

2. *Crecooides osbornii* Shufeldt. This was omitted simply because it was accidentally overlooked.

3. *Piranga 'rubiceps' = rubriceps*. If Dr. Shufeldt makes no protest against *Icterus icterus* and *Spinus notatus*, admitted to the list on Audubon's authority, he should not object to the case of *Piranga rubriceps*, the geographical conditions being similar. So far as known, *P. rubriceps* is not kept as a cage bird; certainly it is not one of the commoner cage birds of our bird stores, as is *Gubernatrix cristatellus*. Many of the common cage birds escape from confinement and are afterwards captured, perhaps after a considerable interval of freedom, and showing very few, if any, traces of previous confinement. Among them are finches, parrots, and parrakeets from Africa, India, Australia and tropical America. Their capture may be recorded as a matter of interest, but no one considers it admissible to include such species in the list of North American birds. On the other hand, wild birds either wander or are carried by storms hundreds and even thousands of miles beyond their usual range, and are captured under circumstances which preclude the supposition of their being escaped cage birds, as in the case of many European stragglers that have occurred once, or a few times in North America. To this class of waifs belongs *Piranga rubriceps*.

4 and 5. Regarding the relationships of the Grebes, Loons, Auks, etc., probably if the A. O. U. Committee were to revise its classification they would make some changes in respect to the position of these groups; but, for reasons given in my former letter (SCIENCE, N. S., No. 73, May 22, 1896), the Committee did not consider it advisable to transpose any of the higher groups. But the Committee doubtless would not follow Dr. Shufeldt in removing the Owls from the Accipitres to place them with or near the Goatsuckers.

J. A. ALLEN.

'THE POLAR HARES OF EASTERN NORTH AMERICA.'—AN ANSWER TO DR. C. H.

MERRIAM'S CRITICISMS.

TO THE EDITOR OF SCIENCE: Dr. C. Hart Merriam has seen fit to devote nearly two pages

of SCIENCE\* to my preliminary paper on the 'Polar Hares of Eastern North America.'

It is difficult to ascertain the motive which prompted this review of my preliminary work on the Polar Hares, the mature results of which I expressly stated in the *American Naturalist*,† would soon be published in the form of a compendious revision of the New World representatives of the *Lepus timidus* group. The importance which Dr. Merriam seems to attach to the paper in question, by devoting thereto three times the space taken by his succeeding review of Sclater and Thomas' new 'Book of Antelopes,' together with the suprising attitude taken on certain questions of nomenclature and diagnostic technique, demand a rejoinder.

Waiving the objections made to my reëstablishment of the specific distinction of the American from the European Polar Hare, and my restriction of the type locality of the latter to southern Sweden, let us consider Dr. Merriam's position regarding my adoption of the name *arcticus* of Ross for the Baffin Land Hare instead of *glacialis* of Leach, which comes nineteen pages later in the same book. In the absence of any statement to the contrary, I proceed on the supposition that Dr. Merriam still agrees with me in taking the Code of Nomenclature of the American Ornithologists' Union for authority in a case of this kind.

His main objections to the use of the name *Lepus arcticus* 'Leach,' Ross, are:

(1) "Capt. Ross was not a naturalist and made no claim to technical knowledge of zoölogy."

(2) "All that he [Ross] knew of the animal came from Leach."

(3) "Ten persons have used the name *arcticus*, while thirty-six have used the name *glacialis*."

(4) "Irrespective of the merits of the two names, *glacialis* would have to be taken if we accept the rule that in cases of names of equal pertinency, the first reviser of the group has the privilege of fixing the name."

The first objection only begs the question. The rules of nomenclature no longer attempt to define what should constitute the standard of authorship, contenting themselves in such a

\* Friday, April 10, 1896, pp. 564, 565.

† March, 1896, p. 256.

case as this to the definition of what constitutes a valid naming and description of genera or species. Would Dr. Merriam have us estimate the personal equation in the authorship of names proposed by such a man as Rafinesque because he fell so far below the scientific standards of a Leidy? Livingstone was 'only a missionary' and Krider a 'gunmaker,' but science is willing to say " 'A man's a man' and priority is priority 'for a' that.' "

The second objection made by Dr. Merriam is not only as irrelevant as the first, but is based on an incorrect statement. Ross knew more about the specimen than Leach did, and the latter was more indebted to Ross for points as to the animal than Ross was to Leach. They described the same specimen, and, besides giving all the diagnostic characters described by Leach, Ross adds two important ones and gives the collector, locality and date of capture of the specimen, which Leach omitted entirely. In short, Ross' description is the better of the two.

As to objection number three, the inconsistency of the numerical argument thus advanced by a member of the A. O. U. Committee on Classification and Nomenclature\* favoring the old standard of 'time-honored' custom, and consensus of opinion in a question of 'equal pertinency' in specific names, strikes me as no less lamentable than subversive of the best interests of that department of American science which aims at canonical permanency in the rules of nomenclature.

The fourth objection is based on a private interpolation into the canonical code even more obviously heterodox than objection number three. I would ask Dr. Merriam where he finds the 'rule that in cases of equal pertinency the first reviser of the group has the privilege of fixing the name?' I do find in the A. O. U. Code of Nomenclature, on which Dr. Merriam has frequently had occasion to publicly pledge his faith, under Canon XVII., relating to 'Preference between competitive specific names published simultaneously in the same work \* \* \*,' a section 3 which reads, 'Of names of undoubt-

edly equal pertinency and founded upon the same condition of sex, age or season, that is to be preferred which stands first in the book.' To my mind this completely covers the matter at issue and justifies my course in adopting *Lepus arcticus* as the proper name of the Baffin Land Hare.\*

Regarding his criticism of my use of the Scandinavian *L. timidus* as the basis of comparison in a paper on American Polar Hares, I need make no apology. Dr. J. A. Allen's monograph of the American Hares was taken as the last authoritative declaration of an American mammalogist on the relations of these animals, and, as he failed to recognize the distinctions which I found to exist, it was reasonable that they should be demonstrated by the plan of comparison adopted in my paper.

Instead of outlining the scope and aim of my paper and stating that I had endeavored to show the close affinity, but specific distinction of the Baffin Land and Scandinavian Hares, and their great differences from the Hare of Greenland, which previous authors have more or less confounded with *L. arcticus* of Ross, my critic chiefly devotes himself to a justification of his own peculiar views on the subject of names, methods and forms of expression.

Dr. Merriam ventures no opinion as to the status of what he spells '*L. greenlandicus*' in his critique, and from his own admissions he evidently knows less about the animal than many of the authors whom he cites to support his 'time honored' but mistaken opinions.

To cap the climax of unjust sarcasm, the chief apostle of generic, specific and subspecific subdivision in this country draws a parallel between my naming of the Labrador and Newfoundland subspecies, *L. a. bangsii*, to the separation of 'weasels that turn white in winter from specimens of the same species that remain brown the year around!' Shall I answer such logic? Not until I have more time and SCIENCE more space for unscientific contro-

\* Dr. Merriam was recently appointed on this Committee in place of Mr. Henshaw. See Check List N. A. Birds, 2d ed., 1895, p. vi., foot-note 1.

\* Since these remarks were written, I find that Dr. J. A. Allen fully endorses the position I have taken, in his answer to an inquiry made by Mr. Witmer Stone, on this and kindred subjects, treated in the 'Correspondence' of the April issue of the *Auk* for 1896.

versy. Then, perhaps, Dr. Merriam will tell us whether he continues to recognize *Lepus americanus* and its subspecies *L. a. virginianus*.

SAMUEL N. RHOADS.

ACADEMY OF NATURAL SCIENCES,  
PHILADELPHIA, April 17, 1896.

AMERICAN POLAR HARES: A REPLY TO  
MR. RHOADS.

THE above wail from Mr. Rhoads respecting my review of his paper on the Polar Hares calls for a brief reply. It was not the importance of Mr. Rhoads' paper, as he seems to suppose, but the importance of certain principles involved in his methods of treatment, that led to the length of my review. My criticisms were aimed mainly at two matters: one, a matter of description; the other a matter of nomenclature. In describing the new American hares, Mr. Rhoads contrasted them with a European species (*Lepus timidus*) instead of with their American relative (*Lepus glacialis*). This struck me as bad systematic zoölogy. In treating the Polar hare of Baffinland he adopted the specific name *arcticus* instead of *glacialis*, though both names appeared simultaneously in the same book. This struck me as bad nomenclature.

The reasons for retaining *glacialis* as the proper name of the animal were stated at length in my review and need not be repeated here. But in his reply Mr. Rhoads implies that I have subordinated priority to the scientific standing of an author. This I deny. Priority of publication is the cardinal principle of nomenclature—the foundation of all modern codes; without it, stability in nomenclature is impossible. But priority of *publication* and priority of *pagination* are two widely different things, and I deny that priority of pagination constitutes priority of publication. It can hardly be gainsaid that the different pages of a book appear simultaneously; hence names on different pages of the same book should be treated in the same way as names appearing simultaneously in different books. Sequence of pagination is a trivial circumstance, not to be considered in fixing specific names except in cases where no other reason for a choice can be found. Even the A. O. U. Code quoted by Mr. Rhoads concedes this, and goes so far as to

accord greater weight to *sex*, *age* and *season* of the type specimen than to priority of pagination. In other words, in choosing between names of even date, sequence of pagination is a last resort.

It is useless to enter into a controversy with Mr. Rhoads over his astonishing statement that of the descriptions of the American Polar hare given by Ross and Leach, "Ross' description is the better of the two." Reference to the work in which both appeared will settle this point.

In reply to Mr. Rhoads' inquiry as to the source of the rule that 'in cases of equal pertinency the first reviser of the group has the privilege of fixing the name,' it may be stated that said rule expresses the practice of most systematic zoölogists—and I think botanists as well—and is in complete accord with the spirit of the A. O. U. Code, though not there formulated as a distinct canon. In closing, I must thank Mr. Rhoads for calling my attention to what he considers would have been a proper review of his paper.

C. H. M.

THE SUBJECT OF CONSCIOUSNESS.

TO THE EDITOR OF SCIENCE: In the number of SCIENCE for May 15th there is a letter from Johannes Rehmke on the subject of 'consciousness,' about which I beg leave to be indulged in a brief statement.

Take two equal weights with handles, one weight being several times the bulk of the other. Ask a blindfolded man to tell which is the heavier, being careful not to let him touch either weight, but only the handle, and he will not judge of a difference. Now let the same man, seeing the weights, but not knowing them to be the same, decide which is the heavier; he will affirm that the smaller is the heavier weight. This is a common experiment in psycho-physics. There are on record a vast number of similar experiments which have been abundantly verified, all leading to the conclusion that there are two elements in sensation, the one of consciousness of the effect upon self and the other an inference relating to the thing observed by any one of the senses. All of these experiments, and a vast body of experiences which every individual undergoes,