

forest fire fanned by a southern breeze; that in their stupor, their instincts teaching them at the time of fear to fly south if anywhere, they flew into the smoke and got suffocated and frightened. No doubt thousands lost their lives and fell into the flames below. The survivors then flew away from the fire, and coming over this city were attracted by the electric lights and flew madly against the walls of buildings. At the Queen's Hotel, where the windows also had to be shut, there is a light as well as at the 'Free Press.' Two years ago a similar stampede was reported in one of the Southern States, but of Ducks alone. They flew in hundreds against the electric masts, and then against buildings. Our lights are on poles only 25 to 30 feet high.

The birds were all small and most of them of this year. Among those picked up or caught were the Redstart, the Black-and-white Creeper, the Tennessee Warbler, the House Wren, Flycatchers, the Hermit Thrush, the Golden-crowned Thrush, and the Chestnut-sided Warbler. The last is a rare visitor here. Small Sparrows, I am told, had been found, but I am not sure of this.

Mr. W. Hurd, our taxidermist, saw next day a Thrush flying along Main Street diagonally and only about two feet above the ground. The birds were all weak, but many, like those which struck my windows, evidently recovered, at least their senses. All were stupified, and many had wounds evidently caused by barbed wire.

In skinning the birds for preservation Mr. Hurd failed to notice anything which could have caused death; the various organs appeared sound and healthy, though the birds were rather small for him to be very certain regarding all of them.

I should have mentioned that the forests were on fire some eight miles south of the city.—ALEXANDER McARTHUR, *Winnipeg, Manitoba.*

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## CORRESPONDENCE.

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

### The Dermo-Tensor Patagii Muscle.

TO THE EDITORS OF THE AUK:—

*Dear Sirs:*—In this letter the writer proposes to reply to a criticism of Leonhard Stejneger, which appeared in 'Science' August 5, of an account of mine of a muscle which is present in certain birds, and which I designated by the name entitling this communication.

To those who are aware of the conditions under which I prosecute my anatomical work no word need be said; my labors in the myology of

birds were chiefly inaugurated and have been pursued during a time while their author found himself removed by several thousand miles from the libraries wherein may be consulted the works of the older anatomists. Under these circumstances my guides have been the general works of Owen, Huxley, Garrod, Mivart, Parker (T. J.), Forbes, and others of similar standing; several of these writers have given very exhaustive accounts of the myology of birds, but none of them, so far as I have been able to discover, have described the muscle in question.

This being the case I was intentionally guarded in my letter to 'Science' (No. 229), and said the dermo-tensor patagii "was a muscle for which at this moment I recall no published description" (p. 624), and by no means proclaimed it a "new discovery," although, so far as I am concerned, it has certainly proved to be an independent observation, but I fail to see that it is any the worse for that circumstance. This answers the first objection to my account made by Dr. Stejneger. Secondly, he charges me with "supposing that it is peculiar to the true passerine birds," when I, in my letter, distinctly said that "I had investigated the matter in but a limited number of birds" and would "look with interest for such future researches that might be made in that direction by others" (p. 624).

Any structural difference in such a group of vertebrates as birds is always to be welcomed, and as the muscle is evidently present in some and absent in others, I still maintain "that it is of taxonomical value," perhaps of greater value than did the authorities whom Dr. Stejneger pleases to quote to me,—dissectors, as a rule, who did not especially look into the structure of birds with the view of determining their affinities as Garrod did, and consequently would naturally not realize the importance to avian classification of such a muscle, were it even a new discovery to them.

Throughout the entire second paragraph of Dr. Stejneger's letter, I am, as it were, directly charged with doing Professor Garrod a "great injustice," and "grossly misrepresenting" him, as if that were the sole aim of my original description; whereas those who may be familiar with my writings in anatomy, know full well that in the many, many instances wherein I have been called upon to allude to his work or name, it has always been with the greatest amount of regard, a regard which I ever sincerely feel, and which is ever increasing as I more fully appreciate the power and force of the work he was enabled to leave us in his only too short career.

The dermo-tensor patagii was entirely absent *on both sides* in the specimen of *Tyrannus tyrannus* which I dissected, and I even went so far as to bring the dissection under a powerful microscope (one inch objective); there was no muscular tissue present, and, as I say, further than that I have not investigated the matter, nor, just now, do I intend to do so, as other anatomical work is engaging my attention.

In closing, perhaps I may be permitted to point out a few of the errors which Dr. Stejneger has unfortunately allowed to creep into his letter of criticism, and more especially into the figures which he published in

'Science' (No. 235) to show me how it ought to be done. These figures (Figs. 1 and 2, p.71) Dr. Stejneger informs us are "both of one-third natural size"; if this be so their author is laboring under the impression that *Colaptes auratus* has a head nearly *five inches* long, and everything else in proportion, to say nothing of the dimensions *Amazona* would attain under the statement in question! And, may I ask, how long since do we see upon "*dorsal view*" of a dissected *Colaptes*, the tips of the shoulder in close anatomical connection with the *side of the middle of the neck?* (See his Fig. 1.) Turning to his "*dorsal view*" of a dissection of the patagial muscles of a Parrot (*loc. cit.*, Fig. 2), this latter error is again repeated, but a far more glaring one here confronts us, for, among other faults, Dr. Stejneger has plainly drawn and lettered his biceps muscle, and would have us believe that it is inserted into the *extensor metacarpi radialis longus*, between the tensor patagii brevis and the humerus. It seems to me on an occasion of this kind, and where the opportunity presents itself to have two new figures added to anatomical science, it is fortunate for us when they prove to be useful ones; such is by no means the case in the present instance, and the true aims and accomplishments of criticism have herein failed in Dr. Stejneger's hands. Upon carefully reconsidering my last letter to 'Science' upon this subject I am at loss to find anything requiring any alteration, nor any adequate reason for changing the name I have given the dermo-tensor patagii muscle; indeed, in the latter instance, I am in full sympathy with Professor Elliott Coues, who has recently, and in the most forcible manner ('N. Y. Med. Record'), shown that the terminology of muscles requires a through reviewing, and the day is with us when we ought, for the sheer sake of clearness and convenience, to lay aside some of the abominable names the old anatomists bestowed upon some of them, and in some instances where the name was five times as big as the muscle.

From this standpoint I think Dr. Stejneger can consider the "*pars propatagialis musculi cucullaris*" of Fürbringer and Gadow as the dermo-tensor patagii of the present writer.

R. W. SHUFELDT.

*Fort Wingate, New Mexico,*  
August 14, 1887.

POSTSCRIPT:—A description of the above muscle was published by the writer in 'Science,' some little time ago (No. 234, July 29, '87), and it called forth, it seemed to me, rather an acrimonious protest from Dr. Leonhard Stejneger in the same journal. That writer so misrepresented the entire matter, that I felt his criticism really required some notice from me, and the above reply was sent to 'Science,' but much to my surprise, the editor of that paper objected to my defending myself in its columns against a criticism which he saw fit to publish. Will 'The Auk' kindly do this matter justice for me, and insert the above rejoinder?

By the first of next month (Oct. 1, '87) I trust to have out a paper covering a description of all the muscles thus far used in the classification of birds, and in it will be given a full account of the present one. Even

until now I have had no time to further investigate this interesting structure, but will briefly remark here that I found it present, *i. e.* the *dermo-tensor patigii* muscle, in all the Acromyodian Passeres that I have thus far examined, and it was *entirely absent* in an alcoholic specimen of *Tyrannus tyrannus*, kindly supplied me by Mr. H. K. Coale, President of the Ridgway Ornithological Club of Chicago.

I introduce two figures here showing the absence and presence of this muscle on the occasion I examined it.

It is quite possible that this muscle may exist in other birds. I have nowhere stated that it does not, so far as I can remember. But I will say

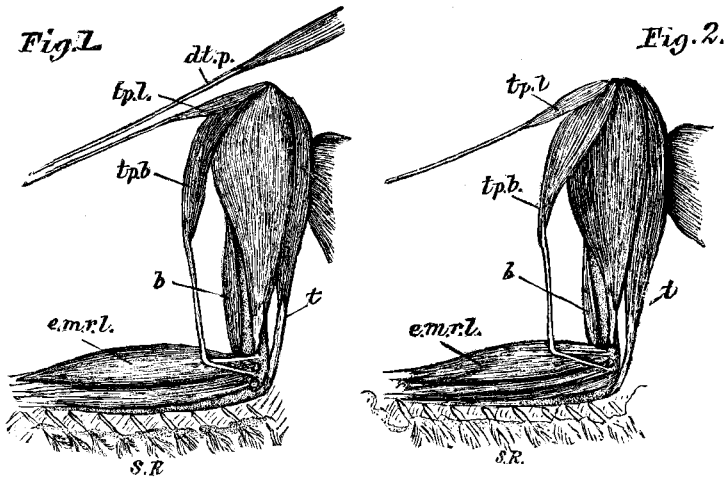


FIG. 1. Outer aspect, right arm of *Xanthocephalus xanthocephalus*, showing a dissection of the muscles of the region in question.

FIG. 2. The same of *Tyrannus tyrannus*; slightly enlarged. Both figures drawn by the author from his own dissections. *dt. p.* dermo-tensor patigii; *tp. l.* tensor patigii longus; *tp. b.* tensor patigii brevis; *b.* biceps; *t.* triceps; *e. m. r. l.* extensor metacarpi radialis longus; *S. R.*, secondary remiges.

that if it is constant for the Acromyodian Passeres, and absent in the Mesomyodian Passeres, the fact will constitute a taxonomic character of value. If it is subsequently found to exist in both, a complete examination of it in our American birds will be a good thing; I do only insist that I found at least one Kingbird wherein it was entirely absent, and that it was present in a long list of Oscines.

With these few brief remarks upon the subject I close the case for the present, with the hope that other dissectors with good eyes will look into the matter. But if you illustrate your work, let us, gentlemen, have intelligent drawings.

Very faithfully yours,

R. W. SHUFELDT.

Fort Wingate, N. Mexico,  
13th Sept., 1887.