CORRESPONDENCE.

Cooke's 'Distribution and Migration of North American Shorebirds.'

To the Editor of 'The Auk:—

Dear Sir:—The comments made by Mr. F. H. Allen in 'The Auk' for October, 1911, on certain shortcomings in Cooke's 'Distribution and Migration of North American Shorebirds' seem to call for a few words in reply. I trust to make these as courteous in tone as were the comments.

It is claimed by Mr. Allen that the facts given under the several species are not complete enough to justify the statement made in the introduction, and on a subsequent page, as to the scope of the bulletin. Thus, to show the insufficiency of the data on the breeding and wintering of certain of the Shorebirds, he states that the Northern Phalarope "is a common summer resident in Labrador and breeds along the entire coast," and adds that Cooke might easily have ascertained this by consulting the 'Birds of Labrador' by Townsend and Allen. The fact is that these authors kindly sent Prof. Cooke a copy of their most excellent work immediately after it was published, that it has been on his desk ever since, and has been frequently and profitably consulted. In their statement in regard to the breeding of the Northern Phalarope in Labrador, however, there is reason to believe that the authors are mistaken. Hence their statement, though by no means overlooked, was not accepted.

The present status of the Northern Phalarope as a breeding bird in Labrador is far from satisfactory. In Vol. III of his 'Ornithological Biography,' Audubon gave a circumstantial account of the discovery of the nest and eggs of the bird in Labrador, ending with the statement that "both young and old had departed by the beginning of August." This account of the breeding of the species is explicit enough, and would be entirely satisfactory were it not for the fact that it sharply conflicts with the equally circumstantial account by the same author in his journal, where we find that the only time he mentions seeing the species, is on July 29, when he states:

"I saw this afternoon two, or a pair, of the *Phalaropus hyperboreus*; they were swimming in a small fresh-water pond, feeding on insects, and no doubt had their nest close by, as they evinced great anxiety at my approach. I did not shoot at them, and hope to find the nest or young; but to find nests in the moss is a difficult job, for the whole country looks alike." It thus appears that up to July 29 he had found no Phalarope nests, and, as it seemed to Prof. Cooke impossible to reconcile the two accounts, he decided to pass by Audubon's breeding record, especially since it has not been substantiated by later investigations.

June is preëminently the breeding month for this phalarope. Numerous June records are at hand for different localities in Canada and Alaska, comparatively few are as late as July. It is significant that of 70 nests found by McFarland at Fort Anderson, July 10 was the latest date, most of them, even in this far northern locality, being in June. In more southern localities, the bird would naturally be expected to nest earlier.

The fall migration of the Northern Phalarope is well under way by the middle of July, so that the mere presence of the birds anywhere after that date cannot be accepted as definite proof of breeding. The two birds seen by Audubon July 29 were doubtless migrants, and the date agrees well with the dates at which migrating phalaropes have been noted by subsequent observers. Brewster found no nests in the Gulf of St. Lawrence region, and does not record the bird as absolutely identified by him until July 25; Bigelow found no nests, and saw no birds until July 23. It is true that in his 'Birds of Northeastern Labrador' published in 'The Auk' for January, 1902, he gives the bird as "Common. Breeding in almost all the suitable marshes; occasionally very abundant off-shore." This again on its face value would be satisfactory as to the essential fact that this phalarope breeds on the Labrador coast but in a recent letter to Prof. Cooke, Mr. Bigelow states that he did not find *nest or eggs of the bird* but found the birds in the marshes at Poutes Cove on July 23 [thus well within the migrating period], and adds that his reason for stating that they breed there was that the natives assured him the birds observed [in 1902] were nesting. While this testimony by the natives may have been convincing to Mr. Bigelow, it cannot, we submit, without further evidence, be accepted as definitely establishing the breeding of the species in the locality in question, especially when is taken into account the late date, July 23.

Townsend and Allen found neither nests nor young, and saw no birds until July 27. Hence the statement in their 'Birds of Labrador' that "The Northern Phalarope breeds along the entire Labrador coast in freshwater marshes on the borders of ponds and lakes" would appear to have been based on the statements of the authors they subsequently name (Audubon, Low, Bigelow, Turner and Spreadborough) rather than on their own observations. If the species really breeds generally on the Labrador coast, it should have been noted by Coues, who had far better opportunity of observation than any of the observers above mentioned. As a matter of fact he did not find the bird at all, while Low, who afterwards visited the same district, does not mention meeting with the bird in the breeding season until June 13, when he was far inland from "Labrador" near the head of the Hamilton River. Spreadborough's account refers to James Bay and hence has nothing to do with the Labrador coast. So that as the matter now stands, "Ungava Bay, about 59° N. (Turner)" is the most southern certain record of the breeding of the Northern Phalarope on the Atlantic coast, as is given in Bulletin 35,'Distribution and Migration of North American Shorebirds.'

A word may be added in regard to the breeding and winter ranges of the

several species treated in the bulletin. It was intended to make the report as complete as possible in this respect. It was being written at the time the third edition of the A. O. U. Check-List was in course of preparation, and both literature and museums were ransacked to make the statements on these two phases of the subject as complete and exact as time and pains could make them. While it is too much to expect that the bulletin is faultless in these regards, nevertheless in the year since it was published no one has pointed out any defects, and during that period of work on ornithological literature, Prof. Cooke has found no omissions that should have been included. No doubt, however, it will be found later that matter was omitted that might profitably have been utilized, and we shall be duly grateful to friends who will call our attention to such omissions with a view to greater completeness in future publications on the same general subject.

Turning now to the migration side of the question, an entirely different problem is presented. No claim is made or even suggested in the bulletin that the data presented on migration are full and complete. In fact the explicit statement is made that the dates of migration "have been obtained principally from the migration schedules" in the possession of the Biological Survey. Ornithological literature, especially of the present time, is too voluminous for one man, however industrious, to transcribe all the dates of occurrence. In the case of this particular bulletin another item had to be taken into account. The size of the bulletin was necessarily limited to 100 pages, and this allowed only a small part even of the migration data now in hand to be used. Our records contain 45,000 cards on the Limicolæ, alone, an amount of material far too great to be more than abstracted in a bulletin of such limited size.

The failure to include in the Shorebird bulletin the data from two such important works as the ones our critic mentions: 'Birds of Essex County' by Townsend, and 'Birds of the Cambridge Region' by Brewster, can neither be explained nor defended except on the ground of human liability to error. In a work which necessitates the consultation and transcription of such a vast amount of literature no author is liable long to escape criticism for errors and omissions, and we shall endeavor to accept our share of merited rebukes with becoming resignation.

In conclusion a word may be added in regard to the great mass of ornithological data now on file in the Biological Survey. This includes over 700,000 cards from all sources, migration schedules, specimens in museums and in the hands of private collectors, and references to ornithological literature. With regard to the last named source, Prof. Cooke has about completed the abstracting of the standard serials, the government publications, the various State lists, and has made considerable progress with the multitude of non-serial publications. He has consulted nearly every American reference quoted by Coues in his three bibliographies and more than twice as many in addition, the whole making more than 20,000 titles and considerably over 200,000 notes on species. It would seem that this amount of material, while confessedly by no means covering the entire

field, is worthy of a more complimentary statement than the one penned by our critic to the effect that the bulletin on Shorebirds contains in addition to official data "*some* reference to the literature."

Very truly yours,

HENRY W. HENSHAW.

Washington, D. C., Nov. 20, 1911.

Mathews's Notes on Nomenclature.

TO THE EDITOR OF 'THE AUK': —

Dear Sir: — In the last number of 'The Auk,' I have been granted an extended review of my notes on Nomenclature published in the Novit. Zool., Vol. XVII, pp. 492–503, Vol. XVIII, pp. 1–22, Emu, Vol. X, pp. 317–326, and Vol. XI, pp. 52–58. That review will be widely read by American ornithologists whereas my original papers will not have such an audience. Inasmuch, therefore, as I feel my views have been somewhat vigorously treated, I would claim space for a short defence of my papers.

The review is principally a defence of the Brissonian genera without recourse to the refutation of the facts I produced against their acceptation. I implicitly obey the "Laws formulated by the International Congress of Zoölogists," and the reviewer wrote: "Instead of accepting, however, the ruling of the Commission on the meaning of its own Code he proceeds to argue that the Commission is wrong"; and then: "It is hard to reconcile this action with his repeatedly professed absolute adherence to 'the laws formulated by the International Congress of Zoölogists.'"

The reviewer has confused the Laws with the Opinions rendered by the Commission. I have never questioned the Laws and "the Commission has no legislative power." Refer to Opinion 16, where after nine pages of discussion the only cases where an Opinion was necessary were left to be decided by the first author who had occasion to use them, and the sentence passed "If any author attempts to construe the cases under the present ruling the burden of proof to show he is justified in this procedure rests upon him."

However the reviewer further wrote: "As a matter of fact, it is perfectly evident that the Commission intentionally employed the term binary for the purpose of conserving genera established by non-binomial authors of dates subsequent to 1758," yet carefully refrained from noticing my appeal to the Laws which I here again quote:

"*Article 25.* The valid name of a genus or species can be only that name under which it was first designated on the condition:

"*a.* That this name was published and accompanied by an indication, or a definition or a description; and

"*b.* That the author has applied the principles of binary nomenclature.

"*Article 26.* The tenth edition of Linné's 'Systema naturæ,' 1758, is the work which inaugurated the consistent general application of the binary