochet commented: ‘A difficult case obviously, since ring speciation is a continuous process which is difficult to interpret in terms of classification. But in this case, *plumbeitarsus* and *viridanus* clearly behave as valid biological species. There is also a gap in distribution between *plumbeitarsus* and the *trochiloides* complex (*obscuratus*) which allows drawing a line between a set of intergrading populations (*Ph. trochiloides*) and the isolated *Ph. plumbeitarsus*. No treatment is perfect, but I firmly believe that a split of *plumbeitarsus* is the best option. After all, this can be considered speciation and if we lump them, we do not acknowledge that. The issue of parphyly of *trochiloides* should not be a major obstacle. Even if the mitochondrial tree is the real taxon tree (which has to be tested yet), paraphyletic species are not “forbidden” when there is strong evidence of speciation from other sources. For *nitidus*, it should be easier: distinctive mtDNA, distinctive song.’

L. Svensson commented: ‘I agree with P.-A. Crochet’s initial remark, but in contrast to him, I believe that with ring species, the drawback with an arbitrary division of the ring exceeds the advantage of “acknowledging that speciation has taken place”. I think it is far better to keep such a complex and plastic species together as one whole, not least for a better understanding of evolution and speciation, rather than splitting and naming down to smallest possible fraction of a whole. Splitting can be beneficial, but only up to a point. Possibly, however, Collinson et al. (2003) exaggerated the difficulties of discriminating *nitidus* on morphology. In my experience this taxon can invariably be separated on morphology. In the future, therefore, at least *nitidus* might be regarded as a separate species. It is more of an appendix and not a part of the ring, where the problems arise.’

Whether ring species are one or more species is also a matter of scale... And scale does not enter the taxonomic model. Hence, clearly, taxonomy cannot represent this situation properly. This not due to the case being particularly difficult, nor to any ‘paradox’ – explaining what a ring-species is with words is rather easy; this is simply a patent failure of the taxonomic model. Whether you lump or split, you will always be ‘wrong’ at some scale. Actually, the only ‘taxonomic treatment’ that might reflect the reality of a ring species accurately would be an open and never-ending disagreement among taxonomists.

The extremes of a ring species are sympatrically allopecific and allopatrically conspecific... Hence the *Guidelines*, because they propose distinct criteria in allo- and sympatry, can be of no help at all. They will inevitably provide two distinct answers according to the scale at which you look at the problem. Knox et al. (2002) wrote: ‘Where *viridanus* and *plumbeitarsus* meet in C Siberia they behave as separate species. This group of taxa appears to comprise a ring species. The (almost) continuous distribution and clinal variation along the chain prevents the constituent taxa being treated as anything other than a single species.’ The counterpart to this affirmation would be something like: ‘The (almost) continuous distribution and clinal variation along the chain linking *viridanus* and *plumbeitarsus* might be interpreted as them being conspecific. This group of taxa appears to comprise a ring species. The fact that where *viridanus* and *plumbeitarsus* meet in C Siberia they behave as separate species prevents these two constituent taxa being treated as anything other than two species.’ Knox’s treatment would be at the cost that true relationships in the contact zone would be lost to taxonomy. The opposite treatment would be at the cost that intermediate populations would not be unambiguously attributable to one of the two species. Either decision is arbitrary. It is difficult, however, to both adopt the *Guidelines for assigning species rank* and not follow what the authors of those *Guidelines* decided regarding Greenish Warbler. This is the major reason to treat the complex as a single species for the time being.

ID: Ticehurst (1938), Alström & Olsson (1987, 1989), Leader (1993) and van der Vliet et al. (2001).

Greenish Warbler complex lumped as one species:

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A				A*

(*) STC is not able to come to a consensus treatment in this case, so will vote Pending and keep the group lumped (as in *Holarctis fāglar*, which serves as a baseline for the Swedish list).

Yellow-browed Warbler *Phylloscopus inornatus* to be treated as two species (accepted by the AERC TAC in Sangster et al. 2002a):

- Yellow-browed Warbler *Phylloscopus inornatus* (monotypic)
- Hume's Leaf Warbler *Phylloscopus humei* (polytypic: *Ph. h. humei*; extralimital: *Ph. h. mandellii*)

Rationale: see Sangster et al. (2002a) and appendix 1.

Pallas's Warbler *Phylloscopus proregulus* to be treated as three species:

- Pallas's Leaf Warbler *Ph. proregulus* (monotypic)
- Lemon-rumped Warbler *Ph. chloronotus* (polytypic; extralimital: *Ph. c. chloronotus*, *simlaensis*)
- Gansu Leaf Warbler *Ph. kansuensis* (monotypic; extralimital)

STC Pallas's Leaf Warbler *Ph. proregulus* used to be considered a wide-ranging polytypic species, breeding in Siberia, N Mongolia and NE China (*proregulus*); C China and the Himalayas west to C Nepal (*chloronotus*); and W Himalayas (*simlaensis*). A fourth taxon, *kansuensis*, from NC China, was treated as a synonym of either *proregulus* or *chloronotus*. Alström & Olsson (1990) proposed that *proregulus* and *chloronotus* / *simlaensis* should be treated as two separate species based on pronounced differences in vocalisations and lack of response of playback to each other's songs. Alström & Olsson (1995) pointed out that also *kansuensis* differed much in vocalisations from the others, and did not respond to playback of song of these, and concluded that it ought to be treated as a separate species. (Martens & Eck 1995; Alström et al. 1997; Alström 2001, Alström & Olsson submitted).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
P	A	A	A*	A

(*) Accepted as 'semi- or allospecies'.

Bonelli's Warbler *Phylloscopus bonelli* to be treated as two species (accepted by the AERC TAC in Sangster et al. 2002a):

- Bonelli's Warbler *Phylloscopus bonelli* (monotypic)
- Balkan Warbler *Phylloscopus orientalis* (monotypic)

Rationale: see Sangster et al. (2002a) and appendix 1.

Chiffchaff *Phylloscopus collybita* complex to be treated as four species (accepted by the AERC TAC in Sangster et al. 2002a):

- Common Chiffchaff *Phylloscopus collybita* (polytypic: *Ph. c. collybita*, *abietinus* and *tristis*)
- Canary Island Chiffchaff *Phylloscopus canariensis* (polytypic: *Ph. c. canariensis* and – recently extinct – *exsul*)
- Iberian Chiffchaff *Phylloscopus ibericus* (monotypic; the recently described *Ph. i. biscayensis* is not recognised here)
- Mountain Chiffchaff *Phylloscopus sindianus* (polytypic: *Ph. s. lorenzii*; extralimital *Ph. s. sindianus*)

Rationale: see Sangster et al. (2002a) and appendix 1.

Firecrest *Regulus ignicapilla* to be treated as two species:

- Firecrest *Regulus ignicapilla*
- Madeira Firecrest *Regulus madeirensis*

CSNA Notice the correct spelling of *Regulus ignicapilla* (David & Gosselin 2002a). The split of the taxon *madeirensis* is accepted by a 4/5 majority of the AERC TAC members, due to significant differences in call, structure, morphology and genetical divergence (e.g. Päckert et al. 2003).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
P	A	A	A*	A

(*) Considered by A.J. Helbig to be part of the 'superspecies' *Regulus [ignicapilla]*.

Common Babbler *Turdoides caudata salvadorii*

Notice the correct spelling (David & Gosselin 2002b).

Fulvous Babbler *Turdoides fulva*

Notice the correct spelling of *Turdoides fulva maroccana* and *Turdoides fulva fulva* (David & Gosselin 2002b).

Brown Flycatcher *Muscicapa latirostris dauurica* becomes *Muscicapa dauurica dauurica*

We follow the views given by Watson (1986) i.e. Pallas's *Muscicapa dauurica* described in 1811, was in wide use in the Russian literature and precedes *Muscicapa latirostris* Raffles, 1822.

Red-breasted Flycatcher *Ficedula parva* to be treated as two species:

- Red-breasted Flycatcher *Ficedula parva* (monotypic)
- Taiga Flycatcher *Ficedula albicilla* (monotypic)

BOURC TSC Taiga Flycatcher is best treated as a separate species based on consistent differences in morphology (incl. female-like winter plumage of adult males, and absence of immature-like first-summer plumage of males), marked differences in vocalisation, genetic data and presumed sympatric breeding ranges (Cederroth et al. 1999). Jännes (1996) treated the identification of *albicilla* and provided some useful comments on its taxonomy. The recent record of 'Taiga' or 'Red-throated' Flycatcher in Sweden (Cederroth et al. 1999) has led to a re-appraisal of the taxonomic status of this form. Hitherto, it has been treated as subspecies *albicilla* of *F. parva*. However, the songs are diagnosably distinct (see BWP Vol. VII p. 36 Figs III and IV). Furthermore, Jännes (2003) described the distinct call notes of these two flycatchers. Svensson (1992) drew attention to the near all-dark underside of the bill in *albicilla*, whereas *parva* has a pale brown or pinkish base to the lower mandible. Adult male *parva* has a more extensive red breast meeting the white of the lower breast and belly; in *albicilla* the reddish breast patch is encircled by grey and restricted to the centre of the throat. One year-old male *albicilla* attain adult plumage, whereas *parva* are more female-like. Finally, the upper tail coverts of *albicilla* are jet black, even blacker than the uppertail, compared with *parva*, which has these brown or black-brown, never darker than the uppertail. Both breast and upper tail coverts seem to be diagnostic. Taiga Flycatcher has now also been recorded in Great Britain (Lassey 2003; Chapman 2003), with recent records in Denmark and France pending.

DNA analyses have not yet been published. Urban Olsson reports that cytochrome *b* differs by c. 6.9% between *parva* and *albicilla*. This is larger than for many similar species, and clearly would support a split. Following the *Guidelines for assigning species rank*, these taxa seem to be parapatric, overlapping slightly in the Ural mountains. They are diagnosably distinct on song, bill colour, upper-tail coverts (apparently in all plumages), and adult male breast pattern. The case for splitting is further strengthened by the difference in one year-old males and the (unpublished) mtDNA data. Thus, *Ficedula albicilla* Taiga Flycatcher should be recognised as a separate species. It is a pity, however, that no information is available from the presumed contact zone. (Martin Collinson)

ID: Jännes (1996), Cederroth et al. (1999)

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A	A*	A

(*) Accepted as 'semi- or allospecies'.

Pied Flycatcher *Ficedula hypoleuca* to be treated as two species:

- Atlas Flycatcher *Ficedula speculigera* (monotypic)
- Pied Flycatcher *Ficedula hypoleuca* (polytypic: *F. h. iberiae, hypoleuca, sibirica*)

BOURC TSC The Eurasian black and white flycatchers have been treated as three separate species: Pied *Ficedula hypoleuca*, Collared *F. albicollis* and Semi-collared *F. semitorquata*. Adult males are diagnosably distinct on the basis of rump colour, size of forehead patch, extent of white on secondaries and tertials, and extent of white on outer tail feathers. Females can also be separated with care, but there is more overlap. Populations of *hypoleuca* from Iberian and NW Africa are allopatric. The Spanish form (*iberiae*) has a larger white forehead than the nominate race, and there is more white in the wings than in populations from further north; adult males of both *iberiae* and *hypoleuca* usually have an all black tail. Birds from the Atlas mountains (*speculigera*) have the greater coverts completely white, but with much less white in the outer tail feathers than in *hypoleuca*. The white forehead is more extensive in *speculigera* than in any other forms of *hypoleuca*. *Speculigera* is probably diagnosably distinct from *iberiae* on size of forehead patch. In many

ways, *speculigera* is closer to *albicollis*, apart from complete neck collar in latter. DNA sequence data published by Saetre et al. (2001a, *Ibis* 143: 494–497) suggest that Pied and Collared Flycatchers form a monophyletic group, with *speculigera* as their sister group, although the bootstrap support for Pied/Collared relationship is not that strong. *Speculigera* is thus more distant from *hypoleuca* than *hypoleuca* is from *albicollis*. Semi-collared is most distinct. Nuclear DNA sequences (Saetre et al., 2001b, *Mol. Ecol.* 10: 727–749) support these findings. The mitochondrial genetic distances between Pied (*hypoleuca* NW Europe), Collared, Semi-collared, *speculigera* and *iberiae* are all of the order of 3–4%, apart from *hypoleuca* and *iberiae*. These are c. 0.5%, which is closer to the intra-taxon differences of 0.12–0.39%. The evidence suggests that *hypoleuca*, *albicollis*, *semitorquata* and *speculigera* are diagnosably distinct, and with genetic divergences (from both nuclear and mtDNA) that are supportive of full species status. The Iberian form seems to be intermediate between *speculigera* and *hypoleuca* in morphology, though closer to the latter. The DNA data also suggest that its affinities lie with *hypoleuca*. The song of *speculigera* is somewhat different from the song of *hypoleuca*; it is perhaps more variable, at times a little reminiscent of *semitorquata*. (P.-A. Crochet and L. Svensson, pers. comm.) It is proposed that the Iberian form retains its subspecific status as *F. hypoleuca iberiae*. (Martin Collinson)

ID: Mild, K. (1994) Field identification of Pied, Collared and Semicollared Flycatchers. *Birding World* 7: 139–151; 231–240; 325–334. Etherington, G. & Small, B. (2003) Taxonomy and identification of Atlas Flycatcher– a potential British vagrant. *Birding World* 16 (6): 252-256.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A	A*	A

(*) Accepted as allospecies.

Blue Tit *Parus caeruleus* proposed by Martin (1988) to be treated as two species:

- African Blue Tit *Parus teneriffae* (polytypic: *P. c. palmensis*, *teneriffae*, *ombriosus*, *degener*, *ultramarinus*, *cyrenaicae*)
- Blue Tit *Parus caeruleus* (polytypic: *P. c. caeruleus*, *obscurus*, *ogliastrae*, *balearicus*, *calamensis*, *satunini*, *persicus*, *orientalis*, *raddei*)

CAF Martin (1988) suggested a division of Blue Tit into two species based on comparative morphological and acoustic data. Sangster (1996a) proposed six species based on differences in plumage, song, various calls and habitat, and strongly reduced reaction to playback in these forms. This proposal was based on BSC and has been adopted by Sibley (1996). Salzburger et al. (2002) and reference therein presented convincing evidence that the Blue Tit represents a paraphyletic assemblage. The analyses of mtDNA of seven subspecies from Eurasia and North Africa revealed a European/Middle Asian clade (that is the sister group to the Azure Tit *P. cyanus*) and a North African clade *P. teneriffae*. However, their data do not support assigning species rank to *P. cyanus flavipectus* as suggested by several authors on morphological grounds. The (mainly) North African clade is thus more distant from the European *caeruleus* clade than *caeruleus* is from *cyanus*. Since *caeruleus* and *cyanus* are good biological species, *caeruleus* and *teneriffae* are best treated as distinct species also. N. Baccetti and G. Fracasso pointed out that *ultramarinus* is also breeding on the (European) island of Pantelleria (Moltoni 1971). L. Svensson commented: ‘It is impossible not to be impressed by the call of *teneriffae*, sounding like a Crested Tit, and nothing like Blue Tit. The song is variable, but one common variant sounds more like Great Tit than Blue Tit. Certainly a good candidate for species status.’ Eduardo de Juana commented that Eduardo García del Rey (C/. Malaquita, 5 E-38005 Santa Cruz de Tenerife) is currently conducting a PhD thesis on *teneriffae*. (P.-A. Crochet)

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
	A	A	A*	A

(*) Considers *caeruleus*, *cyanus* and *teneriffae* as part of the ‘superspecies’ *P. [c.] caeruleus*.

Black-crowned Tchagra *Tchagra senegalus cucullatus*

Notice the correct spelling (David & Gosselin 2002b).

Isabelline Shrike *Lanius isabellinus* (polytypic: *L. i. phoenicuroides*, *isabellinus* and extralimital *arenarius*)

- *Lanius isabellinus phoenicuroides* remains as is (range: breeds from Iran north and east to far NW Xinjiang, through Turkmenistan, Afghanistan, W Pakistan, Uzbekistan, Tadzhikistan and S

Kazakhstan; winters mostly in S Arabia and E Africa (Somalia to Tanzania) although a few are apparently found in NW India. Worfolk 2000)

- *Lanius isabellinus speculigerus* becomes *Lanius isabellinus isabellinus* (range: breeds from the Russian Altai through N China and Mongolia approximately as far east as the Upper Amur river; winters from S Arabia to E and C Africa, generally to the north and the west of *phoenicuroides* although there is undoubtedly much overlap; W African records of Isabelline Shrikes probably refer to *isabellinus*; scarce but regular in Israel in autumn and winter. Worfolk 2000).
- *Lanius isabellinus isabellinus* becomes *Lanius isabellinus arenarius* (range: breeds only in W Xinjiang (Tarim basin), south of the range of *isabellinus*; winters mainly from Iran through Pakistan to NW India. Worfolk 2000)

BOURC TSC These nomenclatural changes were proposed by Pearson (2000) and supported by D. Schodde and W. Bock of the Standing Committee on Ornithological Nomenclature ('Does holotype priority always serve nomenclature?' *British Birds* 95: 593–596.) *L. i. phoenicuroides*, *arenarius* and *isabellinus* may be treated as phylogenetic species (Sangster et al. 1999), whereas recent Russian authors have tended to split *phoenicuroides* as a monotypic species and to lump the other taxa (Kryukov 1995). The situation in the contact zones within the '*cristatus* group' of shrikes is extremely complicated and open to differing interpretations (e.g. review of hybrids in Worfolk 2000, pp 333–335); this group consists of red-backed, isabelline and brown shrikes. More research is needed before they can be split under the species concept that has been adopted by the AERC TAC. L. Svensson commented: 'During a recent field trip in May around Lake Balkhash (with E. Gavrilov, O. Belyalov, A. Lassey, A. Grieve and P. Alström), local *phoenicuroides* (of two occurring forms, morphs or perhaps two valid geographical subspecies, *karelini* in W and N, *phoenicuroides* in E and S) and migrant *isabellinus* were trapped and studied in the field. It became painfully evident that not only were females at times extremely difficult to identify reliably, males too were sometimes appearing as confusing intergrades between the *phoenicuroides* group and *isabellinus*. Clearly a lot more field work and DNA studies remain before we understand the best taxonomic treatment of this group.'

ID: Worfolk (2000) offers a useful review of the identification of redbacked, isabelline and brown shrikes

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A	A	A

Great Grey Shrike *Lanius excubitor* to be treated as two species (accepted by the AERC TAC in Sangster et al. 2002a):

- Great Grey Shrike *Lanius excubitor* (polytypic: *excubitor* incl. '*galliae*' and '*melanopterus*', *homeyeri*, *przewalskii*, *sibiricus*; extralimital: *mollis* incl. *funereus*, *bianchii*, *invictus*, *borealis*)
- Southern Grey Shrike *Lanius meridionalis* (polytypic: *L. m. meridionalis*, *koenigi*, *algeriensis*, *elegans*, *aucheri* incl. *theresae*, *pallidirostris*; extralimital: *lahtora*, *buryi*, *uncinatus*, *leucopygos*)

Rationale: see Sangster et al. (2002a) and appendix 1.

Red-billed Chough *Pyrhcorax pyrrhcorax erythroramphos*

Notice the correct spelling of the subspecies (David & Gosselin 2000a), erroneously *eythroramphus* in Cramp & Perrins (1994).

Citril Finch *Serinus citrinella* to be treated as two species (accepted by the AERC TAC in Sangster et al. 2002a):

- Citril Finch *Serinus citrinella* (monotypic)
- Corsican Finch *Serinus corsicanus* (monotypic) – notice the correct spelling!

Rationale: see Sangster et al. (2002a) and appendix 1.

Crimson-winged Finch *Rhodopechys sanguineus*

Notice the correct spelling of *Rhodopechys sanguineus alienus* and *Rhodopechys sanguineus sanguineus* (David & Gosselin 2002b).

Ovenbird *Seiurus aurocapilla aurocapilla*

Notice the correct spelling (David & Gosselin 2002a).

Rufous-sided Towhee *Pipilo erythrophthalmus* to be treated as two species:

- Eastern Towhee *Pipilo erythrophthalmus* (polytypic: *P. e. erythrophthalmus*; extralimital: *P. e. rileyi*, *alleni*, *canaster*)
- Spotted Towhee *Pipilo maculatus* (polytypic; extralimital: *P. m. arcticus*, *montanus*, *gaigei*, *curtatus*, *oregonus*, *falcifer*, *megalonyx*, *falcinellus*, *clementae*, *umbraticola*, *magnirostris*, *consobrinus*, *socorroensis*, *griseipygius*, *orientalis*, *maculatus*, *macronyx*, *vulcanorum*, *oaxacae*, *chiapensis*, *repetens*)

BOURC TSC cf. BOURC (1996).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A		A*	A

(*) Accepted as ‘allospecies’ by A.J. Helbig.

Savannah Sparrow *Ammodramus sandwichensis* becomes *Passerculus sandwichensis*

BOURC TSC cf. BOURC (1996).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A		A	A

Fox Sparrow *Zonotrichia iliaca* becomes *Passerella iliaca*

BOURC TSC cf. BOURC (1996).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A		A	A

Song Sparrow *Zonotrichia melodia* becomes *Melospiza melodia*

BOURC TSC cf. BOURC (1996). These changes were accepted by the BOURC in 1996 to conform to treatments adopted by the AOU almost a century earlier: *Melospiza fasciata* was adopted for Song Sparrow by the AOU in 1886, and changed into *M. melodia* in 1910; *Passerella iliaca* has been used by the AOU for Fox Sparrow since 1895, *Passerculus sandwichensis* for Savannah Sparrow since 1910. They have not been confronted to modern evidence. Recent work (e.g. Carson & Spicer 2003) could well shake the tree.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A		A	A

Pine Bunting *Emberiza leucocephalos leucocephalos*

Notice the correct spelling (David & Gosselin 2002a).

Cirl Bunting *Emberiza cirlus* suggested to be treated as monotypic by Svensson (1992)

BOURC TSC Two recent reviews (Svensson 1992, Cramp & Perrins 1994, p 182) have suggested that the Sardo-corsican form *nigrostriata* is not recognisable. The BOURC follows this view (Knox et al. 2002).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A			A

House Bunting *Emberiza striolata* to be treated as two species:

- Mountain Bunting *Emberiza striolata* (polytypic: *E. s. striolata*; extralimital: *E. s. jebelmarrae* and *saturator*)
- House Bunting *Emberiza sahari* (polytypic: *E. s. sahari*; extralimital: *E. s. theresae* and *sanghae*)

STC House Bunting is best treated as a separate species based on clear differences in morphology and vocalisation (Kirwan & Shirihai in prep.). This consideration refers only to the Palearctic forms *E. s. striolata* and *E. s. sahari*; the Sub-Saharan forms *jebelmarrae*, *saturator*, *theresae* and *sanghae* need further research.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
	P	A	A	A

Corn Bunting *Miliaria calandra* suggested to be renamed *Emberiza calandra* (polytypic: *E. c. calandra*, *clanceyi* and *buturlini*) according to Grapputo et al. (2001) and Lee et al. (2001)

CSNA There are two genetic studies confirming that Corn Bunting is an *Emberiza* (Grapputo et al. 2001 and Lee et al. 2001). Grapputo et al. (2001) show that '*Miliaria*' *calandra* is nested within the genus *Emberiza*, thus subsumed under that genus. In such a case, the AERC TAC must follow. Generally, it is only useful to maintain separate genera (e.g. *Miliaria*) if there is sufficient evidence to do so. If not, it is recommended to limit the number of genera.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A	A	A

Northern Oriole *Icterus galbula* to be treated as three species

- Baltimore Oriole *Icterus galbula* (monotypic)
- Black-backed Oriole *Icterus abeillei* (extralimital)
- Bullock's Oriole *Icterus bullockii* (extralimital)

CSNA The English name of *Icterus galbula* s.s. becomes Baltimore Oriole and the species is monotypic. cf. BOURC (1996).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A			A

Pending decisions with comments on the progress

The following proposals are pending. We either have not examined the data in sufficient detail to reach a conclusion, or there may not be sufficient data for such an assessment to be worthwhile. In the case of proposed splits, the species discussed below are recommended to be kept conspecific until further information is available. The AERC TAC is well aware that many other taxonomic changes to Western Palearctic species have been suggested by various authors [e.g. on Herald Petrel *Pterodroma heraldica* (Brooke & Rowe 1996), Madeiran Storm-Petrel *Oceanodroma castro* (Monteiro & Furness 1998), Peregrine Falcon *Falco peregrinus* (Brosset 1986), Lesser Sand Plover *Charadrius mongolus* (Garner et al. 2003), Purple Swamp-Hen *Porphyrio porphyrio* (Sangster 1998), Oriental Cuckoo *Cuculus saturatus* (Payne 1997), Reed Warbler *Acrocephalus scirpaceus* (Pearson et al. 2002), Common Magpie *Pica pica* (Ebels 2003), European Robin *Erithacus rubecula* (Bergmann & Schottler 2001) – Tenerife Robin is accepted as an ‘allospecies’ by Helbig (in litt.) – Red-flanked Bluetail *Tarsiger cyanurus* (Thoen & De Smet 2002), White’s Thrush *Zoothera dauma* (Sangster et al. 1998), Myrtle Warbler *Dendroica coronata* (Sangster et al. 1997), several Cape Verde endemics by Hazevoet (1995), etc.] and is considering which of these taxa should be treated next. In some cases, research is ongoing and it therefore seems appropriate to await results and formal publication in the primary literature before taking any action. Taxonomy rekindled the interest in bird forms (e.g. Roselaar 1995, Gantlett 1998, 2001, Clavell 2002), and this will eventually lead to a better understanding of identification, ecology, distribution and many more aspects. Hopefully, the improved knowledge will lead to more efficient conservation of bird forms regardless of their taxonomic treatment.

Tundra Swan *Cygnus columbianus*

suggested by Sangster et al. (1997) to be treated as two species:

- Whistling Swan *Cygnus columbianus* (monotypic)
- Bewick’s Swan *Cygnus bewickii* (monotypic)

BOURC TSC Voous (1973) included *bewickii* in *C. columbianus*, but mentioned that they were sometimes treated as specifically distinct. The STC has currently no information available on the reason for the earlier lumping of the two forms, has no genetic data available for evaluation and furthermore has not been able to detect more than one (bill colour) diagnosable morphological difference. Until these points are elucidated, the STC suggests that Whistling Swan and Bewick’s Swan be kept as one species. The STC considers that it would be interesting to take part of both the BOURC TSC file as well as opinions by the AOU. The CSNA and references therein accepted the split in 1997: ‘Whistling and Bewick’s Swan are specifically distinct (cf. Stepanyan 1990, Gantlett et al. 1996), based on qualitative differences in morphology (Livezy 1996)’ (*Dutch Birding* 19: 22, 1997). The CAF considers that this split would be based on a single morphological difference (bill pattern, see Evans & Sladen 1980), which has not been tested on a sufficiently large sample and may not be diagnostic. Furthermore, there is no evidence that *columbianus* and *bewickii* are monophyletic. Various authors have questioned the presumed discontinuity of the amount of yellow on the bill (e.g. Patten & Heindel 1994, Knapton 2000, *Birding World* 12: 125–127) and it appears that individual variation may have been underestimated by Evans & Sladen (1980). In the Netherlands, at least one putative Whistling Swan has not been accepted by the CSNA as it was probably a Bewick’s Swan with reduced yellow on the bill (cf. *Dutch Birding* 13: 39, 1991; 18: 20–21, 1996). On the other side of the Atlantic, Knapton (2000) pointed out that ‘black-stripe’ Bewick’s Swans are overrepresented among presumed vagrants in North America, possibly indicating variation of Whistling Swan rather than genuine vagrancy. Following a range expansion, both taxa are currently breeding sympatrically in NE Siberia (Syroechkovksi 2002) with hybridisation (to an unknown extent) reported. The BOURC TSC is preparing a file on swans. This case is clearly rejected (two votes); therefore, the status quo (single species) is maintained.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
R	R	A	A*	P

(*) Accepted as ‘semi- or allospecies’.

Bean Goose *Anser fabalis* suggested by Sangster et al. (1997) to be treated as two species:

- Taiga Bean Goose *Anser fabalis* (monotypic)

- Tundra Bean Goose *Anser serrirostris* (monotypic)

BOURC TSC Although a *Bean Goose Complex Task Force* was announced (BOURC 2002), no news has been received by the AERC TAC on this suggested split. For a review of the reasons to split these taxa, see Huyskens (1986) and Sangster & Oreel (1996). The Bean Goose phylogeny suggested by Sangster & Oreel (1996) with *rossicus* and *serrirostris* in one group and *fabalis* and *middendorffii* in the other is only an educated guess. G. Huyskens (1914–2002) always stressed the urgent conservation need for Taiga Bean Geese and was a strong proponent for a specific status of this taxon, which he studied for over 40 years; a list of his publications on Bean Geese can be found in his obituary (Maes 2002). Persson (1990) discussed the occurrence of *rossicus* in Sweden. Lindholm & Tolvanen (2003) wrote on identification and occurrence of *rossicus* in Finland and reviewed different opinions on the taxonomy of the Bean Goose complex. Sometimes pair formation of geese is said to occur on the wintering grounds, which would make wintering grounds even more important than breeding grounds for taxonomic studies. This is, however, an oversimplification (see Ruokonen 2001 for a discussion and additional references). One of the major problems in the Bean Goose Complex is that ‘intermediates’ cannot be studied in the field; even pure individuals are sometimes so difficult to identify that classifying ‘intermediates’ objectively is impossible. The very existence of ‘intermediates’ has been questioned, e.g. by Huyskens (1977, 1986) and Van Impe (1980a,b). J. Van Impe (in litt.) commented: ‘At the time we were lucky to be able to study large and pure flocks of *fabalis* and *rossicus*. At present, mainly mixed flocks are occurring in the Dutch province of Noord-Brabant; it is very difficult to define the exact number of *fabalis* and *rossicus* in such flocks. It would be wrong, however, to return to the year 1936 when the word ‘Mischform’ was first used; by maintaining this mistake, even some geese experts have done a lot of harm to the conservation of the endangered Taiga Bean Goose.’ At the geese conference in Kleve, *A. f. fabalis* and *A. f. rossicus* were formally recognised again, after years of confusion (Madsen 1991). This point of view is also followed in Russia (Mooij & Zöckler 1999), who wrote on p 113: ‘Before accepting species status for Tundra and Taiga Bean Geese more information is needed, especially from the breeding grounds. Exact limits of the breeding range of both Bean Goose representatives are still unclear, especially in the western part of their distribution...’ The Siberian Bean Goose expert V. I. Emel’yanov has measured thousands of birds over many years and has regularly published on the subject, information ignored by Western European taxonomists so far. He includes *A. f. johanseni* in *A. f. fabalis* and wrote an interesting book on Bean Geese (in Russian) (Emel’yanov 2000). One possible method of advancing is taking measurements in different areas (like Burgers et al. 1991 did in the Netherlands). Lindholm & Tolvanen (2003) tried this on a very small sample from Finland, with confusing results. The other possible method is molecular studies. Currently, DNA of Bean Geese is being studied at Oulu University (M. Ruokonen; A. Lindholm pers. comm.). The taxonomic position of all five forms in this complex (*fabalis*, *rossicus*, *johanseni* and the extralimital *middendorffii* and *serrirostris*) needs further research. Again, an obvious pending case; the status quo as a single species is maintained.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
P	R	A	P	P

Canada Goose *Branta canadensis* suggested by Sangster et al. (1998) to be treated as two species:

- Greater Canada Goose *Branta canadensis* (polytypic: *B. c. canadensis*, *fulva*, *interior*, *maxima*, *moffitti*, *occidentalis*, *parvipes*)
- Lesser Canada Goose *Branta hutchinsii* (polytypic: *B. h. hutchinsii*, *leucopareia*, *minima*, *taverneri*)

BOURC TSC A number of important phylogenetic studies have been published recently or are awaited. Sangster et al. (1998), and references therein, accepted the split: ‘Lesser Canada Goose and Greater Canada Goose are specifically distinct (cf. Sibley 1996) based on congruence of phylogeographic analyses of mtDNA restriction fragments (Shield & Wilson 1987, Van Wagner & Baker 1990, Quinn et al. 1991), mtDNA sequences (Quinn et al. 1991, Baker & Marshall 1997) and morphometry (Van Wagner & Baker 1990). Pending further analysis, *leucopareia*, *minima* and *taverneri* are provisionally retained conspecific with *hutchinsii*; *fulva*, *interior*, *maxima*, *moffitti*, *occidentalis* and *parvipes* are provisionally retained conspecific with *canadensis*.’ *Dutch Birding* 20 (1): 25. Since the publication of Sangster et al. (1998), more results have been published suggesting that *B. canadensis* is paraphyletic, cf. Sorenson et al. (1999) and particularly Paxinos et al. (2002). Phylogenetic analysis of 1.35 kb of mtDNA sequences from fossils revealed a previously unknown lineage of Hawaiian geese, of which only one representative survives (Hawaiian Goose *B. sandvicensis*). This radiation is nested phylogenetically within Canada Goose *B. canadensis* and is related most closely to the large-bodied *canadensis* lineage. Barnacle Goose *B. leucopsis* is

also nested within the Canada Goose complex and is most closely related to the small-bodied *hutchinsii* lineage. Pearce et al. (2000) combined morphology and genetics to identify shot Canada Geese and provided the first population-level approach based on a large number of birds collected on the breeding grounds. They analysed 45 *parvipes* (two localities), 69 *occidentalis* (3 localities), 18 *fulva*, 16 *moffitti*, 8 *taverneri*, 8 *leucopareia* (2 localities) and 20 *minima*. They found that control region haplotypes formed two very distinct clades (P.-A. Crochet's analysis of their sequence in GenBank) and, more interesting, that there is no lineage sharing between large-bodied (*parvipes*, *occidentalis*, *fulva*, *moffitti*) and small-bodied (*taverneri*, *leucopareia*, *minima*) geese. All individuals from the large-bodied form had a large-bodied haplotype and all individuals from the small-bodied form had a small-bodied haplotype. We thus have now evidence that mtDNA segregates with 'species' at a population level. All the samples are from Alaska and adjacent Canada, so come from a restricted geographic area. Although it would be better to have the same data for birds from all over the continent, it is difficult to interpret the pattern observed by Pearce et al. (2000) without admitting a strong reproductive isolation between large-bodied and small-bodied Canada geese. This adds up to other arguments about paraphyly and makes a split the most logical position.' Pierson et al. (2000) documented the molecular genetic status of *leucopareia*. Talbot et al. (2002) examined the genetics of *orientalis*. A paper on the phylogeography of Canada Geese in W North America (Scribner et al. 2003) is mainly a rewriting of the results of Pearce et al. (2000) and does not provide new evidence (sampling is still limited to NW North America). Large-bodied (*parvipes*, *occidentalis*, *fulva*, *moffitti*) and small-bodied (*minima*, *taverneri*, *leucopareia*) forms constitute reciprocally monophyletic lineages in the mtDNA tree even if sampling localities are quite close. The lack of lineage sharing combined with the amount of divergence (14% for hypervariable control region, difficult to relate to the more common cytochrome *b* divergence, but probably at least 2%) is strongly indicative of speciation under all species concepts. The real 'proof' would be to see the other small bodied form (*hutchinsii*) from E North America to group with the Alaskan small bodied forms, and all large bodied birds to group together. Baker & Marshall (1997) answered this question (although sample size and sequence length was small). This chapter includes a phylogeny of the Canada Goose complex. All subspecies were sampled, with the exception of *leucopareia*. Large- and small-bodied subspecies formed separate clades (bootstrap support 98–100%). Of two samples of *taverneri* (a small-bodied form), one from Washington turned up in the large-bodied clade and the other in the small-bodied clade; the authors suggested hybridisation and misidentification as possible explanations for the 'wrong' position of the Washington specimen. Alternatively, some races might be of hybrid origin and hold mitochondria of both lineages... Notice that *taverneri* is considered invalid, and treated as an intergrade link between *minima*, *occidentalis* and *parvipes* by Madge & Burn (1988).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
P	A	A	A*	P

(* Accepted as 'semi- or allospecies'; A.J. Helbig includes Barnacle Goose *Branta leucopsis* as part of the 'superspecies' *B. canadensis*.

Brent Goose *Branta bernicla* suggested by Sangster et al. (1997) to be treated as three species:

- Dark-bellied Brent Goose *Branta bernicla* (monotypic)
- Pale-bellied Brent Goose *Branta hrota* (monotypic)
- Black Brant *Branta nigricans* (monotypic)

BOURC TSC For a review of the reasons to split these taxa, see Sangster et al. (1997), going further than the interpretation by Shields (1990). Since the publication of this summary, some new information came to light, which is still being studied by the AERC TAC. See e.g. Millington (1997), Garner (1998), Ogilvie & Young (1998), Reed et al. (1998), Shields & Cotter (1998), Syroechkovski et al. (1998), Hagmeier (2000), Sangster (2000b), Sibley (2000), Bloomfield & McCallum (2001), Garner & Millington (2001). The STC considers that there is lack of convincing evidence at present. With two rejections, the AERC TAC rejects the treatment as three species.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
	R	A	R	P

Common Scoter *Melanitta nigra* suggested by Stepanyan (1990) and others to be treated as two species:

- Common Scoter *Melanitta nigra* (monotypic)
- Black Scoter *Melanitta americana* (monotypic)

BOURC TSC Martin Collinson is actively working on the ‘Taxonomic status of the scoters *Melanitta*’, and during the 2003 AERC meeting in the Danube Delta it was therefore formally agreed to keep decisions on scoter taxonomy pending. Numeric cladistic studies based on phenotypic characters (e.g. Livezey 1991 on which this split is partially based) are disregarded completely by A.J. Helbig (in litt.) because they are unlikely to be phylogenetically informative and generally not congruent with molecular and careful morphological studies. Range overlap is unknown, races seem almost to meet on lower River Lena, but no intermediate specimens are known (Madge & Burn 1988). The split was accepted by Sangster et al. (1997): ‘Common and Black Scoter are specifically distinct (cf. *Dutch Birding* 11: 21–22, 1989), based on qualitative differences in morphology (cf. Stepanyan 1990, Livezey 1991, Gantlett et al. 1996)’ *Dutch Birding* 19 (1) 1997: 23. The STC also estimates that Black Scoter is best treated as a separate species based on rather obvious and clear-cut differences in morphology. From a BSC approach it is unlikely that clear-cut differences would be maintained without a large gap in range without some intrinsic barriers to gene flow (i.e. reproductive isolation).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
P	A	A	A*	A

(*) Accepted as ‘semi- or allospecies’.

Velvet Scoter *Melanitta fusca* suggested by Livezey (1995) to be treated as two species:

- Velvet Scoter *Melanitta fusca* (monotypic)
- White-winged Scoter *Melanitta deglandi* (polytypic: *M. d. deglandi* incl. ‘*dixoni*’ and *M. d. stejnegeri*)

CSNA Martin Collinson is actively working on the ‘Taxonomic status of the scoters *Melanitta*’, and during the 2003 AERC meeting in the Danube Delta it was therefore formally agreed to keep decisions on scoter taxonomy pending until the results of this review are known. According to the STC, White-winged Scoter is best treated as a separate species based on differences in morphology, occurring abruptly in C Siberia. Numeric cladistic studies based on phenotypic characters (e.g. Livezey 1995 on which this split is partially based) are disregarded completely by A.J. Helbig (in litt.) because they are unlikely to be phylogenetically informative and generally not congruent with molecular and careful morphological studies. Bill and trachea structure differ (Dwight, J. 1914 *Auk* 31: 293–308; Kortright F.H. 1942 *The Ducks, Geese and Swans of North America*. Harrisburg; Vaurie 1965). The ranges of *M. f. fusca* and *M. d. stejnegeri* are not known to meet. *M. d. stejnegeri* breeds from the Yenisey basin eastwards to Kamchatka and south to Mongolia. *M. d. deglandi* breeds from Alaska across N Canada to Hudson Bay and south into Manitoba (Ogilvie & Young 1998). Livezey (1995) suggested the split of *M. fusca* and *M. deglandi*. There is one accepted record of *M. d. stejnegeri* in Finland: 27 May–8 June 1996 Kemiö Smedaböle (Lindroos 1997) and several records of *M. d. deglandi* in Iceland (at least in 1993, 1998 and 2000–2002). The only recent paper dealing with the breeding biology of *stejnegeri* is by Yumov. (Abstracts of a conference in Moscow, 25–27 Sep. 2001, Problems in research and protection of Anseriformes in E Europe and N Asia, pp 135–136.). Unfortunately, this paper offers no clues whether *stejnegeri* should be treated as specifically distinct from *fusca*. The author includes *stejnegeri* in *M. deglandi*. *M. fusca* is not mentioned in the paper. G. Mauersberger wrote that I. Neufeldt knew no sympatric breeding [*Mitt. Zool. Mus. Berlin* (1982) 58: 19]. L. Kalbe (*Mitt. Zool. Mus. Berlin* 70, *Ann. Orn.* 18: 44) mentions *M. stejnegeri* in a review of the avifauna of a Mongolian lake and writes that according to Mauersberger, *M. fusca* never occurred in Mongolia. An updated version of the book of Rogachewa *The Birds of Central Siberia*, published for the first time in Russian in 1988, was published in 1992 (and can be found at http://birds.krasu.ru/txt/txt_mede.shtml). In a review of the first edition by Mauersberger in *Mitt. Zool. Mus. Berl.* 66 (1990), p 149. ‘*M. deglandi* dringt im Süden allein noch bis...lässt weiter im Norden aber als Brutvogel keine eindeutige sympatrie mit *M. fusca* erkennen; von Hybriden ist auch nichts gesagt.’ This implies that there is no clear sympatry; nothing is mentioned about hybrids. The updated text on the Internet is identical: *M. fusca* and *M. deglandi* are regarded as distinct species, with a lot more information about the former. Where the breeding areas meet, the reader is kept guessing; the text mentions: ‘judging is complex’. This probably implies that if hybridisation occurs, it is likely to be limited. J. Van Impe kindly added some comments on references to this section.

ID: Some of the best paintings of ducks can be found in Gooders & Boyer (1986). This is one of the few popular books which covers identification of scoters well (e.g. differences in head pattern and bill shape of both male and female scoters of all taxa are well illustrated). The identification of Whitewinged and Velvet Scoters is discussed by Proctor & Pullan (1997) and Garner (1999).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC

P	A	A	A*	A
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(*) Accepted as 'semi- or allospecies'.

Hooded Merganser *Mergus cucullatus* proposed to become *Lophodytes cucullatus*

STC cf. Livezey (1995), AOU (1983), BOURC (1996) and Sangster et al. (1997). Although three TCs and the AOU accept this generic change, the CAF would either put both Smew and Hooded Merganser in *Mergellus* or maintain all mergansers in *Mergus* based on Donne-Goussé et al. (2002). All decisions by TCs on the generic position of Hooded Merganser were published before the Donne-Goussé et al. (2002) paper. To minimise the risk that the generic name needs to be changed back in a near future, the AERC TAC prefers to maintain *Mergus cucullatus* as a status quo (Voous 1977c).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	R	A	A	P

Smew *Mergus albellus* proposed to become *Mergellus albellus*

STC Although it is now widely accepted to elevate the subgenus *Mergellus* to genus rank (cf. Livezey 1995, AOU 1983, BOURC 1996 and Sangster et al. 1997), its supposed relationships with Goldeneye *Bucephala clangula* (Livezey 1995) based on morphological data were only supported by poor bootstrapping and are not supported by genetic data. On the contrary, a recent genetic analysis of *Mergus serrator*, *M. albellus*, *M. cucullatus* and other waterfowl species suggest monophyly of these species, even if the support is not extremely high (Donne-Goussé et al., 2002). *M. serrator* is nevertheless quite distantly related to *M. albellus*, with genetic distance comparable to distance between *Bucephala clangula* and the *Mergus* / *Mergellus* clade. One may wonder whether it is a consistent approach to recognise three genera within *Mergus* s.l. (consisting of only six extant species world-wide), whereas pronounced subgenera in larger genera (e.g. within *Anas*, *Larus*, *Sterna*, *Acrocephalus*, *Sylvia* and *Parus*) are not elevated to genus rank, mainly for preserving 'taxonomic stability'. Donne-Goussé et al. (2002) showed that *cucullatus* and *albellus* are very closely related, with genetic distance far less than between different genera and even less than between many waterfowl species. *M. serrator* is more distantly related. So, the first conclusion is that *cucullatus* and *albellus* could be kept in the same genus. This genus can be *Mergus* (which remains monophyletic in all their analyses) or can be *Mergellus* (first described in 1840) which has priority over *Lophodytes* (from 1852), if one wishes to make two genera.' If the genus were recognised, the CAF would also place Hooded Merganser in *Mergellus*. Although this generic change is accepted by at least three TCs and the AOU, more time is needed for a proper evaluation of the Donne-Goussé et al. (2002) paper, in order to avoid needing to change back in the near future. Therefore, *Mergus albellus* is maintained as status quo. It should also be noted that at the time of writing *Mergus albellus* is still much more commonly used than *Mergellus albellus* (try a search on <http://www.google.com/> to see if this has changed since).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A*	A	A	P

(*) The CAF would either put both Smew and Hooded Merganser in *Mergellus* or maintain all mergansers in *Mergus* based on Donne-Goussé et al. (2002).

Genus *Calonectris* proposed to be merged in *Puffinus*

CAF Available phylogenies of shearwaters based on cytochrome *b* gene (Heidrich et al. 1998, Nunn and Stanley 1998) unambiguously place *Calonectris* within *Puffinus* as presently used. Two main lineages of *Puffinus* are identified, a 'small species' clade (with among others *P. puffinus*, *lherminieri*, *assimilis*) and a 'large species' clade (with *bulleri*, *pacificus*, *carneipes*, *gravis*, *griseus*). The two *Calonectris diomedea* and *leucomelas* form a clade, which is the closest relative of the 'small species', group. This topology is found by all methods of data analyses and is supported by rather high bootstrap values. A generic arrangement consistent with true relationships would thus require either moving the large species to another genus or merging *Calonectris* with *Puffinus*. The first option would necessitate allocating a complete analysis of all species and would also require more taxonomic changes. We thus recommend the allocation of *diomedea*, *leucomelas* and *edwardsii* to the genus *Puffinus*, pending a complete study of the relationships within *Puffinus*. (P.-A. Crochet)

P. Yésou, however, prefers a status quo, as ongoing research may reveal three or more genera within *Puffinus*, including the resurrection of *Calonectris*.

The AERC TAC recently received this proposal.

Cory's Shearwater *Calonectris diomedea*

suggested by Sangster et al. (1998) to be treated as three species:

- Cory's Shearwater *Calonectris borealis* (monotypic)
- Scopoli's Shearwater *Calonectris diomedea* (monotypic)
- Cape Verde Shearwater *Calonectris edwardsii* (monotypic)

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
	R	A	R	P

Suggested by Helbig & Barthel in Svensson et al. (2000) to be treated as two species:

- Cory's Shearwater *Calonectris diomedea* (polytypic: *C. d. diomedea* and *borealis*)
- Cape Verde Shearwater *Calonectris edwardsii* (monotypic)

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
	A	R	A*	P

(*) Accepted as 'semi- or allospecies'.

CAF The Cape Verde taxon *edwardsii* is clearly more distinct from *diomedea* and *borealis* than these two taxa are from each other (Mougin et al. 1991, Thibault & Bretagnolle 1998). This is true for morphology (see BWP, Porter et al. 1997) and voice (BWP). Although no genetic data are available, the levels of differentiation between *edwardsii* and *diomedea* / *borealis* in appearance and vocalisations are certainly more typical of species than subspecies. This is why this taxon has been split by e.g. BWP Concise Edition, a position supported by AERC TAC in these recommendations.

The systematic status of *diomedea* and *borealis* is more difficult. The two taxa differ in morphology and colouration (BWP, Granadeiro 1993, Gutiérrez 1998, Thibault & Bretagnolle 1998, Camphuysen & Van der Meer 2001). These differences are apparently consistent but slight (not clearly exceeding amount of differences among other subspecies of birds). Furthermore, there are overlaps in measurements between *diomedea* and *borealis* according to Mougin et al. (1991). We are not aware of studies looking at overlap in colouration in large series of specimens, although neither Camphuysen & Van der Meer 2001 nor Gutiérrez 1998 mentions intermediate specimens for the underwing pattern. Consistent differences also exist in vocalisations (Bretagnolle & Lequette 1990, Thibault & Bretagnolle 1998) but are much less marked than with *edwardsii*.

Genetically, the two taxa are weakly differentiated in nuclear DNA, with a level of differentiation more typical of intraspecific population structure than interspecific differentiation (Randi et al. 1989, Rabouam et al. 2000). Nevertheless, none of these studies use fully appropriate sampling and genetic methods and are thus not fully conclusive. Studies of mtDNA suggest that *diomedea* and *borealis* might be reciprocally monophyletic (Heidrich et al. 1996, 1997) but again, the low number of individuals (a maximum of 3 *borealis* per study) make this conclusion unreliable. On the other hand, the genetic distance between *borealis* and *diomedea* haplotypes is very low (1.0 to 1.6% for cytochrome *b*) and not larger than among haplotypes found within *diomedea* (1.2% between two *diomedea* haplotypes found in different individuals of the same colony in Marseille, see Heidrich et al. 1996). The amount of divergence between *diomedea* and *borealis* for mtDNA is lower than between different lineages in *Puffinus yelkouan* (see Heidrich et al. 1997).

Last, ringing recoveries suggest the occasional reproduction of *borealis* with *diomedea* individuals, and a number of intermediate individuals suggesting genetic introgression have been detected in a small colony off Corsica where vocalisations and haplotypes typical of *borealis* have been identified. It seems therefore that gene flow is not fully interrupted between *diomedea* and *borealis*.

The amount of genetic divergence between *diomedea* and *borealis* is thus typical of intraspecific variability and not of interspecific differentiation. Morphological and vocal differences are slight even if consistent (which is not fully proven for all characters), and not as large as between most sister species within closely related genera. Last, there is no evidence that these differences suffice to prevent interbreeding when these taxa meet. Based on evidence available at the time of writing, *diomedea* and *borealis* are best kept as subspecies of the same species (but see Sangster et al. 1998, 1999 for another opinion). The STC would need more information before being in a position to give any recommendation. Twofold split suggested by CAF and Germany and threefold split by the CSNA, no opinion on *edwardsii* received from BOURC TSC.

ID: The field identification of *diomedea* and *borealis* is discussed by Gutiérrez (1998).

Little/Audubon's Shearwater *Puffinus assimilis* / *lherminieri* complex

Austin, Bretagnolle & Pasquet have submitted a very detailed analysis of *lherminieri* / *assimilis* taxonomy (based on genetics) which will show that the taxonomy of this group must be completely reshaped. It seems that at least six distinct groups emerge from this complex. The results of this forthcoming study to be published in *Auk* should be awaited before taking any further action.

Little Shearwater *Puffinus assimilis* (*P. a. baroli*, *boydi*; extralimital: *P. a. elegans*, *tunneyi*, *assimilis*, *haurakiensis*, *myrtae*, *kermadecensis*)
 suggested to consider the taxa *P. a. baroli* and *P. a. boydi* as subspecies of Audubon's Shearwater *P. lherminieri* based on Austin (1996)

CAF The treatment of the *assimilis* / *lherminieri* complex is clearly wrong: Austin (1996) found that *boydi* and *baroli* are genetically indistinguishable and are closely related to *lherminieri* but not to *assimilis*. These results need to be validated and there is a risk of misidentification (but *baroli* samples originate from Tenerife and *boydi* from Razo). Still, even if there has been a mix of *baroli* and *boydi* samples, the results show that at least one of either *baroli* or *boydi* should be classified as blue-footed *lherminieri*. More research is needed to decide whether *baroli* should be treated as a full species or a subspecies of *lherminieri* (but not of *assimilis*). *Boydi*, on the other hand, could be treated as a subspecies of *lherminieri*: both taxa are genetically very close and morphologically perhaps not even diagnosably distinct. Further sampling is required. It should be noted that Hazevoet (1995) considered *boydi* as a full species. There is no doubt that *lherminieri*, *boydi* and *baroli* belong to a single group, but their relationships need to be examined further. Relationships and species limits within *P. assimilis* and *P. lherminieri* are highly complex and unclear. It is certain, however, that *P. assimilis* does not occur in the Western Palearctic. (P.-A. Crochet)

Audubon's Shearwater *Puffinus lherminieri* suggested to be split into

- Audubon's Shearwater *Puffinus lherminieri*
 [polytypic; possibly (see Little Shearwater)]
 1. *P. l. baroli* (Bonaparte) 1857 (small islands off Madeira, Selvagens, Canaries and Azores)
 2. *boydi* Mathews 1912 (Cape Verde Islands);
 extralimital:
 3. *P. l. bailloni* (Bonaparte) 1857 (Mauritius, Réunion, Seychelles & Madagascar; including 'nicolae' Jouanin 1971 from Aldabra-Maldives)
 4. *temptator* Louette & Herremans 1985 (Moheli Island)
 5. *bannermani* Mathews & Iredale 1915 (Bonin Islands)
 6. *gunax* Mathews 1930 (New Hebrides; *nugax* Mathews 1912 being an old name of *gunax*)
 7. *dichrous* Finsch & Hartlaub 1867 (Palau, Phoenix and Christmas Islands)
 8. *polynesiae* Murphy 1927 (Samoa, Society, Marquesas and Tuamotu Islands)
 9. *subalaris* Ridgway 1897 (Galápagos Islands; *becki* Mathews 1912 being a synonym)
 10. *lherminieri* Lesson 1839 (West-Indies, Bahama and Bermuda Islands)
 11. *loyemilleri* Wetmore 1959 (Costa Rica to Guyana)]
- Persian Shearwater *Puffinus persicus* Hume 1873 (monotypic?; NW India, Iran, Kuria Muria, Gulf of Oman; seen in Israel)

Audubon's Shearwater (sensu stricto)

Puffinus [sic] *Lherminieri* Lesson, 1839, *Revue Zoologique [de Paris]*, vol. 2, no. 3: Apr. (May), p. 102 (type from 'ad ripas Antillarum' = Antilles, Straits of Florida). *P. lherminieri* (sensu lato) is usually considered a sedentary polytypic species. The species may need to be split further.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
	P			A

Persian Shearwater

Puffinus Persicus Hume, 1872 [Peters Checklist 1(2): 99 has 1873; this publication started in 1872, as stated by Vaurie: 29], *Stray Feathers, A Journal of Ornithology for India and its dependencies*, vol. 1, p. 5 (type from at sea between Guadar & Muscat, Gulf of Oman). Originally thought to be a subspecies, but now widely considered to be a full species (see e.g. *British Birds* 85 (3): 127, Sibley & Monroe 1990: 327, Heinzel, Fitter & Parslow (fifth edition): 32, CINFO 1993, Clements 4th and 5th edition, most if not all

recent checklists from the Middle East). Treated as a species by Inskipp et al. (1996, *Checkl. Oriental Reg.*), Stattersfield & Capper (2000, *Threatened Birds of the World*), contra Mayr & Cottrell (1979, 'Peters' Checklist') and del Hoyo et al. (1992, *HBW*). Often treated (as is the taxon *boydi*) as a subspecies of *P. lherminieri* (including *bannermani*), but sometimes of *P. assimilis* (Hüe & Etchécopar 1970, *Oiseaux Proche et Moyen Orient*). The relationships of *persicus* and *temptator* should be examined further. *Temptator* of Moheli Island is breeding in trees (recent adaptation to introduced predators?) and its vocalisations differ from *bailloni* which is breeding as near as Anjouan Island (only 40 km) (R.-M. Lafontaine pers.comm.).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
	P			A

Mascarene Shearwater

Puffinus atrodorsalis was described in 1995 (Shirihai et al.) but now considered to be juvenile *Puffinus lherminieri bailloni* (Bretagnolle & Attié 1996). In the same paper, the taxa *bailloni* (including *nicolae*, *bailloni* having priority) and *gunax* were given specific status. (Oscar van Rootselaar)

European Storm-Petrel *Hydrobates pelagicus*; new information is available to evaluate the validity of the Mediterranean subspecies *H. p. melitensis*.

CAF This taxon is not mentioned in Peters (Peters 1931, p 72, Mayr & Cottrell 1979, p 111), Vaurie (1965), Clements (2000), p 13 or Howard & Moore (1980, p 59; 1984, p 59; 1991, p 10) and only briefly in del Hoyo et al. (1992), p 269: 'Mediterranean population might form distinct subspecies (*melitensis*). Monotypic.' Furthermore, the name Schembri (1843) is not mentioned in Wolters (1982, 'Autoren', p 457), Anker (1990), Wynne (1969), Gijzen (1938) and Mearns & Mearns (1988, 1992, 1998). Martín & Lorenzo (2001, p 132–135) mentioned that although considered monotypic by Cramp & Simmons (1977) some authors recognise the Mediterranean population as a distinct subspecies, *H. p. melitensis* Schembri, 1843', referring to Hémery & d'Elbée (1985). Cramp & Simmons (1977), pp 163–168, indeed states: 'Geographical variation. None. Formerly recognised subspecies *melitensis* (Schembri, 1843), Mediterranean, said to be darker and larger, but differences from British birds too small to warrant subspecies recognition'. This was probably (or partly) based on Witherby et al. (1948), Vol. 4, pp 25–29: '*H. p. melitensis* (Mediterranean) has been separated but this does not appear to be justified'. The EBCC Atlas (Massa & Merne in Hagemeyer & Blair 1997, pp 24–25) states: '...whereas the larger *melitensis* is typical of the Mediterranean', referring to Catalisano et al. (1988). The subspecific status of *melitensis* was resurrected by Hémery & d'Elbée (1985) who noticed that Mediterranean Storm-Petrels showed a larger bill than Atlantic Storm-Petrels. Hémery in Lalanne et al. (2001) also pointed out that there are no ringing recoveries confirming movements from Mediterranean Storm-Petrels into the Atlantic and vice versa. The CAF recognised *melitensis* (see Dubois et al. 2000). Cagnon et al. (2000) analysed cytochrome *b* sequences and found that there is no gene flow between *melitensis* and *pelagicus* and therefore considered these two metapopulations to be at least subspecifically distinct. The results of Lalanne et al. (2001) based on discriminant analysis confirm that Mediterranean Storm-Petrels differ markedly from those of the Atlantic in wing length and bill height and indicate significant variations within Mediterranean populations (beware of the small sample size, though). Measurements of 20 birds trapped in the Catalan Mediterranean (Estrada 1988, Gutiérrez own data) are similar to the Mediterranean data in Lalanne et al. (2001) and are unlike those of the Atlantic. (Oscar van Rootselaar)

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
	A			P

Darter *Anhinga melanogaster* proposed to be treated as three species:

- African Darter *Anhinga rufa* (polytypic: *A. r. chantrei*; extralimital: *A. r. rufa*, *vulsini*)
- Oriental Darter *Anhinga melanogaster* (monotypic; extralimital)
- Australian Darter *Anhinga novaehollandiae* (monotypic; extralimital)

CSNA The AERC TAC has not yet studied the taxonomy of the darters. Relationships among the Old World darters are uncertain. Oriental Darter *A. melanogaster* is closest to Australian Darter *A. novaehollandiae* and they are often considered conspecific and sometimes not even subspecifically distinct. African Darter *A. rufa* is next closest and the Old World populations may constitute a single species. American Darter (or Anhinga) *A. anhinga* is the most distant from the others, but the entire genus may be a single species. They are treated by Sibley & Monroe (1990) as 'allospecies'. The baseline treatment of the STC is to treat *A. anhinga* as a

different species from *A. rufa*. The subspecies occurring in the Western Palearctic *A. r. chantrei* (Oustalet, 1882) (in Roselaar list) is not recognised by Cramp et al. (1977); ‘said to have foreneck paler and greater upperwing-coverts greyer than *rufa*, but these characters variable in *rufa* (Ticehurst, C.B. (1922) *Bull. Brit. Orn. Club* 42: 120–121)’. Darters are treated as three species by Vaurie (1965) *A. melanogaster*, *rufa* and *novaehollandiae*. Voous (1973), starting point for the considerations by the AERC TAC recognised two species: *A. anhinga* and *A. melanogaster* (incl. *melanogaster*, *rufa* and *novaehollandiae*). Therefore, Anhinga *A. anhinga* (polytypic; extralimital: *A. a. anhinga*, *leucogaster*) is excluded from the above list. The taxonomic position of *papua* (erroneously included in *A. rufa* by Howard & Moore 1980) needs elucidation. Is it a valid subspecies of *A. novaehollandiae*? Is Australian Darter monotypic or polytypic? What is the treatment in HANZAB? Johnsgard, P.A. (1993) *Cormorants, darters, and pelicans of the world*. Smithsonian Institution Press. Washington and London. 445 pp (ISBN 1-56098-216-0) and other relevant sources still need to be consulted by the AERC TAC. See online reference list on Anhingidae: <http://www.damisela.com/zoo/ave/otros/pelecan/anhinga/biblio.htm>

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
∅	∅			

Reef Heron *Egretta gularis*

N. Baccetti and G. Fracasso have requested to add some comments on the taxonomic treatment of this taxon as there seems to be a trend to lump it with *E. garzetta* (e.g. Clements 2000, Kushlan & Haffner 2000). Increasing records of dark egrets of unknown origin in Italy motivates their interest in the species. In the future, the AERC TAC should provide feedback to his question.

Intermediate Egret *Egretta intermedia* proposed to become *Mesophoyx intermedia*

STC DNA-DNA hybridisation data place *Mesophoyx intermedia* (= *Ardea intermedia*) and *Bubulcus ibis* closer to *Ardea* than to *Egretta* (Sheldon 1987b). Its three subspecies are widely allopatric and morphologically distinct in colour of soft parts, especially the bill and head; they may be considered as species. As *Casmerodius albus* was accepted as status quo for the unsatisfactory *Egretta alba*, *Mesophoyx intermedia* should be applied for Intermediate Egret as ad interim status quo.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
∅				P

Great White Egret *Egretta alba* proposed to become *Casmerodius albus*

STC Although Voous (1973) preferred to call this species *Ardea alba*, he maintained *Egretta alba* in his *List of Recent Holarctic Bird Species*. The inclusion of this species in *Ardea* is supported by DNA-DNA hybridisation data (Payne & Risley 1976, Sheldon 1987b, Sheldon et al. 1995 and Sibley & Monroe 1990); Great White Egret is more closely related to *Ardea* than to *Egretta*. It was listed as *Ardea alba* by Mayr and Cottrell (1979, *Peters' Check-list of Birds of the World*, Vol. 1, 2nd ed., pp 203–204). The relationships among the races are not clear. *A. a. modesta* may be a separate species, but extensive comparisons among all forms have not been done. Accepted as *Ardea alba* by AOU 1995 and in BOURC (1996). The CSNA and A.J. Helbig, however, preferred to place Great White Egret in the genus *Casmerodius* (Sangster et al. 1997; A.J. Helbig in litt.). Phylogenetic analyses based on DNA-DNA hybridisation indicate that Great White Egret is not closely related to the *Egretta* clade and instead suggest a closer relationship with *Bubulcus* and *Ardea*. However, given the unresolved relationships between *Ardea*, Great White Egret, Intermediate Egret *Mesophoyx intermedia* and Cattle Egret *Bubulcus ibis*, the CSNA believes that the inclusion of Great White Egret in *Ardea* (e.g. AOU 1995, BOURC 1997) is premature. Until the relationships of Great White Egret are better understood, the CSNA prefers to place it in a monotypic genus *Casmerodius* (cf. Inskipp et al. 1996) (Sangster et al. 1997). A.J. Helbig (in litt.) commented on his personal point of view: ‘Molecular studies confirm two monophyletic families (Sheldon et al. 2000). Great White Egret is equidistant from *Ardea* and *Bubulcus*, thus retained in a separate genus (*Casmerodius*). Relationships of Intermediate Egret (*Mesophoyx* / *Egretta*) are unknown.’ It should be noted, however, that in the field, *intermedia* shows intermediate features between *Bubulcus* and *Casmerodius*, suggesting a position in between these two taxa rather than in *Egretta*. If Great White Egret were to be placed in *Ardea*, the relationships of *Bubulcus* and *Mesophoyx* would need to be re-examined as well. In the field, Intermediate Egret shows more similarities to Cattle Egret and Great White Egret than to any of the *Egretta* species (G. De Smet, pers. observations). Although it is clear that the status quo (retaining *intermedia* and *alba* in *Egretta*) is wrong, this case clearly shows the need of *Guidelines for assigning generic status* and consultation among TCs before adopting

generic changes. The rule of ‘monophyly’ seems to be applied in various ways by different committees. In addition, more research on the genetic relationships of *intermedia* is crucial, before a satisfactory decision can be reached. With two rejections for each option (*Casmerodius albus* or *Ardea alba*) only an arbitrary decision is possible. There is agreement that Great White Egret must leave *Egretta* but no consensus that it must enter *Ardea* or *Casmerodius*. Given the uncertainty about the relationships of Intermediate Egret and the possible implications for Cattle Egret, a cautious approach is needed. Therefore, Great White Egret is provisionally placed in *Casmerodius albus* until its relationships are resolved.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
R	R	A	A	P

Lesser Flamingo *Phoenicopterus minor* suggested to become *Phoeniconaias minor*

A.J. Helbig Lesser Flamingo was placed in the monotypic genus *Phoeniconaias* by Kear & Duplaix-Hall (1975), particularly because of its more specialised bill and narrower food requirements. The distribution of feather lice among flamingos supports the retention of three flamingo genera – including *Phoenicoparrus* for Andean Flamingo – (Dowsett & Dowsett-Lemaire 1980). With two rejections, Lesser Flamingo is maintained in *Phoenicopterus* by the AERC TAC.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
∅	R		R	P

Cape Verde Islands Buzzard *Buteo buteo bannermani*

A.J. Helbig Clouet & Wink (2000) published the first results of a genetic analysis based on nucleotide sequences of the cytochrome *b* gene. The taxonomic implications of this paper need to be assessed. A molecular phylogeny of the genus *Buteo* was published by Riesing et al. (2003). The CAF stated that qualitative differences in morphology are insufficient to recognise a species.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
	P			P

Lesser Spotted Eagle *Aquila pomarina*

suggested by Parry et al. (2002) to be treated as two species:

- Lesser Spotted Eagle *Aquila pomarina* (monotypic)
- Indian Spotted Eagle *Aquila hastata* (monotypic)

CSNA Parry et al. (2002) suggest to recognise *A. pomarina* of W Eurasia and Africa, and the resident *A. hastata* of the Indian subcontinent as distinct species by presenting observations and measurements on hundreds of specimens of both, as well as the closely related Greater Spotted Eagle *A. clanga*. Differences between these two allopatric taxa demonstrate that they should be treated as distinct species. Specifically, Parry et al. (2002) present (1) evidence of differences in plumage (both for adults and juveniles), (2) external morphology, (3) osteology, (4) clutch size and (5) behaviour. Particular emphasis is placed on differences in gape size and general cranial structure. Lesser and Indian Spotted Eagle differ considerably in plumage. *A. pomarina* has a pale head contrasting with darker back, and a rufous nape patch. *A. hastata*, on the other hand, has head and back uniformly dark brown, and no nape patch. Adult iris colour also differs: brown in *hastata*, as opposed to yellow or amber in *pomarina*. There are also clear differences between these taxa in skull and bill measurements. More useful evidence that *pomarina* and *hastata* should be treated as different species comes from a comparison with *A. clanga*. The most striking difference is in gape width, which is greatest in *hastata*, intermediate in *clanga*, and smallest in *pomarina*. Because *hastata* and *pomarina* are less similar to one another than each is to *A. clanga* (a different species), all three should be considered separate species. Lesser Spotted Eagle *A. pomarina* is then distinct from *A. hastata*, for which the authors suggest the English name ‘Indian Spotted Eagle’.

Remark: Lesser Spotted Eagle *A. pomarina* is closely related to Greater Spotted Eagle *A. clanga* (Bergmanis 1996, Meyburg 1974, Meyburg et al. 1999, Wendland 1959, Zhezherin 1969). It has been supposed that a separation between the mitochondrial lineages of Lesser and Greater Spotted Eagle could have occurred slightly less than one million years ago, assuming a substitution rate of 2% per million years for mitochondrial genes (Seibold et al. 1996). Although there is a large area of overlap of the two species there are only very few cases known where members of both species seem to have formed a mixed pair (Bergmanis et al. 1997). A. Helbig considers *A. [c.] clanga* and *A. [c.] pomarina* as semispecies.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC

∅	A			A
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Little Tern *Sterna albifrons* to be treated as two species:

- Little Tern *Sterna albifrons* (polytypic: *S. a. albifrons*, *guineae*; extralimital: *S. a. sinensis*)
- Least Tern *Sterna antillarum* (polytypic: *S. a. antillarum*; extralimital: *S. a. athalassos*, *browni*)

STC Least Tern was described from the West Indies by Lesson (1847). It was considered specifically distinct from Little Tern of Europe, *Sterna albifrons* [Pallas] 1764, and named *Sternula antillarum*. For the recognition of the genus *Sternula*, see Timmerman (1957), *Parasit. Schriftenreihe* 8: 183. In 1921 Hartert combined the two species (Hartert 1921). In 1983 the taxonomy was again revised, and Least Tern was restored to the status of a full species (AOU 1983) based on research that documented differences in vocalisations and morphology (Massey 1976). Three subspecies of Least Tern have been recognised in the U.S. – *antillarum* on the east and gulf coasts, *athalassos* in the Mississippi drainage system, and *browni* on the West Coast (AOU 1957). Least and Little Terns show assortive breeding behaviour on Midway atoll, confirming their specific status (Pyle et al. 2001). The first record in the Western Palearctic is described by Yates & Tafts (1990), but this record has not yet been accepted by the BOURC (*Ibis* 139: 200, 1997). One of the reasons in the delay of acceptance of this record is the study of *S. albifrons guineae*. The vocalisations of this taxon, however, are similar to *albifrons* and not to *antillarum* (unpublished recordings from Senegal by D. Vangeluwe). An immature Least or Little Tern was photographed on Flores, Azores in October 2002 by T. Frandsen.

ID: For a complete description of plumages, both adult and juvenile, see Massey (1978) and Olsen & Larsson (1995).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
P			A*	A

(*) Accepted as ‘semi- or allospecies’.

Laridae: P.-A. Crochet proposes a change of the generic treatment of the Larini

CAF The current generic treatment of the Laridae does not reflect true relationships (Crochet et al. 2000, Pons & Crochet in prep.). Recognising *Rissa*, *Pagophila*, *Xema* and *Rhodostethia* while keeping all other species in *Larus* is not consistent with the species relationships. Contra to comments from Helbig, outgroups were included in our analyses (two terns and Dunlin in Crochet et al. 2000, two terns, Dunlin and Black Oystercatcher in Pons & Crochet submitted). Our favoured solution is to recognise additional genera (Crochet et al. 2000). The alternative treatment is to lump all taxa into *Larus*. This solution was proposed in earlier versions of the recommendations to minimise nomenclatural changes. Since then, additional opinions have been received, but none of them is advocating lumping all taxa in *Larus*. We are aware that changing generic names of many species is undesirable for practical purposes, but unless we want a classification which is more ‘easy to remember’ than scientifically founded, there is no alternative for the classification of gulls other than lumping several genera in *Larus* or moving many species to additional genera. We thus propose the following classification of Western Palearctic Laridae:

Pagophila eburnea
Xema sabini

eburnea and *sabini* are sister species (strong bootstrap support under MP and distance methods, very high probability under bayesian approaches). For reasons to maintain two genera, see Crochet et al. (2000).

Rissa tridactyla
Hydrocoloeus roseus
Hydrocoloeus minutus

Roseus and *minutus* are sister species. This is in agreement with their overall morphology and immature plumage, and is supported by high bootstrap in parsimony (78) and very high probability in bayesian approaches. There is probably not enough divergence among them to maintain separate genera, in which case *Hydrocoloeus* Kaup 1829 (note the correct spelling) has priority over *Rhodostethia* Mac Gillivray 1842.

Chroicocephalus philadelphia
Chroicocephalus ridibundus
Chroicocephalus cirrocephalus

Chroicocephalus genei

The small hooded species form a well-supported monophyletic group. In none of the analyses is this group the sister group of the *Larus* clade. It is either identified as the most divergent group of gulls or is grouped with *Rissa*, *Hydrocoloeus*, *Pagophila* and *Xema*. If these genera are recognised, the small hooded species cannot be retained in *Larus*.

The remaining species form a monophyletic group in all analyses. This group includes *marinus* (the type species of *Larus*). They could all be included in *Larus*, although the amount of divergence (genetic, morphological, and behavioural) among them is similar to the divergence among the other genera of gulls. Best treatment is therefore:

Atricilla atricilla
A. pipixcan

Genus *Atricilla* Bonaparte 1854 seems to be the oldest available name for this group.

Ichthyaetus melanocephalus
I. ichthyaetus
I. leucophthalmus
I. hemprichii
I. audouinii

Genus *Ichthyaetus* Kaup 1829 seems to be the oldest available name for this group. Both groups are strongly supported by all methods of analyses. The remaining species are the true *Larus*. (P.-A. Crochet)

There is no consensus about this in the AERC TAC. Voous (1973) recognised the genera *Larus*, *Rhodostethia*, *Rissa* and *Pagophila* within the Holarctic Larini. It is certain that the current generic treatment of the tribe Larini is wrong. Either all gulls should be placed in the genus *Larus*, or more subgenera should be elevated to genus rank. A.J. Helbig (in litt.) commented: 'The only molecular study of overall relationships within this family (Crochet et al. 2000) was based on rather short mtDNA sequences. However, it strongly indicates separation of *Xema* from *Larus* and distinctness of *Rissa*. Monophyly of *Larus* was not tested (no outgroup), but strong divergence among two groups of "hooded" species suggests that species with a blackish hood (incl. some that lost it secondarily) are basal within the genus. *Larus dominicanus* is derived within the *fuscus* group (Liebers et al., submitted).' P.-A. Crochet replied: 'I don't see how we can maintain the current classification. It is a matter of choice to recognise more genera or to use *Larus* for all species, but keeping *ridibundus* and *argentatus* in the same genus while using *Rissa* for *tridactyla* is just not tenable on scientific grounds.'

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
	A*		R	P

(*) CAF accepts the proposal of Pierre-André Crochet. If the AERC TAC does not agree on elevating the subgenera mentioned above to generic rank, CAF would prefer to put all gull taxa in *Larus*.

Lesser Black-backed Gull *Larus fuscus* on current knowledge to be treated as a single species, comprising *L. f. graellsii*, *intermedius*, *fuscus*, *heuglini* and *barabensis*.

CAF The AERC TAC is still awaiting some important publications in the primary literature and provisionally retains the following Western Palearctic taxa within *L. fuscus*: *graellsii*, *intermedius*, *fuscus*, *heuglini* and *barabensis*. Some AERC TAC members favour a further split of Lesser Black-backed Gull *L. fuscus* (with subspecies *graellsii*, *intermedius* and *fuscus*) and Heuglin's Gull *L. heuglini* (with subspecies *heuglini*, *taimyrensis* and *barabensis*). Yésou (2002) summarised the reasons for such a treatment, but did not recognise *taimyrensis* as a valid taxon. The CSNA split Lesser Black-backed Gull further into two species: Baltic Gull *L. fuscus* and Lesser Black-backed Gull *L. graellsii* (Sangster et al. 1998). There is a wide consensus, however, to include five Western Palearctic taxa in the *fuscus* group: *L. f. graellsii*, *intermedius*, *fuscus*, *heuglini* and *barabensis* (Yésou 2002); as Voous (1977c) did not mention any subspecies, this arrangement is proposed as status quo. The taxonomical validity of the extralimital *taimyrensis* (generally included in the *fuscus* group) is uncertain. (texts on *Larus fuscus* complex by P.-A. Crochet and P. Yésou).

Baltic Gull *L. f. fuscus*

The split of Baltic Gull by Sangster et al. (1998) was contradicted by many authors (e.g. Jonsson 1998a, Liebers et al. 2002, and Yésou 2002). Within *L. fuscus* there is no indication of any gene flow barrier between the subspecies *L. f. graellsii*, *L. f. intermedius* and *L. f. fuscus* (Liebers & Helbig 2002). Phenotypic differentiation of *L. f. fuscus* is probably due to strong directional selection related to feeding and migration strategy. The field study of *L. f. intermedius* from Norway passing through the Netherlands and Belgium (including colour-ringed birds) show that these can show some or all features supposedly diagnostic of *L. f. fuscus* (at least until 2nd summer) (Adriaens 2002). In Groningen, the Netherlands, *L. f. fuscus*-like birds are recorded with some regularity but *L. f. intermedius* showing *fuscus*-like moult patterns occur, confusing the picture (Winters & Bakker in prep.). The criteria outlined by Jonsson (1998a) to identify *L. f. fuscus* are insufficient to identify such individuals. Unless they are colour-ringed, it may be impossible to tell out-of-range *L. f. fuscus* reliably from the most *fuscus*-like *L. f. intermedius*. Since the groundbreaking publications by Barth (1968, 1975) it still needs to be established firmly which taxa are breeding in Norway.

ID: For identification of Baltic Gull, see e.g. Jonsson (1998a) and Gruber (1999); see also references listed in Adolffsson & Cherrug (*Bird Identification*, pp 164-169) for gull identification in general.

Is Baltic Gull *L. fuscus* (monotypic) a distinct species (*L. f. fuscus* of Voous)?

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
P	R	A	R	R

Heuglin's Gull *L. f. heuglini*

Heuglin's Gull *L. f. heuglini* is well differentiated from Baltic Gull *L. f. fuscus* in the contact zone; the extent of possible interbreeding, however, is unknown. V. Rauste (in litt.) comments: 'There are apparently growing numbers of records in Finland of birds which show more or less clearly intermediate characters between *fuscus* and *heuglini*', indicating possible interbreeding. Haplotype frequencies among the five northern taxa are said to form a stepped cline with significant gene flow restriction between the forms *heuglini* and *fuscus*, probably indicating a secondary contact with (partial?) reproductive isolation (Liebers & Helbig 2002). Nevertheless, the genetic data are not as clear cut as Liebers & Helbig suggest in their discussion. Their samples of *fuscus* from C and E Finland are in fact closer to *heuglini* than *intermedius* in term of haplotype frequencies, and their samples of *fuscus* from W Finland have about 50% with *graellsii* (western) haplotypes, and 50% with *heuglini* (eastern) haplotypes. In terms of genetic estimates, gene flow is higher between *fuscus* and *heuglini* (4.71 migrants per generation) than between *fuscus* and *intermedius* (2.86 to 4.01 migrants per generation). There is thus a pattern of isolation by distance, with amount of gene flow determined by distance between populations and not by taxonomic affinities. This is why *heuglini* and *fuscus* are retained as conspecific here.

Steppe Gull *L. f. barabensis* is clearly very closely related to *heuglini*. These taxa are poorly differentiated in mtDNA (Liebers et al. 2001) and differ mainly in size and adult mantle colour, as in the case of *fuscus* and *graellsii*. Based on vocalisations and behaviour, Buzun (2002) reached the same conclusion about the close relationships of *heuglini* and *barabensis*. These two taxa occupy a very different habitat: Heuglin's Gulls are breeding on the tundra and Steppe Gulls breed in reed marshes on steppe lakes. The juvenile plumage of *barabensis* is said to differ quite markedly from the juvenile plumage of Heuglin's and Lesser Black-backed Gulls (P. Yésou, pers.comm.). The taxon *barabensis* is most likely recently derived from *heuglini* and phenotypic differences quickly evolved by selection in a new environment. Genetic introgression from *cachinnans* is also evident from mitochondrial data and might have played a role in the evolution of its distinct phenotype.

The taxonomic position and validity of the extralimital taxon *taimyrensis* is still a matter of debate.

ID: For identification of Heuglin's and Baltic Gulls, see e.g. Rauste (1999a,b).

Is *L. heuglini* (incl. *heuglini*, *taimyrensis* and *barabensis*) a distinct species?

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
P	R*	A	A**	P***

(*) On present knowledge CAF prefers not to accept *L. heuglini*, as in *fuscus* haplotypes of both *graellsii* and *heuglini* are present.

(**) Accepted as 'semi- or allospecies'.

(***) Until more information is available, the STC accepts to keep all the above forms together.

Thayer's Gull *Larus thayeri* suggested to remain as is;

Iceland Gull *Larus glaucooides* suggested to become monotypic.

Traditionally, Iceland Gull is composed of two subspecies, nominate *glaucoides* and *L. g. kumlieni*; following that approach, birds that appear intermediate between Kumlien's and Thayer's are called hybrids. Based on historical evidence, however, Weir et al. (2000) suggest that 'kumlieni' may be best treated as a hybrid swarm between Thayer's Gull and Iceland Gull. This paper is based on journals and museum documents from historical and more recent expeditions to the Arctic, examination of museum skins and includes about 124 cited references (which see). 'Three Iceland Gull taxa were defined mainly from adult wingtip melanism. Up until about 1860, nominate *glaucoides* (no melanism) was known to breed from Greenland to W High Arctic Canada, but by about 1900 it was essentially confined to Greenland. Until 1860, *thayeri* (most melanism) was known only from W High Arctic Canada, but from 1900 to 1980 it was found throughout High Arctic Canada and a small part of NW Greenland. At high latitudes in Canada it replaced *glaucoides*, with which it was formerly sympatric in the west and probably interbred. The first known *kumlieni* (intermediate, variable melanism) were from West Greenland in the 1840s, and by 1900 the western and northern limits of most of its breeding range in the E Canadian Arctic were known. The range of *kumlieni* lies between those of *thayeri* and *glaucoides* and overlaps both: *kumlieni* bred in Greenland by 1964. It freely interbreeds with *thayeri* and probably with *glaucoides*. Winter ranges of *glaucoides* and *thayeri* have changed little since they were first determined for *glaucoides* by 1860 and for *thayeri* by the 1920s. However, winter adult *kumlieni* was unknown from Greenland to the British Isles until 1900; there were a few records prior to 1915 and progressively more after 1950. The study adds to the evidence that *kumlieni* represents introgressive hybridisation by western *thayeri* into eastern *glaucoides*.' Mind, however, that Voous (1977c) recognised *L. thayeri* and *L. glaucoides*. Banks & Browning (1999) pointed out a number of questions that need to be addressed regarding the taxonomy of Thayer's Gull. Caution is also needed with the published molecular results (Crochet et al. 2002) based on a single specimen of 'thayeri' collected in Louisiana. P.-A. Crochet commented: 'I had not realised at the time when I requested this specimen (LSUMZ B-21816) that it was out-of-range and of contentious ID. We have now sequenced five more specimens of *thayeri* from California and the Pacific coast. A short note should appear soon in *the Auk* to complete our previous paper. None of these specimens group with our previous "thayeri". None groups with *glaucoides* or *kumlieni* either... but with *glaucescens*. Taxonomy of white-winged gulls needs more research.'

ID of Thayer's Gull: Garner & McGeehan (1998) (on juvenile and firstwinter plumage); Howell & Elliott (2001) (on variation in adult wing-tip pattern with some taxonomic comments)

ID of Kumlien's Gull: Howell & Mactavish (2003)

Kittiwake *Rissa tridactyla* suggested by Chardine (2002) to become polytypic:

BOURC TSC Chardine (2002) has recently re-examined geographic variation in the Kittiwake and confirmed that there are differences between the Atlantic and Pacific populations. Indeed, there is a complete (diagnostic) discontinuity in the amount of black in the wing tip, and a variety of other (overlapping) character differences. These differences are sufficient to recognise Kittiwake as a polytypic species with two subspecies *R. t. tridactyla* and *R. t. pollicaris*; Chardine (2002) even suggested that these taxa could be treated as different species, at least under the PSC. Only nominate *tridactyla* has been shown to occur in the WP. (Currently treated as monotypic by the BOURC TSC) For the AERC TAC, this merely implies a status quo, as both subspecies are recognised in BWP III. (Martin Collinson)

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	P		P

African Collared Dove *Streptopelia roseogrisea*

N. Baccetti: 'Why has not Linnean *risoria* (used for domestic birds) a complete priority over *roseogrisea*? *Gallus gallus* was also originally described on domestic forms, but the same binomen is in use for wild birds.' M. Gosselin commented: 'Names "based on domesticated animals" are specifically included in the scope of the ICZN Code (Art. 1.2.1). However, just a few weeks ago, ICZN (Opinion 2027) has ruled that the names of 17 wild species have precedence over the names their domestic derivatives even though the latter are older names (e.g. *Felis silvestris* now replaces *Felis catus*). I haven't read the ruling yet [I have only seen its summary on the ICZN web site], but I intend to do it a.s.a.p. Although the dove is not among these 17 taxa, the ruling may give some clues as to how to proceed in such a case. Notice that the name of the Rock Pigeon is *Columba livia* Gmelin 1789, not *Columba domestica* Linnaeus 1758. In short, I have no opinion for the moment.'

Yellow-billed Cuckoo *Coccyzus americanus* to be treated as monotypic (Banks 1988, 1990; BOURC 1991).

BOURC TSC cf. Banks (1988) and Banks (1990) for details. BWP IV recognises extralimital *C. a. occidentalis* next to nominate *americanus*. There is one A missing for this decision to be included.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	P			A

Eagle Owl *Bubo bubo* suggested by Wink & Heidrich (1999) to be treated as two species:

- Eagle Owl *Bubo bubo*
- Pharaoh Eagle Owl *Bubo ascalaphus*

CSNA This will be further investigated before final recommendation. In particular, the apparently rather distinct taxon *desertorum* needs to be properly assessed in relation to the other two. There is supposedly a contact zone between the *bubo* group and the *ascalaphus* group in the Middle East and in N Africa. In the Middle East, Shirihai (1996) mentions intergradation. There is no precise information on N Africa. There are, moreover, important phenotypical differences between *ascalaphus* and ‘*desertorum*’, which may indicate a cline (or at least progressive variation) from the north towards desert areas. It seems, however, difficult to make a clear distinction between these two groups. A study of past (with *B. b. hispanus*) and present interbreeding (with *B. b. interpositus*) of *ascalaphus* would be welcome. Additionally, more information is needed on the other small desert forms (*B. b. nikolskii* and *B. b. omissus*). An analysis of plumage variation, biometry, vocalisations and molecular data of the entire complex is desirable. Wink & Heidrich (1999) studied mtDNA of *B. Bubo*, *B. ascalaphus*, *B. nipalensis*, *B. magellanicus*, *B. africanus*, *B. bengalensis*, *B. sumatrana* and *B. lacteus*. According to phylogenetic relationships and distances these are all distinct species. In this analysis, nucleotide substitutions differ by 3.5% between *B. bubo* and *B. ascalaphus*. Moreover, *B. b. interpositus*, which is morphologically distinct from *B. b. bubo* and lives in the Israeli desert, is also genetically distinct (distance 2.8%). Notice, however, that *interpositus* has a much wider range (western and northern shores of the Black Sea, Turkey, and from the Levant to NW Iran and the Caucasus area) (Roselaar 1995). Since a sequence divergence of more than 1.5% is indicative of species level (exceptions occur!), the authors regard it justified to treat both taxa as distinct species particularly if supported by morphological and acoustic evidence. Helbig & Barthel in Svensson et al. (2000) supported this split. If, however, *interpositus* is as distinct as *ascalaphus* from *bubo*, one may question a split in two species. Genetic divergence within species is not always evidence for reproductive isolation. A population level approach is required. Also the reports of intergradation by Shirihai (1996) require more research. The World Owl Trust http://www.owls.org/Species/bubo/savigneys_eagle_owl.htm is working to determine the exact taxonomy of Eagle Owls in the Middle East. Researchers of the World Owl Trust consider that morphology, vocalisations, and mtDNA of *B. Bubo* and *B. ascalaphus* are distinct. They include *desertorum* as a subspecies of *B. ascalaphus* and define the ranges as follows:

(1) *B. a. ascalaphus*: N Africa from Morocco to Egypt, Sinai, Israel and Palestine.

(2) *B. a. desertorum*: W Iraq, Arabia and S Sahara from Ethiopia and Sudan to Mali.

Peter Symens has commented that in Saudi Arabia, *ascalaphus* is restricted to E Saudi Arabia, whereas *desertorum* is occurring in C Saudi Arabia. *B. africanus* is occurring there in the South-West (and may spread into the desert during wet years). He has the impression that *desertorum* and *ascalaphus* may well be separated geographically in Saudi Arabia. In Saudi Arabia, P. Symens has only found ground nests of *ascalaphus* (often situated near hillocks in the desert).

Guy Kirwan commented on the situation in Turkey, regarding recent claims of intergradation in that country by Hadoram Shirihai: ‘Most, if not all, are apparently *interpositus*. König et al. (1999) remarked that DNA evidence suggested that *interpositus* was sufficiently distinct to be recognised at species level, and considered the forms *ascalaphus* and *desertorum*, which have traditionally been placed in *Bubo bubo*, as a separate species, Pharaoh Eagle Owl *B. ascalaphus*. However, the relationships between *ascalaphus* and *desertorum* clearly merit further research. The DNA results for *interpositus* are interesting, but thus far do not appear to be supported by other data, e.g. vocalisations, unlike the case for *B. ascalaphus*. Given that Roselaar (1995) posited that *interpositus* might grade into *nikolskii* in SE Turkey and that H. Shirihai (in *Birding World* 9: 218) suggested that those at Birecik appeared to represent intergrades between *interpositus* and *desertorum* (though photographs from this area examined by us do not appear to support this proposition), we prefer to await additional evidence before making any judgement over the taxonomic position of Turkish populations.’

Molecular information on owls was published by Wink & Heidrich (1998, 1999, 2000a,b).

This is a clear case where further research is needed.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
P	P	A	A*	P

(*) Accepted as 'semi- or allospecies'.

Little Owl *Athene noctua* suggested by Wink & Heidrich (1999) to be treated as two species:

- Little Owl *Athene noctua* (polytypic: *A. n. noctua*, *vidalii*, *glauca*, *indigena*, *bactriana*; extralimital *A. n. orientalis*, *ludlowi*, *plumipes*, *spilogastra*, *somaliensis*)
- Desert Little Owl *Athene lilith* (monotypic)

CAF Wink & Heidrich in König et al. (1999) analysed the phylogeny of the owls (*Strigiformes*) based on the analysis of mtDNA and noticed that within *A. noctua*, two genetic clusters are apparent, and are supported by high bootstrap values; genetic differences between both groups account for 6.4% nucleotide substitutions, a value more typical of specific divergence in avian taxa. Both clusters (one with samples collected in Europe, the other with samples from Israel and Turkey) might represent distinct species (*A. noctua* and *A. lilith*). In this case, also morphological and acoustical differences should exist between both forms. The song of *lilith* is said to differ in lacking the upward inflected 'interrogative' character of *A. noctua* (König et al. 1999). This split was supported by Helbig & Barthel in Svensson et al. (2000). All these elements should be further evaluated before final recommendation. Contact zones between the *lilith* group and the *noctua* groups (in Turkey for example) should be better investigated. Examination of museum specimens would be a first step.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
P	P	P	R?	P

Alpine Swift *Apus melba* proposed to become *Tachymarptis melba* by Brooke (1972)

CSNA The generic affinities of Alpine Swift need to be clarified. Brooke (1972) justified the separation of *Tachymarptis* from *Apus* because of the difference in nestling foot structure, larger size and different *Mallophaga* (feather lice) There were some proponents (e.g. Fry et al. 1988, Chantler & Driessens 1995) and opponents (e.g. Snow 1978, Dowsett & Dowsett-Lemaire 1980, Cramp et al. 1985, Sangster et al. 1997) of this generic change. On current knowledge, there is no evidence for a sister relationship between Mottled Swift *A. aequatorialis* and Alpine Swift and monophyly of the other species traditionally placed in *Apus* (Sangster et al. 1997). More information is needed to decide and in the meanwhile the genus name of Alpine Swift should remain unchanged.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
R	A	R	R	P

Lesser Spotted Woodpecker *Dendrocopos minor* proposed to become *Picoides minor* by Weibel & Moore (2002)

CAF If we split the black and white woodpeckers, Lesser Spotted Woodpecker is not a *Dendrocopos*. It is closely related to North American woodpeckers such as Downy Woodpecker *Picoides pubescens*, Nuttall's Woodpecker *P. nuttalli* and Ladder-backed Woodpecker *P. scalaris*. There are two solutions: either all black and white woodpeckers should be placed in *Picoides* (AOU 1983) or *minor*, *pubescens*, etc. should be placed in a separate genus (yet to be defined). Mind, however, that the AOU later added Great Spotted Woodpecker (a vagrant to Alaska) to their list under the name *Dendrocopos major*, which seems inconsistent with their earlier treatment. If we maintain *Dendrocopos* for most European species, then only *tridactylus* should stay in *Picoides*. The genus name for *minor* is not *Picoides*. (P.-A. Crochet)

Picoides is the largest genus of woodpeckers and member species are found on most major land masses. Current systematic arrangement of this group, based on morphological, behavioural, and plumage characters, suggests that New World species evolved from a single invasion by a Eurasian common ancestor and that all New World species form a monophyletic group. No clear link has ever been established between the relationships of Old World and New World species other than to infer that the most primitive species is Eurasian. Weibel & Moore (2002) employ DNA sequences for two protein-coding mitochondrial genes, cytochrome oxidase I and cytochrome *b*, to reconstruct phylogenetic relationships among all New World species and several Eurasian representatives of the genus *Picoides*. A well-resolved mitochondrial gene tree is in direct conflict with proposed species relationships based on non-genetic characters; monophyly among New World species is rejected, the evolution of New World species likely resulted from as many as three independent Eurasian invasions, and *Picoides* is paraphyletic with two other woodpecker genera, *Veniliornis*

and *Dendropicos*. These results strongly suggest that this large, cosmopolitan genus be in need of systematic revision in order to reflect evolutionary history.

This generic change was recently proposed to the AERC TAC (31 July 2003). Hence, there are no votes yet.

The CAF will oppose to any changes until the phylogeny of the group is resolved.

Lesser Short-toed Lark *Calandrella rufescens* treated by several Russian taxonomists, e. g. Stepanyan, and by Roselaar (1995), as two species:

- Lesser Short-toed Lark *Calandrella rufescens* (polytypic: *C. r. rufescens*, *apetzii*, *minor*, *heinei*, *persica*, *aharonii*)
- Asian Short-toed Lark *Calandrella cheleensis* (polytypic: *C. ch. leucophaea*; extralimital: *C. ch. cheleensis*, *kukunoorensis*)

STC Alström, Mild & Zetterström (in press: *Larks of Europe, Asia and North America*) suggest that the alleged sympatry between *heinei* and *leucophaea* needs to be confirmed and the phylogeny of all taxa needs to be reconstructed before *C. rufescens* sensu lato is divided into two or more species (P. Alström is currently working on the phylogeny). Following taxa should therefore currently be kept together as one species under *C. rufescens*: *rufescens*, *apetzii*, *minor*, *heinei*, *persica*, *aharonii*, *leucophaea*, *kukunoorensis* and *cheleensis*. *C. cheleensis* is sometimes separated from *C. rufescens* due to supposed overlap in breeding range of *C. r. heinei* and *C. ch. leucophaea* in Transcaspia (Stepanyan 1967, 1990). Roselaar (1995) includes 'niethammeri', restricted to the barren fringes of salt and soda lakes of the Turkish Central Plateau in *C. cheleensis*. L. Svensson commented: 'Judging by morphology, of the Asian taxa in this series, only *leucophaea* is distinct (and may well be a separate species on this account, and on published evidence in Kazakhi literature of sympatric breeding S and SW of Lake Balkhash, near River Ili). To me, there is very little difference between e.g. *aharonii*, *heinei* and *cheleensis* as to morphology, behaviour and vocalisation. Differences in wing formula between these can largely be credited to differences in migration habits. Note that *leucophaea* seems to be extremely rare or even extinct now, or it fluctuates in numbers, or lives partly nomadic. It was impossible to find it during a three weeks trip around Lake Balkhash May 2003, in spite of guided efforts to locate it.'

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
P	P	A	R	R

Red-rumped Swallow *Hirundo daurica* suggested by Winkler & Sheldon (1993) to become *Cecropis daurica*

A.J. Helbig Winkler & Sheldon (1993) first adopted this treatment based on nest structure. P.-A. Crochet commented: 'The species used in Sheldon et al. (1999) is *Cecropis semirufa*, not *daurica*. Is there strong evidence that *semirufa* and *daurica* are closely related? This treatment suggests that *fuligula* (and *rupestris*?) are more closely related to *rustica* than *daurica* is to *rustica*. A bit hard to swallow (!) given the voice similarity and similarity in structure and colours between *rustica* and *daurica*. Given the limited taxon sampling in Sheldon et al. (1999) and the conflict between the trees based on DNA-DNA hybridisation and cytochrome *b* sequences, I wonder whether this change is ripe. An alternative treatment is to merge all species of the "Hirundo" clade within the genus *Hirundo* until more taxa are analysed. The only change required would thus be to move *urbicum* to *Hirundo*.' *Cecropis* is the name of the daughters of Cecrops (founder of Athens), hence feminine (cf. Jobling 1991). Thus: *Cecropis daurica* (M. Gosselin in litt.). M. Gosselin commented: 'In my opinion, merging all light-rumped swallows that build enclosed mud nests (*Delichon*, *Petrochelidon*, *Cecropis*) into one genus (*Cecropis*) is just as good an option.'

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
	P		A	P

American Cliff Swallow *Hirundo pyrrhonota* suggested by Sheldon et al. (1999) to become *Petrochelidon pyrrhonota*

A.J. Helbig (in litt.) commented: 'To avoid paraphyly of the genus *Hirundo*, *Cecropis* and *Petrochelidon* have to be accepted as separate genera (otherwise *Delichon* would have to be merged into *Hirundo* as well; cf. Sheldon et al. 1999). The AERC TAC did not yet discuss this.'

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
	P		A	P

Yellow Wagtail *Motacilla flava* suggested by Sangster et al. (1998) to be split in eleven species

- Blue-headed Wagtail *Motacilla flava* (incl. ‘*beema*’)
- Yellow Wagtail *Motacilla flavissima* (monotypic)
- Grey-headed Wagtail *Motacilla thunbergi* (monotypic)
- Spanish Wagtail *Motacilla iberiae* (monotypic)
- Ashy-headed Wagtail *Motacilla cinereocapilla* (incl. ‘*pygmaea*’)
- Black-headed Wagtail *Motacilla feldegg* (monotypic; incl. ‘*melanogrisea*’)
- Yellow-headed Wagtail *Motacilla lutea* (monotypic)
- Green-headed Wagtail *Motacilla taivana* (monotypic)
- Kamtchatka Wagtail *Motacilla simillima* (monotypic)
- Alaska Wagtail *Motacilla tschutschensis* (monotypic)
- White-headed Wagtail *Motacilla leucocephala* (monotypic)

STC Until more research is done on this complex it was decided to keep all the taxa combined as one species. A review of the taxonomy of *M. flava* is given by Alström & Mild (2003). DNA data were published by Ödeen & Alström (2001) and Alström & Ödeen (2002). One should notice, however, that the tree obtained by analysis of nuclear DNA differs markedly from the one obtained by analysis of mtDNA. Phylogenetic trees may reflect limitations of research methods rather than true evolution. This shows that one must be very careful to use ‘(non-)monophyly’ in taxonomic decisions. In fact, more and more biological species are suggested to be paraphyletic and paraphyly may well be a normal event in biological species. Alström and Mild (2003) consider that there is a deep split between, on the one hand, *taivana*, *macronyx* and *tschutschensis* and on the other hand, all the others (position of *leucocephala* unknown, but thought to belong to the western group). A.J. Helbig (in litt.) prefers to treat the *Motacilla* [*flava*] complex as a ‘superspecies’, because there appear to be clear barriers to gene flow (no clinal variation) wherever two of their component taxa come into contact. The ‘superspecies concept’ is not supported by the rest of the AERC TAC.

P.-A. Crochet commented: ‘Most of these “species” are clearly not species. *Flava* and *thunbergi* seem to hybridise along a very broad hybrid zone with no sign gene flow restriction. The situation with *iberiae* and *cinereocapilla* is similar in S France. Whereas in the case of the crows, the hybrid zone is narrow, which suggest some kind of reduction of gene flow (but not strong and not very clear), there does not seem to be such barriers between most of the subspecies of Yellow Wagtail. There might be more than one species in the *flava* complex, but even the situation of the contact zone between *flavissima* and *flava* (one of the cases where reproductive isolation seems to be more apparent) is far from clear.

Comments on paraphyly: gene trees and species trees should not be mixed. In a tree based on mtDNA, relationships between the DNA genes, not the taxa, are reproduced. Discrepancies between reconstructed gene trees and real taxon trees are twofold. Firstly, the phylogenetic methods might fail to recover the real gene tree based on the available data. This is inherent to the analytical procedures and the mode of DNA evolution (random process with probabilistic evolution, rate variation among lineages, etc...). Secondly, the true gene tree and the true taxon tree might differ because the coalescence process is also a random process, because there might be exchange of genes among taxa, etc... Thirdly, we have to keep in mind that seeing evolution as a tree process is only valid when there is no gene flow: taxa are fully isolated once they diverged. Most analysis of low-level divergence (such as taxa within the *flava* complex) study situations where evolution is reticulate: there is ongoing exchange of genetic material among taxa. In this case, genetic divergence can be influenced more by current level of gene flow than by time since divergence. A “tree” will then not reflect history but level of genetic exchange. When using a mtDNA tree to reconstruct taxa evolution, we all make several assumptions which allow to bet that the mtDNA gene tree is indeed the taxon tree. These assumptions are more likely to be true as the divergence time between the taxa and their reproductive isolation increase. For genus and good, old biological species, these assumptions are not too risky. For recently diverged and/or not fully isolated taxa, these assumptions are more likely to be false than true. It means you cannot interpret a mtDNA tree between genera the same way as a tree between subspecies.’

G. Fracasso and N. Baccetti commented on the breeding of *feldegg* in Italy: ‘*M. f. cinereocapilla* is widespread as a breeding bird in the whole Po plain of N Italy, along the northern coast of the Adriatic (Friuli, Venetia and Emilia-Romagna) and the central coast of the Tyrrhenian Seas (Tuscany). More or less isolated and small populations are present in the main valleys of C Appennine and along the Sardinian coasts. We know only very few breeding sites in the South of the Italian Peninsula (Apulia, Basilicata and

Calabria) and in Sicily. Sporadic records of breeding *M. f. feldegg* (usually male birds with food for, supposedly, nestlings) are anecdotally reported for scattered localities in eastern regions of N (Friuli, Emilia) and C Italy (Marche), but in western ones too (Tuscany and Sicily). A small *feldegg* population is known to breed regularly in the marshy areas to the north (Varano and Lesina lakes) and south (near Manfredonia) of the Gargano promontory (Apulia, SE Italy). Recent (but unsystematic) observations (N. Baccetti et al.) confirm that the only wagtails breeding there (but in small numbers) are apparently all *feldegg*. On the ground of the more recent published maps (P. Bricchetti), this population is not clearly isolated from *cinereocapilla* populations, supposedly breeding in a, more or less, continuous range from the nearby valley of Fortore river to inner Molise and C Appennine, but detailed information about the plumages of pairs nesting in this contact (or very near) zone are missing.’

The distribution and interbreeding of Yellow Wagtail taxa in France is described by Dubois (2001a).

Pavlova et al. (2003) found that there seems to be sympatric breeding of two types of Citrine wagtails *Motacilla citreola* in S Russia (*quassatrix* and *werae*), with each morphotype associated with very divergent mtDNA lineages. This paper might also have important implications for the systematics of the *flava* complex.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
	R	A	A*	P

(*) A.J. Helbig recognises seven allospecies in Europe.

White Wagtail *Motacilla alba* suggested by Sangster et al. (1998) to be split in nine species:

- White Wagtail *Motacilla alba* (incl. ‘*dukhunensis*’)
- Pied Wagtail *Motacilla yarrellii* (monotypic)
- Moroccan Wagtail *Motacilla subpersonata* (monotypic)
- Masked Wagtail *Motacilla personata* (monotypic)
- Himalayan Wagtail *Motacilla alboides* (monotypic)
- Black-backed Wagtail *Motacilla lugens* (monotypic)
- East Siberian Wagtail *Motacilla ocularis* (monotypic)
- Amur Wagtail *Motacilla leucopsis* (monotypic)
- Baikal Wagtail *Motacilla baicalensis* (monotypic)

STC Until more research is done with this complex suggested to be monophyletic by Alström & Mild (2003) and references therein, it was decided to keep following taxa combined as one species *M. a. alba, yarrellii, baicalensis, ocularis, lugens* and *subpersonata*. Following an observation of *personata* in Norway in November 2003 (and earlier records in Cyprus, Israel and Egypt), the AERC TAC will need to examine the taxonomic status of that taxon as well. A.J. Helbig (in litt.) prefers to treat the *Motacilla [alba]* complex as a ‘superspecies’, because there appear to be clear barriers to gene flow (no clinal variation) wherever two of their component taxa come into contact. From a BSC point of view, P.-A. Crochet commented: ‘As in the *flava* complex, available data demonstrate that this complex is of very recent origin. There is still no convincing evidence that efficient reproductive isolation has evolved between forms that have developed distinctive plumages.’

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
	R	A	A*	P

(*) A.J. Helbig recognises three ‘allospecies’ in Europe.

Stonechat *Saxicola torquatus* suggested by Sangster et al. (1998) to be treated as three species:

- African Stonechat *Saxicola torquatus* (polytypic: extralimital *S. t. felix, albofasciatus, jebelmarrae, moptanus, nebulorum, adamauae, pallidigula, axillaris, promiscuus, salax, stoney, clanceyi, torquatus, oreobates, sibillus, voeltzkowi, tectes*)
- Common Stonechat *Saxicola rubicola* (polytypic: *S. r. rubicola, hibernans*)
- Siberian Stonechat *Saxicola maurus* (polytypic: *S. m. variegatus, maurus, armenicus, przewalskii, stejneri*; extralimital: *indicus*.)

CSNA *Saxicola* is a masculine noun. Notice the correct spelling of *torquatus, variegatus, armenicus* and *maurus* (David & Gosselin 2002b). Authors prior to this publication have used the feminine endings to these adjectives, which are not repeated here to avoid confusion. There are, however, other differences in the Latin binomen used for the suggested new species. The CSNA has used the binomen *S. rubicola* for the two

Western Palearctic subspecies *hibernans* and *rubicola*, whereas Wink et al. (2002) have used the binomen *S. torquatus* (in fact, *torquata* at the time, but see David & Gosselin 2002b). Wittmann et al. (1995) and Wink et al. (2001, 2002) both use the binomen *S. axillaris* for African Stonechat. This would only be correct if subspecies *S. t. torquatus* from South Africa was grouped with the European populations. A grouping including *S. t. torquatus* with the rest of the African populations under African Stonechat *S. torquatus* is far more likely than a grouping of *S. t. torquatus* under European populations (G. Sangster in litt.). Urquhart & Bowley (2002) clearly show the discrepancy between the nomenclature used by Wink et al. (2002) and the one used by the CSNA (and their own).

The CSNA recommended European Stonechat *S. rubicola*, Siberian Stonechat *S. maurus* and African Stonechat *S. torquatus* to be treated as specifically distinct (cf. Sibley 1996) based on qualitative differences in morphology (Cramp 1988, Svensson 1992) and phylogeographic analysis (Wittmann et al. 1995). Sangster et al. (1998) also argued that there is no evidence that populations inhabiting W Europe are diagnosably distinct from those in C and N Europe. Therefore, they considered the form ‘*hibernans*’ a synonym of *S. rubicola*. They were also of the opinion that evidence is lacking that populations inhabiting E Siberia (‘*stejnegeri*’) are diagnosably distinct from W Siberian populations and therefore included ‘*stejnegeri*’ in *maurus*. Svensson (1992), p 124, however, did recognise the subspecies *hibernans* and *stejnegeri* but calls them ‘very similar’ to *rubicola* and *maurus*, resp. (p 124). Pending further analysis, the CSNA provisionally retained *variegatus*, *armenicus*, *indicus* and *przewalskii* as conspecific with *maurus* (Sangster et al. 1998). The STC commented that the entire complex needs to be better researched, and is therefore provisionally best kept as one species.

Wink et al. (2002) provided evidence based on sequence data of the mitochondrial cytochrome *b* gene and genomic fingerprinting that the geographically separated taxa of the *S. torquatus* complex represent distinct genetic lineages, which became separated more than one million year ago. The distinct genetic pattern implies that hybridisation and gene flow between these lineages no longer takes place to a significant degree. Since these lineages also differ in morphology, breeding behaviour, vocalisations and physiological control of their annual cycles, they suggested treating Common Stonechat *S. torquatus*, African Stonechat *S. [t.] axillaris*, Reunion Stonechat *S. [t.] tectes*, Canary Islands Stonechat *S. dacotiae* and Siberian Stonechat *S. [t.] maurus* as distinct species. There is, however, disagreement on the nomenclature applied by these authors. The use of *S. torquatus* is not advocated for European birds by the CSNA and *S. rubicola* is proposed instead.

L. Svensson commented: ‘Although Wink et al. in Urquhart (2002) provided a molecular analysis of seven of the 14 species of *Saxicola*, covering four (out of 25) from the *torquatus* group, plus *S. dacotiae*, I fail to see this as a conclusive instrument for taxonomic change of the Stonechat complex (but an interesting start). DNA is still missing from several taxa, like *armenicus*, *variegatus*, *przewalskii*, *stejnegeri* and *indicus*, and Urquhart can only refer to rather loose arguments for a three-way split, like divided range (can occur between two subspecies of the same species as well, and indeed occurs within the resulting species), and differences in morphology (but these are partly more dramatic within the proposed species *S. maurus* and *S. torquatus* than between the three). A closer study of the breeding biology and behaviour of the three proposed Stonechats is missing. Urquhart refers to vocal differences between the three claimed Stonechats, but this is in preparation still and not included in the book. The drawing on p. 182 of wing shapes of European and Siberian Stonechats, taken from the German handbook (Glutz & Bauer 1988), implies dramatic differences between these two but is unfortunately unrealistic and misleading.’

Vernacular name: Guy Kirwan requested that Eastern Stonechat be used instead of ‘the longstanding misnomer of Siberian Stonechat’.

ID: Corso (2001) pointed out that *S. t. archimedes* (Clancey 1949) from Sicily can appear very similar to *maurus*.

Is *rubicola* the correct name of the Stonechats breeding in W Europe?

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
	P	A	A*	A

(*) But treated as *S. [torquatus] rubicola*.

Who recognises the twofold split?

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
P	R	A*	A**	P

(*) Accepted but to be split further.

(**) In Europe, includes the following three taxa in the ‘superspecies’ *S. [torquatus]*: *rubicola*, *maurus* and *dacotiae*; this implies that the group should be split further.

Northern Wheatear *Oenanthe oenanthe* proposed by Walters (1997) to be treated as two species:

- Seebohm's Wheatear *Oenanthe seebohmi* (monotypic)
- Northern Wheatear *Oenanthe oenanthe* (polytypic: *Oe. oe. libanotica*, *oenanthe*, *leucorhoa*)

A.J. Helbig Seebohm's Wheatear was described in 1882 by C. Dixon (1858–1926), a naturalist, author, journalist and lecturer who studied the migration of birds and geographic distribution of species. Seebohm's Wheatear could be treated as a separate species based on consistent differences in morphology, including male-like female plumage, and song. There is no consensus on this, however, within the AERC TAC. Its audibly different song is more measured, melodious and sonorous than that of nominate *oenanthe*; units are longer and lower-pitched, mostly below 4 kHz, whereas much of *oenanthe* song exceeds 4 kHz. Also pauses between units are longer (cf. Cramp et al. 1988 for sonograms and discussion). Recognised as a distinct species by Walters (1997) and A.J. Helbig in Svensson et al. (2000).

C.S. Roselaar commented that 'reduced sexual dimorphism is in itself not a valid reason to regard *Oe. seebohmi* as a distinct species. It should be noted that study skins from southern populations of *Oe. oenanthe* (e.g. "argentea" and "nivea") can be very difficult to sex: female Northern Wheatears from Spain and Cyprus can be hard to tell from 2nd calendar year males since they also tend to develop a black(ish) mask, grey mantle and largely white underparts. The black throat of *seebohmi* may be a reason according to some species concepts to split *seebohmi*, but it is recommended to await results of further research (e.g. mtDNA) before taking any decision.' O. van Rootselaar added: 'In general, the intersexual change of external characters (e.g. males becoming female-looking or females resembling males), is not considered genetically determined, but rather caused by irregularities or disorders in the endocrine system (i.e. hormonal secretion) of an individual organism. Hence, I would not consider this phenomenon of any taxonomic value, at least not as long as these abnormalities are considered to be caused by external (environmental or physiological) influences.' *Oenanthe [oenanthe] seebohmi* (Seebohm's or Black-throated Wheatear) is sometimes regarded as an incipient species, the two taxa are allopatric.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
P	P	P	A*	A

(*) Accepted as 'semi- or allospecies'.

Mourning Wheatear *Oenanthe lugens* to be treated as two species:

- Eastern Mourning Wheatear *Oenanthe lugens* (polytypic: *Oe. l. lugens*; extralimital: *Oe. l. lugentoides*)
- Western Mourning Wheatear *Oenanthe halophila* (monotypic)

A.J. Helbig According to the STC, Western Mourning Wheatear may best be treated as a separate species based on differences in morphology, incl. presence of clear sexual dimorphism. This proposed split is not supported by the CSNA because the evidence has not been summarised and compared with the Guidelines. C.S. Roselaar commented that this complex consists of several forms, which are hard to tell by plumage or morphology. It is therefore recommended to await the results of DNA research. English names are tentative only. P.-A. Crochet commented: 'Presence of sexual dimorphism can evolve very quickly under sexual selection and be variable within species (cf. Pied Flycatcher *Ficedula hypoleuca*: some populations are dimorphic, some – "muscipeta" form – are not). Wheatears often show marked intraspecific plumage polymorphisms, further evidence that plumage alone is not a very good predictor of relationships and/or distinctiveness in this group. Allopatric taxa that differ in plumage only are plentiful and most are not split by AERC TAC. So in the absence of other differences (voice, DNA,...) the split should be rejected. If other arguments exist, please provide them.'

Traditionally several widely allopatric populations are lumped with *O. lugens*, being *halophila*, *persica*, *lugentoides* (incl. *boscaweni*), *lugubris* (incl. *vauriei*) & *schalowi*. The *lugentoides* group is treated as a subspecies of *Oe. lugens* by Cramp (1988) and Dowsett & Dowsett-Lemaire (1993), considered an incipient species by Hall & Moreau (1970), and treated as specifically distinct by Hollom et al. (1988) and Sibley & Monroe (1990), so obviously more information is required. Currently, however, *halophila*, *lugubris* (incl. *vauriei*) and *schalowi* are often treated as separate species, being highly allopatric and *lugubris* and *schalowi* being sexually dimorphic, differing in plumages, in male as well as in female.

Variation: (4 to 8 races): Western Palearctic taxa are in bold face.

1. **lugens** (Lichtenstein) 1823: E Egypt & Near East
2. *persica* (Seebohm) 1881: Iran
3. **lugentoides** (Seebohm) 1881: SW & S Arabia & W Yemen: South Arabian Wheatear

4. *boscaweni* Bates 1937: E Yemen & S Oman
5. *halophila* (Tristram) 1859: N Africa, from Morocco east to Cyrenaica in Libya
6. *lugubris* (Rüppell) 1837: Highlands of Eritrea, Ethiopia, Kenya & Tanzania (Abyssinian Black, Eastern Black or East African Wheatear). Treated as a separate species by Zimmermann et al. (1996), said to differ in behaviour, voice and plumage.
7. *vauriei* Meinertzhagen 1949: NE-Somalia
8. *schalowi* (Fischer & Reichenow) 1884: Highlands of Ethiopia (Schalow's Wheatear). Subspecies of *lugens* in Mayr & Paynter (1964), Cramp (1988) and Dowsett-Lemaire (1993), of *lugubris* in Sibley & Monroe (1990) and Zimmermann et al. (1996) and treated as a separate species by Stevenson & Fanshawe (2001).

ID: Clement & Harris (1987a, b) offer general information on the identification of wheatears. Andrews (1994), Tye (1994) and van der Vliet & de Lange (1997) provide information on the black morph of *O. l. lugens* 'Basalt Wheatear' in Jordan. These birds were first erroneously identified as Variable Wheatears *O. picata ophistoleuca*, a species which is not reliably recorded in the Western Palearctic according to Beaman & Madge (1998) (see, however, Shirihai 1999). P. Rasmussen is splitting the different 'morphs' of *picata* in her forthcoming Indian Subcontinent book (G. Kirwan, pers. comm.).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
∅	P	R	A*	P

(*) Accepted as 'semi- or allospecies'.

Naumann's Thrush *Turdus naumanni* suggested by some to be treated as two species:

- Dusky Thrush *Turdus eunomus* (monotypic)
- Naumann's Thrush *Turdus naumanni* (monotypic)

Information on these taxa was summarised by Clement et al. (2000), allowing no solution at this stage. We know no molecular studies and information on the contact zone is still limited. More research is needed.

ID: Clement et al. (2000)

Dark-throated Thrush *Turdus ruficollis* suggested by some to be treated as two species:

- Black-throated Thrush *Turdus atrogularis* (monotypic)
- Red-throated Thrush *Turdus ruficollis* (monotypic)

Arkhipov et al. (2003) described the differences in song between these two taxa. Molecular studies are still lacking and the contact zone is poorly studied. See Clement et al. (2000) for a review of currently available information. More research is needed.

ID: Clement et al. (2000)

Subalpine Warbler *Sylvia cantillans*

Remark: The situation in Italy and W Europe requires further research. Moltoni's Warbler *Sylvia cantillans moltonii* was reported to be breeding in mainland Italy (Festari et al. 2002, see also De Smet & Goossens 2002). N. Baccetti commented: 'What Orlando described as *moltonii* from Sardinia (by comparison with Sicilian specimens?) is probably the same taxon breeding in mainland Italy, according to identification criteria proposed by Shirihai et al. (2001). The wren-like call of N Italian birds, in particular, has been known for centuries (Savi 1828 first gave useful details on this matter). See C.S. Roselaar's short comment in Shirihai et al. (2001), on which I fully agree. The fact that (mainland) Italy is the type locality of Pallas's *cantillans* may put subspecific nomenclature upside down, as far as birds of mainland Spain and Sicily are concerned. The question cannot be solved until we can examine Orlando's series, now closed in boxes due to a move of Terrasini museum.'

Lesser Whitethroat *Sylvia curruca* complex suggested by some to be treated as three species:

- Lesser Whitethroat *Sylvia curruca*
- Desert Lesser Whitethroat *Sylvia minula*
- Hume's Lesser Whitethroat *Sylvia althaea*

STC This entire complex is currently being researched by several taxonomists, and pending new evidence best kept as one species (see e.g. Martens & Steil 1997, King 1998, Shirihai et al. 2001 for details). Although a genetic analysis remains to be done, the Lesser Whitethroat complex can be seen as a ring species, where the widespread nominate (*curruca*, including extremely similar '*blythi*' in Siberia) in one direction, in the east and going southward, becomes paler and warmer brown above, and generally smaller, adapting to

deserts and arid plains in Central Asia (*halimodendri* and *minula*; *margelanica* same but large), in the other direction southward in Europe and east through E Asia Minor and the Caucasus (*caucasica*) becoming darker and greyer above and progressively larger, to culminate in the mountains of Iran, Pakistan, N India and Central Asia (*althaea*). Where *althaea* in the mountains on occasions meets or comes near *halimodendri* and *minula* in the valleys and on the desert plains they behave as separate species. However, along this chain of populations it is impossible to indicate a step or a break which could serve as a division between two or more species; all are grading smoothly and steplessly into each other. This is the picture emerging when making a careful study of available study skins in museums, and from trapping live birds in various parts of the wide range of distribution. However, it is possible that a morphometric study is insufficient to resolve the taxonomy, and so a systematic collection of DNA has taken place the last five years in collaboration with Urban Olsson. The future will show whether some cryptic species are hiding among this cline of very similar birds. – For field study, it has become clear over the years that the various taxa are extremely difficult to identify reliably in nature, and even after recording the song and trapping the bird it can prove difficult without sample specimens to compare with. (This is in bright contrast to the optimistic picture given in some fieldguides and identification papers.) – It should be noted, also, that the song differences as given by Martens & Steil (1997) have proved difficult to follow. In practice, all birds west of a line from W Siberia to E Turkey have the familiar song consisting of a brief warble followed by the characteristic rattle (*curruca*, *caucasica*), whereas birds east of this line basically have a longer scratchy warble and no rattling end (*halimodendri*, *minula* and *althaea*; many exceptions or intermediate singers near the line). This means that in Central Asia you have limited help by the song. – A brief reference also to the taxon *margelanica*, afforded ‘allospecies’ status by Shirihaei et al. 2002 (but this based solely on morphological distinctness!) It breeds in C China (contra BWP) and migrates west through Central Asia to unknown winter grounds. Until we know DNA, song, detailed distribution range, etc., I find no sound reason to split it as an ‘allospecies’. – In summary, research is in progress, research which will hopefully cast more light on the relationship between these closely similar taxa. It seems unwise to make any taxonomic changes before this research is concluded. Also, if a molecular analysis confirms the morphological picture of a ring species, I strongly advocate keeping it together as one whole rather than arbitrarily cutting it up in little pieces (cf. comments on the Greenish Warbler complex). The Lesser Whitethroat is an old and interesting, plastic species which has been able to adapt to various environments, and changed with them. A fragmentation of it will not help our understanding of this. (Lars Svensson)

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
P	P*	P**	A***	P

(*) Recognise *S. curruca* and *S. minula*, but not *S. althaea*.

(**) Recognise a two way split (*althaea* versus *curruca*), but there seems to be a large intergradation with *minula*.

(***) Accepted as ‘semi- or allospecies’.

Goldcrest *Regulus regulus* to be treated as two species:

- Goldcrest *Regulus regulus*
- Tenerife Goldcrest *Regulus teneriffae*

CAF There is no consensus on this split. It was proposed by Löhrl et al. (1996) on account of significant differences in call, structure and morphology, but this was countered by evidence presented by Sturmbauer et al. (1998) and Martens et al. (1998). Based on genetic results suggesting that *regulus* (excluding eastern taxa such as *himalayensis*, *tristis* and *japonensis*) and *teneriffae* might be monophyletic (Sturmbauer et al. 1998, Päckert et al. 2003, but beware of very small sample sizes!) and on morphological diagnosability, they could be treated as species under a PSC approach. On the other hand, although *teneriffae* and *regulus* have very different songs, amount of divergence in song is lower between *teneriffae* and *azoricus* or *sanctaemariae* than between these taxa and *regulus* (Päckert et al. 2003). Amount of genetic divergence between *teneriffae* and *regulus* is very low: 0.3% in 16S RNA (a slowly evolving mitochondrial gene, Sturmbauer et al. 1998) and 3% in cytochrome *b* (Päckert et al. 2003). As a comparison, *ignicapilla* and *madeirensis* differ by 11–12% in mtDNA (cytochrome *b*) and have very different song structure. *Regulus* and *teneriffae* are thus genetically poorly differentiated taxa and some Azorean subspecies of *regulus* are apparently similar in song to *teneriffae*. This taxon is thus closer to a well marked subspecies than to a valid species. Following the Guidelines to recognise as valid species pairs of taxa that diverge by a similar amount than closely related pairs of ‘proven’ species, *teneriffae* would be maintained as a subspecies of *R. regulus*. (P.-A. Crochet)

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
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P	P	A	A*	P
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(*) Considered by A.J. Helbig to be part of the ‘superspecies’ *Regulus [regulus]*.

Pygmy Sunbird *Anthreptes platurus* suggested by Cheke et al. (2001) to become *Hedydipna platura*

STC

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
∅	∅			P

Nile Valley Sunbird *Anthreptes metallicus* suggested by Cheke et al. (2001) to become *Hedydipna metallica*.

STC

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
∅	∅			P

Palestine Sunbird *Nectarinia osea osea* suggested by Cheke et al. (2001) to become *Cinnyris oseus oseus*.

STC

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
∅	∅			P

Azure-winged Magpie *Cyanopica cyanus* suggested to be treated as two species by Fok et al. (2002):

- *Cyanopica cooki* (monotypic)
- *Cyanopica cyanus* (polytypic; extralimital: *C. c. cyanus*, *pallescens*, *stegmanni*, *swinhoei*, *japonica*, *koreensis*, *interpositus*, *kansuensis*)

CSNA Notice the correct spelling of *Cyanopica cyanus* (David & Gosselin 2002a). Cooper & Voous (1999) and Fok et al. (2002) showed that the Iberian Azure-winged Magpie *C. cooki* should be treated as a distinct species. P.-A. Crochet, however, commented: ‘The split of *cooki* from *cyanus* rests almost entirely on the genetic divergence (6% for control region, which is probably equivalent to about 2% for cytochrome *b*, at least according to the authors’ estimates). In the absence of any other information, this genetic distance is not really conclusive. Are the plumage differences really consistent? Any difference in vocalisation? This is really a borderline case. May be better to wait until more information is available?’

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
∅	P		P*	P

(*) Could be accepted as ‘allospecies’.

Carrion Crow *Corvus corone* proposed by Parkin et al. (2003) to be treated as two species:

- Carrion Crow *Corvus corone* (polytypic: *C. c. corone*, *orientalis*)
- Hooded Crow *Corvus cornix* (polytypic: *C. c. cornix*, *pallescens*, *sharpii* and *capellanus*)

BOURC TSC See Parkin et al. (2003) for an appraisal of a twofold split, based on obvious differences in plumage, good evidence of non-random mating, reduced fitness of hybrids between Carrion and Hooded Crows. Differences in vocalisation and ecology support this distinction.

If, however, the sharing of a black plumage between *corone* and *orientalis* is regarded as a shared ancestral (symplesiomorphic) trait, it cannot be used to indicate any kind of evolutionary relationships within this group. In that case, keeping these two taxa together based only on plumage colour is arbitrary. One consequence is that a twofold split, that would keep *orientalis* and *cornix* conspecific but separate only the western *corone*, should enter the considerations as well. Due to this, results from eastern hybrid zones should be proven to be relevant to a *corone* – *cornix* split, not assumed relevant in an a priori fashion (L. Raty in litt.).

According to the STC, Carrion Crow is best treated as three species based on differences in morphology, vocalisation (at least between *cornix* and *orientalis*) and parapatric breeding ranges as well as only limited hybridisation zones between the three species (Sangster et al. 1999; Knox et al. 2002). An isolated taxon

from lowland Iraq, Mesopotamian or Iraq Crow *C. c. capellanus*, strikingly different to adjoining populations of Hooded Crows, is suggested by Madge & Burn (1994) to be possibly a good species. The two black taxa *C. corone* and *C. orientalis* are well separated, and some authors believe that they probably evolved independently in the wetter, maritime regions at the opposite ends of the Eurasian continent (Madge & Burn 1994). Many experts believe, however, that the last ice ages cleaved and isolated the *corone* and *orientalis* populations, and that mutation produced the *cornix* population in one of the enclaves. Then, after glaciation, the once-separated populations merged. The intraspecific variability of crows is currently studied by Haring, Gamauf & Kryukov (in prep.).

Corone and *cornix* usually interbreed in areas of contact, as in S Scotland, Denmark, S Switzerland, N Italy and N Afghanistan, but the hybrid zones are narrow and relatively stable, although shifting slightly westward in Europe in recent years. There are some areas, e.g. C Russia and Iran, where they are sympatric with little or no interbreeding. This complex situation requires a detailed study to fit into any species concept and these two taxa may be viewed either as two species, as subspecies of *corone*, or as 'semispecies'. Kryukov & Blinov (1994. *Journ. f. Orn.* 135: Sonderheft p. 47) studied the zone of hybridisation between the Ob and Yenesei rivers in Siberia. The hybrid zone is c. 150 km wide and introgression of plumage characters can be detected for 700 km. Up to 30% of the birds in the centre of the zone are intermediate and 11 colour morphs were identified. Genetic variability in allozymes and DNA increase within the hybrid zone. There is no evidence of reduction of fertility or viability in mixed pairs, but evidence of positive assortative mating was found. The two differ in migration behaviour, wintering areas and habitat preferences. Rolando & Saino (1994. *Journ. f. Orn.* 135: Sonderheft p. 48) analysed the composition of breeding pairs in six areas in the alpine hybrid zone in alpine valleys and the Cuneo highlands of Italy. Their results indicate that positive assortative mating is present, i.e., mixed pairs are less frequent than would be expected if random mating was present.

Although the CAF formally accepted the split, P.-A. Crochet comments that in his opinion the split is not ripe: Assortative mating: found in some studies (see Parkin et al. 2003) but not in others (Picozzi 1976, but note small sample size). Counterselection of hybrids: slight evidence in N Italy (see reference in Parkin et al. 2003) but not everywhere either (Picozzi 1976 also, but small sample size). Even the paper by Saino & Villa (1992), which seems to be the basis of the split, is not that clear. The authors report that 'no difference in reproductive success was observed in the hybrid zone between pairs containing only hybrid phenotypes and pairs containing at least one hybrid' and even conclude that their data support the 'bounded hybrid superiority' model, which is just the opposite to counter selection of hybrids... Admittedly, the data in Saino & Villa suggest that female hybrids reproduce less well than pure females, a result also reported by Saino and Bolzern, but again only for Italy. Genetic consequences of differentiation: no study has reported genetic differences in either mtDNA or nuclear DNA between Hooded and Carrion Crows. In other words, there is no evidence that the two species or three species are evolutionary units. There is even evidence of the contrary: Kryukov & Suzuki (2000) found that mtDNA separates crows from S Sakhalin from all other crows, and that all Carrion, Hooded and hybrid populations from France to N Sakhalin had the same haplotype. Ufyorkina et al. (1995) similarly state that the hybrid population has 'a single gene pool' implying lack of linkage disequilibrium. To summarise, most of the data showing some kind of hybrid disadvantage come from a single hybrid zone (N Italy). The hybrid disadvantage is at best very weak (hybrids of both sexes are fertile, but female hybrids are less fit). Assortative mating is demonstrated in several hybrid zones (Germany, Siberia) but is always statistical (i.e. there are less hybrid pairs than expected but they are still very frequent). There is apparently no barrier to gene flow and a very low genetic divergence among Hooded and Carrion crows in the hybrid zone in Italy (Saino et al. 1992) and no genetic separation of Hooded and Carrion crow as a whole. If we split Carrion and Hooded crows, it will be the first case of avian species, which are not separated by mtDNA, but are parapatric and hybridising nearly freely. All other species are either mostly identifiable by mtDNA (even if some gene flow occurs) or – e.g. *Loxia* – non-differentiated in mtDNA but nearly fully reproductively isolated. It is important to realise that reproductive isolation in crows is very weak: there are still plenty of hybrid pairs, and hybrids are nearly as fit as parents. My opinion is that, although Hooded and Carrion Crows have started on the way to reproductive isolation, they are still much closer to intraspecific divergence than interspecific divergence.

Additional remarks:

N. Baccetti & G. Fracasso: 'Taxonomic identity of Hooded Crow populations of mainland Italy and Sardinia should be checked in view of remarkable differences that in the past suggested placing our insular birds (but not mainland ones) in *sardonius* (cf. Arrigoni degli Oddi 1929; Vaurie 1959).' This remark points out the fact that Roselaar in Cramp & Simmons (1994) and Parkin et al. (2003) have elected one of the two co-existing intraspecific subdivision systems. Traditionally, the '*cornix*-group-without-*capellanus*' has been

divided into three subspecific taxa: one primarily eastern race, *sharpii*, occupying the complete Asian range of the complex but barely reaching WP in the Caucasus, and two western taxa: *cornix* in N Europe, *sardonius* in the whole Mediterranean basin. This subdivision is still widely in use (e.g. Madge & Burn 1994, Shirihai 1996, Beaman & Madge 1998, Shirihai 1996,...). The other subdivision system, used by Roselaar, is different. Instead of starting with an east-west divide, Roselaar starts with a north-south divide. He extends the range of *cornix* to all northern populations, both European and Asian, and that of *sharpii* to most southern populations, both European and Asian, to the exception of the Levant; he drops *sardonius* by partitioning it between *sharpii* (most populations), *cornix* (Corsican population) and a Levantine race *pallascens*. Incidentally, both systems use three races, two of which share their name with a race in the other system, but none of which is equivalent to a race in the other system (L. Raty in litt.).

A. Lindholm: ‘The differing vocalisation of *orientalis* was mentioned but without reference. This has to be based on finer details, because the calls are in fact quite similar. I have a recording of *orientalis* call and I can send it to the AERC TAC.’

The voting, admittedly undertaken before the above discussion was made available, shows that most if not all TCs are in favour of a split. There is disagreement, however, over a twofold or a threefold split. Some individual taxonomists, however, still prefer to keep all taxa lumped.

Please state your formal opinion on a twofold split, based on Parkin et al. (2003).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A*	A**	P***

(*) The CSNA has been in favour of a threefold split.

(**) Considered as semi- or allospecies.

(***) The STC has been in favour of a threefold split, but needs to examine new evidence.

House Sparrow *Passer domesticus* suggested by Johnston (1969) to be split in

- **House Sparrow** *Passer domesticus* (polytypic: *P. d. tingitanus*, *balearoibericus*, *domesticus*, *biblicus*, *mayaudi*, *hyrcanus*, *persicus*, *niloticus*, *indicus*, *rufidorsalis*, *bactrianus*)
- **Italian Sparrow** *Passer italiae* (polytypic: *P. i. italiae*, *brutius*, *maltae*)

CSNA *Italiae* was included by Voous (1977c) as a stabilised hybrid population between House Sparrow *P. domesticus* and *P. hispaniolensis*. Helbig (2000a) pointed out that this is not a convincing case of speciation through hybridisation as both parent species are only ‘semispecies’. The *Guidelines* hypothesise that ‘hybridogenetic speciation is unknown in birds, although it may be possible.’ N. Baccetti and G. Fracasso commented: ‘Listing Italian Sparrows as × *italiae*, × *maltae*, etc. within *hispaniolensis* is not a satisfactory solution, and is not homogeneous with all other taxa in the checklist. In current Italian literature (and Corsican: Thibault & Bonaccorsi 1999), *Passer italiae* is the used binomen, for practical reasons if not for more substantiated arguments. I imagine that when this subject would be re-examined according to modern views on species concept, a similar solution will be adopted. At least because there are *hispaniolensis* colonies isolated on the Italian mainland, which do not mix with surrounding *italiae*.’ Publications dealing with Italian Sparrow include Johnston (1969), Summers-Smith (1988) and Massa (1989). Recent work by Domenico Fulgione and Mario Milone, however, has shed new light on the subject. The results summarized on his poster presented at the 23rd IOC (Beijing, 2002), titled ‘Genetic approaches to the systematics and range of the Italian Sparrow *Passer italiae*’ (page 332) say that *P. italiae* is a good species: ‘The Italian Sparrow has traditionally been identified variously as a stabilised hybrid or subspecies of either *P. domesticus* or *P. hispaniolensis*, or even separated as a good species and emergent interspecies. Our recent studies suggest that the Italian Sparrow might be derived from populations of *P. hispaniolensis* in N Africa. Nevertheless, its systematic status, range and contact zones with sister taxa remain unclear. Modern biomolecular techniques, such as microsatellite DNA analysis, allow investigation at different scales by use of markers that discriminate between populations and species. Accordingly, we applied microsatellite amplification primers screened for *P. domesticus* to an analysis of 31 populations of Italian Sparrows sampled along the Italian Peninsula between the ranges of the two parapatric species. All populations were classified by current morphological criteria Gradient analysis of the molecular data indicates that “pure” populations of Italian Sparrow are confined to peninsular Italy, north to the Po valley and south to C Calabria. Throughout this region, the populations show the consistent traits of a good species, and are delimited at either end by zones of hybridisation with *domesticus* and *hispaniolensis* respectively. The northern zone spreads in an east-west direction and is characterised by three well distinct components resulting, probably, from interactions with three different groups of *P. domesticus*. The southern zone includes Sicily and S Calabria, beyond which Sardinia, Corsica and Tuscan Archipelago can be considered genotypic corridors linking *hispaniolensis* and *italiae*. As well as being the first genetic investigation of Italian Sparrows, the present study contributes to speciation models involving fast rates of evolution in birds.’

F. Jiguet commented: ‘Two major hypotheses have been proposed on the evolution and taxonomic status of Italian Sparrow. On the one hand, it could be a stabilised hybrid *P. domesticus* × *P. hispaniolensis*, but other such cases are unknown in birds. On Mediterranean islands, the founder population (either more “House Sparrow” or more “Spanish Sparrow”) would explain the variation observed between populations. In N Africa, the more recent contact between the parental taxa would explain the numerous intermediate phenotypes between all three forms. In that case, the form “*italiae*” would not be considered a valid taxon. On the other hand, *italiae* could have evolved from a common ancestor of all these sparrows, hybridising locally with the two species. Its recent origin may not have allowed efficient reproductive barriers and the more or less recent contact between the different taxa may have caused locally important hybridisation.

Following the second hypothesis, *italiae* could either be regarded as a subspecies of *domesticus* based on the colouration of the crown feathers (Burkhard 1999) and genetic studies (Allende et al. 2001), an allospecies forming part of *Passer* [*domesticus*], or a recently evolved full species. Genetic studies in the contact zone between *domesticus* and *italiae* should provide the answer to that question. Also, there would be local hybridisation with e.g. *italiae* × *domesticus* in France (Lockley 1992, Lockley 1996, Bonaccorsi & Jordan 2000), *italiae* × *hispaniolensis* in Italy, hybrids of both types in Algeria, Tunisia and Libya, and *domesticus* × *hispaniolensis* in Morocco and Greece. The geographic variation of the phenotype “*italiae*” (Italy, Malta, and N Africa) could be considered as subspecific variation.

Finally, it should be noted that the name *italiae* predates *hispaniolensis* (1817 versus 1820, cf. Burkhard 1999). If *italiae* and *hispaniolensis* are considered to form a single species, the scientific name of “Spanish Sparrow” would become *Passer italiae hispaniolensis* (and *P. i. transcaspicus* in the Middle East) and Italian Sparrow would become *P. i. italiae*. If both are regarded as conspecific, then Spanish Sparrow becomes a subspecies of Italian Sparrow.’

D. Fulgione and M. Milone commented: ‘The Italian Sparrow shows some morphological traits (plumage colouration) overlapping those of two parapatric species *P. domesticus* and *P. hispaniolensis*. However, there is evidence of its southern derivation (from a putative ancestral form of *P. hispaniolensis*):

(1) **Morphological analyses** (results were presented in the *Proceedings of the First European Ornithological Congress*, Bologna, Italy) have shown significant differences between *P. domesticus* and *P. hispaniolensis* in wing length, tarsus and bill robustness index according to Alonso (1985). Bill, tarsus and wing of Italian Sparrow populations show intermediate phenotypes as compared to the two parapatric species. The km distance between 11 populations examined along the Italian Peninsula is positively correlated with each considered variable. The clinal variation in some morphometric characteristics (bill and wing) is disconnected by a strong variability in correspondence of the alpine hybridisation area, while all other populations follow a cline that gradually leads to the populations of *P. hispaniolensis*.

(2) Using **genetic markers** (4 polymorphic loci of DNA microsatellite) we observed the same clinal pattern, although the break of the north was found to have shifted a little south (to level of the Padana plain) as compared to that observed with morphological traits. This is probably due to the recent anthropization of the alpine valleys that allow a *P. domesticus* allele introgression. The results of the genetic cline analyses were presented in 2002 at the International Ornithological Congress (IOC) in Beijing, China, and they are currently contained in a paper in preparation which will be sent to the AERC TAC shortly. In the light of recent genetic results, we believe that the northern hybridisation zone (with *P. domesticus*) is a true zone of contact typically characterised by hybrid populations. This may not apply to the southern contact zone where our first results suggest that variation develops more gradually. We ignore, however, whether this is the case in the entire southern zone, as we have only sampled on Sicily without finding hybrid populations. At this stage, we cannot exclude that other southern Mediterranean islands could reserve us some surprise. In fact the use of genetic markers confirms this type of variation. The evidence following actual genetic analyses is in contrast with our preliminary assertion in the abstract for the China congress (IOC), in which we speak of two zones of hybridisation.

(3) **Behaviour**

(3.1) In an analysis of **song display** in Italian Sparrow we have used the male territorial marker song as distinctive display between populations (using spectrogram). By multivariate analysis we have found a clinal variation from *P. hispaniolensis* to peninsular Italian Sparrow. This pattern is broken in the Alps where Italian Sparrow populations (morphologically similar to Italian Sparrows) and European *P. domesticus* show a similar song structure (the first note forming a “rounded” peak not exceeding 5000 Hz like in *P. domesticus*). Spectrogram of Corsican sparrows results different from *P. hispaniolensis*’ song, but it seems similar to that of some peninsular populations. In fact Corsicans’ syllable shows a first peak characterised by

a distortion absent in *P. hispaniolensis*; moreover, the third peak which in *P. hispaniolensis* has the same frequency as the first, is much lower in the Corsican population. These two traits are present in a lot of peninsular populations like in Tuscany, Campania and Calabria (Fulgione et al. 2000).

(3.2) Regarding the **biology of reproduction**, it is interesting to notice that:

(a) *P. domesticus* basically has a single breeding period each year.

(b) *P. hispaniolensis* shows considerable variation throughout its range (see Summers-Smith 1988), but more southern populations (Cape Verdes) show two distinct breeding periods, from April to May and from September to January (Keulemans 1866, Bourne 1955, Bannerman & Bannerman 1968). Our results, from Italian Sparrows underline a clear resumption of the reproductive activity immediately after the autumnal refractory period. The testis activity during the period November-December shows a consistent growth of mass and a production of spermatozoa, even if they appear to be adhered to the wall of the tubules in messy way (compared with the orderly disposition- arrayed shown during the spring reproductive period). Such gametogenesis will reveal abortive and the SPZ are most probably reabsorbed by the testis tissue so that in the following period (February) it appears as if in a state of rest. The results regarding this trait are contained in an article in preparation that I will send shortly to the AERC TAC and in Fulgione et al. (1998).

(4) **Chromosomes**

Morphologically, the chromosome set of *P. italiae* differs from that of *P. domesticus* in chromosome W, which is acrocentric in the former and submetacentric in the latter. *P. hispaniolensis* shows an acrocentric chromosome-W morphology. A comparison of the C-banding patterns between *P. italiae* and *P. domesticus* revealed other remarkable differences. In fact, the short arms of the eighth pair of autosomes are euchromatic in *P. italiae* but entirely heterochromatic in *P. domesticus*. The involvement of simple chromosomal rearrangement such as inversion, fusion and translocation must be ruled out, since the morphology of these and other autosomes remains unchanged. The comparison of the C banding patterns between *P. domesticus* and *P. italiae* also displayed a difference in chromosome Z, in particular in the location of a heterochromatic peritelomeric band, which is on the short arm in *P. italiae* and on the long arm in *P. domesticus*. The simplest mechanism that might be argued for such a difference is a pericentric inversion (Fulgione et al. 2000a).

The last two aspects, combined to the fact that the Italian populations show a genetic distance over 0.2 (Nei distance in Fulgione et al. 1998) comparing it with the two parapatric species, and the pattern of the range of *P. italiae* regarding to *P. hispaniolensis*, induces us to consider *P. italiae* a “good species” sufficiently distinct from *P. hispaniolensis* from which it would have originated. Paradoxically, if we held *P. italiae* derived from *P. domesticus*, we could not speak of “good species” given the contiguity of the ranges. The southern islands of the Italian Peninsula deserve a separate treatment. Sicilian birds (of which we examined a few samples from Palermo) show a strong similarity to *P. hispaniolensis* and could be considered as subspecies *P. h. italiae*.’

So there are two conflicting proposals on Italian Sparrow. There is a proposal backed by CAF to treat Italian Sparrow as a subspecies of House Sparrow and a proposal by several Italian ornithologists to treat Italian Sparrow as a full species. The AERC TAC is awaiting the publication of the results of ongoing research before it can make a decision.

Pale Rockfinch *Petronia brachydactyla* suggested to become *Carpospiza brachydactyla*

CSNA No longer thought to be closely related to Rock Sparrow *Petronia petronia* (and may possibly be more closely related to the Fringillidae than the Passeridae), so now usually placed in the monotypic genus *Carpospiza* (Beaman & Madge 1998). This treatment highlights this rather unique bird and the difficulties surrounding its classification. Makes a sparrow-like woven cup-nest in dense scrub, in contrast to *Passer*, *Gymnoris*, and *Petronia*, and this, together with the colour of the eggs and some peculiarities in voice and behaviour, points to cardueline relationships rather than to a position in the sparrow family (Roselaar 1995). Without a phylogenetic framework and peer-reviewed publications, this proposal is retained in the pending category.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
∅	A			A

Chestnut-shouldered Sparrow *Petronia xanthocollis* suggested to become *Gymnoris xanthocollis*

CSNA Not now thought to be closely related to Rock Sparrow *Petronia petronia* and so placed in the genus *Gymnoris* (Roselaar 1995, Beaman & Madge 1998). Regarded as *Petronia xanthocollis* by e.g. CINFO 1993, Clements (4th, 5th edition), Sibley & Monroe (1993, 1996) and BWP. Chestnut-shouldered Sparrow and its three Afrotropical relatives differ markedly from Rock Sparrow in plumage, structure, habitat and behaviour, warranting recognition of a separate genus *Gymnoris* (Roselaar 1995). The CAF accepts that it is not a *Petronia*, but needs more information on its placement in *Gymnoris*. As this proposal is lacking both a phylogenetic framework and peer-reviewed publications, it is provisionally retained in the pending category. The AERC TAC is well aware, however, that the position of Pale Rockfinch and Chestnut-shouldered Sparrow in *Petronia* is no longer considered valid by many observers. These species certainly look very different in the field.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
Ø	P			A

Indian Silverbill *Euodice malabarica* suggested to become *Lonchura malabarica*

and **African Silverbill** *Euodice cantans cantans* suggested to become *Lonchura cantans cantans*

CSNA Delacour (1943) gave a comprehensive review of the taxonomy of munias (including the silverbills). He considered there to be three subgenera, *Padda* which embraced the Java and Timor Sparrows, *Amadina*, Cut-throat and Red-headed Finch, now regarded universally as falling within the Estrildidae, and *Lonchura* which contained all the munias, divided into four subgenera (*Heteromunia*, *Euodice*, *Lonchura* and *Munia*). Subsequently Wolters (1957), Steiner (1960) and Guttinger (1970, 1976) gave further conclusions. Voous (1977c) and some other authors placed Indian and African Silverbills in the genus *Euodice*, comprising the African species, all three silverbills and the Madagascar Munia. Goodwin (1982) merged *Padda* with *Lonchura*. Sibley & Monroe (1990) recognised *Heteromunia* (Pictorella Mannikin), *Lemuresthes* (Madagascar Mannikin) and *Padda* in addition to *Lonchura*. Clement et al. (1993) and Restall (1997) found no good reasons to subdivide *Lonchura*. Indian and African Silverbill are very similar species, which led Delacour (1943) to synonymise them. Harrison (1964), however, found that the call notes are similar, but the songs are distinctly different in form, although they appear to share a common basic pattern. The two silverbills are sympatric in the south of the Arabian Peninsula and there is no record of natural hybridisation. Restall (1997) found that the species evince preference for its own kind in captivity. Kakizawa & Watada (1985) confirmed the specific status of these silverbills by means of protein electrophoresis. P.-A. Crochet commented: 'From what I understand from the historical review by Restall (1997), there is no convincing evidence either for splitting *Euodice* from *Lonchura* or for merging the two genera. If so, I believe we should adopt the "lumping" option. In general, a genus name should not be used unless there is good evidence that it is justified. Furthermore, the AOU uses *Lonchura*.' (Restall 1997).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
Ø	A			A

- **Common Redpoll** *Carduelis flammea* (polytypic: *C. f. flammea*, *rostrata*)
- **Lesser Redpoll** *Carduelis cabaret* (monotypic)

proposed by Ottvall et al. (2002) to be lumped

STC An appraisal of the reasons to separate these two taxa on the basis of differences in morphology, vocalisations, and behaviour, and sympatric breeding of the two forms in S Norway in 1994 was published by Knox et al. (2001) in the June issue of *British Birds*. In September 2001, however, it became clear that several AERC countries believed that the split of the redpoll complex into *C. flammea* and *C. cabaret* was premature and in the minutes of the 6th Conference of the AERC at Hel Peninsula, Poland (http://www.aerc.be/Hel_2001.htm) the AERC TAC was asked to add 'new, unpublished scientific evidence from Poland, Switzerland, Finland and Sweden' to the redpoll file for reconsideration. At least Finland, Poland and Sweden did not accept the split on their national lists. Next, the decision was accepted in the *Taxonomic recommendations for European birds* by Sangster et al. (2002a), p 156, in the January issue of *Ibis*. The first AERC report was already accepted by *Ibis* at the time of the meeting. Earlier, the CSNA had advocated a PSC split in *Dutch Birding* (Sangster et al. 1998) and in August 2000 the split by the AERC TAC was announced in a press release without substantiating the decision ([Marr] 2000). The split of Common and Lesser Redpoll was also formally accepted in the 27th report of the BOURC (October 2000) *Ibis* 143: 171–175) and by the AOU (Banks et al. 2002). The proposal by Knox et al. (2001) was mainly based on their interpretation of a paper by Lifjeld & Bjerke (1996), going further than the one in the original paper. The first author of Lifjeld & Bjerke (1996), J. Lifjeld (in litt. 23 May 2003), however, agrees with the

new AERC TAC and explains why: 'I fully agree that the split seems premature. I feel somewhat responsible for this, due to my paper on assortative pairing by *cabaret* and *flammea* in the Norwegian journal *Cinclus* in 1996 [Lifjeld & Bjerke 1996]. However, it was based on a very small sample, and it seems that there are intermediates in both the alpine and coastal populations in Norway, making the situation more complex than I first anticipated. The situation is therefore far from clear. I have taken on a PhD student now, and over the next three years she will look for diagnostic markers, using AFLP and microsatellites of *cabaret* and *flammea*, as well as *hornemanni*. I am not confident we will find any clear differentiation between the former two. Given the current strong research interest in this issue I think it is quite sensible to await further results before making any changes in the taxonomy. The second author of the paper, B. Bjerke, is my museum technician. He may not have any strong opinions in this matter. He collected the birds described in our 1996 paper. Otherwise, he hasn't worked much on this issue.' Ottvall et al. (2002) – among others co-authored by J. Lifjeld – and important new information since the publication of Knox et al. (2001) found no genetic differentiation between *flammea* and *cabaret*. The lack of genetic differentiation among the phenotypic redpoll forms could either be the result of high present-day gene flow or morph differentiation following a rapid and recent expansion. According to the *Guidelines* molecular divergence is not a character (a particular sequence is) but sequence divergence estimates can be used as an objective measure of overall divergence in comparative analysis and gives a 'rough indication of how likely it is that reproductive incompatibilities have evolved between two taxa.' P.-A. Crochet commented: 'To "overcome" the genetic results (which show either ongoing gene flow or very recent isolation, in the last 15 000 years) strong arguments of reproductive isolation would be needed. These arguments are not available. So there is nothing strong enough to demonstrate that the genetic homogeneity of *cabaret* and *flammea* results from a very recent separation and that the species have evolved some mechanisms of reproductive isolation strong enough to prevent them from interbreeding now and to keep them apart if they meet.' The last sentence of Ottvall et al. (2002) is: 'we recommend that the two taxa should be treated as subspecies.' S. Bensch (in litt.) did a preliminary test on *cabaret* and *flammea* using AFLP and there was no detectable difference, supporting the mtDNA analysis. T. Aalto, A. Lindholm and M. Putkonen are studying *cabaret*, *flammea* and *exilipes* in Finland. A. Lindholm commented: 'We have studied the problem but some difficulties are remaining. In the *Guidelines*, much importance is laid on the intergradation zones, their stability and width. I think that I have seen several intermediate redpolls but it is very hard to prove a bird to be an intermediate, statistically or otherwise. I could easily show that there is a cline in Finnish non-breeding birds from small brown through medium-sized greyish-brown to larger greyish birds (corresponding to *cabaret* – *flammea* – *exilipes*). But it is not so easy to show that this cline is even, genetically true and not only caused by individual variation of the taxa. I studied a smallish sample (about 20) of British *cabaret* skins and they were quite consistent, smallish and brownish. In Finland, it is much more difficult to classify all the birds.' There is also ongoing research on *cabaret* in Norway, Sweden, and Poland (T. Stawarczyc, pers. comm.). The contact zone between *flammea* and *cabaret* now extends to Poland, an important reason not to focus on the Norwegian situation only. So far, Redpoll has been considered as a breeding species in Poland exclusively in the Tatra and Sudete mountains. The first possible breeding records of Redpolls in northern Poland were noted in 1989 at Jastarnia and in 1992 near Mierzeja Sarbska bar. A study by Sikora (2001) indicates the presence of a large and permanent breeding population on the Baltic coast. At present it is unknown which taxa are breeding on the Polish coast and this requires further research. It may be wise, however, to await more information on the breeding of Redpolls on the northern Baltic coast of Poland before splitting *flammea* and *cabaret* (both *flammea* and *cabaret* have been observed at Hel Peninsula, during the September 2001 AERC meeting and breeding is likely to be sympatric). At Falsterbo ringing station, an increasing amount of individuals difficult to assign to one taxon or the other is reported (e.g. 10% in 1999, reported anecdotally by Wirdheim 2000 and Nilsson 2003). The increasing occurrence in Fennoscandia of such birds should be further studied; notice, however that some intermediate individuals – presumed to be hybrids – are not possible to identify either when handling the bird or in the laboratory. Similarly, the relationship between Arctic Redpoll *C. hornemanni* *exilipes* and Common Redpoll *C. f. flammea* should be further studied (Seutin et al. 1995; M. Putkonen is currently investigating genetic differences between Common and Arctic Redpolls), but for reasons of stability no change is proposed for this pair until new and conclusive evidence is published. Redpoll taxonomy in general needs more research [e.g. comments on Redpoll *C. flammea* and Arctic Redpoll *C. hornemanni* in BOURC (1988) and BOURC (1990)]; particularly the taxonomic position of *C. f. rostrata* and '*islandica*' is insufficiently studied. Both STC and CAF propose to keep *cabaret* and *flammea* lumped until there is more convincing evidence for splitting and request to keep the Voous list as status quo. The TAC recommends that individual countries should not make any change in their list concerning redpolls as

long as the case is pending. In the case of both rejected and pending proposals, the status quo is Voous (1977c) for species and BWP for subspecies.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
	A			A

Azores Bullfinch *Pyrrhula murina*

CSNA Considered as a species distinct from *P. pyrrhula* by Aubrecht (2000) and Ramos & Nunes (2001). The decision is pending in the AERC TAC. The geographical variation of Bullfinch needs to be evaluated and it needs to be assessed whether the Azores Bullfinch is clearly more distinct than any other subspecies of Bullfinch.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
P	A	A	A*	P

(*) Accepted as 'semi- or allospecies'.

Evening Grosbeak *Hesperiphona vespertina* suggested to become *Coccothraustes vespertinus* by AOU (1983).

This proposed name change has not yet been addressed by the AERC TAC. The AOU named this species *Coccothraustes vespertina* (1886), *C. vespertinus* (1895), *Hesperiphona vespertina* (1910, 1931, and 1957) and more recently *Coccothraustes vespertinus* (1983, 1998) again. So far, this name change has not been adopted by any of the European TCs. Clement et al. (1993) state that *Hesperiphona* Grosbeaks show close similarities with *Coccothraustes* but that no link between the two genera has ever been established.

Snow Bunting *Plectrophenax nivalis* suggested to be treated as *Calcarius nivalis* by Klicka et al. (2003) and Carson & Spicer (2003)

CAF Klicka et al. (2003) place the two *Plectrophenax* (*nivalis* and *hyperboreus*) and the four *Calcarius* (*lapponicus*, *ornatus*, *pictus* and *mccownii*) in the same ancient clade, sister-clade of the Emberizidae (Parulinae, Emberizinae, Icterinae, Thraupinae, Cardinalinae) and advocate shifting *nivalis* and *hyperboreus* to *Calcarius nivalis* and *C. hyperboreus*. There is, however, no consensus within the AERC TAC about this. A.J. Helbig commented: 'The *calcarius* clade is basal and thus given subfamily rank. *Calcarius* may be paraphyletic (Klicka et al. 2003), but this requires confirmation. Even if this is, it is not mandatory to subsume *Plectrophenax* under *Calcarius* (instead, McCown's Longspur *C. mccownii* could be assigned to *Plectrophenax*).' P.-A. Crochet (CAF) replied: 'A.J. Helbig provides some good arguments against splitting, but even the position of *lapponicus* in the Klicka tree is not well supported. It could group with the *nivalis* clade rather than *ornatus* / *pictus* clade. Given uncertainties on the relationships within this clade, wouldn't it be better to keep one genus, to be on the safe side? Carson & Spicer (2003) confirm that the generic assignment of *P. nivalis* should be changed. Using 1700 bp of three mitochondrial genes, they recovered a sister relationship of *P. nivalis* and *C. lapponicus*, with *C. ornatus* the sister taxon of the *nivalis* / *lapponicus* clade. This result is supported by all analyses, often with strong support. Together with Klicka et al. (2003) it provides good evidence to subsume *Plectrophenax* in *Calcarius* (note that *lapponicus* being the type species of *Calcarius*, it is NOT possible to move *mccownii* and *lapponicus* to *Plectrophenax* and to keep *Calcarius* for the *ornatus* / *pictus* clade).'

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
	A*		R	P

(*) Accepted based on Carson & Spicer (2003).

Blue Grosbeak *Guiraca caerulea* proposed to become *Passerina caerulea* by Klicka et al. (2001)

BOURC TSC This proposal has not yet been dealt with by the AERC TAC as no records of this species have been accepted in category A anywhere in the WP.

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Appendix 1. Splits already accepted by Sangster et al. (2002a)

Soft-plumaged Petrel *Pterodroma mollis* complex to be treated as three species (accepted by the AERC TAC in Sangster et al. 2002a):

- Soft-plumaged Petrel *Pterodroma mollis* (polytypic; extralimital *P. m. mollis, dubia*)
- Fea’s Petrel *Pterodroma feae* (monotypic, incl. ‘*deserta*’)
- Zino’s Petrel *Pterodroma madeira* (monotypic)

CAF This split is accepted unanimously by the AERC TAC. Fea’s and Zino’s Petrels are already treated as separate species in the Swedish Holarctic checklist (SOF 1995). This threefold split gradually gained general acceptance since Bourne (1983). Nunn & Stanley (1998) concluded on the basis of cytochrome *b* sequences that *P. mollis* and *P. feae* are not even sister-taxa. See Sangster et al. (1998) and references therein for reasons to split the Soft-plumaged Petrel complex into three species. Since the review in Sangster et al. (1998) Zino & Bischoff (2001) published comparative biometrics of *P. madeira* and *P. feae*. Nunn & Zino (in prep.) will reveal molecular genetic information on the Madeiran taxa but did not include birds from the Cape Verde Islands. The split of these *Pterodroma* petrels (together with 12 other splits) was first announced in a press release as accepted by the AERC and the BOURC TSC by [Marr] (2000). The information in this press release was later expanded upon in Sangster et al. (2002). For a review of literature relating to the soft -plumaged petrel complex in the Western Palearctic and a detailed review of its taxonomic history, see Harrop (in press). This paper also infers that there is no hard evidence for the occurrence of *P. mollis* in the Western Palearctic. *P. mollis* is not monotypic but polytypic, with subspecies *dubia* in the Indian Ocean (Kerguelen, Crozet, Marion, Prince Edward, Amsterdam Islands) which has darker upperparts, broader collar, and intermediate and dark morphs (see Mathews, G.M. (1924) *Bulletin of the British Ornithologists’ Club* 44: 70; Clancey et al. 1981; Carboneras 1992; Bretagnolle 1995; Shirihai & Garrett 2002). There is disagreement over the subspecific identity of Soft-plumaged Petrels breeding on the Antipodes.

ID: field identification was discussed by Tove (2001). In both bill length and bill depth, Fea’s is larger than Zino’s without overlap. Wing shape and body size (weight) also differ.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A	A	A

Manx Shearwater *Puffinus puffinus* to be treated as six species (accepted by the AERC TAC in Sangster et al. 2002a):

- Manx Shearwater *Puffinus puffinus* (monotypic)
- Yelkouan Shearwater *Puffinus yelkouan* (monotypic)
- Balearic Shearwater *Puffinus mauretanicus* (monotypic)
- Black-vented Shearwater *Puffinus opisthomelas* (monotypic; extralimital)
- Fluttering Shearwater *Puffinus gavia* (monotypic; extralimital)
- Hutton’s Shearwater *Puffinus huttoni* (monotypic; extralimital)

CAF With the exception of Townsend’s Shearwater *P. auricularis* (polytypic; extralimital *P. a. auricularis, newelli*), Voous (1973) included six taxa in Manx Shearwater *P. puffinus*. Jehl (1982) recommended to split Manx Shearwater into at least five species, but included *newelli* in *P. auricularis*, whereas *yelkouan* and *mauretanicus* were provisionally kept in *P. puffinus* (see Bourne et al. 1988 for a review). Following Bourne et al. (1988), BOURC (1991) separated Mediterranean Shearwater *P. yelkouan* (including the race *mauretanicus*) and treated Manx Shearwater *P. puffinus* as monotypic (implicitly recognising a number of other splits). Mediterranean Shearwater is best treated as two species, *P. yelkouan* and *P. mauretanicus*, based on differences in morphology, vocalisations, genetic data, feeding habitat and timing of breeding. Sangster et al. (1997) initially summarized the reasons to split *P. yelkouan* into two species, which they considered as originating from different stocks. However, the sister relationships of Balearic and Yelkouan shearwaters have thereafter been confirmed by bio-acoustics and genetics. The vocalisations of both forms show marked similarities, strongly suggesting close relationships (Bretagnolle & Zotiér 1998). Also, the observed 2.2–2.9% divergence in mtDNA between *mauretanicus* and *yelkouan* compares with 2.9–3.5% divergence between *yelkouan* and *P. puffinus* or 3.5% divergence between *P. puffinus* and *P. assimilis*; moreover all phylogenetic reconstructions have placed *mauretanicus* and *yelkouan* as neighbour species at the top of the same branch, and the authors concluded that ‘*P. mauretanicus* and *P. yelkouan* are “good” species

which appear to have evolved from allopatric populations of an ancestral member of the *Puffinus* clade about 1 to 2 million years ago' (Heidrich et al. 1998, 2000). See Yésou & Paterson (1999) and Sangster et al. (2002c) for a more detailed reviews of the reasons to split these two species, highlighting plumage differences, osteological differences and molecular phylogenetics. It should be added, however, that in 2000 birds referred to as *P. yelkouan* (plumage characteristics of this species by unpublished photographs, specific identification seemingly supported by preliminary genetic evidence, but biometrics much closer to *mauretanicus*) have been found breeding sympatrically with *P. mauretanicus* in Menorca. No hybridisation has been recorded, at least for sure in Mallorca, Cabrera, Ibiza and Formentera, where most breeding sites are in cliffs and remain inaccessible to ornithologists (R. Gutiérrez in litt.). Obviously, the situation in Menorca is worthy of further study. Accepted by the BOURC in its 27th report (October 2000) *Ibis* 143: 171–175.

ID: for *P. yelkouan*, see Blomdahl et al. (2003)

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A*	A	A**	A***

(*) CAF agrees on the split of *yelkouan* and *mauretanicus* but has not (yet) studied the other taxa.

(**) Accepted as 'semi- or allospecies'.

(***) STC accepts a split in the case of the first three mentioned species. For the remaining four we wish to state that although we are uninformed, apart from reading the paper cited above, we have no other knowledge to support the arrangement as given above. According to the cited report, the work by Austin (1996) and Heidrich et al. (1998) both are said to 'indicate' rather than conclusively prove.

Common Teal *Anas crecca* to be treated as two species (accepted by the AERC TAC in Sangster et al. 2002a):

- Common Teal *Anas crecca* (polytypic: *A. c. crecca*; extralimital: *A. c. nimia*)
- Green-winged Teal *Anas carolinensis* (monotypic)

CAF Accepted by CSNA (Sangster et al. 1998, p 25), BOURC in its 27th report (October 2000) *Ibis* 143: 171- 175 and AERC TAC (Sangster et al. 2002a). Green-winged Teal is best treated as a separate species based on differences in morphology, courtship behaviour and genetic data (Sangster et al. 2001). For general information on the phylogeny of dabbling ducks, cf. Johnson & Sorenson (1999). This split is unanimously accepted by the AERC TAC. Note, however, that the AOU has not accepted this split yet. Furthermore, genetic arguments put forward to justify this split are ambiguous: the suggested closer relationships of *carolinensis* with the South American *A. flavirostris* rather than with *crecca* (Johnson & Sorenson 1999) could be due to (ancient?) horizontal gene transfer (introgression) between *carolinensis* and *flavirostris*. The alternative hypothesis that *crecca* and *carolinensis* are really highly divergent genetically (as much as *A. platyrhynchos* from *A. acuta*) and share their extremely similar male and female plumage by convergence or long retention of ancestral characters is rather unlikely. Furthermore, Zink et al. (1995) found that the *carolinensis* haplotype also occurs in *crecca* from Russia, indicating that the two taxa are not reciprocally monophyletic in mtDNA. This is not totally incompatible with their species status but indicates that the situation is more complex than stated by Sangster et al. (2001, 2002a). (P. -A. Crochet)

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A	A*	A

(*) Accepted as 'semi- or allospecies'.

Steppe Eagle *Aquila rapax* to be treated as two species (accepted by the AERC TAC in Sangster et al. 2002a):

- Tawny Eagle *Aquila rapax* (polytypic: *A. r. belisarius*; extralimital: *A. r. vindhiana* and *rapax*)
- Steppe Eagle *Aquila nipalensis* (polytypic: *A. n. nipalensis* and *orientalis*)

CSNA Steppe Eagle and Tawny Eagles are treated as two species because of differences in size, structure, colour of iris, plumage (mainly in juvenile and immature plumages), and age at which the juvenile plumage is acquired (see comments, not all of them in favour of a split, in Ripley 1961, pp 278 –279; Clancey 1966, p 17; Brooke et al. 1972; Dowsett & Dowsett-Lemaire 1980, p 155; Cramp et al. 1980, p 216–225 and particularly Clark 1992). Steppe and Tawny Eagles are treated as separate species in the Swedish Holarctic checklist (SOF 1995). CSNA adopted this split in 1997 (Sangster et al. 1997). The split is now widely accepted e.g. Brown et al. (1982), del Hoyo et al. (1994) and Ferguson-Lees & Christie (2001). The spelling of *A. r. belisarius* is correct: Belisarius is the name of a Byzantin general (c. 505–565) under Justinian I, hence a noun (M. Gosselin in litt.).

ID: For identification of Steppe Eagle, see Forsman (1991, 1999). The separation of Tawny and Steppe Eagles in Israel is treated by Shirihai (1994).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A	A	A

Imperial Eagle *Aquila heliaca* to be treated as two species (accepted by the AERC TAC in Sangster et al. 2002a):

- Imperial Eagle *Aquila heliaca* (monotypic)
- Spanish Imperial Eagle *Aquila adalberti* (monotypic)

CAF Spanish Imperial Eagle is best treated as a separate species based on differences in morphology (incl. rather clear differences in juvenile and immature plumages), genetic data, and on fine but apparently consistent differences in structure of adults (cf. Hiraldo et al. 1976, González et al. 1989, González 1991, Wink & Seibold 1994, Heredia 1996, Forsman 1999 and Ferrer 2001).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A	A*	A

(*) accepted as 'semi- or allospecies'

Lesser Golden Plover *Pluvialis fulva* to be treated as two species (accepted by the AERC TAC in Sangster et al. 2002a)

- American Golden Plover *Pluvialis dominica* (monotypic)
- Pacific Golden Plover *Pluvialis fulva* (monotypic)

STC Widely accepted split. Conover (1945) showed that the species overlapped in Alaska without interbreeding. Connors (1983) and Connors et al. (1993) confirmed sympatric breeding; though some intermediate specimens were found, their occurrence was not higher in the area of sympatry than elsewhere. On account of this, the BOURC accepted the split in its 12th Report (*Ibis* 128: 602). Identification criteria for both taxa have evolved and it is not clear whether the intermediate specimens reported by Connors would still be unidentifiable. Stepanyan (1990) reported sympatric breeding in NE Asia. Byrkjedal & Thompson (1998) confirmed consistent differences in vocalisations and habitat choice of American and Pacific Golden Plovers, thus assortative mating.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A	A	A

Water Pipit *Anthus spinoletta* to be treated as three species (accepted by the AERC TAC in Sangster et al. 2002a)

- **Water Pipit** *Anthus spinoletta* (polytypic: *A. s. spinoletta*, *coutellii*; extralimital: *A. s. blakistoni*)
- **Rock Pipit** *Anthus petrosus* (polytypic: *A. p. petrosus* incl. 'meinertzhageni' and 'kleinschmidti' and *A. p. littoralis*)
- **Buff-bellied Pipit** *Anthus rubescens* (polytypic: *A. r. rubescens* and *japonicus*; extralimital: *A. r. geophilus*, *alticola*, *pacificus*)

STC Some authors (e.g. Vaurie 1959, Hall 1961 and Williamson 1965) divided Water Pipit *A. spinoletta* s.l. into two ecologically distinct groups, Water Pipit inhabiting the mountains of Europe, Asia and North America and Rock Pipit along the coastlines of NW Europe. Bannerman (1953) accepted them as two valid species. Voous (1977c) included *japonicus* and *petrosus* in *A. spinoletta*. Glutz von Blotzheim & Bauer (1985) on the other hand did not accept Rock Pipit *A. petrosus*, but divided the Water Pipit complex into *A. spinoletta* and *A. rubescens*. This was based on research by Nazarenko (1978) who showed that *A. s. blakistoni* and *A. r. japonicus* occurred sympatrically in Transbaikalia (former USSR), but in mainly different habitats without hybridising. Devillers (1980), BOURC (1986), Alström & Mild (1987), Knox (1988), Alström et al. (2003) and many others divided the complex into three species, an opinion which has gained wide acceptance. The poorly differentiated forms *A. p. 'meinertzhageni'* and '*kleinschmidti*' can be included in *A. p. petrosus*. In the Western Palearctic, *A. spinoletta* and *A. petrosus* are allopatric, but the morphological and ecological differences are at least as well-marked as between *A. spinoletta* and *A. rubescens* in the Eastern Palearctic.

ID: Alström & Mild (1997, 2003).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A	A	A

Yellow-browed Warbler *Phylloscopus inornatus* to be treated as two species (accepted by the AERC TAC in Sangster et al. 2002a):

- Yellow-browed Warbler *Phylloscopus inornatus* (monotypic)
- Hume's Leaf Warbler *Phylloscopus humei* (polytypic: *Ph. h. humei*; extralimital: *Ph. h. mandellii*)

STC Hume's Leaf Warbler is already treated as a separate species in the Swedish Holarctic checklist (SOF 1995). Svensson (1992) treated it as a distinct species. Irwin et al. (2001a) suggested that Mandelli's Leaf Warbler *Ph. h. mandellii* would be recognised as a distinct species under the PSC. This was followed by the editors of Dutch Birding (*Dutch Birding* 22, 2002: 22), but not by the CSNA, nor by the STC. The AERC TAC awaits additional research and provisionally maintains *mandellii* as a subspecies of *Ph. humei*. See BOURC (1996). This split is accepted unanimously by the AERC TAC.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A	A	A

Bonelli's Warbler *Phylloscopus bonelli* to be treated as two species (accepted by the AERC TAC in Sangster et al. 2002a):

- Bonelli's Warbler *Phylloscopus bonelli* (monotypic)
- Balkan Warbler *Phylloscopus orientalis* (monotypic)

STC Bonelli's Warbler is best treated as two separate monotypic species based on differences in morphology, vocalisation (both call and song) and genetic data (Helbig et al. 1995), see BOURC (1996) and references therein. This split is accepted unanimously by the AERC TAC.

ID: The identification of Eastern and Western Bonelli's Warbler is treated by Preddy (1998) and Svensson (2002a,b).

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A	A*	A

(*) accepted as 'semi- or allospecies'.

Chiffchaff *Phylloscopus collybita* complex to be treated as four species (accepted by the AERC TAC in Sangster et al. 2002a):

- Common Chiffchaff *Phylloscopus collybita* (polytypic: *Ph. c. collybita*, *abietinus* and *tristis*)
- Canary Island Chiffchaff *Phylloscopus canariensis* (polytypic: *Ph. c. canariensis* and – recently extinct – *exsul*)
- Iberian Chiffchaff *Phylloscopus ibericus* (monotypic)
- Mountain Chiffchaff *Phylloscopus sindianus* (polytypic: *Ph. s. lorenzii*; extralimital: *Ph. s. sindianus*)

STC Chiffchaff is best treated as four species based on differences in morphology, vocalisation, and genetic data (Helbig et al. 1993, Helbig et al. 1996, Clement et al. 1998). The form *tristis* (Siberian Chiffchaff) is currently best treated as conspecific with *collybita* until further research has been conducted, including molecular, regarding interaction with *abietinus*. Chiffchaff bibliography was usefully summarized by O. van Rootselaar (<http://www.birder.com/science/taxonomychat/0343.html>); this text is adapted to the current situation below. This split was accepted by the BOURC in its 25h report (October 1998) *Ibis* 141: 175–180 (<http://www.bou.org.uk/recrep25.html>).

(1) Common Chiffchaff *Phylloscopus collybita*

(1.1) *Phylloscopus collybita collybita* (Vieillot) 1817

(1.2) *Phylloscopus collybita abietinus* (Nilsson) 1819

Sylvia abietina Sven Nilsson, 1819, K. Vetenskaps Acad. Nya Handlingar, Stockholm, p. 115 ('north of Trondheim, in the spruce forests of Stjørdalen, Inderøya, and Namdalen, Norway'; erroneously said to be Sweden in Ticehurst, 1938, p. 42; restricted to Stjørdalen by Holgersen, 1955, *Sterna*, 18, p. 3). In Hartert 1910, said to be 'Schweden, Trondjem u.a.'.

Range: Scandinavia and E Europe (from Poland & Romania) east to somewhere between the Pechora and the Ob (Ural mountains?) and possibly in the Caucasus; wintering in Arabia and Somalia. – Swedish populations of Common Chiffchaff *Ph. c. collybita* and *Ph. c. abietinus* reacted more strongly to their own song than to the other subspecies and in conjunction with e.g. differences in

habitat choice, Hansson et al. (2000) predicted there would be only limited interbreeding if these subspecies were to meet in the future.

(1.3) *Phylloscopus collybita tristis* Blyth 1843

Phylloscopus. *tristis* Edward Blyth, 1843, *Journ. Asiat. Soc. Bengal*, Vol. 12, p. 966 (Calcutta).

Range: NC Asia, from the Yenisey east to the Kolyma River; wintering in N India and Bangladesh. Occurrence in Italy is described in *Riv. ital. Orn.* 65:3–13 (N. Baccetti & G. Fracasso in litt.). – Sometimes treated as a distinct species. Early reports (Meves, 1875, *Journ. f. Orn.*, p. 430) mention this form from around Perm 56° E, 57° N). Song recorded on L. Svensson (1984) *Soviet Birds*, and K. Mild (1987) *Soviet Bird Songs*, no. 86. See also *Birding World* 10/4: 153–154 for occurrence in Britain. – A. Lindholm (in litt.) recorded the song of 16 different individuals of Chiffchaff *Ph. collybita* near Syktyvkar, the capital of Komi republic, N Russia in June 2002 (hundreds of km west of Urals). Most of these were very different from *Ph. c. abietinus* – in fact quite *tristis*-like. The song of *tristis* is well described by Martens & Meincke (1989). In comparison to recordings published by Mild (1987) many of the Komi songs were somewhat odd, and looking at the sonograms all of them somewhat intermediate between *collybita* and *tristis*. The results will possibly be published in a publication about Komi and other ‘sub-*tristis*’ songs. It is not clear whether ‘*tristis* call’ is really confined to birds which sing like *tristis* and vice versa.

(1.4) *Phylloscopus collybita menzbieri* Shestoporov 1937

Phylloscopus collybitus menzbieri Shestoporov, 1937, *Keys Vertebrata Turkomania, Aves*, Vol. 4, p. 244 (Kopet Dag, SW Transcaspia).

Range: NE Iran and Turkmenistan (SW Transcaspia). – Sometimes included in (or synonymous with) *tristis*, but recognised by Wolters. See Marova & Leonovich (1997) for reasons to recognise this taxon.

**Subspecies of *Ph. collybita* which are not recognised here
(in chronological order by year of description):**

(1) *Phylloscopus collybita brevisrostris* (Strickland) 1837

Sylvia brevisrostris Hugh Edwin Strickland, 1837, *Proc. Zool. Soc. London*, p. 98 (near Smyrna, Turkey).

Range: NW Turkey, wintering in Persia & Arabia (Tristram, *E Persia*, Vol. II, p. 181) and Israel (Tristram, 1877, *Annals and Mag. Nat. Hist.*, p. 28). – Closer to *abietinus* than to *lorenzii*, but possibly also related to the latter. Erroneously listed as ‘Strickland 1836’ in Cramp. Michel Menzbier (1882, *Revue comparative de la faune ornithologique des Gouvernements de Moscou et de Toula*) has published a description of over 40 birds in *Izdanje Imperatorskago Moskovsk. Ovsjestva Ispytatelej Prirodi*, Vol. LVI (1881), No. 3, pp 23–24. – This taxon was not discussed by Ticehurst (1938). For *Birding World*, there was a paper in preparation on this form, compared to *lorenzii* (BW 9/6: 246), for which good photographs were urgently needed (BW 10/6: 201).

(2) *Phylloscopus collybita naevia* Severtzov 1873

Ficedula fulvescens (et var. *naevia*) Nicolai Severtzov, 1872 (1873), *Vertikalnoje i Gorizontavnoje Raspredylenie Turkestanskichr Zivotnychr* [‘The Distribution of Animals of Turkestan’], p. 126 (Turkestan, U.S.S.R.).

Range: Turkestan, between Lake Aral and Lake Balkhash. – Merely listed as a synonym of *tristis* in Cheng (p. 798), but might also be related to *sindianus*. Lars Svensson questions: ‘Do any Chiffchaffs breed there at all? If so, in what habitat? Deserts? Riverine forests along Syrdar’ya? Or is this taxon just based on migrant *tristis*?’

(3) *Phylloscopus collybita altaiana* Sushkin 1925

Phylloscopus tristis altaiana Peter Petrovic Sushkin, 1925, *Sposok i Raspr. Ptits. Russk. Altai* [‘List and distribution of birds of the Russian Altai and nearest parts of NW Mongolia, with a description of new or imperfectly known forms’], p. 71 [in 1–78] (Biisk, Kotanda, C Altay mountains).

Range: Russian Altai mountains (c. 85° E, 50° N). – Listed as a synonym of *tristis* in Cheng (p.798). Included in *tristis* by Ticehurst (1938), who also included *fulvescens*, *altaiana* and *avillaris* (Sushkin, *Bull. Brit. Orn. Club* 14, p 44; type locality: Yenesei river at 55°N) in *tristis* – and sensibly so according to L. Svensson.

(4) *Phylloscopus collybita caucasica* Loskot 1991

Phylloscopus collybita caucasica V.M. Loskot, 1991, *Phylloscopus collybita caucasica*: new subspecies of Chiffchaff (Aves, Sylviinae). *Vestnik Zoologie*, Vol. 3, p. 91 (Caucasus).

Range: Lower zone in the Caucasus, altitudinally separated from *lorenzii* in most areas. – Recently split from *abietinus*. Intermediate in appearance between *abietinus* and *tristis* or *lorenzii* (probably not safely separable), and where *caucasica* and *lorenzii* come into contact further study is necessary to establish their taxonomic relationship. Because of gender agreement, the correct name is *Ph. c. caucasicus*. L. Svensson commented: ‘Loskot has briefly shown the type series to me. I could not see it as sufficiently separate from *abietinus*. A firm opinion would require a more thorough study, something which I was not allowed to perform at the time.’

Traditionally, within the chiffchaffs, only one species was recognised (in one genus: *Phylloscopus* Boie 1826), covering large parts of Europe, Asia Minor and the Far East (e.g. Vaurie 1959; Gruson 1976: 125 and Walters 1980: 224). The OBC-Checklist (Inskipp et al., p. 172) mentions *tristis* as a candidate for being separated as a distinct species (as did Wolters 1980, p. 364), although further studies seem to be necessary.

Canary Islands Chiffchaff *Phylloscopus canariensis*

(1) *Phylloscopus canariensis canariensis* (Hartwig) 1886

Phyllopneuste rufa canariensis W. Hartwig, 1886, *Journ. f. Orn.*, Vol. 34, p. 486 (Tenerife).

Range: W Canary Islands: El Hierro, La Palma, La Gomera, Tenerife and Gran Canaria. – Illustrated in Moreno (Guía de las Aves de las Islas Canarias, p. 67) and Heinzel, Fitter & Parslow (5th ed., p. 297). The author’s name is sometimes spelled ‘Hartweig’.

(2) *Phylloscopus canariensis exsul* Hartert 1907

Phylloscopus collybita exsul Ernst Johann Otto Hartert, 1907, *Vög. pal. Fauna*, p. 505 (Lanzarote, E Canaries).

Range: E Canary Islands: Fuerteventura and Lanzarote (Valle de Haría). – More chestnut-backed and shorter-winged than *canariensis*. Now probably extinct. Illustrated in Heinzel, Fitter & Parslow (5th ed., p. 297).

Iberian Chiffchaff *Phylloscopus ibericus*

(1) *Phylloscopus collybita ibericus* Ticehurst 1937

Phylloscopus collybita ibericus Claud Buchanan Ticehurst, 1937, *Bull. Brit. Orn. Club*, Vol. 57, p. 64 (Paul d'Argila, near Coimbra, Portugal).

Range: Iberia, Algeria. – Recognised in Howard & Moore 1984.

(1a) *Phylloscopus collybita brehmii* (Homeyer) 1871

Phyllopneuste Brehmii Eugen Ferdinand von Homeyer, 1871, *Erinnerungsschrift Versammlung Deutschen Ornithologen* (Görlitz), 1870, p. 48 (Portugal).

Range: From the W Pyrenees south to several scattered locations on the NW African coastline. – Erroneously listed as 'Homeyer 1870' by Cramp.

Ticehurst (1937) and Svensson (2001b) advocate *Phylloscopus ibericus* as the right name for Iberian Chiffchaff. Salomon, Voisin & Bried (2003) recognise two subspecies: *Ph. i. ibericus* (southern form: C Portugal & Andalusia) and *Ph. i. biscayensis* ssp. nov. (northern form: N Portugal, Galicia, Cantabrian Cordillera, Spanish Basque province, Navarre, French Basque country). It should be noted, however, that these subspecies are based on statistically different means, one of the less widely accepted 'subspecies concepts'. L. Svensson commented that he opposes the recognition of subspecies on such grounds: 'The demonstrated differences are very slight (e.g. male wing length differs by 1.28 mm on average). The claimed range divide between the two populations has been questioned by other workers, and natural habitat differences in the north and the south explain habitat choice differences. The claimed two races would never pass the 90% rule, probably not the 75% rule either.' As Iberian Chiffchaff is also a breeding bird in France, the CAF also examined this matter and equally decided to reject the two newly proposed subspecies. Earlier papers on *ibericus* include Niethammer (1963) and following Spanish references: Bernis (1945, 1946, 1962), Bernis & Bernis (1963), Del Junco (1963) and Valverde (1967). Salomon authored a series of important papers on *Ph. ibericus*, mainly focusing on the vocalisations, morphology and relationship with *Ph. c. collybita* in the contact zone (Salomon 1987, 1988, Erard & Salomon 1989, Salomon 1989, Salomon & Hemim 1992, Salomon 1997 and Salomon et al. 1997). The female offspring of hybrid pairs *Ph. collybita* × *Ph. ibericus* is sterile (or shows a reduced fertility); this postzygotic reproductive barrier is a fine example of Haldane's rule (Helbig et al. 1996; Helbig 2000a). Some descriptions of vagrants can be found in Paul (1984) and Mülstegen et al. (1994). Iberian Chiffchaff has turned up as far north as Sweden (confirmed by mtDNA analysis).

Mountain Chiffchaff *Phylloscopus sindianus*

1. *Phylloscopus sindianus lorenzii* (Lorenz) 1887

Phyllopneuste Lorenzii Theodor (Fédor) Karlovic Lorenz (ex Severtsov MS), 1887, *Beitr. Kennt. Ornith. Fauna Nordseite Kaukasus*, p. 28, pl. 2, figs. 2–4 (Kislovodsk, N Caucasus, fide Ticehurst, 1938, *Syst. Review Genus Phylloscopus*, p. 49).

Range: High-Caucasus, Trans-Caspia, Armenia, possibly NE Turkey (*Dutch Birding* 1: 117) where it might meet with *brevirostris* in the Pontic mountains. Vagrant to Israel (Paz & Eshbol: 218; Shirihai: 529). – Photographs and a colour plate of this form are to be found in Shirihai (1987) *International Bird Identification*, pp 62–63, and another plate in Porter, Christensen & Schiermacker-Hansen, pl. 90 (p. 181), where also *brevirostris*, *abietinus*, *tristis* and *sindianus* are illustrated in colour by Brian Small. Treated as a separate species in Knystautas (*Birds of Russia*, p. 191), Sibley & Monroe (supplement), Roselaar, p. 124 and Stepanyan in *Birding World* 4: 402.

2. *Phylloscopus sindianus sindianus* Brooks 1879

Phylloscopus sindianus W. E. Brooks, 1879, Ornithological observations in Sikkim, the Punjab and Sind. *Stray Feathers*, A Journal of Ornithology for India and its dependencies, vol. 8, p. 476 [in 464–489] (Sukkur, Sind, Pakistan; E Kashmir; hills of Shache, China).

Range: Breeds between c. 2500 and 4400m in willow and poplar groves, tamarisks, gardens and *Hippophae* scrub in E Afghanistan, Pakistan and the Pamir mountains to the NW Himalayas; wintering in N India. – Sometimes (erroneously?) listed as 'Brooks 1880'. Treated as a separate species in Voous and Heinzel, Fitter & Parslow, as a subspecies of *collybita* in Vaurie and Glutz, Bauer & Bezzel and recently Sibley & Monroe supplement, p. 69. For photographs and colour-plates, see *lorenzii*-account.

Subspecies of *Ph. sindianus* which is not recognised here:

1. *Phylloscopus sindianus subsindianus* Zarudny 1913

Phylloscopus collybita subsindianus Nikolai Alekseevich Zarudny, 1913, *Orn. Vestn.*, p. 269 (Pamir Plateau).

Range: Pamir mountains, SE Tajikistan. – Usually considered synonymous with *sindianus*. Ticehurst (1938) explained that '*subsindianus* was described by Zarudny from an adult (at the end of June!) and a moulting juvenile obtained in the Pamir-Altai, E. Bokhara – hopelessly inadequate material on which to found any race of *Phylloscopus*.'

The splitting of *sindianus* into a separate species was first suggested by Williamson in 1967 (and followed by Voous 1977c, Peters 1986, p 230, Cramp (1992) BWP V: 605–639 and Sibley & Monroe 1990:620). Helbig (2000c) suggests that splitting Mountain Chiffchaff only, would leave the rest of the Chiffchaff complex

Ph. collybita incl. *canariensis*, *ibericus*, *collybita* and *tristis*) paraphyletic. Sibley & Monroe have corrected their views in their 1993 supplement (p. 69), where they include *sindianus* in *collybita* again (as did Glutz, Bauer & Bezzel [1991] Handbuch 12/II: 1232–1292), but now split *lorenzii* as a distinct, monotypic species. Roselaar in his *Songbirds of Turkey* (pp 124–127) and a few others have supported this treatment. Beaman summarized the vocal differences in his *Palaearctic Birds* (pp 87–88), and concluded: ‘... if *lorenzii* deserves specific status then so may *canariensis* (together with *exsul*), *tristis*, *sindianus* and possibly *brehmii*.’ For reasons to include *lorenzii* in *Ph. sindianus*, see Williamson (1974) p 56 and literature cited there. There are indications that ‘*brevirostris*’/ ‘*caucasicus*’ is not mixing with Mountain Chiffchaff. From a phylogenetic point of view, it is recommendable to assign species rank to all Chiffchaff forms showing gene flow barriers or differences in song. They can be considered as ‘allospecies’ within a ‘superspecies’ (Helbig 2000). Many Chiffchaff forms have been described in the eastern part of the range, indicating complicated and often subtle geographical variation. Most of these forms have not been studied in such a detail as the western ones.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A*	A**	A***	A

(*) *Canariensis* and *sindianus* still pending.

(**) *Tristis* also recognised as a distinct species, *sindianus* to be split further.

(***) *Tristis* also accepted as ‘semi- or allospecies’, *sindianus* incl. ssp. *lorenzii*.

Great Grey Shrike *Lanius excubitor* to be treated as two species (accepted by the AERC TAC in Sangster et al. 2002a):

- Great Grey Shrike *Lanius excubitor* (polytypic: *excubitor* incl. ‘*galliae*’ and ‘*melanopterus*’, *homeyeri*, *przewalskii*, *sibiricus*; extralimital: *mollis* incl. *funereus*, *bianchii*, *invictus*, *borealis*)
- Southern Grey Shrike *Lanius meridionalis* (polytypic: *L. m. meridionalis*, *koenigi*, *algeriensis*, *elegans*, *aucheri* incl. *theresae*, *pallidirostris*; extralimital: *lahtora*, *buryi*, *uncinatus*, *leucopygos*)

STC This split is accepted unanimously by the AERC TAC. Southern Grey Shrike is best treated as a separate species based on slight but consistent differences in morphology, behaviour, habitat and partly sympatric breeding ranges (Isenmann & Bouchet 1993, Lefranc & Worfolk 1997). Sangster et al. (2002a) argue for elevation to specific status based on diagnostic plumage, behaviour, habitat, and demonstration of no interbreeding in contact zones.

The form *pallidirostris* is currently treated as conspecific with *meridionalis* based on available evidence. Based on characters like wing pattern and slender tarsi, *pallidirostris* belongs to *L. meridionalis*; its paleness in general plumage and the pale bill base in immature plumages are shared with other pale forms of *L. meridionalis*, like *L. m. elegans*. Sangster et al. (1999) consider *pallidirostris* to be a species separate from *L. meridionalis*. However, also some other taxa within the traditional *L. meridionalis* show distinctive characters, like *meridionalis* proper and *lahtora*, and these may also require separation. Much of the evidence to separate *pallidirostris* goes back to original research by Panov (1983): he found that *pallidirostris* differed from other Russian taxa he studied in small bill, narrower tail-feathers, unbarred underparts of juvenile, different call (especially advertising call) – notice, however, that L. Svensson (in litt.) could not find any significant difference in song – two broods annually, courtship behaviour, etc; moreover, *pallidirostris* overlapped in range with other taxa in Central Asia without apparent interbreeding (Panov 1983). One should realise that the other taxa Panov studied all belong to *L. excubitor* and not to *L. meridionalis*, as no member of *L. meridionalis* other than *pallidirostris* breeds in the former USSR, and the characters cited are shared with the Mediterranean and Middle East races of *L. meridionalis* (see Cramp & Perrins 1993). Without a more detailed study of voice, behaviour, and other characters of *pallidirostris* in comparison with, e.g., *L. m. elegans* or *aucheri*, separation of *pallidirostris* from *L. meridionalis* seems premature, and the same applies to, e.g., the separation of *lahtora*. Also, the situation in Iran and W Pakistan is in need of study, as *L. m. aucheri*, *lahtora*, and *pallidirostris* meet here and either grade into each other or show touching or overlapping breeding ranges (L. Svensson pers. comm., De Smet & Roselaar in press). See BOURC (1996).

According to C.S. Roselaar *leucopterus* Severtzov, 1873 (type locality: ‘Turkestan’) is a nomen nudum, as given only in a list without description and without designation of a type (*Madarász's Zeitschr. Ges. Orn.* 4: 16, 24, 1888), and *leucopterus* Severtzov, 1875 (TL: upper Naryn River in C Kyrgyzstan) is invalid as cited in synonymy of *L. homeyeri* and moreover preoccupied by *L. leucopterus* Heuglin, 1871, a junior synonym of *L. nubicus* Lichtenstein, 1823. Stepanyan (1990) rejected *przewalskii* and its invalid synonym

'*leucopterus*', but Cramp & Perrins (1993) erroneously accepted the invalid name '*leucopterus*'. Eck (1994), who examined 1255 Great Grey Shrikes, including many *leucopterus* but only relatively few *homeyeri*, also concluded that '*leucopterus*' (= *przewalskii*) was a valid taxon. Lars Svensson (in litt.) considers *leucopterus* / *przewalskii* to be merely a colour morph of *homeyeri*.

M. Gosselin commented: 'I don't have the references quoted by Roselaar, so your recent question is somewhat theoretical for me. However, I notice that Cramp & Perrins are quoting Severtzov, 1875, not Severtzov, 1873. And according to Roselaar, "*L. leucopterus* Severtzov, 1875, is invalid as cited in synonymy of *L. homeyeri*, and moreover preoccupied by *L. leucopterus* Heuglin, 1871". He may well be right, but notice that a name first published as a junior synonym is not necessarily invalid (see ICZN Art. 11.6.1), and a junior homonym, too, is not necessarily invalid (see ICZN Art. 23.9). So, more investigations may be needed here. As for Severtzov, 1873, I can only quote Article 12.1 of the ICZN Code: "To be AVAILABLE, every new name published before 1931 must satisfy the provisions of Art. 11 and must be accompanied by a description or a definition of the taxon that it denotes, or by an indication." Article 12.2 defines what an "indication" is (essentially, a reference to a description in another publication, or an illustration). Article 12.3 also says: "The mention of any of the following does not in itself constitute a description, definition, or indication: a vernacular name, locality, geological horizon, host, label, or specimen." [So, the existence of a type-specimen or a type-locality is irrelevant for our purpose.] Article 23.9, which deals with maintaining the prevailing usage, describes the process for deciding which of two competing names is the valid one, but the first condition a name must meet in order to be valid is to be AVAILABLE (Art. 23.1).'

L. Svensson (in litt.) commented that according to his own research (Svensson, in prep.) both taxa '*przewalskii*' (= '*leucopterus*') and '*funereus*' are mere synonyms of '*homeyeri*' and '*mollis*', respectively. Hartert (1910) states that *leucopterus* is a 'nomen nudum', and hence uses *przewalskii*, but since then all authors and main handbooks have used *leucopterus*. – There are of course several more valid taxa than listed above.

Vernacular name: L. Svensson prefers 'Saxaul Grey Shrike' (saxaulvarfågel in Swedish) for *pallidirostris*; 'Steppe' is to be avoided in the name in any language. The use of 'Steppe Grey Shrike' is of comparatively recent date, and there seems to be little justification for preserving a totally misleading name of young age.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A	A*	A

(*) Accepted as 'semi- or allospecies'.

Citрил Finch *Serinus citrinella* to be treated as two species (accepted by the AERC TAC in Sangster et al. 2002a):

- Citрил Finch *Serinus citrinella* (monotypic)
- Corsican Finch *Serinus corsicanus* (monotypic) – notice the correct spelling!

Serinus is a masculine noun

Michel Gosselin (in litt.) has commented on the gender agreement within the genus *Serinus*: *Serinus* Koch, 1816 is decidedly masculine, derived from the French noun 'serin'; originally established to include by monotypy *Serinus hortulanus* Koch (= *Fringilla serinus* Linnaeus) it is masculine under ICZN Articles 30.2.3 and 30.2.4. It was not included in David & Gosselin (2002a) because it is universally treated as masculine, and rightly so. Adjectival words combined with *Serinus* must agree in gender and e.g. *pusillus*, *syriacus*, *thibetanus*, *capistratus* are spelled correctly.

Serinus canaria must remain as is

Coomans de Ruiter et al. (1947) believe that canarius, -a, -um is probably a new Latin adjective, derived from the Canary Islands, i.e. occurring there, and consider it a less correct spelling of canariensis as Johnstone (1650), Willughby (1676) and Ray (1713) called it *Passer canariensis* and Linnaeus, Fauna svecica, 1761, no.207 mentions 'Habitat: in Canariis Insulis'. *Fringilla Canaria* Linnaeus, 1758, however, must be spelled *Serinus canaria* because the word 'Canaria' is not an adjective in this instance, but the name of this bird (as quoted by Linnaeus 1758 from Gesner 1555), thus a noun (ICZN Art. 31.2). 'Canariae insulae' (Islands of Dogs) is the classical name of the Canary Islands, as 'canarius, -a' means 'canine'. However, the word 'Canaria' has, by extension, been applied to a bird associated with these islands (just as canine has come to designate a kind of tooth, or Jersey has come to designate a breed of cattle). This semantic process had already taken place when Linnaeus described *Fringilla Canaria* in 1758 because two

of the authors he was quoting from (Gesner 1555 and Aldrovandi 1599-1603) were calling this bird 'Canaria'. The same pattern is repeated in many European languages (En.: Canary; Du.: Kanarie; Fr.: Canari; Es.: Canario; Ru.: Kanareyka). Again, ICZN Art. 31.2.2 applies: Canaria is meant here to be name of this bird and not the Latin adjective canarius (canine). And again, the evidence of usage points to this interpretation.

Serinus citrinella must remain as is

Similarly, *Fringilla citrinella* [Pallas], 1764, has apparently been spelled *Serinus citrinella* (as per Peters 1968, p 211, etc.) because *citrinella* is an Italian name for this bird (Jobling 1991; Italian dictionaries). Species names that are not Latin are invariable, see ICZN Art. 31.2.3. In other combinations, *citrinellus* is used in the masculine too, e.g. in *Atlapetes citrinellus*. In a treatment of etymology of bird names, Coomans de Ruiter et al. (1947) explains *citrinella* in *Emberiza citrinella* as a diminutive of *citrinus*, -a, -um (Lat. adj.) = citrine; derived from *citrus*, -i f. (Lat.) = lemon tree [compare to Greek κίτρινος (*kitrinos*) derived from κίτρον (*kitron*) = lemon]. Although Coomans de Ruiter et al. (1947) are right to say that the original etymology of this [Italian] word lays in the Latinized Greek adjective *citrinus*, there may be enough evidence to support the fact that *Citrinella* is here an Italian word, and not anymore its Latin source -word.

Fringilla citrinella is described in the *Adumbratiunculae* of Vroeg's catalogue ([Pallas] 1764), a remarkably rare work, discussed by Davies Sherborn & Richmond (1905). Only three copies are known to exist. Considering all evidence, it is clear that A. Vosmaer (1720–1799) should be credited with the text in the main body of Vroeg's catalogue (pp 1–49), while P.S. Pallas (1741–1811) was responsible for the *Adumbratiunculae* (appendix pp 1–7) (Rookmaker & Pieters 2000). Although the scientific name *Fringilla citrinella* is credited to Pallas, he seems to have lifted it from earlier authors whose works are not 'available' under the ICZN Code. In the 1766 edition of his *Systema Naturae*, Linnaeus also used *Fringilla Citrinella* for this bird, and quoted the name 'Citrinella' from earlier authors (Gesner 1555, Aldrovandi 1599–1603, Olina 1622, Willughby 1676 and Ray 1713). However, he didn't quote Vroeg's catalogue ([Pallas] 1764) published just two years earlier. It looks like the bird (and its name, *Citrinella*) was known to European naturalists of the XVIth–XVIIth–XVIIIth centuries (perhaps as a regular cage-bird from Italy), and Pallas simply happened to be the first to use a binominal name acceptable by today's standards.

[Vosmaer] (1764) gives the following Dutch description on p 15 in Vroeg's catalogue: '142 BERG KANARIE *Fringilla Canariae subsimilis*). Lin. Sp. o. Deze is groen, achter op de stuit geel, en hier in het Duin gevangen.' [142 MOUNTAIN CANARY ... It is green, yellow on the rear of the rump and captured here in the dunes]. This shows that Vosmaer was aware of the montane origin of the species. Hence, Pallas may have linked this bird with an Italian local name. No other Citril Finches have been recorded in the Netherlands since. One may wonder whether the lost type specimen is really from Den Haag or not simply a mounted specimen in Vroeg's collection (perhaps originally a cage bird). According to the annotated copy in the Royal Library, on 6 October 1764, the day of the sale of Vroeg's collection, items numbered 1–239 were auctioned in the morning, items 240–331 in the afternoon, making a total price of 362 guilders for the birds and 108 guilders for the quadrupeds. Vosmaer bought 87 lots for the natural history cabinet of Stadholder Willem V. If the type specimen of Citril Finch was acquired by the Stadholder, it could have been taken to Paris after the French invasion in 1795 (Vlek R.J.J. in litt.).

It seems that *Citrinella* is, or, at the very least, can be the original Italian name of Citril Finch, which would make it a noun under ICZN Art. 31.2.2 ('Where the author of a species-group name did not indicate whether he regarded it as a noun or as an adjective, and where it may be regarded as either and the evidence of usage is not decisive, it is to be regarded as a noun in apposition to the name of its genus ...'). Moreover, the evidence of usage in this case is indeed decisive, and points to a noun. It is interesting to note that Pallas has used capital letters for *Fringilla Citrinella* in the *Adumbratiunculae* and that Linnaeus used capital letters for both *Citrinella* and *Canaria*, which was (usually) his way of indicating that he was dealing with a noun and not an adjective.

Serinus corsicanus is the correct spelling

On the other hand, *Citrinella corsicana* Koenig, 1899, must be spelled *Serinus corsicanus* because *corsicana* is a latinized adjectival word ending in the adjectival suffix *-anus* (*-ana*, *-anum*). The erroneous spelling '*Serinus citrinella corsicana*' (Peters 1968, p 211) was overlooked by David & Gosselin (2002a). Notice that

e.g. Wolters (1982), Barbagli & Violani (1997) and Sangster et al. (2002a) among others, had it right (*Serinus citrinella corsicanus*).

Corsican Finch is best treated as a separate species based on differences in (1) morphology (Cramp et al. 1994); (2) plumage; (3) vocalisations (Chappuis 1976) – although these need perhaps be better documented – and (4) genetic data (Pasquet & Thibault 1997). Songs and calls are notably different and plumages also show consistent differences. Cytochrome *b*-sequences of four individuals of *corsicanus* differed by 2.7% from three populations (12 individuals) of *citrinella* (but additional sampling is desirable to investigate mtDNA variation) (Pasquet & Thibault 1997). Although Pasquet & Thibault (1997) concluded that there is no significant difference between the island and the mainland populations of the Citril Finch, Sangster (2000) published six arguments to counter these conclusions. Sangster (2000) advocated splitting Citril Finch, because the concordant variation patterns of morphology, vocalisations and mtDNA sequences is strong evidence that both taxa have unique evolutionary histories. Arnaiz-Villena et al. (1999) described a rapid radiation of the canaries (genus *Serinus*) and pointed out that *citrinella* and *corsicanus* seem to belong to the genus *Carduelis* (a view followed by the CAF). Another cladistic study (van den Elzen & Khoury 1999), however, reached the opposite conclusion. More research is needed whether the Cardueline Finches form a monophyletic group (e.g. Chu 2002). This split is accepted unanimously by the AERC TAC.

BOURC TSC	CAF	CSNA	A.J. Helbig	STC
A	A	A	A	A

Common Redpoll *Carduelis flammea* proposed to be treated as two species by Knox et al. (2001)

- **Common Redpoll** *Carduelis flammea* (polytypic: *C. f. flammea, rostrata*)
- **Lesser Redpoll** *Carduelis cabaret* (monotypic)

This matter has been moved to the pending section.

Appendix 2

CR/SEO & GAE comments on the AERC WP list and Recommendations v.9.2

José Luis Copete
Jordi Clavell
Ricard Gutiérrez

During the last meeting of the Spanish Rarities Committee (CR/SEO) held in march 2003 it was agreed to establish a joint commission between the CR/SEO and its counterpart dealing with the treatment of escaped species and the E list, the *Grupo de Aves Exóticas* (GAE) also from SEO/Birdlife, to deal with the categorization of birds in the Spanish list paying special attention to the establishment of escapes as breeding species in the wild besides the application of AERC decisions on taxonomy and systematics into the Spanish list.

Several members of the joint commission CR-GAE (SEO/BirdLife) have worked on the Recommendations (5th and 6th drafts) and we would like to contribute with some proposals to the discussion. As a general comment, we strongly believe that the taxonomy of Iberian birds must be better studied in the forthcoming years. In many cases the information we currently have is insufficient to allow taxonomic decisions. We hope, however, that in the next years, ongoing research will clarify some of these cases.

Puffinus mauretanicus* / *yelkouan

It seems that the case of a mixed pair recorded at Illa de l'Aire was an error. The bird observed was an incubating *P. yelkouan* (erroneously ringed as *P. mauretanicus* but later identified as *P. yelkouan* after checking the bird while it was incubating); the identity of the other member of this pair remained unknown (D. Bigas pers. comm.). Thus, there is no known case of hybridisation or mixed pairs. According to data from Balearic Islands Government (2002) the *yelkouan* population of Menorca is evaluated at 160 pairs and the remaining Balearic shearwater colonies gathers 1687 ± 240 pairs in Menorca, Cabrera, Eivissa and Formentera Islands. Further data on different biological requirements of both species pointed out by Carboneras (1998) show that *mauretanicus* would be a highly specialized species which is perfectly adapted to the environmental variations in productivity of the Balearic sea, thus avoiding periods in which this sea becomes poorer, being a specialist which prefers rich and cool sea waters being forced to abandon the

Mediterranean in summer. In turn, *yelkouan* would be less specialised, being able to live year round in the Mediterranean. The narrow trophic requirements of *mauretanicus*, highly specialised in Clupeidae (see Arcos & Oro 2002a, 2002b) would support its higher potential threatening. Further research on the Menorca population is in due course and more data are expected before the AERC meeting in Romania.

Arcos, J.M. & Oro, D. (2002a). The role of nocturnal purse-seiners as a feeding resource for seabirds in the Ebro Delta area (NW Mediterranean). *Marine Biology* 141: 277–286.

Arcos, J.M. & Oro, D. (2002b). Significance of fisheries discards for a threatened Mediterranean seabird, the Balearic shearwater *Puffinus mauretanicus*. *Marine Ecology Progress Series* 239: 209–220.

Carboneras, C. 1998. Notes sobre la fenologia i selecció de l'hàbitat de la baldritja de les Balears *Puffinus mauretanicus*. *Anuari Ornitològic de les Balears* 13: 13–19.

Calonectris diomedea / borealis

The at sea identification characters pointed out by Gutiérrez (1998) were successfully tested in a S African upwelling area off Namibia (Camphuysen & van der Meer 2001) where both forms were recorded but mixture of *borealis* and *diomedea* within flocks was not seen, although both species shared certain feeding areas. There are some cases of *borealis* breeding in the Mediterranean mixed with *diomedea* and raising chicks. Some cases are supported not only through biometrics but also by ringing recoveries (for a review see Martínez Abraín et al. 2001). These authors found exchanges between Atlantic and Mediterranean colonies of about 4–19 birds per generation, supporting previous data published by Randi et al. (1989) and suggesting all these colonies form a metapopulation and that gene flow from Atlantic colonies could decrease the short-term probability of local extinction of small Mediterranean colonies. Until more research is done and despite the low (but constant) exchange in colonies, the current subspecific status is better kept. However, former splits of seabirds (particularly Procellariiformes) prevent a final categorisation of these taxa until the differences among the different Macaronesian and Mediterranean populations – which eventually could form a cline (Massa & Lo Valvo 1986) – are fully understood.

Gutiérrez, R. (1998). Flight identification of Cory's and Scopoli's Shearwaters. *Dutch Birding* 20(5): 216–225.

Camphuysen, C.J. & van der Meer, J. (2001). Pelagic distribution, moult and (sub-)specific status of Cory's Shearwaters *Calonectris [d.] diomedea / borealis* wintering off Southern Africa. *Marine Ornithology* 29: 89–96.

Martínez-Abraín, A., Sánchez, A. & Oro, D. (2001). Atlantic Cory's shearwaters (*Calonectris diomedea borealis*) breeding in a colony of Mediterranean Cory's shearwaters (*C.d. diomedea*). *Waterbirds* 25: 221–224.

Massa, B & Lo Valvo, M. (1986). Biometrical and biological considerations on the Cory's Shearwater *Calonectris diomedea*. In MEDMARAVIS & Monbailliu, X. *Mediterranean Marine Avifauna. Population Studies and Conservation*. NATO ASI Series: 293-313.

Randi, E., Spina, F. & Massa, B. (1989). Genetic variability in Cory's Shearwater (*Calonectris diomedea*). *Auk* 106: 411–418.

Aquila adalberti

Forsman (1999) mentions a mixed pair *A. adalberti / chrysaetos* in NE Spain as having bred in the area in 1994–1995. This information is unknown to us. NE Spain includes Aragon and Catalonia. In Catalonia, the Avifauna Committee (CAC) regards *A. adalberti* as a rarity, seen at most once a year and the species is also rare in Aragon region. D. Forsman (pers. comm.) had no direct knowledge about this; he only mentioned this record in *The raptors of Europe and the Middle East* because of a record published in *British Birds*, without remembering the complete reference. A review of *British Birds* volumes (1993–1998) has given no clue on the origin of this reference, which we, eventually, consider unproven, or in error. We have also checked Ferrer (2001) where there are no mentions of any hybrid records between both species.

Ferrer, M. (2001). *The Spanish Imperial Eagle*. Barcelona: Lynx Edicions.

Forsman, D. (1999). *The raptors of Europe and the Middle East. A Handbook of field identification*. London: T & A D Poyser.

Clavell, J. (2002). *Catàleg dels ocells dels Països Catalans. (Catalunya, País Valencià, Illes Balears, Catalunya Nord)*. Bellaterra: Lynx Edicions. In Catalan with English summary. [Checklist of birds of Catalan countries. (Catalonia, Valencian country, Balearic Islands, N Catalonia)].

Tyto alba

There is some interbreeding between *alba* and *guttata* in Catalonia (Clavell 2002), and there are some recoveries in Catalonia of Barn Owls ringed as pulli within *guttata* breeding range (Neubradenburg, Oberriexingen and Kunzelsau-Mausdorf, three German localities; Clavell 2002). The reference to Zuberogoitia & Campos (1999) refers to Euskadi, while Clavell (2002) refers to Catalonia. More information is needed to assess the variation within Barn Owl; it is unclear whether it is warranted to recognise the subspecies *T. a. alba* and *T. a. guttata* or not.

Clavell, J. (2002) *Catàleg dels Ocells dels Països Catalans (Catalunya, País Valencia, Illes Balears, Catalunya Nord)*. Lynx Edicions. 382 pp.

Zuberogoitia, I. & Campos, L. F. (1999) Hibridación de lechuzas, *Tyto alba alba* y *T. a. guttata*, en el norte de la Península Ibérica [Barn owls hybridization, *Tyto alba alba* and *T. a. guttata*, in the north of Iberian Peninsula]. *Est. Mus. Cienc. Nat. de Álava* 14: 187–192.

Picus viridis sharpei

It seems that *sharpei* may best be treated as a species, but it has not yet been proven. Vocalisations of Iberian birds were recorded by Roché (1990), J. L. Romero, A. B. van den Berg (pers.comm.) and others, but

comparisons with nominate *viridis* have not yet been done as far as we know. Personal impressions are that *sharpei* voice would be closely linked to *levallantii* and more distant to *viridis*. It is important to point out that V. López Seoane (1870?:1894) committed a scientific fraud, changing the publishing date of his description of *Gecinus viridis galliciensis* (see Reig 2001), described as *Gecinus sharpei* by Saunders (1872). As distinctive characters, Saunders refers to 'grey-ash' coloured cheeks, presence of black on the head, golden rump, more brilliant red on the head, and shorter and slender bill. De Sagarra (1915) described and illustrated a new taxon, *Picus sharpei* (Saunders) Sub. sp. nov. *levantinus*, and an extraordinary larger bird with a reddish rump, called '*Picus sharpei levantinus* Sag. ab. rhodopygia n. ab.

López Seoane, V. (187?-1894). *Aves nuevas de Galicia*. La Coruña, Imprenta y Estereotipia de Vicente Abad. 11 pp.

Reig, A. (2001). Víctor López Seoane (1832-1900) como ornitólogo europeo en el centenario de su fallecimiento. La relación de Víctor López Seoane con Alfredo y Reinaldo Brehm y su importancia para la ornitología ibérica. *Ingenium* 7: 345-377.

Saunders, H. (1872). *Proc. Zool. Soc.* p. 153

de Sagarra, I. (1915). *Noves ornitològiques*. Treballs de la Institució Catalana d'Història Natural, vol. 1. Barcelona.

Anthus richardi

Schodde & Mason (1999) proposed four taxa: **Anthus australis rogersi*, **Anthus australis australis*, **Anthus australis bistratus*, **Anthus australis bilbali*. 'Species: Limits unsettled and controversial. Whereas Vaurie (1959; et al. 1960), Hall (1961) and Hall & Moreau (1970) combined major Australasian and Afro-Asian allotaxa in a single species (*novaeseelandiae* Gmelin), Haffer in Glutz & Bauer (1985) made a case for separating them (also Prigogine 1981). The first arrangement was followed by Schodde (1975), Wolters (1975-82), Cramp (1988), Keith et al. (1992) and Christidis & Boles (1994), and the second by Prigogine (1985), Clancey (1984, 1986, 1990) and Sibley & Monroe (1990). Wolters (l.c.) also presaged separation of Afro-Asian, Australasian and New Zealand groups as species. Here we follow the second, treating the *novaeseelandiae* complex as a superspecies because Australasian populations (Australia, New Guinea, New Zealand) differ in structure of wing and hind toe from Afro-Asian forms east to Timor (n=9). Such traits are species-specific in the otherwise cryptic members of *Anthus* (Keith et al. 1992). Whereas the hind claw is usually about as long as the hind toe of Afro-Asian members of the complex, or a little shorter (Keith et al. 1992), it is longer in the Australasian, particularly in the New Zealand main islands (up to 1.5 x toe, n=9). In the folded wing, the distance between the tips of p6 and p5 is consistently less than that between p5 and p4 in Australasian populations, the reverse of the condition in Afro-Asian forms and resembling more the wings of *A. leucophrys* Vieillot and *A. valeensis* Shelley figured in Keith et al. (l.c.: 217-218). Hall (1961: 254, pl. 60) also noted that Australasian populations had 'blunter' wings, however adaptive. That Australasian populations are 'linked' to those in Asia through 'Melanesia and Philippines' has not been substantiated (pace Hall l.c.; Hall & Moreau 1970). Moreover, the interrelationships of the two major Asian groups, *richardi* Vieillot and *rufulus* Vieillot, which may be linked by intermediate forms in East China and Viet Nam (Hall 1961; Hall & Moreau 1970; Roselaar in Cramp 1988), are not relevant to Australasia.' ... 'Within Australasia, furthermore, there are three principal groups of taxa: *novaeseelandiae* in New Zealand and outlying islands, *australis* Vieillot in all Australia and Tasmania south of north coast, and *rogersi* Mathews on the N Australian coast and mid montane New Guinea...' 'Although *australis* may abut patchily on *rogersi* in N Australia, nothing is known of interaction nor of the breeding limits of *australis* there (cf. Vaurie et al. 1960: 148; Blakers et al. 1984). The issue needs investigation'. 'the ultrataxon concept circumscribes regional interbreeding populations of birds that differ discontinuously from neighbouring relatives in at least one morphological character that is presumed to be genetically based. Single traits which are seemingly ecophenotypic and reflect environmental differences, or which cline gradually from one state to another across wide regions, are avoided as criteria'.

Schodde, R. & Mason, I.J. (1999) *The directory of Australian birds. A taxonomic and zoogeographic atlas of the biodiversity of birds in Australia and its territories. Passerines*. Canberra: CSIRO.

Phoenicurus ochruros gibraltariensis / aterrimus

In 2002, *gibraltariensis / aterrimus* was studied in Asturias (Spain) and Sweden (Álvarez-Laó 2002) and this study seems to conclude that both subspecies are the same, because there are no valid criteria to distinguish them. Álvarez-Laó concludes that there is a cline of *P. ochruros* in Europe, without subspecies.

Álvarez Laó, C.M. (2002) Colirrojos tizones: aproximación a su variabilidad en plumajes, densidad y fenología en Asturias. // *Encuentros Ornitológicos Asturianos*: 11-23. COA. Xixón.

Oenanthe pleschanka, Oe. hispanica hispanica & Oe. h. melanoleuca

Whereas plumage differentiation was already known, Magnus Ullman (2003) abstracted several papers confirming that these taxa are also morphologically diagnosably distinct. The most widely accepted view is considering these forms as subspecies. In fact, only the CSNA formally accepted four species, based on a PSC approach. The taxonomic position of these taxa remains to be compared to the Guidelines. Hybridisation between *Oe. h. melanoleuca* and *Oe. pleschanka* is relatively common. Furthermore, the frequency of 'black-throated' birds is higher in *melanoleuca* than in *hispanica*. It is hypothesized that the frequency of this character typical of *pleschanka* may originate from the splitting of a common ancestor (into *hispanica* and *pleschanka*) and further introgression of characters from both taxa. On the one hand, there is

a westward introgression of the 'black-throated' character in *hispanica* ('spatazina' form) and on the other hand an eastward introgression of the 'white-throated' character in *pleschanka* ('vittata' form). Suárez (1990) gives percentages of black-throated birds in an east-west cline. The proportions of characters initially typical of *pleschanka* into *hispanica*, and vice versa, has an additional interest when considering the hybridisation of the three taxa (*hispanica*, *melanoleuca* and *pleschanka*).

Suárez, F. (1990). El dimorfismo de la collalba rubia (*Oenanthe hispanica* L.): variaciones geográficas y entre hábitats. *Ardeola* 37 (2): 291–298.

Ullman, M. (2003). Separation of Western and Eastern Black-eared Wheatear. *Dutch Birding* 25: 77–97.

Turdus merula algirus

We were initially surprised that C.S. Roselaar mentioned this subspecies (*algira*) in Spain. We had never heard about its occurrence in Spain. Clement & Hathway (2000), Clements (2000), & Deignan *et al.* (1964) do not recognise *algira*. Deignan *et al.* (1964) mentions *Turdus merula mallorcae* proposed by von Jordans, 1950, *Syll. Biol.*, Leipzig, p. 172. Vaurie (1959), in the text of *Turdus merula mauritanicus* says: 'Morocco. (*) *Merula algira* Madarász, 1903, *Ann. Hist.-Nat. Mus. Natl. Hungarici*, I, p. 559, Bone, Algeria. Range: NW Africa from Morocco to Tunisia. The more northern populations ("algira") average very slightly smaller.' It seems, thus, that *algira* (or *algirus*?) may be a 'form' of the taxon *mauritanicus*. Eloïsa Matheu (<http://www.sonidosdelanaturaleza.com>) has made recordings of Balearic birds, and vocalisations seem to differ from peninsular Blackbirds, but further research is required.

Clement, P. & Hathway, R. (2000). *Thrushes*. London: Christopher Helm.

Clements, J. F. (2000). *Birds of the world. A checklist*. Sussex, Pica Press.

Deignan, H. G., Paynter, R.A., Jr. & Ripley, S.D. (1964). Prunellidae, Turdinae, Orthonychinae, Timaliinae, Panurinae, Picathartinae, Polioptilinae. Volume X. In: Mayr, E. & Paynter, R.A. Jr. eds. 1964. *Check-list of the birds of the world. A continuation of the work of James L. Peters*. Cambridge, Massachusetts: Museum of Comparative Zoology.

von Jordans, A. (1950). *Syllogom. Biol.*: 165–181. Leipzig. [from Vaurie (1959) and Cramp (1988). Vol. 5].

Vaurie, C. (1959). *The birds of the Palearctic fauna. A systematic review*. Order Passeriformes. London, H. F. & G. Witherby.

Serinus corsicanus

Perhaps it would be better to include this taxon in the 'Pending decisions', until the vocalisations of both taxa have been studied in detail.

Vocal types of Crossbill *Loxia curvirostra* in Spain

(a) Pyrenean Crossbills

Clouet & Joachim (1996) described differences in bill morphology between Pyrenean, Alpine and Corsican Crossbills. Bill length, however, can vary a lot within individuals due to growth and wear, and is thus of very limited value. Therefore, bill depth is the bill trait that is most often used nowadays (P. Edelaar in litt.). Corsican Crossbills present patterns similar to the variation of the Balearic ones (longer bills and tarsus). Although morphological differences do not seem sufficient to propose new taxa (in agreement with Amadon & Short 1992), acoustic differences (supported by sonograms), may be evidence in the line of the results of Groth (1988, 1993a, 1993b, 1996) in the USA. Mind, however, the very small sample size (only 5 Pyrenean birds). [Note that the 8th vocal type in the USA (New Foundland Crossbill, now extinct) is described from just a single bird!]. Clouet & Joachim (1996) suggest further research on crossbill vocalisations, as their data do not allow to make taxonomic statements. Pyrenean and Corsican birds are specialized in one (or two) species of pine seeds, whereas the Alpine ones feed on several species. Pyrenean Crossbills might constitute a different taxon, since they are sedentary based on ringing recoveries (Senar *et al.* 1993) and specialized in Mountain Pine *Pinus uncinata* seeds, a seasonally regular and predictable source of food. Note, however, that Mountain Pine also exists in the Alps and even in small enclaves further east and south (M. Robb in litt.). In North America the distribution of conifers seems to form extensive zones with predominance of one or few species, causing heterogenous *Loxia* distributions. More importantly, the distributions (also during breeding) largely overlap. Groth (1988) proposed that cryptic species could exist based on the analysis of vocalisations and morphology and sympatric distributions. In Europe this subject is beginning to be studied (see Robb 2000).

There has been no research to see whether 'Pyrenean Crossbills' are limited to the Pyrenees or possibly merge into other populations. Probably this Crossbill population is a vocal type sensu Groth (1988, 1993a, 1993b, 1996), Benkman (1992) and Robb (2000), and in the end the most consistent approach will be to treat all including Scottish Crossbill and Parrot Crossbill as species, or all as sympatric 'subspecies' whatever that means! (M. Robb in litt.). We do not know if sufficient recordings of Pyrenean and Iberian birds are available, although it seems that one ringer (from Navarre, Daniel Alonso loxriadaniel@navegalia.com) is studying the subject and that Magnus Robb has recordings from a few areas but not enough. More Pyrenean *curvirostra* voices should be recorded. As some Pyrenean birds start breeding already in late summer and autumn (Clouet 2000) recording breeding birds in any time of the year would be best. Pim Edelaar (graesc@uolsinectis.com.ar) has performed a postdoctoral study (directed by Benkman) of North American vocal types, and will now continue to study the European cryptic species, including those of the Pyrenees. He organized a Workshop on this subject in August in Germany (review and summary paper will soon be published in *Avian Science*). David Jardine (dcjardine@freeuk.com) has also come to the Pyrenees to record

Crossbills. Michel Clouet (michel.clouet@wanadoo.fr) has also studied Pyrenean Crossbills (e.g. Clouet 1987, 2000). Questiau et al. (1999) found a genetic difference between North American and European crossbill populations, but within each continent, the vocal types did not differ genetically. In the 'Fonoteca del Museu de Ciències Naturals de Barcelona' there are some recordings of Crossbills, but they would have to be analysed.

(b) Balearic Crossbills

In the Balearic Islands (mainly Mallorca) Altaba (2001) studied Crossbills and concluded that *L. balearica*, should be treated as a new species based on morphological differences. M. Robb commented: 'This needs also to be studied by sonogram and preferably with a large sample size from several areas. Also, the large Crossbill populations in Aleppo Pine *Pinus halepensis* (like *balearica*) in Alicante etc. would need research.' P. Edelaar agrees: 'A vocal, morphological and genetic comparison between *balearica* and Crossbill populations found in *halepensis* forest on the Spanish mainland should be performed first before concluding *balearica* is sufficiently distinct for specific status. In fact, it may prove to be indistinguishable from mainland *halepensis* Crossbills, and *balearica* actually may falsely have been given subspecific status due to a confounded comparison between *halepensis*-specialised Crossbills from the Balearic islands and Crossbills from N Europe using spruce (*Picea* spp.).'

In general, more research into crossbill types is necessary (see also Robb 2000, Summers & Piertney 2003); the specific status of Scottish Crossbill *Loxia scotica* (sensu Voous 1977c) has been questioned by Piertney et al. (2001). These authors found no genetic differentiation between Scottish Crossbill and Parrot Crossbill; notice, however, that they compared the mtDNA of *scotica* with Parrot Crossbills *L. pytyopsittacus* breeding in Scotland and that it would be desirable to compare it with continental Parrot Crossbills too. A.J. Helbig (in litt.) prefers to treat *scotica* as a subspecies of *L. curvirostra*. M. Robb, however, commented that 'if treating *scotica* as a subspecies of *L. curvirostra* one should be consistent and treat Parrot Crossbill as a subspecies of Common Crossbill as well. There is no evidence that these two populations should receive a different taxonomic status.'

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Summers, R.W. & Piertney, S.B. (2003) The Scottish Crossbill – what we know and what we don't. *British Birds* 96: 100–111.

Emberiza schoeniclus

Byers et al. (1995) mention *lusitanica* for Portugal, but it is still unknown whether it also reaches Galicia (NW Spain) and extends to the east to Basque country, just to the French border, according to material gathered by J.C. Atienza, who is working now to clarify this subject. His work includes the analysis of blood samples for DNA analysis of *lusitanica* and *witherbyi*. On current knowledge *lusitanica* is a valid taxon, endemic to the Iberian Peninsula, with less than 200 pairs. *Witherbyi* and *lusitanica* seem to be clearly isolated geographically, the basic requirement to eliminate the possibility of clinal variation. This allows to affirm that they are indeed subspecies. *Witherbyi* is also isolated from *schoeniclus*, the reason why its allocation as a taxon on its own seems properly based. On present knowledge, *witherbyi* is in the Mediterranean zone (Catalunya, Balearic Islands, Valencian Community, Aragón and Castilla-La Mancha). *Lusitanica* is in the Atlantic-Cantabrian zone (C and N Portugal, Galicia, Asturias, Cantabria and Euskadi). According to data gathered recently by J.C. Atienza, *lusitanica* is confirmed as a breeding taxon in Galicia (NW Spain) and surely also in the entire Atlantic part of the Iberian Peninsula, according to photos checked by him and J.L. Copete. The identity of the forms sometimes breeding in Navarre and La Rioja (which would be transition zones) and those of S Portugal is still unproven (if there are any pairs there, since in neighbouring Andalusia Reed Bunting seems to have become extinct). The whole Iberian population is very fragmented and is somewhat isolated (although *witherbyi* is occurring in Catalunya, the Balearic and Castilla-La Mancha). Each

of the Iberian subspecies can also have another type of isolation since they belong to different groups of *E. schoeniclus*. *Witherbyi* is of the southern group (generally larger, dark subspecies with a heavier more rounded bill); it is believed to specialize in capturing insects in reed marshes. *Lusitanica* on the other hand, comes from the group of the nominal subspecies and has a finer bill, more adapted to capture insects in reed edge vegetation, but stronger and larger (at least visually) compared to *schoeniclus*. In any case *lusitanica* has a more powerful bill than *schoeniclus* (but almost straight, not curved as in *witherbyi*) and is darker. Females of *lusitanica* are also dark, in fact the darkest of all European females (J.C. Atienza pers. comm.). The birds observed in the Basque Country seem to belong to *lusitanica*. In the map of Cramp & Perrins (1994), *schoeniclus* appears to be breeding in the French Basque Country. *Lusitanica* and *schoeniclus* could possibly be in contact in the border area between Spain and France. However, it would be necessary to verify which is the south-western limit of *schoeniclus* and if it reaches the French Basque Country. So, in conclusion, *witherbyi* and *lusitanica* are the taxa occurring in Spain. *Witherbyi* is shown to be isolated from *lusitanica* and *schoeniclus*. *Lusitanica*, however, could be in contact with *schoeniclus* near the French border in Basque country. This point remains to be investigated.

Atienza, J.C. & Copete, J.L. (2003). Escribano palustre, *Emberiza schoeniclus*. Pp. 604-605 in: Martí, R. & del Moral, J.C. eds. (2003). *Atlas de las aves reproductoras de España*. Dirección General de Conservación de la Naturaleza & SEO/BirdLife, Madrid.

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Leiothrix lutea

Euplectes afer

Estrilda troglodytes

These three species are being under GAE consideration, to promote them into category C, because they presumably hold self-sustaining populations in Spain. Current data seem to corroborate that these three species are well established (*L. lutea* in Catalonia; *E. afer* & *E. troglodytes* in Andalusia), but more research is needed. The new Atlas of breeding birds perhaps will allow us to decide about this.

Appendix 3: Example of AERC TAC proposal by George Sangster

AERC TAC proposal: generic placement of Dwarf Bittern *Ardeirallus sturmii*

PROPOSAL Include Dwarf Bittern in *Ixobrychus* to become *Ixobrychus sturmii*

BACKGROUND In the European literature, Dwarf Bittern is often placed in the monotypic genus *Ardeirallus* (e.g. Verheyen 1959, Cramp & Simmons 1977, Voous 1977c) but many authors have used *Ixobrychus* as its generic name (e.g. Peters 1931, Bock 1956, Payne 1979, Hancock & Kushlan 1984, Howard & Moore 1991, del Hoyo et al. 1992, Sibley 1996).

HYPOTHESES Placement in *Ardeirallus* was suggested by Sharpe (1895), Verheyen (1959) and Curry-Lindahl (1971) and is based on (i) reduced sexual dimorphism compared to species placed in *Ixobrychus*, and (ii) behavioural and ecological differences with species generally placed in *Ixobrychus*. Curry-Lindahl (1971) stated that Dwarf Bittern has 'different postures, habits, environmental demands and food biology'. He wrote that he has never seen some of the characteristic habits of *Ixobrychus* in Dwarf Bittern (i.e. their acrobatic movements, and downward position and sidewise movements of tail when excited).

Placement in *Ixobrychus* was suggested by Payne & Risley (1976) and Hancock & Kushlan (1984), who argued that (i) reduced sexual dimorphism is also found in the South American Streaked Bittern *I. involucris*, (ii) that the supposed behavioural distinctions may reflect a lack of knowledge about the behaviour of Dwarf Bittern, and (iii) that examination of the skeleton suggests no marked distinctiveness of Dwarf Bittern from the species of *Ixobrychus*.

RELEVANT DATA Payne & Risley (1976) performed cladistic and phenetic analyses of 33 osteological characters of 49 heron species, using a hypothetical heron ancestor as an outgroup. In all cladograms and phenograms, Dwarf Heron was placed among *Ixobrychus* species, the cladograms indicating a close relationship to Least Bittern *I. exilis*, Cinnamon Bittern *I. cinnamomeus*, Schrenck's Bittern *I. eurhythmus* and Yellow Bittern *I. sinensis*. Support for the relevant nodes was not given.

McCracken & Sheldon (1998) re-analysed the data set of Payne & Risley (1976) using modern phylogenetic methods. They corrected and added character states for some species and analysed 30 skeletal characters of 49 heron species with PAUP. They performed two analyses, one using a hypothetical outgroup and another using 9 non-heron outgroups. In both analyses, Dwarf Heron emerged within the *Ixobrychus* clade, as part of a subclade formed by Cinnamon Bittern, Schrenck's Bittern and Yellow Bittern. Support for this subclade was 81% and 100%, respectively. Monophyly of *Ixobrychus* (excl. *Dupetor flavicollis*) was supported by bootstrap values of 95% and 85%, respectively.

INTERPRETATION / DISCUSSION Reduced sexual dimorphism does not represent good evidence for generic separation of Dwarf Bittern because it is also found in the Streaked Bittern *I. involucris*. However, there has been no suggestion, or any evidence, that Dwarf Bittern is closely related to Streaked Bittern. Therefore, their shared reduction in sexual dimorphism may be homoplasious and hence cannot be cited in support for placement of Dwarf Bittern in *Ixobrychus*.

Because comparisons of behaviour and ecology (Curry-Lindahl 1971) have not been extended to all relevant species and have not been placed in a phylogenetic context, behavioural characters should not be cited in support for or against a particular hypothesis of systematic relationships.

This leaves us with the cladistic study of Payne & Risley (1976) and the re-analysis of their data by McCracken & Sheldon (1998). Although Payne & Risley's (1976) study supports inclusion of Dwarf Heron in *Ixobrychus*, their analyses are pretty thin because tree building algorithms were not so sophisticated in those days. McCracken & Sheldon (1998) showed that the osteological data clearly support placement of Dwarf Heron in *Ixobrychus*. Thus, this seems the best-supported treatment based on all available data.

SUGGESTED ACCOUNT FOR AERC REPORT Dwarf Bittern is currently placed in the monotypic genus *Ardeirallus* based on reduced sexual dimorphism compared to species placed in *Ixobrychus* and behavioural and ecological differences with species generally placed in *Ixobrychus* (Verheyen 1959, Curry-Lindahl 1971). However, the validity of these reasons has been questioned (Payne & Risley 1976). Cladistic analysis of osteological characters (Payne & Risley 1976), and re-analysis of this data set (McCracken & Sheldon 1998), indicate that Dwarf Bittern is a member of the *Ixobrychus* clade. Therefore, Dwarf Bittern is placed in *Ixobrychus* and becomes *Ixobrychus sturmii*.

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PS: There are four records of Dwarf Bittern in the Western Palearctic, all from the Canary Islands (Velmala et al. 2002). P. Herroelen (in litt.) pointed out that the name 'Dwarf Bittern' may not be the best choice. In fact, this species is not a 'dwarf' compared to African Little Bitterns (*payesii*) from Congo: 6 *payesii* from Congo weighed 84–119 g, whereas 2 imm. males *sturmii* (120, 160 g) and 2 ad. females *sturmii* (130 and 173 g) were heavier (personal data P. Herroelen). Verheyen noted an adult male *sturmii* of 175 g in 1951. P. Herroelen prefers the name Rail Bittern (also used in *Birds of Gambia*). Sturm's Bittern is another option.

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