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 *middendorfii* 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 stud ies. 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 mo re 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'yano v 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 *middendorfii* 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 patte rn 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. (2 000) 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 mtDN A 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 stron gly 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 & Mars hall (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 a re 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 un likely 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 so uth 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….läst 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 refe rences 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 whichs 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 White-winged 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

(*) 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 Mergan ser 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 Donné -Goussé et al. (2002) paper. To minimise the risk that the generic na me 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 t o 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 prese rving '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 evaluatio n 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 w ould 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 a llocation 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 o f 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 loo king 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 interspecifi c 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 suggest ing 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 varia bility and not of interspecific differentiation. Morphological and vocal differences are slight even if con - sistent (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 spl it 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 t his 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)
Pufflnus [*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.*), Statterfield & 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* Hagemeijer & 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 recognis ed 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 th is 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 relationshi ps 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 famili es (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

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 Indi es 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). Th ree 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 Midw ay 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 photograph ed 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 no menclatural 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 classifi cation 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 classificatio n 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 Croc het 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 suppose dly 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 Adolfsson & 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 Fin land 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 restrictio n 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 differentiat ed 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 Heugli n'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 glaucoides* 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 first-winter 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*, *glaux*, *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*, Nutall's Woodpecker *P. nutalli* 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. Brichetti), this populat ion 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 a s 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 develo ped 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, nebularum, adamauae, pallidigula, axillaris, promiscuus, salax, stonei, 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, stejnegeri*; 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. T here 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 <u>within</u> 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 long-standing 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 specie s. 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 measur ed, 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). Recog nised 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 ski ns 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. ho rmonal 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 bes t 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 dimorph ism 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 (vo ice, 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 p lumages, 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 Wh eatear

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 t hese 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. Molton i'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 de scribed 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 pe nding 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 v arious 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 s ong 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 Shirihai 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 piece s (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 significan t 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 stru cture. *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

P	P	A	A*	P

(*) 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 <u>proven</u> 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. Ufyrkina 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 hybrids 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 *pallescens*. 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 'hybridogenic 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. One 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 *hispanoliensis* 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
∅	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 (includin g 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 g enus *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 not es 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 revi ew 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
∅	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 'n ew, 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 decisio n ([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.