ON THE AFRICAN BARBETS

BY R. E. MOREAU

I have read Ripley's revision of the Barbets (Auk, 62: 542–563, 1945) with interest, especially that part concerned with the African barbets, which comprise so large a proportion of the whole family. I have no criticisms of the new generic arrangement, except to remark that *Buccanodon* and *Pogoniulus* (in the sense used by Sclater in the 'Systema Avium Aethiopicarum') seem to me well characterized natural groups that nothing is gained by confounding. Generic arrangements are, of course, as several authors have recently insisted, largely matters of convenience and of individual judgment. I do wish, however, to make a protest at the method of presenting the "list of accepted subspecies," which too often leaves the reader guessing whether Ripley has omitted a form through inadvertence or after the examination and rejection of material; and whether all those he has admitted (especially those bearing the asterisk that denotes "not examined") are after consideration of all the available data.

At the outset it seems obvious that Ripley is under a great debt to the 'Systema Avium Aethiopicarum': a large proportion of the geographical ranges he gives are in Sclater's words, and, with two exceptions, none of the forms which Sclater omits, specifically rejects, or casts doubt upon, appears in Ripley's list. (The fact that no acknowledgement is made to Sclater is no doubt a tribute to the fact that his 'Systema' is indispensable to any taxonomic study of African birds.)

What, then, are we to make of the fact that Ripley makes no mention of the following forms that Sclater admits and upon which, so far as I know, no subsequent author has cast doubt?

(a) *Lybius chaplini* Clarke
(b) *Lybius rubrifacies* (Reichenow)
(c) *Lybius vieilloti buchanani* (Hartert)
(d) *Buccanodon belcheri* Sclater

It may be useful to add that *B. belcheri* is now accepted as a (very distinct) subspecies of *B. olivaceum* (Shelley)—*see* Bull. B.O.C., 58: 84, 1938, and Ibis, (14) 4: 426, 1938; while *L. chaplini* may be a subspecies of *L. rubrifacies* [Ibis, 13 (2): 304–308, 1932].

Another form admitted by Sclater and not by Ripley, *Lybius zombae* (Shelley), has recently (Bull. B.O.C., 58: 104–106, 1938) been regarded as a subspecies of *L. torquatus* (Dumont), with *L. z. albigularis* Neumann as a synonym. It may be that Ripley cannot re-

gard *L. zombae* as a subspecies (although it occupies an exclusive geographical area), because Salomonsen has regarded it as a mutant of *L. torquatus* (Proc. Eighth Internat. Orn. Congr. 1934: 190–198, 1938). If so, it is a point of view worth discussing.

Difficulties arise also about other barbets, which have been described since the publication of the 'Systema' and for the decisive rejection of which I find no published references, namely:–

(1) *Lybius bidentatus friedmanni* Bannerman
(2) *Trachyphonus margaritus berberensis* Bowen
(3) *Viridibucco leucomystax chyulu* van Someren
(4) *Pogoniulus pusillus lollesheid* van Someren (rejection in Bull. B.O.C., 58: 140, 1938, cancelled by acceptance in Bull. B.O.C., 63: 20, 1943).

Are these omitted from Ripley's list because they were forgotten or were they examined by him and rejected as synonyms? The first alternative must naturally be suspected in the case of (1) and (2) because the localities given by the authors for these new forms are outside the ranges given by Ripley for the species as a whole.

Since the 'Systema' was published, much taxonomic work has been done on the African barbets and conclusions on the validity of subspecies have been published especially by Chapin (Bull. Amer. Mus. Nat. Hist., 75: 488–534, 1939), Friedmann (U. S. Nat. Mus. Bull. 153: 436–466, 1930), Bannerman (embodied in 'Birds Trop. W. Afr.,' 3, 1939), Grant and Mackworth-Praed (Bull. B.O.C., 58: 82–84, 104–107, 116–117, 140–141, 1938 and 63: 19–20, 1943), Macdonald (Ann. Mag. Nat. Hist., (11) 2: 73–76, 1938[1] and Ibis (14) 3: 346–349, 1939), Grote (Orn. Monatsber., 46: 8–12, 1938) and Roberts (Ann. Transvaal Mus., 16: 109, 1935). Of all these authors, Chapin and Friedmann are the only ones included in the "literature cited" by Ripley, and inevitably the reader wonders whether due consideration has been given to the views of the others.

Yet Chapin has not been followed consistently. It is surprising to find the name *Lybius levaillantii,* which Chapin (*loc. cit.*) has noted as preoccupied, used by Ripley in preference to *Lybius minor* (Cuvier), which Chapin favors.

In such a case it is surely desirable that a reviewer of a group should cite the reference or other reasons that swayed him. In fact I would submit that every review of this nature would be improved and would be more readily acceptable if it gave evidence that all

[1] In which Ripley's inclusion of *Pogonorhynchus* and *Erythrobucco* in *Lybius* was anticipated and detailed arguments for that course were presented.

relevant sources had been examined, if all synonyms were cited, and if the grounds for each conclusion were stated, whether personal study of material or a specified publication. The size of such a review would be greater than one on the present model, but the increase in size would be thoroughly justified: and in cases where a sound general basis such as the 'Systema Avium Aethiopicarum' is available only departures from it need to be documented.

In a group review of this nature, which lists subspecies, it would also be useful to have some indication of the principles that have guided the author in a field of taxonomy that still causes so much controversy. It is, for example, noticeable that Ripley does not mention any of those subspecies which Grote described on size alone, *e.g., Lybius torquatus pumilio, Pogoniulus bilineatus rovumensis, Pogoniulus pusillus eupterus,* nor *P. p. lollesheid,* which is in the same category except that it has been accepted, not rejected, by Grant and Mackworth-Praed. One would like to know whether Ripley omits these forms because their measurements overlap with those of other forms (the grounds used by other authors, though it is not clear that adequate series of birds of the same sex have always been compared), or because they do not satisfy the 75% convention, or because some of these forms distinguished by Grote are part of a cline. If the last is true it would have been of interest to have the clinal tendencies indicated.

I take the opportunity of correcting some of the geographical ranges given by the author:—

(a) *Lybius leucocephalus lynesi* Grant and Mackworth Praed. The original description gave the range as "the Dodoma and Iringa Districts of Tanganyika Territory."

(b) *Lybius u. undatus* Rüppell. The range is given by Ripley as "Ethiopia" although four more subspecies are admitted, the ranges of which are all given (in Sclater's words) as including parts of Ethiopia. Sclater should be followed also for *L. u. undatus* and its range given as "Central Ethiopia from Addis Ababa north to Lake Tsana."

(c) *Buccanodon leucotis kilimense* (Shelley) and *B. leucotis leucogrammicum* Reichenow have their ranges given as "East Africa" and "Rufigi valley, Tanganyika" respectively. The range of *B. l. kilimense* should read "Mount Kenya to northern Tanganyika Territory (Usambara)" and that of *B. l. leucogrammicum* as "Rufiji valley and Uluguru, Tanganyika Territory" [Ibis, (12) 4: 82, 1928].

(d) *Pogoniulus kandti* (Reichenow), which was described from Kivu, has been omitted altogether. Presumably it has been regarded as a synonym of *P. bilineatus jacksoni* (Sharpe), the course already taken by Grant and Mackworth-Praed (Bull. B.O.C. 58: 82, 1938), Chapin, Peters and Loveridge (Bull. Mus. Comp. Zoöl., 89: 241, 1942). But in that case it is not sufficient to give the range of *P. b. jacksoni* as "Mau Plateau at Nairobi, Mt. Elgon" as Ripley has done. The Kivu population of *P. b. jacksoni* appears, in fact, to be isolated from the Kenya population of the same form by about four hundred miles, in which *P. leucolaima nyansae* (Neumann) occurs. Ripley has treated as conspecific those forms regarded by Sclater, and by Chapin and others, as belonging to the two different species *P. bilineatus* and *P. leucolaima.* The question naturally arises whether before doing so he considered whether any difficulty is raised by the Kivu *P. b. jacksoni.* (This western population may extend from Lake Kivu across Urundi and Ruanda, for I have recently obtained *P. b. jacksoni* from Kasulu, which is about 180 miles SSE of Lake Kivu and the same distance SW of the SW corner of Lake Victoria.)

(e) The ranges given for *Lybius guifsobalito ugandae* Berger, *L. bidentatus aequatorialis* (Shelley), *Pogoniulus chrysoconus schubotzi* (Reichenow), *P. c. zedlitzi* (Neumann) and *Tricholaema hirsutum chapini* Bannerman are unsatisfactory in the light of the information given by Bowen ('Cat. Sudan Birds,' 1926–1931) and Woodman ('Sudan Notes and Records,' 21: 315–324, 1938).

So far as the African barbets are concerned the statement that "almost all the species are found in areas of high trees, either deep forest or old gardens," is misleading. The statement is correct for the genus *Gymnobucco* (all three species), for three species of the four usually placed in *Buccanodon* (but not for *anchietae*), for six of the eight species of *Pogoniulus* (but not for *pusillus* or *chrysoconus*), for two species included in *Lybius* (*hirsutus* and *bidentatus*), and for *Trachylaemus purpuratus* Verreaux—sixteen species in all. The statement does not apply to the four species comprising the (old) genus *Trachyphonus,* nor to the remaining thirteen species in *Lybius* (to which *L. rubrifacies* + *L. chaplini,* omitted by Ripley, may be added). There are, in fact, no less than twenty species of African barbets that do not depend upon "areas of high trees, either deep forest or gardens," a view that I base on a wide range of published references as well as personal experience. Instead they are birds of the drier, often semi-arid, areas of Africa, which fall into three main vegetation types:

(a) "Thornbush," dominated by *Acacia* and *Commiphora,* of which few trees exceed fifteen feet in height; (b) what has often been called "savanna"—grassland swept by fire every year, and scattered with what in the dry season look like dead apple trees (the *Obstgarten-steppe* of the Germans), with here and there clumps of semi-deciduous bush or of taller trees; (c) leguminous woodland, especially *Brachystegia—Isoberlinia* trees up to about sixty feet high, leafless for much of the year, and at all times throwing only light shade. Types (b) and (c) often include strips of more or less evergreen trees along the watercourses, and occasional great fig trees that are a prime attraction to some barbets. Type (c) and some of type (b) can be described as "well-wooded," but they are nothing like "deep forest."

A final point: specific and subspecific names of forms transferred from *Buccanodon* and *Tricholaema* to *Pogoniulus* and *Lybius* need to be given masculine endings in place of neuter.

Amani
Tanganyika Territory

SOUTHWARD INVASION IN GEORGIA

BY EUGENE P. ODUM AND THOMAS D. BURLEIGH

Plate 9

THE extensive changes in natural conditions resulting from the spread of civilization in North America have naturally affected bird populations. Many species have been forced to retreat, and their abundance as well as their ranges have been greatly decreased. On the other hand, other species have profited by white man's alterations and have not only increased in abundance but in many cases have also extended their ranges. Because of the large amount of publicity given to vanishing species, bird students often forget the possible compensation provided by the advancing species. True, these latter are mostly small birds which do not have the glamour or popular appeal of such birds as Whooping Cranes and Ivory-billed Woodpeckers, but from the biological and economic standpoint they are equally worthy of study. When a species increases and invades new territory, it not only introduces a new and perhaps important factor into the biotic community, but speciation may also be affected since a new subspecies or species may eventually evolve in the new environment. In other words, just as artificial selection speeds up the evolution of domestic plants and animals, enabling man to learn something of the causative factors involved, so artificial changes in the environment speed up changes in natural populations, enabling