

# Systematic notes on Asian birds. 11.<sup>1</sup> A preliminary review of the Alaudidae

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The taxonomic treatment of Asian taxa of larks (Alaudidae) is discussed and recommendations are made for further evaluation or awareness of competing hypotheses as to treatment.

## Introduction

In preparing the text for a planned 'Synopsis of the Birds of Asia' (see Introduction to 'Systematic notes on Asian birds': Dickinson & Dekker, 2000) we compare treatment in the Check-list of Birds of the World (Peters, 1960), in this case of the larks, with more recent treatments and we comment on points arising from the Check-list itself. Proposals made in some recent works have been examined, and in the case of some of these the context is clarified. Our recommendations, as to the treatment to adopt in the Synopsis, are intended to be consistent with the tradition of requiring the publication of convincing evidence for change, in as much detail as is needed from case to case. Our recommendations are explained where other views are preferred in major publications.

In this review of the Alaudidae we have been able to exchange views with the authors of a new monograph (Alström et al., in prep.) and a new Atlas (Roselaar & Shirihai, in prep.), but when the exchange took place neither their more thoroughly researched conclusions nor our more summary views were final. The major reviews of Asian larks (Meinertzhagen, 1951; Vaurie, 1951) predate Peters (1960), but are essential reading for a detailed understanding of the subject.

### *Mirafra javanica* Horsfield, 1821

The broad species ranging through Africa and India to Australasia, as recognised by Vaurie (1951) and Peters (1960), has been very widely replaced by the adoption of the view of Hall & Moreau (1970), in both regional and global works of reference (e.g. Sibley & Monroe, 1990, Grimmett et al., 1998), that *Mirafra cantillans* Blyth, 1845,

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should be separated. In the context of Africa these views have been accepted by Keith et al. (1992) and Dowsett & Dowsett-Lemaire (1993).

The focus of Hall & Moreau (1970) was on African birds, and Hall wrote in the introduction that "in recognising species in this Atlas we tend to split rather than to lump", a decision based on the conviction that previous lumping had gone too far (a conviction with which we tend to agree). She also wrote of the "choice of making a few detailed studies of single groups, or of trying to give an overall, but necessarily more superficial, picture of speciation in the whole avifauna of the continent". The result was powerful and persuasive. Because the Atlas was designed to reveal speciation, it was standard practice to assess whether African species belonged to super-species. Often the superspecies that Hall & Moreau discussed reached well beyond Africa, and it must be reasonable to suppose that the depth of coverage of related taxa beyond the confines of Africa would be one area where the admitted superficiality would be most probable. Rightly further detailed studies were encouraged.

It is easy to empathise with the conclusions of Hall & Moreau (1970), but whether sufficient evidence has been put forward to support their intuitive splits requires examination on a case by case basis. In practice, at least in the context of the relationships between the larks of Africa and the larks of Asia, the detailed work has barely begun. The need, as we see it, is to focus attention on the value of these studies.

In the present instance previous work, in our region, is insufficient. Vaurie (1951) treated *cantillans* as a race of *javanica*, commenting on the "abrupt change from the dark pigmentation of the more eastern race (*williamsoni* Baker, 1915a) from Siam to the rufous pigmentation of the Indian race (*cantillans*)" and adding "The conspecificity of these two forms is not questioned, even though .... intermediates are lacking". He did not pursue this, but said "I lack specimens from other parts of the range". It is unclear how broad a study Vaurie might have made had his overriding objective not been the identification of the Koelz material. In fact, however, Baker (1926, 1930) treated *Mirafra cantillans* as a separate species and Vaurie's statement about conspecificity being unquestioned seems ill-founded. Baker (1926) was influenced not by any significant morphological difference but by a "very wide geographical area between these two in which there is no representative form" with a corresponding difference in colour but not pattern.

There is then a case supporting a reversion so as to give specific status to *cantillans*. However, a careful re-evaluation has not been carried out. Although Australian and New Guinea birds have been discussed by Mees (1962, 1982), no study has looked at the populations of mainland and archipelagic south-east Asia. Mees (1962) stressed how significant the substrate colour of the habitat is as a determinant of morphological variation in these larks, and there may be significant variations in annual rainfall levels which may be more important in determining pigment but not, perhaps, genetic distinctness. Obviously these factors must be allowed for in any broad geographic study. There is evidently a need for caution over the interpretation of abrupt changes in pigmentation of the kind noted by Vaurie (1951).

There is a range gap west of Indian *cantillans* as well as to its east. It is entirely possible that *cantillans* is a monotypic species; distinct from African and Middle Eastern forms placed under this name by Hall & Moreau (1970). In our view it is prudent to retain the treatment of Peters (1960) until the emerging evidence is

properly published and allows a measured judgement.

In summary, we retain a broad concept for the species *javanica* but we suggest that research should be sought to substantiate a split, and even a split into three species: Afro-Arabian *M. simplex* (Heuglin, 1868), Indian *M. cantillans* and Australasian *M. javanica*.

### *Mirafra assamica* Horsfield, 1840

Peters (1960) treated a single broad species *assamica* occurring from the Indian sub-continent through Burma and Thailand to Cambodia and Vietnam. This cloaked a history of disagreement.

The treatment by Baker (1926) of four geographical subspecies was disputed by Whistler & Kinnear (1934: 103), who believed that *affinis* Blyth, 1845, was closer to *Mirafra erythroptera* Blyth, 1845, than to *Mirafra assamica*. They wrote of *affinis* that it could "not be considered a race of *Mirafra assamica*. The change in colour is too abrupt and striking and hardly likely to be correlated with the reappearance of the dark *affinis*-like form *microptera* Hume, 1873, in Burma. It must either be treated as a separate species or as a race of *Mirafra erythroptera*". They thought the ranges of *affinis* and *erythroptera* to be complementary "in the Peninsula" and examined reported sympatry, considering the only substantiated case of this to be from south-eastern Hyderabad. Unsure whether this implied breeding sympatry with some ecological separation or seasonal overlap they recommended further observation.

Vaurie (1951) mapped the Indian distribution and showed the localities in Hyderabad concerning *M. e. erythroptera* and *M. a. affinis*. His map also showed that in the Himalayan foothills *M. e. sindhiana* Ticehurst, 1920, occurs in close proximity to, and probably overlaps with *M. a. assamica*. He concluded that he could not unite *affinis* and *erythroptera*. Stating that there was a parallel between the case of *Mirafra cantillans* and *M. javanica* on one side and the case of *Mirafra assamica* and *M. affinis* on the other, Vaurie followed Baker (1926) in treating *affinis* as a race of *assamica* and his map gave no hint that the two came close in their distribution. In both decisions, he was followed by Ripley (1961, 1982). However Voous (1977) separated *cantillans* from *javanica*, citing Hall & Moreau (1970), and Roberts (1992) followed Voous, so that Roberts offers an alternative view to Ripley. The issue of whether *affinis* should be split from *assamica* was apparently not considered, presumably because the problem lay beyond their borders (Hall & Moreau, 1970; Voous, 1977; Roberts, 1992).

It was therefore wholly appropriate that a new study of this puzzle be undertaken. Using sound recordings and carefully targeted field work, Alström (1998) concluded that *affinis* should be considered distinct from *assamica*. His map gives ranges for *affinis* and *assamica* that meet in the area of south-east Bihar, and reflect old reports of sympatry there, but he did not visit Bihar and he did not mention whether there were specimens, among the many examined, that substantiated these old records.

He did mention specimens of rather grey *affinis* from Orissa that had been discussed by Abdulali (1976a). These do show that *affinis* occurs further north than Vaurie's map suggested, but there must be some question as to whether Alström's map suggests a proximity here between these two taxa that is greater than has been proven.

In central Burma Alström (1998) looked for sympatry between *microptera* (type locality Thayetmyo) and *assamica* and found only *microptera*, leading him to state that Smythies (1953) erred in listing nominate *assamica* (and implicitly placing *microptera* in its synonymy). That this, however, must have been Kinnear's error is suggested by the case of Tenasserim. It is perhaps here that Alström thought there might be sympatry between *microptera* and *marionae* Baker, 1915b, although Smythies (1953) had reported that Armstrong's specimen "referred by Stuart Baker to *marionae*, is *assamica* (Kinnear in litt.)".

Alström's map, showing *marionae* occurring in much of south-east Asia including southwest Thailand lapping over into neighbouring Tenasserim (which Alström did not visit), suggests that Baker was right and Kinnear and Smythies wrong. The reinterpretation of Smythies needs care, Alström accepts that *assamica* was correctly recorded from Arakan, and thus accepts all three forms from Burma. It is likely that the map, showing large swathes of Burma unoccupied by these taxa, reflects firstly the restricted areas of suitable dry zone habitat apparently favoured by *microptera* and secondly our present rather inadequate knowledge of the distribution of Burmese birds. However, if *microptera* is a dry zone bird (and Alström associates *affinis* and *marionae* with this habitat too), *assamica* is not and the apparent gaps in Burmese distribution may well be greater than reality will prove. Nonetheless there has been agreement through the years that the two more easterly populations are isolated from the Indian ones and, apparently, from each other.

Remembering that the problem in India began with whether *assamica* and *affinis* are conspecific, an examination of the sonagrams given by Alström (1998) indicates that the songs of *assamica* and *affinis* are quite different. Indeed he argues that Ali & Ripley (1972) erred in calling their vocalizations the same.

It is striking that, of the four taxa examined by Alström (1998), *assamica* differs from the other three in its behaviour; alone it seems not to perch up on bushes or trees and to sing from them. Taking together the morphological evidence (Alström says "in some respects *marionae* and especially *affinis* and *microptera* are more similar to *Mirafra erythroptera* than either is to *assamica*), and the habits and habitat, as well as the evidence from the vocalizations, there seems to be a considerable affinity between three forms that is not shared by *assamica*. On this basis we accept that *assamica* is a distinct monotypic species.

Looking at the evidence Alström (1998) presented, of the songs and calls of the four related taxa he discussed, we find the picture for the other three hard to interpret. Clearly the discrete populations each have more than one song type and more than one call. Our inclination is to treat *microptera* and *marionae* as geographical representatives of *affinis* and to continue to treat them as subspecies.

We recognise that Alström's decision to treat each as a species may be at least as well justified as ours<sup>2</sup>. As he writes (in litt., 3 Jan. 2001) to us, "the differences in song between the different taxa in the *M. assamica*/*M. erythroptera* complex are considerably more pronounced than within any other Eurasian lark genus. The songs of *microptera* and, especially, *erythrocephala* Salvadori & Giglioli, 1885, differ much more from the song of *affinis* than from the song of *erythroptera*." In central India *affinis* and *ery-*

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<sup>2</sup> And in separate on-going work ECD will tentatively recognise Alstrom's arrangement.

*throptera* are reported to overlap. It may, therefore, be better to treat *M. affinis* as monotypic and to attach *microptera* and the Indochinese form to *erythroptera*.

He tells us that he has DNA evidence to support his views. Such evidence may also resolve the relationship with *erythroptera*. We look forward to seeing that in print, until then we are reluctant to follow his arrangement and the table in the accompanying paper on types reflects that.

### *Miraфра affinis* Blyth, 1845

Alström (1998) found no morphological distinctions between *marionae* Baker, 1915b (central Thailand) and *subsector* Deignan, 1941 (of northern Thailand) and *subsector* was placed in synonymy.

He argued in favour of retaining the name *marionae* for the population of Thailand and Indochina although he was aware that a prior name existed. Violani & Barbagli (1999) have shown that the prior name, *erythrocephala*, was used several times between 1915 and 1996 and considered that it should stand. As the name has been used in the 20th century Art. 23.9. of the International Code of Zoological Nomenclature, 4th Edition (ICZN, 1999) cannot be invoked and, unless the Commission is asked to intervene and place the older name on its list of rejected names, *erythrocephala* must be used.

A subsidiary issue is the distinctness of the Ceylonese population. Ripley (1946) wrote of *M. affinis ceylonensis* Whistler, 1936, which on measurements alone the race did not seem justified. Vaurie (1951) agreed and placed it in the synonymy of *affinis*. More recently the matter has been raised again: Abdulali (1978) thought *ceylonensis* a valid race, but Phillips (1978) did not agree and Alström (1998) left it in synonymy. As Phillips may not have seen the paper by Abdulali (1978), which was much delayed, the potential validity of *ceylonensis* needs reassessment.

### *Ammomanes phoenicurus* (Franklin, 1831) and *Ammomanes cincturus* (Gould, 1841)

There is now general acceptance that *Ammomanes cincturus* and *Ammomanes phoenicurus* are separate species, although Hartert (1904) considered them conspecific. He did not strongly defend this view (Hartert, 1919), but continued to uphold it although acknowledging a considerable difference (Hartert, 1921: 2084).

Meinertzhagen (1951) perceived *zarudnyi* Hartert, 1902, as a connecting link between the Afro-Arabian subspecies of *cincturus* and Indian *phoenicurus*. The range of *zarudnyi* lies between them but seems to be isolated from them. Vaurie (1951) found *cincturus* a smaller bird than *phoenicurus* with a very pale, adaptive, cryptic colouration and to have a different wing pattern and wing formula, and a weaker, much less massive and less strongly decurved bill. In his view *zarudnyi* clearly belonged to *cincturus*. This view, not initially adopted by Ali & Ripley (1972), has since become generalised (Ripley, 1982, Roberts, 1992).

### *Ammomanes deserti* (Lichtenstein, 1823)

Most of the range of *Ammomanes deserti* is outside our limits. Within our range there have been different views on the subspecies to accept. Although most authors

follow Peters (1960), *iranicus* Zarudny, 1911, is not always accepted, sometimes being placed in the synonymy of *parvirostris* Hartert, 1890 (Dementiev & Gladkov, 1954).

Vaurie (1959) and Peters (1960) both accepted *iranicus* with *darica* Koelz, 1951, in synonymy; *darica* was recognised by Cramp (1988), but with a range outside our area.

### *Melanocorypha bimaculata* (Ménétriés, 1832)

Vaurie (1951) resurrected the name *torquata* Blyth, 1847, for the population of eastern Iran and Afghanistan, east of the nominate form.

The species visits Japan in small numbers (Brazil, 1991) and is usually credited to the nominate form, but the use of this name probably dates back to the time when *torquata* was in synonymy. Vaurie (1951) did not address this, but questioned it later (Vaurie, 1959). Roselaar (in litt.) considers nominate *bimaculata* to be confined to the Caucasus region. We treat vagrants to Japan as *torquata*, as does the Ornithological Society of Japan (2000). We consider this a provisional view, based on the breeding ranges of nominate *bimaculata* and *torquata* as given by Cramp (1988). Trapping is needed to provide confirmation.

### *Melanocorypha maxima* Blyth, 1867<sup>3</sup>

Understanding of *Melanocorypha maxima* at subspecific level has been hampered by limited material.

Meinertzhagen (1951) recognised *subgrisea* Stegmann, 1937, and *flavescens* Stegmann, 1937, but in fact examined neither. Vaurie (1951) examined *subgrisea* but not *flavescens* and thought the former, and probably the latter, to be synonyms of *holderei* Reichenow, 1911, which he showed to be the oldest name for the northern and eastern birds (whereas Meinertzhagen had considered *holdereri* a synonym of the nominate form). Roselaar (in litt.) believes *flavescens* to be valid. We tentatively retain just two races assuming clinal variation between the two.

The race *kashmirica* Koelz, 1939, was reviewed and rejected by Whistler (1941) but was tentatively supported by Vaurie (1951, 1954), although when revising Peters's manuscript he did not require it be recognised (Peters, 1960). No doubt the situation Vaurie (1954) perceived remains the same: collecting of fresh breeding material of *kashmirica* and of *flavescens* is needed to be certain that these names belong in synonymy.

### *Calandrella brachydactyla* (Leisler, 1814)<sup>4</sup>

In contrast to a broad species *Calandrella cinerea* (Gmelin, 1789) preferred by Meinertzhagen (1951), who included *Calandrella acutirostris* Hume, 1873, and by Vaurie (1951) who did not, Hall & Moreau (1970) preferred to treat *Calandrella cinerea*, *brachy-*

<sup>3</sup> We were not aware of recent disagreement with the treatment of *Melanocorypha mongolica* (Pallas, 1776) when we wrote this paper. However, Roselaar (in litt.) considers that *M. m. emancipata* Meise, 1933, requires fresh review.

<sup>4</sup> Leisler's original spelling was *brachydactyla*. The date may in fact be 1812 not 1814.

*dactyla* and *acutirostris* as members of one superspecies, reverting to the treatment of Meise (1933a).

Within a broad species *cinerea* the Eurasian *brachydactyla* group is relatively distinct from the southern African *cinerea* group. Hall & Moreau (1970) argued that *brachydactyla* is a migrant, wintering as far south as the southern edge of the Sahara and Somalia, while *cinerea* is mainly resident. In addition *cinerea* is distinguished from *brachydactyla* by the bright rufous cap and has, in most populations, rufous patches on the side of the breast, although both these characters had been considered and rejected as a basis for specific separation by Meinertzhagen (1951). The issue is whether the distinctions are bridged by the intervening populations of NE Africa and Arabia in a manner that supports the concept of conspecificity. Although we believe the jury is still out on this, and some treat the intervening populations as a third species, here we elect to follow our inclinations and accept the narrow species concept.

Voous (1977), like Hall & Moreau (1970), chose to revert to treating the northern forms as a separate species *C. brachydactyla* and most authors since then, including Cramp (1988), have followed him, although Ripley (1982) did not. This misled Roberts (1992) into a curious melange of names. The northern race *dukhunensis* (Sykes, 1832), which Roberts thought might breed as far south as the NW Himalayas, he listed as a form of the species *cinerea* and *longipennis* (Eversmann, 1848) of Turkestan, which winters in Afghanistan and the northern subcontinent, he made a part of *C. brachydactyla*.

If there are specimens from Ladakh taken during the breeding season, these need to be re-evaluated to determine the species and subspecies. Osmaston (1925) mentioned collecting the nests of *C. acutirostris* in Rupshu, Ladakh (near and just south of Leh) in 1923, and later reported finding *C. brachydactyla dukhunensis* nesting in the Suru Valley, just south of Kargil and about 100 km west of Leh (Osmaston, 1926; Roberts, 1992). Although Osmaston was considered a reliable ornithologist the latter record was not accepted by Ali & Ripley (1972) because no specimen was collected in the Suru Valley, so there may be no Ladakh breeding specimens.

At the subspecific level other questions remain. The breeding ranges are not well worked out and consequently subspecific recognition varies. Migrant forms pass through the breeding ranges of other forms so that, in general, comparison needs to be based on birds taken in the breeding season, but at this time the plumage is often very worn (Vaurie, 1951). Migrants in worn plumage continue to pose problems as apparently we do not yet understand the species well enough to associate such birds with a given breeding form. Only winter collection of ringed breeding birds, or captive birds held through the moult into winter plumage, will illuminate these problems.

The wintering birds of Korea and Japan have been treated as belonging either to *longipennis* (Eversmann, 1848), with *orientalis* Sushkin, 1925, and *puii* Yamashina, 1939, in synonymy, by Vaurie (1959) and by the Ornithological Society of Japan (1974; 2000); or to *orientalis* with *puii* in synonymy, by Peters (1960). Meanwhile Cheng (1987) recognized the breeding form of Xinjiang as *longipennis* (with *orientalis* in synonymy). He seems not to have identified this as a migrant, with at least part of the population moving eastward through China to Korea and Japan. Cheng also recognized the breeding form of Tibet, Qinghai, NW Yunnan, SE Gansu and eastern Nei Mongol as *dukhunensis* and thought *puii* a synonym of this.

The only inconsistencies between these views are whether *orientalis* should be

recognised or not, and where *puii* belongs. On the former Cheng is consistent with Vaurie, and Peters differs. This is strange for we know that Vaurie revised the Peters manuscript for the Alaudidae between the time of Peters's death in 1952 and publication. Vaurie (1951, 1959) is consistent between his paper on Asian larks and his book on the Palearctic. In 1951 he explained that he disagreed with Russian authors who had perceived *orientalis* (type locality the Russian Altai) as a little darker than *longipennis* above and a little paler below and that the name *orientalis* appeared to have been applied to *longipennis* (type locality Dzungaria, i.e. the Djarkent area). Presumably Mayr & Greenway (1960) exercised editorial discretion in retaining *orientalis*.

Vaurie (1951) stated that "no breeding specimens of *dukhunensis* are available but owing to the characteristically heavy pigmentation, particularly that of the underparts, the identity of winter visitors is never in doubt". In separate places in this paper Vaurie referred to this taxon as "living in higher regions" (p. 471), as the "most eastern" (p. 472) and "breeds in Tibet" (p. 474). In fact Vaurie treated all the birds breeding from Dzungaria in the west to north-eastern Mongolia and north-western Manchuria as *longipennis*, as long as they bred north of the Tibetan uplands. In doing this he followed the views of Kozlova (1933), except that she believed that *orientalis* could be recognised in a long series. Ludlow & Kinnear (1933), Vaurie (1951) and more recently Stepanyan (1990) thought it could not.

Vaurie noted that the name *puii* had been attributed to Manchurian birds, but he synonymized this with *longipennis* (and with *orientalis*). Brazil (1991) thought the species a regular spring migrant in Japan in small numbers. Perhaps Cheng attached *puii* to *dukhunensis* on the basis of wing length. Yamashina (1939) distinguished it partly on the basis that it had a longer wing than *orientalis*, however it fits well with *longipennis* on this character.

The holotype of *puii* was collected on April 28 evidently on spring migration. The description reads: "The colour of plumage is similar to that of *C. c. orientalis* having no isabelline colour on the under tail-coverts and no fulvous colour on the under surface, but the size is larger. The length of wings is 90.5-98 mm instead of 81.5-93 mm as in *C. c. orientalis*. This subspecies is distinguished from *C. c. dukhunensis* in its paler colour of under surface and under tail-coverts" (Yamashina, 1939). Vaurie (1951) gave the wing length of *dukhunensis* as 92.5-103.5 mm and of *longipennis* as 85-98 mm. Although Yamashina's sample may have been small *puii* is clearly close to *longipennis* and belongs in its synonymy unless it shows the "characteristically heavy pigmentation" of *dukhunensis* that Vaurie referred to. As we have not seen a specimen we take the same tentative view as that taken by Morioka in Ornithological Society of Japan (2000).

The range of *longipennis* (including that of *orientalis*) covers a vast expanse of southern Siberia. There may well be clinal variation in wing length and probably differences in migratory behaviour at each end of the range, with western birds reaching Pakistan (Vaurie, 1951), and eastern birds, perhaps originating in or north of north-western Manchuria, travelling down the Pacific flyway. The longest-winged birds may prove to be at the eastern end of the cline.

It has been suggested that *artemisiana* Banjkowski, 1913, occurs in winter in the Indian subcontinent (Abdulali, 1976a). Ripley (1982) footnoted the view that "paler, thinner and long billed examples from Pakistan" were suggestive of this form.



Although recognised by Peters (1960) and Cramp (1988), *artemisiana* was made a synonym of *longipennis* by Stepanyan (1990) and we tentatively follow him on the presumption that he will have had the largest material available to make a judgement. Like Roselaar (in litt.) we doubt that true *artemisiana* from the Caucasus wanders enough to reach the Indian subcontinent.

### *Calandrella acutirostris* Hume, 1873

Vaurie (1951) agreed that *acutirostris* should be treated as a separate species from *brachydactyla* stressing the evidence of Schäfer (1939) of representative taxa breeding "beside one another and at the same altitudes" in eastern Tibet.

Earlier Ludlow (1928) treated *acutirostris* as monotypic, as did Whistler (1932) after reviewing the issue of where nominate *acutirostris* might meet *tibetana* Brooks, 1880, generated by interest in the affinity of birds from Rupshu, Ladakh, collected by Osmaston.

Vaurie (1951) considered there to be "a cline of decreasing pigmentation and increasing size" running from west to east; although aware of a degree of distinctness in the north-central Afghan population Vaurie recognised just the two established forms. The positions of both Whistler (1932) and Vaurie are tenable, depending on how one treats a clinal population. As we have not studied this we tentatively follow Vaurie.

### *Calandrella raytal* (Blyth, 1845)

The distinctness of *Calandrella raytal* is universally accepted. The subspecies *adamsi* (Hume, 1871) often has its type locality miscited as Agrore Valley, Hazara District (e.g. Peters, 1960; Ripley, 1982). It has been reported from there by Roberts (1992), although this may be based upon the literature rather than upon museum skins.

Hazara District lies east of the Indus, and this "type locality" has been inferred from text immediately following the original description. However, careful reading shows that the two paragraphs on which this view is based (Hume, 1871: 406), actually preface the description of *Caprimulgus unwini* Hume, 1871. On p. 404 in the similar preamble to this lark Hume says it came "from Murdan", which a page earlier he tells us is west of the Indus. This locality is now known as Mardan [34° 19' N., 71° 56' E].

Ripley (1961, 1982; and in Ali & Ripley, 1972) placed *vauriei* Koelz, 1954, in the synonymy of the nominate form, but did not give his reasons, however the range Peters (1960) gave for the nominate form would be expected to cover lowland Assamese birds.

### *Calandrella rufescens* (Vieillot, 1820)

For the benefit of those who may wish to research this species it will be helpful to examine the succession of names used for this. Sharpe (1890) considered the name *Alauda pispoletta* Pallas, 1811, the senior name.

In the West change began with Hartert (1904) who reported that Pallas's name was derived from *Alauda spinoletta* Linnaeus, 1758, and that Pallas had simply

changed the orthography to *pispoletta* to align it with his own conception of Italian, but that Pallas was still referring to a pipit. In seeking to use the next oldest name for the group Hartert applied the name *minor* (Cabanis, 1851) and this was widely used as the specific name for almost 20 years. In fact Hartert (1904) overlooked the prior name *rufescens* (Vieillot, 1820) which he listed two pages earlier as applicable to the Tenerife form. Eventually, recognising this, *rufescens* was advocated (Hartert, 1923).

The name *pispoletta* continued in use in Russia for many years. Bianchi (1906) responded to Hartert (1904) arguing for its retention and was followed by Dementiev (1934), Dementiev & Gladkov (1954) and even Flint et al. (1984). The move towards common ground began with Portenko (1954) who adopted *rufescens*, footnoting the explanation that Pallas's description did not allow discrimination between *Calandrella brachydactyla* and *C. rufescens* and that it should be considered indeterminate.

### *Calandrella cheleensis* (Swinhoe, 1871)

The issue of whether *C. rufescens* required splitting to recognise a sibling species arose when the ranges of *Calandrella leucophaea* Severtsov, 1873, and *pispoletta*, actually the race *heinei* (Homeyer, 1873), were reported to overlap in part of the Syr Darya region north of the Kyzyl Kum (Stachanow & Spangenberg, 1931).

Stachanow & Spangenberg reported significant morphological differences, especially in bill structure. They also said that Sushkin (1908) had found "hybrids". However, the evidence of overlap must have been slight, for in spite of the perception of a different wing formula (based on too limited a sample to satisfy him) Vaurie (1951) kept these populations within a broad species *rufescens*. Vaurie (1959) repeated his previous suggestion that further study may show that the eastern races constitute a separate species. Since he did not himself adopt two species one must conclude that he had not expanded his own studies to include the wing formulas of these eastern races.

The concept of two species was not followed by Dementiev & Gladkov (1954)<sup>5</sup>. They also rejected the theory that *leucophaea* was a distinct subspecies, arguing that it was a colour variant sometimes found in the same localities as '*pispoletta*' and always in association with solonchic soils. However, in a striking contradiction, they appear to report *leucophaea* to be a permanent resident, while making '*pispoletta*' a migrant.

Korelov (1958) studied 150 specimens of "eastern short-toed larks" from the museums in Alma-Ata, Moscow, St. Petersburg and Tashkent, plus substantial comparative material of *C. rufescens* (M. Kalyakov, pers. comm. 19 Jan. 2001). In addition to commenting on the bill, as had Stachanow & Spangenberg (1931), Korelov, according to Stepanyan (1967), expanded our understanding of the distinctness of the wing structure. He provided information on the wing formula, the apex of the wing, the distance between the wing tip and the tip of the 4th primary and the emargination of the 4th primary, but added that these are useful only in fresh plumage. Stepanyan (1967) affirmed that this wing structure is mirrored in all the eastern races and associated *leucophaea*, *kukunoorensis* (Przewalski, 1876), *seebohmi* (Sharpe, 1890), *beicki* Meise,

<sup>5</sup> This text is attributed to Voltchanetzky in Dementiev & Gladkov by Stepanyan (1967).

1933b, and *tangutica* Hartert & Steinbacher, 1933<sup>6</sup>, with the oldest named form *Calandrella cheleensis* (Swinhoe, 1871), a species distinct from *rufescens*. He reported having examined substantial numbers of specimens of almost all these races.

Korelov's data on the ecological distinctness of *leucophaea* from *heinei* is also presented in tabular form. Stepanyan did not comment on the duration, extent or intensity of the fieldwork needed to establish this table, and it seems Korelov may not himself have done fresh fieldwork, so it is not easy to know what is new and how much reliance to place on this part of the text.

We already knew from Dementiev & Gladkov (1954) that there was an association between *leucophaea* and solonchic soils, now apparently covered by the phrase "salt-marshes and Takyri in deserts". Dementiev & Gladkov (1954) had also said that *leucophaea* was non-migratory. On the basis of his experience with related populations, but probably not with *leucophaea*, Stepanyan (1967) qualified this, but it is not clear that he meant his qualification to extend from the eastern subspecies to *leucophaea* itself. Korelov (1958) apparently provided new information, beyond just confirmatory detail on habitats, in relation to breeding and moult. It appears that *leucophaea* raises a single brood and then begins a prolonged moult, and that it does not flock (at least during the seasons when the study took place). By contrast *heinei* seems to raise two broods and begins to moult before the second brood is raised, to have a quicker moult, and to form large flocks.

Stepanyan (1967) stressed that *leucophaea* is the only race to have been found to respond to its ecological niche with a significant change to the colour of the upperparts. This he perceived as an adaptation to competition from sympatric *heinei*. The related forms are, Stepanyan thought, less restricted in their choice of habitat and not facing the same competition.

On this evidence it must be accepted that there is a zone of contact between distinguishable groups of forms. On this basis we tentatively accept the value of treating *cheleensis* as a separate species and adopt this<sup>7</sup>. However, the quantity and quality of the information may not be sufficient to preclude the alternative of limited hybridization in an area of secondary contact, and more detailed studies would undoubtedly help. There may also be more than two species involved here (Alström, in litt.). We note that Cramp (1988) did not adopt the concept of two species, although Roselaar (1988) mentioned his own preference for that in his text therein. Roselaar also drew attention to potential overlap situations in Syria and elsewhere in the Middle East. Flint et al. (1984) used the name *Calandrella leucophaea* for the species *cheleensis* apparently mistaking the prior name.

Stepanyan (1976, 1983, 1990) applied his view in naming a new eastern race *Calandrella cheleensis tuvunica* Stepanyan, 1976, from Tuva, Lake Tere-Khol (30 km south of

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<sup>6</sup> Peters (1960) wrongly implies that the MS name was that of Hartert & Steinbacher and that Tugarinov published in Vög. Pal. Fauna, Ergänzungsab., Heft 2. In fact Hartert & Steinbacher clearly show it as Tugarinov MS. Our attempts to trace the type have not been successful.

<sup>7</sup> Both our reviewers (Alström, in litt.; Roselaar, in litt.) suggest that *cheleensis* may be a grouping of forms distinct enough to require treatment as two or more species. We await the publication of their work with great interest. Roselaar (in litt.) would include in *cheleensis* forms from SW Asia including *persica* Sharpe, 1890, which we have tentatively left with *C. rufescens*.

Arzin and well east of the contact zone), in his book on sibling species in the USSR, and in his *Conspectus of the Ornithological Fauna of the USSR*. Others such as Sibley & Monroe (1990) and Inskipp et al. (1996) have followed. The facts suggest that the alternative hypothesis of retaining a single species should not be forgotten.

The subspecific treatment of *cheleensis* may require review, particularly in China. It was suggested by Dementiev & Gladkov (1954) that *seebohmi* should probably be synonymized with *kukunoorensis*. In addition, the idea that *tangutica* may be a synonym of *cheleensis* rather than of *kukunoorensis*, which was put forward by Meise (1933a), has probably not been reviewed. Meinertzhagen (1951) had no specimens and Vaurie (1951) did not mention it, although he later placed *tangutica* in *kukunoorensis* (Vaurie, 1959).

Afghanistan too requires further work. Paludan (1959) was unable to assign five *breeding* specimens from Mukur in eastern Afghanistan to any named form. Paludan ensured that Vaurie saw these and reported that Vaurie found them different from October-taken birds examined earlier for his 1951 paper. The reports of breeding in Baluchistan and near Peshawar referred to by Roberts (1992) should also be treated with caution.

#### *Galerida cristata* (Linnaeus, 1758)

No particular issues arise in respect of resident populations in our area, but care is needed over the application of names. Vaurie (1951) used the name *magna* Hume, 1871, to cover a number of closely related, but possibly distinct, populations that were "very pale, not gray, more buffy than sandy, and of a very delicate soft shade". He explicitly lumped *ivanowi* Loudon & Zarudny, 1903, *vamberyi* Härms, 1907, *retrusa* Bangs & Peters, 1928, and, although somewhat redder, even "perhaps" *alashanica* Meise, 1933a, with *magna*. However, Vaurie (1951) noted that *retrusa* and *alashanica* were not available to him for study. He did not mention *submagna* Zarudny & Bilkevitch, 1918; its type locality might seem to be within the range defined for *magna* by this lumping, and it was listed as a synonym of that by Vaurie (1959) and by Peters (1960).

Vaurie's lumping has not been followed by Russian authors. Dementiev & Gladkov (1954) recognised *magna* as the breeding form of the Tian Shan and the Altai, but to the west and southwest of there recognised *ivanowi* (with *vamberyi* [sic] and *submagna* in synonymy) and in China mapped *retrusa* (although this was extra-limital to their scope). The treatment of Stepanyan (1990) is not consistent with this. He did not recognise *magna* from Russian territory, thus like Dementiev & Gladkov recognising *ivanowi*, but unlike them he placed *submagna* in the synonymy of *caucasica* Taczanowski, 1888. Cheng (1987) accepted that *magna* should embody *retrusa* and *alashanica*. Until shown by further studies the boundary between *magna* and *ivanowi* must therefore follow the political border.

The type locality of *magna* is Yarkand, and Vaurie (1951) considered birds from Chinese Turkestan to be topotypical. The type of *magna*, taken in late August, might be expected to be in moult, but Meinertzhagen (1951), who believed that there was a resident population in Yarkand, found moult to be nearly complete and yet the type was in "shocking condition". Fresh topotypical breeding material may be needed to

establish whether Vaurie's broad range is more helpful.

The populations that seem to be migratory are those that nest at relatively high elevations and their movements are presently thought to be relatively small. For this reason birds occurring in winter at lower elevations in Pakistan and northern India are said to be *magna*. However this label presumably has been given to them in the context of Vaurie's broadly-lumped taxon, and, if a smaller breeding range of *magna* is agreed, fresh plumaged winter birds may be worth closer scrutiny if enough specimens taken from October to January can be brought together. Such birds should have completed the moult and not yet be too abraded for comparative purposes (Vaurie, 1951).

### *Galerida malabarica* (Scopoli, 1786)

A relatively recent suggestion that *Galerida theklae* (C.L. Brehm, 1858) and *G. malabarica* could be considered conspecific, with the latter just "an incipient species" (Hall & Moreau, 1970), was not accepted by Ripley (1982) or Cramp (1988). There is a substantial gap between the ranges of *theklae* of the Mediterranean basin and Indian *malabarica*. These authors maintained the previous taxonomic position, to the benefit of stability, and presumably in the belief that the suggestion of Hall & Moreau should await some detailed evidence in its support.

Of more local interest is the relationship between *malabarica* and *deva* (Sykes, 1832). Whistler & Kinnear (1934) noted that their ranges seemed "in the main complementary" and suggested that the apparent overlap might not be sustained in the face of careful field investigation. We have not seen a careful subsequent review of this and would be interested to see one. The calls appear to be very different (Grimmett et al., 1998).

### *Alauda arvensis* Linnaeus, 1758, and *Alauda gulgula* Franklin, 1831 One species or two?

At the end of the 19th century it was customary to treat these as two species (Sharpe, 1890; Hartert, 1905), but in the 'Nachtrag' to his 'Vögel der Palaarktischen Fauna' Hartert (1923) treated the two as conspecific<sup>8</sup>. This was largely because he perceived the Chinese and Japanese populations as intermediate between the northern *arvensis* and the more southern *gulgula* group.

Hartert was particularly interested in which form bred in the Himalayas. If he was to treat the two as conspecific his hypothesis depended on there being no proof of sympatric breeding anywhere. Yet in the Himalayas there were two candidates: *dulcivox* Brooks, 1873, and *guttata* Brooks, 1871. Sharpe (1890), and indeed Hartert (1905), had not doubted that the former was a representative form of the *arvensis* group or that the latter was a representative of the *gulgula* group.

However, migrants of the *arvensis* type had been collected in the plains of northern India and, although these might be *dulcivox* (attributed to a *terra typica* of Nepal), such

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<sup>8</sup> More detail on this will be found in Appendix 1 to Dickinson et al. (2001) this issue, discussing type material of Asian larks.

migrants might not breed in the Himalayas, but might originate further north.

Matters of nomenclature complicate our understanding of the issue. Both *dulcivox* Brooks, 1873, and *guttata* Brooks, 1871, have since been placed in synonymy. In the case of *dulcivox* the citations listed by Hartert (1905) gave first, correctly, Hodgson's nomen nudum of 1844, and then the usage by Brooks (1873). But it has since been shown that *dulcivox* Brooks, 1873, falls to the priority of *dulcivox* Hume, 1872, by a matter of months (Vaurie, 1959). These and related names, derived in one way or another from Hodgson (1844), are examined in an annexe to our accompanying paper (Dickinson et al., 2001 – this issue).

Similarly, *Alauda guttata* Brooks, 1871, from Kashmir was discovered to be preoccupied by *Alauda guttata* Lafresnaye, 1839<sup>9</sup>. There being no valid alternative, the name *Alauda arvensis lhamarum* Meinertzhagen & Meinertzhagen, 1926, given to Ladak birds, was brought into use, as it was considered that it also applied to the breeding bird of Kashmir (Whistler, 1932; Hartert & Steinbacher, 1933: 127).

The issue of whether there is a Himalayan breeding form of *arvensis* as well as of *gulgula* was addressed by Ticehurst (1922). Ticehurst discussed the type locality of *dulcivox* and suggested that Siberian migrants might be concerned, both as types and in respect of wintering birds in the Punjab and elsewhere in the plains of India. Ticehurst could find no specimens of *dulcivox* that supported earlier assertions of breeding, and looking at the birds from the plains of India ascribed to *dulcivox* considered that they matched Siberian birds. The Siberian birds had been named *cinerea* Ehmcke, 1903, but this had been found to be preoccupied, and replaced by *cinerascens* Ehmcke, 1904. Ticehurst (1922) concluded that *dulcivox* Brooks should be applied to such migrants, since this name antedated Ehmcke's names.

Hartert (1922) changed his mind and decided to treat *arvensis* and *gulgula* as one species, but Ticehurst (1922) was too late to affect the "Nachtrag" (Hartert, 1923). In this Hartert accepted two subspecies from the Himalayas: *A. a. intermedia*<sup>10</sup> Swinhoe, 1863, treated as a migrant (with *dulcivox* and Ehmcke's names in synonymy), and *guttata* as breeding.

In the "Erganzungsband" Hartert & Steinbacher (1933: 127) incorporated Ticehurst's view and listed both *dulcivox* (from western Siberia) and *intermedia* as migrants, also restricting the breeding range of *intermedia* to Transbaikalia. As in 1923, only one form appeared to breed in the Himalayas. This was *lhamarum* (this name replacing *guttata*). It was against this background that Mayr (1941) treated the two as conspecific.

However, Vaurie (1951) re-examined structural differences of the wing shape (longer and more pointed in *arvensis* and short and more rounded in *gulgula*) and the shape of the bill (rather straight and stubby with a relatively broad base in *arvensis* and narrower, more pointed and decurved in *gulgula*). He wrote "the two species are separated by the deserts of inner Asia and come into contact only in the west. In this region they overlap over a very broad zone extending from north-eastern Afghanistan

<sup>9</sup> Not this species.

<sup>10</sup> Much later Vaurie (1951) reported that the type, taken in Shanghai in January, matched a June bird from southern Ussuriland.

over the greater part of Russian Turkestan.”<sup>11</sup> There was sympatry, he said, at Kirman and Kuhistan in Iran, as reported by Zarudny (1911), and also from Zaisan Nor (Johansen, 1944). To this Vaurie added evidence from the Koelz material from Afghanistan. Vaurie (1951) mapped the suspected scope of the overlap and marked four points where he was satisfied there was proof of sympatry. Roselaar (in litt.) has reminded us that the nature of this overlap is in the context of distinct altitudinal ranges: *gulgula* being a relatively lowland bird and *arvensis* inhabiting the mountain steppe.

If this settles the matter of Himalayan sympatry, and we believe that it does, it does not wholly resolve matters. It has been suggested recently by Martens & Eck (1995) that the aerial song of *lhamarum*, on suspected breeding grounds at very high elevations in Nepal, is “as in *arvensis*” and that “differences between *gulgula* and *arvensis* remain to be worked out”.

### *Alauda arvensis* Linnaeus, 1758: local issues

Vaurie (1951) considered that the status of *A. japonica* Temminck & Schlegel, 1848, remained in doubt, although he elected to place it with *arvensis* not *gulgula*.

Suspensions of sympatry in Japanese territory, or in the Kuriles, have led some writers (e.g. Cramp, 1988; Stepanyan, 1990) to believe that *japonica* should be treated as a separate monotypic species. The evidence for this appeared in a footnote in Vaurie (1959), where there is a suggestion of sympatric breeding of *lonnbergi* Hachisuka, 1926, and *japonica* on Yagishiri, an island off north Japan, attributed to Udagawa (1953). Roselaar (in litt.), who considers *japonica* a representative form of *gulgula*, has advised us that in southern Sakhalin and the Kuriles it is clear that *japonica* breeds, and that *arvensis* may have done so. It is our understanding that Japanese ornithologists do not consider this evidence conclusive and that it is not supported by specimens (H. Morioka, pers. comm. 19 June 1998; Ornithological Society of Japan, 2000: 293). Pending clearer understanding of the situation we prefer to treat the Japanese form as part of the *arvensis* group, although the affinities to the *gulgula* group noted by Vaurie (1951) should not be forgotten.

The status of the Korean population has also not been clearly summarised. Either it has been misunderstood or there are still differences of opinion. At the time of Austin (1948), *arvensis* was still seen as one broad species including *gulgula* although Korea is to the north of the range of the *gulgula* group. Neither Won (1969) nor Gore & Won (1971) suggested a trinomial for the birds found there.

Vaurie (1951, 1954) observed that Russian authors had consistently used the name *intermedia* for northern birds breeding east of the range of *dulcivox* (based in part on the views of Ivanov, 1929). Upon re-examining the type of *intermedia*, Vaurie (1951) found it a perfect match for a specimen from Lake Khanka in southern Ussuriland, although actually taken near Shanghai in January. It is thus typical of breeding birds

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<sup>11</sup> Vaurie (1951), although knowing that *dulcivox* Brooks, 1873, was based on Hodgson's nomen nudum of 1844 and thus on Nepal birds, choose to designate Djarkent in Russian Turkestan as the type locality. This has no effect on Hume's name which is now used (Peters, 1960), of which the type is from Murdan.

of a restricted area further east than suspected, perhaps extending from Lake Khanka as far west as Nei Mongol (Cheng, 1987). South of it *intermedia* reaches the coast and further east yet, in Kamtschatka, there is *pekinensis* Swinhoe, 1863. As a name applicable to Lake Khanka birds *intermedia* displaced *nigrescens* Kistjakovskij & Kotshubej, 1929, named from that area. This left breeding birds just east of the Yenessei River without a name and Vaurie (1951) applied the name *kiborti* Zaliesski, 1917 (type locality: district of Kansk, about 200 km east of Krasnoyarsk). This created a range for *intermedia* running from the Yenessei through southern Siberia and Mongolia to northeastern China and Korea (south to Quelpart Island).

Dementiev & Gladkov (1954) had probably not yet read Vaurie's conclusions on the applicability of the name *intermedia*. They continued to apply this name to northern birds breeding east of the Yenessei. Also, to the south of this, they recognised *dementievi* Korelov, 1953, from the Tian Shan mountains and *alticola* Sushkin, 1925, from the south eastern Altai. Because they retained the name *intermedia* for this population they naturally also retained *nigrescens* for that of Ussuriland. Dementiev & Gladkov therefore differed in their application of names and in the taxa recognised, which also included *buxtoni* Allen, 1905, although they remarked that *buxtoni* was very similar to *pekinensis*. The type of *buxtoni*, in New York, was examined by Vaurie (1951: 514), but probably not by Dementiev & Gladkov. Vaurie (1951), who was confident that *buxtoni* was a synonym of *pekinensis*, suggested that if birds of inland Kamtschatka differed from coastal birds it might be possible to restrict the name *pekinensis* to the inland birds and to recognise *blakistoni* Stejneger, 1844, for coastal birds. This might repay further research.

Much more recently Stepanyan (1990) has largely brought the treatment of Russian populations into line with Vaurie (1951). He recognised *alticola*, but not *dementievi* nor *buxtoni*. In addition he lumped *lonnbergi*, of Sakhalin with *intermedia* of Ussuriland. In the context of this last action we draw attention to the following comments of Vaurie (1951), "the vividly coloured races of *arvensis* from Sakhalin (*lonnbergi*) and the Okhotsk Sea (*pekinensis*)" "abruptly differ from" "the coastal populations of *arvensis* directly facing Japan in Korea (*kiborti*) and Ussuriland (*intermedia*)". We have rearranged these phrases to clarify this point, but we have not changed Vaurie's meaning. Perhaps Sakhalin birds belong with those of Kamtschatka. Or perhaps the type of *lonnbergi*, collected in October, was a migrant from Kamtschatka and residents in Sakhalin are indeed a good match for *intermedia*, as Stepanyan suggests; in this case the name *lonnbergi* belongs in the synonymy of *pekinensis* and not of *intermedia*.

There remains another intriguing mystery. In general eastern populations of *A. arvensis* do not migrate further south than China. There are no records from Thailand (Lekagul & Round, 1991), peninsular Malaysia (Medway & Wells, 1976) or the Philippines (Dickinson et al., 1991). However, the species has been seen and collected in Kuching (Harrisson, 1958; Smythies, 1960). The one specimen taken although "too worn for racial identification", is said to match "similarly worn winter birds from Sining Ho, N. Kansu, China". In Kansu such birds would presumably be winter visitors or passage migrants from unknown breeding grounds.

One other report of a migrant requires comment. Ripley (1982) listed the subspecies *cantarella* Bonaparte, 1850, from Baltistan based on Richmond (1896). This



name may have seemed appropriate in 1896, but subsequent taxonomic revision suggests the specimen, which probably exists in the USNM, should be re-examined. This is a southern European form breeding east to the Crimea, which winters mainly in the Mediterranean basin (Peters, 1960).

### *Alauda gulgula* Franklin, 1831: local issues

As regards *A. gulgula* we have not re-examined the case of *A. gulgula dharmakumarsinji* Abdulali, 1976b, from Gujarat, distinguished essentially by a long hind claw. Ripley (1982) placed it in the synonymy of *inconspicua* Severtsov, 1873, but recently Mohapatra & Rao (1990) reported it in February in Madhya Pradesh, where it was caught with other birds with shorter hind claws.<sup>12</sup>

The situation in Sri Lanka may also deserve further study. There used to be disagreement over whether the population should be attributed to nominate *gulgula* or to *australis* (Whistler, 1944). Vaurie (1951) agreed with Whistler that Sri Lankan birds were best treated as nominate *gulgula*, but Ripley (1982) treated lowland birds as *gulgula* and birds from the hill country as *australis*. As lark plumages often reflect substrate colours (Vaurie, 1951: 442), and wet conditions also often produce more saturate colours in many species (Meinertzhagen, 1927: 365), it is unsurprising that Sri Lankan hill birds might be darker than dry zone birds, whether this justifies associating them with *australis* of the Nilgiris is open to question.

The Philippine population, *wolfei* Hachisuka, 1930, was not reviewed in depth by Vaurie (1951) as he had too little material. It has since been reviewed and accepted by Parkes (1969).

Birds from Taiwan and the Pescadores were named *wattersi* Swinhoe, 1871. Subsequently, birds from the Pescadores were separated as *pescadoresi* La Touche, 1922. However, Swinhoe (1859) described *coelivox* before he separated *wattersi*, which is recognised as distinct, so the implication is that either Swinhoe's syntype(s) from the Pescadores was a visitor or breeding birds are not identical to Taiwanese birds. This does not seem to have been resolved, as although Peters (1960) treated this as a synonym of *coelivox* Swinhoe, 1859, of southern China, the Pescadores is not explicitly included in the range statement given for *coelivox* or for *wattersi*.

### *Eremophila alpestris* (Linnaeus, 1758)

The taxonomy of this species, which was much debated prior to the works of Vaurie (1951, 1959), has been essentially stable in our area since Peters (1960). The treatments of Dementiev & Gladkov (1954) and of Stepanyan (1990) are consistent as well.

The only difference that we have noted in recent literature is that Cheng (1958, 1976, 1987, 1994) treated *nigrifrons* (Przewalski, 1876) as a synonym of *elwesi* (Blanford, 1872). Clearly Cheng knew that his consistent treatment in this manner was at odds with that of Peters (1960), who recognised *nigrifrons*, and we can be sure that it was deliberate. It is a position that reflects the doubts of Hellmayr (1929). Although there

<sup>12</sup> Roselaar (in litt.) believes that punjaubi Whistler, 1936, should be recognised. This may affect the recognition of *dharmakumarsinji*. We have not been able to re-examine this.

is likely to be a reasonable quantity of Chinese material from the range of *nigrifrons* in collections outside China, on which basis judgements have been made, there may be important gaps and the two may intergrade smoothly. However, there is probably no topotypical material of *elwesi* (type locality: Sikkim) available inside China. The northern limit of *elwesi* in Tibet was undefined in Peters (1960), and perhaps Cheng had no access to specimens from the part of southern Tibet that has been judged to lie within the range of *elwesi*. Apparently the northern and eastern limits of *elwesi* remain to be determined.

In the accompanying paper on type material (Dickinson et al., 2001) a lectotype has been designated for the name *sibirica* Swinhoe (1871), which places this name in the synonymy of *flava* (Gmelin, 1789) and not in *brandti* Dresser, 1874, where it was listed by Cheng (1987).

### Acknowledgements

Particular thanks are due to Per Alström for sharing some of the taxonomic views that he and Krister Mild have developed for their forthcoming book. As stated earlier their research has evidently been very thorough both in the field and in the museum and they will have important findings to present. We have tried to avoid introducing their unpublished ideas as our own, but their work has influenced our thinking. In particular it caused deeper examination of the reported relationship between *Calandrella rufescens* and *C. cheleensis*. This led us to obtain the valuable help of Mikhail Kalyakin over the content and interpretation of the paper by Korelov. Cees Roselaar also offered his unpublished results, but we agreed that pending his co-author's approval these were "off limits".

We also benefited from the participation of Michael Walters in discussions over the occurrence of *Eremophila alpestris* in easternmost China and the decision to designate a lectotype. Much help with bibliographic research was kindly provided by Mrs F.E. Warr and supplemented by copies of Russian literature made available to us by Linda Birch, Vladimir Loskot, Mikhail Kalyakin and Mike Wilson to all of whom we are most grateful. Murray Bruce reviewed our list of references and offered helpful suggestions which we have often used. Hiroyuki Morioka kindly discussed Japanese treatment with us in the context of his preparatory work on the passerine texts for the latest Check-list (Ornithological Society of Japan, 2000). The occurrence of *Alauda arvensis* in Borneo was discussed with Clive Mann. Indirectly we also drew on help received in connection with our work on Asian type material and our thanks to all those involved is detailed in the following paper.

We are also most grateful to Per Alström and Cees Roselaar, who read one or more drafts of this paper. We have cited their views in some cases but not where we interpret their remarks to be better held for inclusion in their forthcoming work. If, in such cases, we have misjudged the situation we apologise.

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<sup>14</sup> The initial L, used here, for the name Ladislas may also be spelled Wladislas so that the initial W is sometimes used.

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<sup>15</sup> Also spelled as Sarudny.