

Classification of the Macrochires.

TO THE EDITORS OF THE AUK:—

Dear Sirs:—Dr. Shufeldt's letter in the October 'Auk,' last year, requires only a few lines in reply from my side.

I am sorry that Dr. Shufeldt in "carefully reading a number of times" the sentence commencing: "internally they differ," etc., failed to see that the whole was a case of typographical error, and still more sorry that he did not know "the kind of comparison he [I] wishes to institute between the sternum of a Swift and a Swallow," when I referred to the bifurcate manubrium and deeply "two-notched" sternum of the latter. Dr. Shufeldt will probably believe me, when I state, that in the original, from which the sentence in question was quoted, the kind of comparison was made clear, and that the words "*pointed manubrial process and no posterior notches to the*"—an entire line—has fallen out between "*a*" and "*sternum*." I cannot prevent Dr. Shufeldt from taking exception to the remark that the sternum is 'two-notched' in the Swallows, notwithstanding the fact that he admits its having "*a pair of notches in its xiphoidal extremity*," and my own belief that "a pair of notches" and "two notches" mean exactly the same thing, but I must protest against his remark that "the two-notched sternum is seen in such forms as *Picus*." To the uninitiated mind it would seem to be a decided misnomer to call the sternum of *Picus* two-notched when the fact remains, that it possesses *four* notches. "Such forms as *Picus*" of course, have two notches *on each side of the mesial line*, but Dr. Shufeldt will probably pardon me for not calling a horse a two-legged animal, or a man a one-legged animal, notwithstanding the fact that they have respectively two legs and one leg on each side of the mesial line. But if Dr. Shufeldt calls a horse a four-legged animal, why object to calling the sternum of the Woodpeckers four-notched?

In regard to the similarity or dissimilarity of the flight of the Swifts as compared with that of the Swallows or Hummingbirds, I shall only remark that Dr. Shufeldt's supposition that I would never have asked, "what differences are there in the Swifts' flight from that of the Swallows' that should have caused such a remarkable modification towards the Hummingbirds," if I "had ever had the opportunity to compare in nature the flight of two such birds, for example, as *Micropus melanoleucus* and *Tachycineta thalassina*," will not hold for the simple reason that I have had the opportunity to compare in nature the flights of several species of Swifts and Swallows. I am also familiar with the flight of the Hummingbird, and in spite of this, or rather just on account of my observations, do I reiterate that the flight of the Swift is decidedly more like that of the Swallow than it is like that of the Hummingbird. And I also insist that I am still without an answer to the question, What in the nature of these birds' flight has brought about such an extraordinary similarity, osteologically, myologically, and pterylographically in the wing-structure of the Swifts and Hummingbirds, as compared with that of the Swallows? For surely, it cannot be denied, that the flying apparatus of Swifts and Hummers pos-

sesses features and combination of features quite unique, and shared by no other birds, and especially not by the Swallows. That a Swift hovers in front of its nest before entering it "like a Hummingbird over a flower," shows certainly no special relationship, for I have seen despised English Sparrows do the same, and as for swift and precipitous flight and its instantaneous checking I might quote numerous birds which in their wing-structure show no analogy to that of the Macrochires. The *superficial* similarities of certain structures in the Swallows' and the Swifts' wings can undoubtedly be traced "to the modification of these structures gradually brought about by the habits or actions of the forms in question," to use Dr. Shufeldt's own phraseology. It is upon the recognition of the essential and the unessential similarities, and of the superficial analogies and the radical affinities, that the present question hinges.

Yours, very truly,

LEONHARD STEJNEGER.

Smithsonian Institution,
December 25, 1886.

TO THE EDITORS OF THE AUK:—

Sirs:—Will you kindly allow me a little space in which to reply to Dr. Shufeldt's comments on the footnotes of my recent paper on 'The Affinities of *Chætura*'?

At the outset let me say that I object less to the separation of Swift and Hummingbird than to the union of Swift and Swallow. As Dr. Shufeldt now concludes (or did in October last) that the Swifts are not a family of *Passeres* placed next the Swallows, but an order by themselves, we are less at variance than when the paper on *Chætura* appeared.

In one and the same paragraph Dr. Shufeldt objects to my statement that Professor Huxley united the Swifts and Hummingbirds, while quoting Huxley's own words, which show the statement to have been correct! (p. 86). The remark that Professor Huxley "evidently believed that Swifts were but profoundly modified Swallows" is purely an assumption; but even if it be a correct one, the fact remains that he believed them to be so *very* "profoundly modified" as to require a place in quite a different order. In view of the fact that Dr. Shufeldt has not been in Washington for over two years, it is a little surprising that he should assume to know exactly what material is contained in the collections of the National Museum. Nevertheless, Dr. Shufeldt is this time correct in his supposition, for at the time of writing neither *Panyptila*, nor *Tachycineta thalassina* (*T. bicolor* I did have) were in my possession, although since then crania of both species have been extracted from skins, supplied by the courtesy of Mr. Ridgway, and verify my statement that the *maxillo-palatines* as figured by Dr. Shufeldt are imperfect. While my specimen of *Panyptila* is a poor one, having suffered from decalcification, traces of the slender maxillo-palatines still remain, and show them to be practically of the same shape as those of *Chætura*, *Cypselus apus*, and *Dendrochelidon mystacea*,

this latter bird having been kindly furnished me by Professor Henry A. Ward. Dr. Shufeldt's very figure of *Tachycineta* shows at a glance that the expanded ends of the maxillo-palatines have been broken off, and I have yet to learn that doubling the size of a drawing doubles its accuracy.

I should have been very glad to have found myself in error concerning *Panyptila*, as it would have given me another, although slight, point of resemblance between the Swifts and Hummingbirds.

The material in the National Museum has already taught me that the sternum may be notched or entire in Auks of the same species, and the same thing will be found to occur in the Loons; also, if my memory is not treacherous, in other water fowl. The reason for this is, it seems to me, very evident, while the fact itself has no bearing whatever on the present case. That Dr. Shufeldt is aware of this is shown by his haste to remark that "Of course in recording what I have just done in the preceding paragraph, I by no means wish it to be understood that I in any way underrate the significance of the 'notching' of the xiphoidal end of the sternum, in the *vast majority* of birds." I would also note that the entirety of the posterior margin of the sternum was but one of *four* good characters pointed out. Since Dr. Shufeldt places but little reliance on the structure of the bony palate as a taxonomic character, has had his faith in the sternum shaken, and rejects the modifications of the limbs (aside from the modification of the phalanges, on which he lays considerable stress!), it would seem that but little of the skeleton was left on which to found comparative distinctions.

That the 'osteologist-in-chief' is not conversant with a large amount of ornithological literature is unluckily too true, and he has always regarded it as a great misfortune. Still, had my commentator been less engrossed by the footnotes, he might have inferred from a paragraph almost at the very outset, that I was not *entirely* ignorant of Dr. Parker's opinions on the subject under consideration.

In conclusion, allow me to express my surprise at the concluding paragraph of Dr. Shufeldt's letter, the sarcastic tone of which leads me to infer that he prefers to evolve opinions which do *not* compare favorably with those held by living masters in morphology.

Very respectfully,

FREDERIC A. LUCAS.

Washington, D. C., Jan. 25, 1887.

The Sense of Smell in *Cathartes aura*.

TO THE EDITORS OF THE AUK:—

Sirs:—In his article in the January number of this Journal, Mr. Ira Sayles has added another instance to the already long list of fallacious 'proofs' of the remarkable power of scent in the American Vultures. Ignoring the fact that there is certainly room for some difference of opinion as to what constitutes a remarkable power of smell, he sets aside as utterly



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