

In looking at a case of his birds this specimen at once attracted my attention as a strange looking Tanager, different from any I remembered to have seen, and on inquiry I learned its history, as above given.

As far as I can learn this is a bird new to California, and also to the United States. If so it seems worthy of record. (No. 2697. ♂, Coll. of W. E. B.)

In 1884 I took east with me a specimen of *Tringa fuscicollis*; it was so named by some good authority, Mr. Ridgway I think. By the A. O. U. Check List it appears that it has not been found in California. It was a solitary individual, shot by myself on the marsh near Oakland, Cal. No. 1080. ♀, Oct. 8, 1883. Iris dark brown, feet and legs yellow. Coll. of W. E. B. — WALTER E. BRYANT, *Oakland, Cal.*

CORRESPONDENCE.

[Correspondents are requested to write briefly and to the point. No attention will be paid to anonymous communications.]

Scarcity of Adult Birds in Autumn.

TO THE EDITORS OF THE AUK:—

Sirs: Between the first of September and the twenty-second of November of this year I collected 367 bird skins; 258 during the month of October in Colorado, and the remainder in Kentucky. Of this aggregate of 367, 348 were birds of the year. The question at once presents itself, whence this glaring discrepancy? Where were the adult birds? I made no effort to secure young birds (in nine cases out of ten the young fall bird is indistinguishable from the adults by external characters), but 'took them as they came.' It may be asked how I determined the birds in question to be 'birds of the year.' For several years I have noted that nearly all the birds shot by me in the fall had skulls that were more or less incompletely ossified, and in 1885 I began to systematically examine the skulls and other skeletal parts with the view of determining the relative age of the birds, assuming that those individuals exhibiting a soft or incompletely ossified skull, must have been hatched during the immediately preceding breeding season.

Of the nineteen adult birds collected between the dates above given, *eleven* of them were species resident where collected.

Apparently the only legitimate inference from the above facts is, assuming my method of determining the relative age of birds correct, that the adults migrate as soon as they are relieved of the care of the young birds, and that the latter form the great bulk of the autumnal migration stream. Opposed to this theory we have the *negative* evidence that ex-

tremely few adult 'transients' are recorded as observed in July and August. Are there not some members of the A. O. U. who can throw light upon the subject?

Respectfully yours,

CHARLES WICKLIFFE BECKHAM.

Bardstown, Ky., Nov. 23, 1886.

Classification of the Macrochires.

TO THE EDITORS OF THE AUK:—

Sirs:—Once more I must ask your indulgence in the matter of a little space, as I have a word or two to say in regard to Mr. Lucas's paper on 'The Affinities of Chætura' which appeared in the last number of this journal (Oct., 1886), and from the reading of which I find that I have on my hands another ornithologist who takes exception to the further separation of the Cypseli and Trochili, more than is now generally agreed to by the majority, perhaps, of systematists in their schemes of classification.

It is not my intention on the present occasion either to add or subtract anything to what I have already contributed to the morphology of the Macrochires, for by so doing I would forestall the conclusions of my further researches in this matter that I now have in hand.

Mr. Lucas says, "Nevertheless, until still more evidence to the contrary is adduced, I will hold fast to Huxley's union of Hummingbirds and Swifts" (p. 444).

Now at the present writing I have been over two years in a position where I have not been able to avail myself of either the libraries or the museums, and have at my command but a limited working field library; so that it is quite possible that Professor Huxley may have recently changed his views in regard to the taxonomy of the Macrochires, and I not have known of it. But, I do know that in 1867 he wrote the following sentences, to wit: "In their cranial characters, the Swifts are far more closely allied with the Swallows than with any of the Desmognathous birds, the Swift presenting but a very slight modification of the true Passerine type exhibited by the Swallow. No distinction can be based upon the proportions of the regions of the fore limb; since in all the Swallows which I have examined [*H. pacifica*, *H. riparia*, *H. rustica*, and *H. urbica*], the manus and antibrachium respectively, greatly exceed the humerus in length, though the excess is not so great as in *Cypselus*" (P. Z. S., Apr. 1867, p. 456). And again in the same paper he says "The *Cypselidæ* are very closely related to the Swallows among the Coracomorphæ" (p. 469). Mark you, Professor Huxley here says "very closely related." In other words, at the time that this eminent biologist formulated his 'Classification of Birds' in the memoir in question, he evidently believed that Swifts were but profoundly modified Swallows. Believing this as he did, I am the more