

1. Two-year plumage-cycle (like *Larus philadelphia*):—*Xema sabini*, *Rhodostethia rosea*, *Larus minutus*, *Larus franklini*, *Larus atricilla*, *Rissa brevirostris* and *Rissa tridactyla*.

2. Three-year plumage-cycle:—*Larus heermanni*, *Larus canus*, *Larus brachyrhynchus*, *Larus delawarensis* and *Pagophila alba*.

3. Four-year plumage-cycle (like *Larus argentatus*):—*Larus californicus*, *Larus vegae*, *Larus affinis*, *Larus occidentalis*, *Larus schistisagus*, *Larus marinus*, *Larus nelsoni*, *Larus kumlieni*, *Larus glaucescens*, *Larus leucopterus* and *Larus hyperboreus*.

43 W. 70th Street, New York, N. Y.

THE SUBSPECIES OF BRANTA CANADENSIS (LINNAEUS)¹

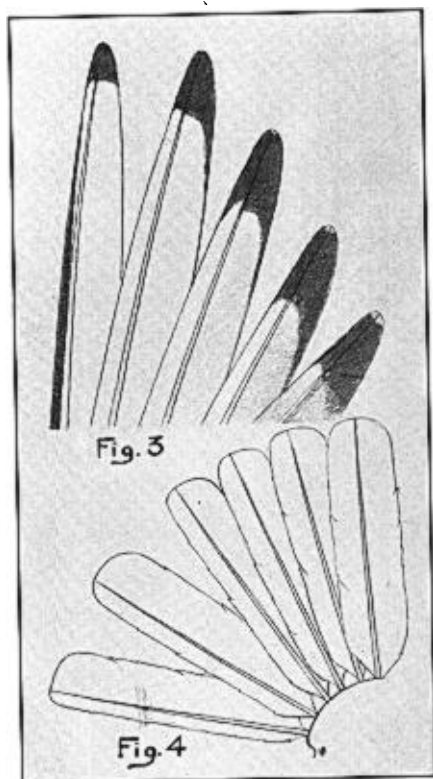
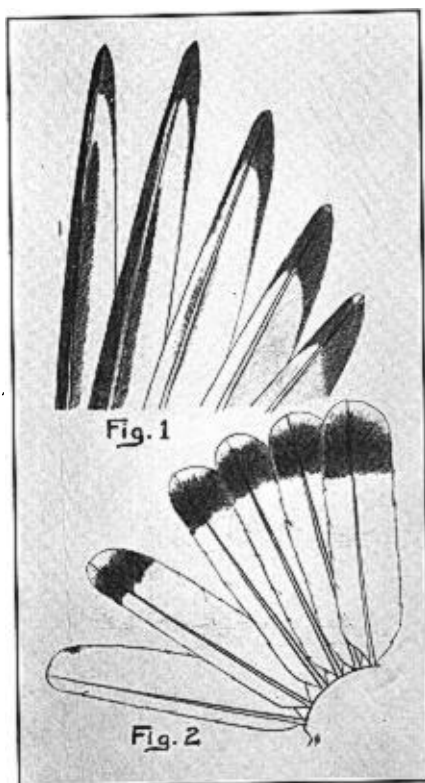
BY H. S. SWARTH

IN the January, 1920, issue of 'The Auk' (pp. 94-102) Mr. J. D. Figgins has a paper on "The Status of the Subspecific Races of *Branta canadensis*." This paper is devoted in large part to severe criticism of a publication of my own upon the same subject.² I could not possibly take exception to Mr. Figgins for differing from me in matters of opinion, nor for publishing his conclusions. I am, however, perfectly justified in feeling resentful at the ungracious wording of his argument. I object to such statements, for example, as that measurements I have taken are unreliable and that I have suppressed such measurements as did not answer my purpose. I object to having statements ascribed to me that I did not make. I object to having statements of mine "interpreted"—I do not think they need it.

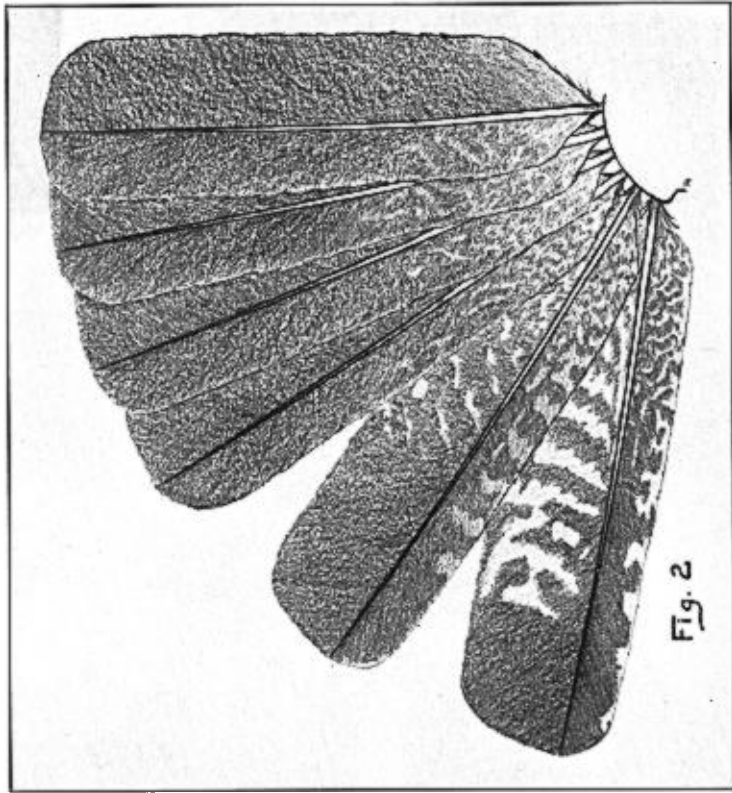
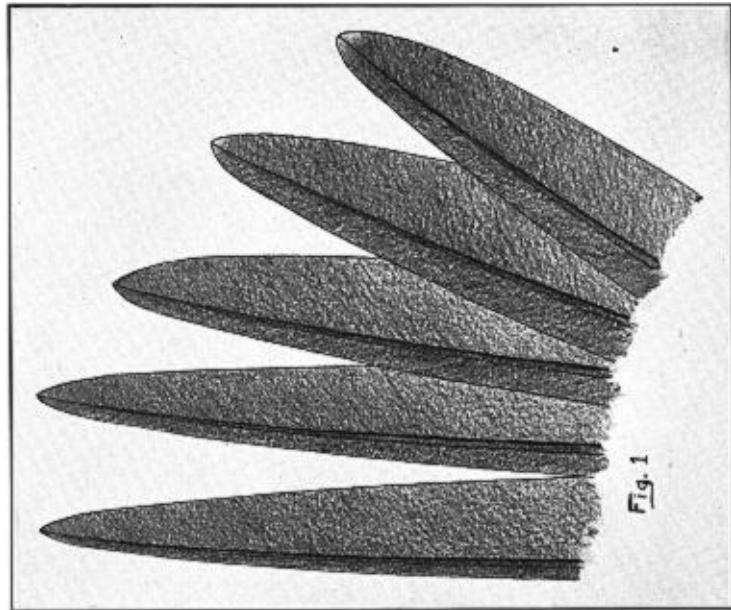
Before discussing in detail some of the statements he has made, it is best, perhaps, to give Mr. Figgins' conclusions, then some of my objections to them. He says finally: "It is, therefore, proposed that '*hutchinsi*' and '*occidentalis*' be eliminated as subspecific forms, that *minima* be raised to specific rank and that the occas-

¹Contribution from the Museum of Vertebrate Zoology of the University of California.

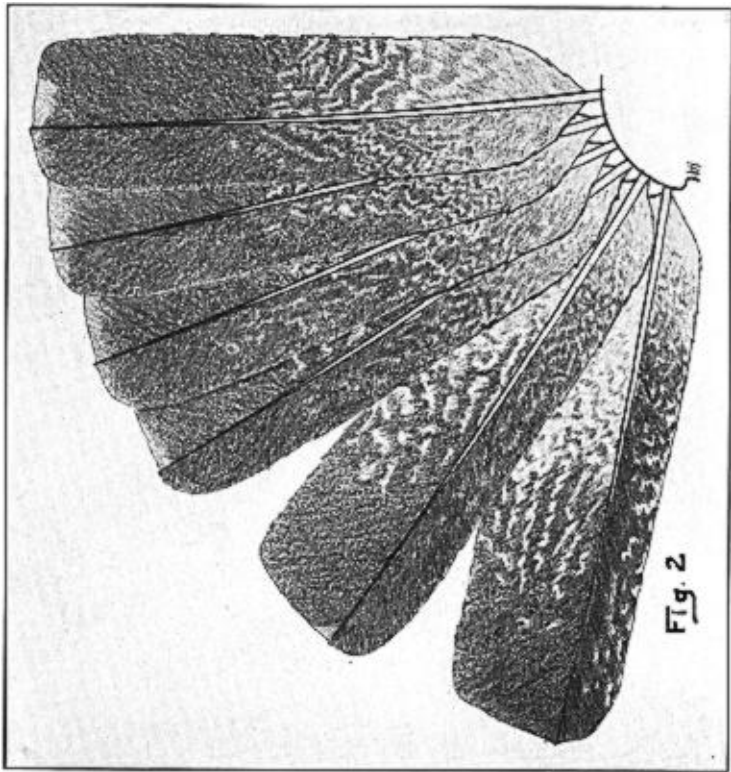
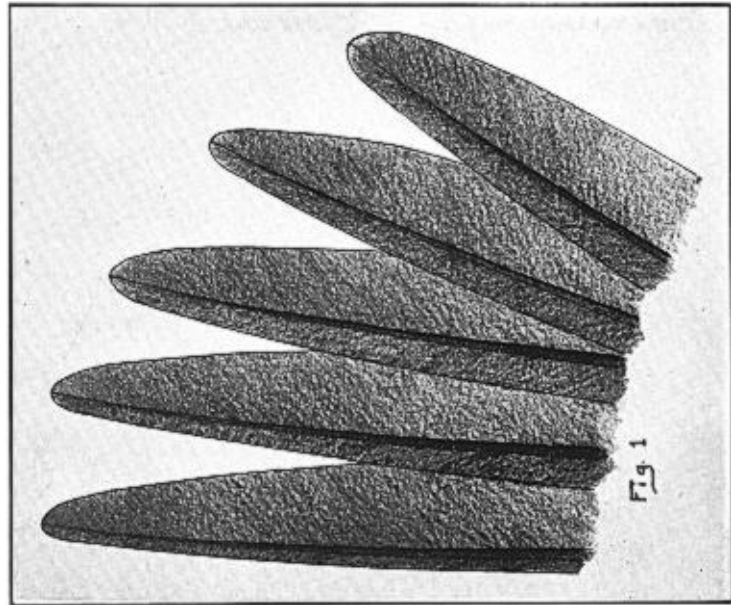
²A study of a collection of geese of the *Branta canadensis* group from the San Joaquin Valley, California. Univ. Calif. Publ. Zool., vol. 12, 1913, pp. 1-24, pls. 1-2, 8 text figs.



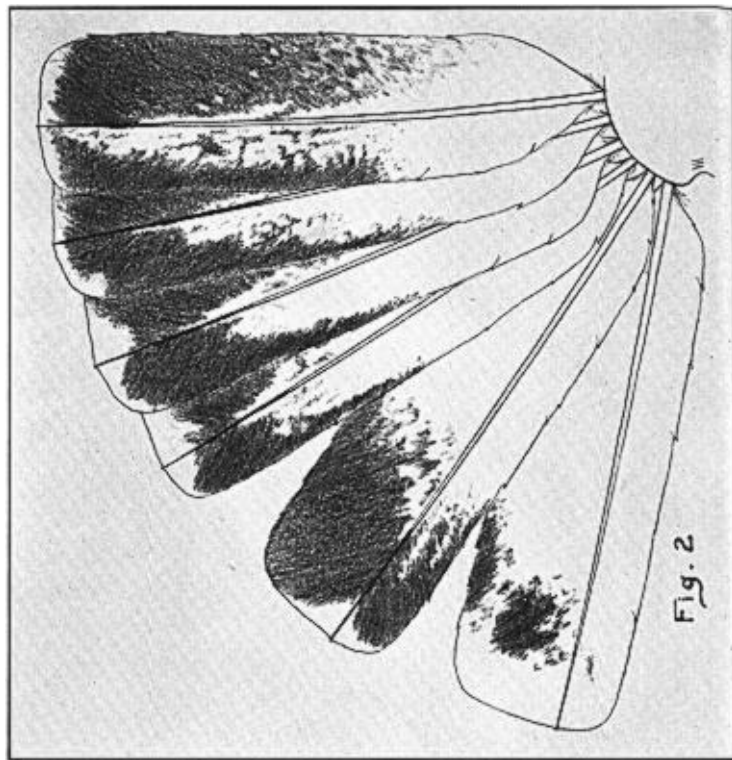
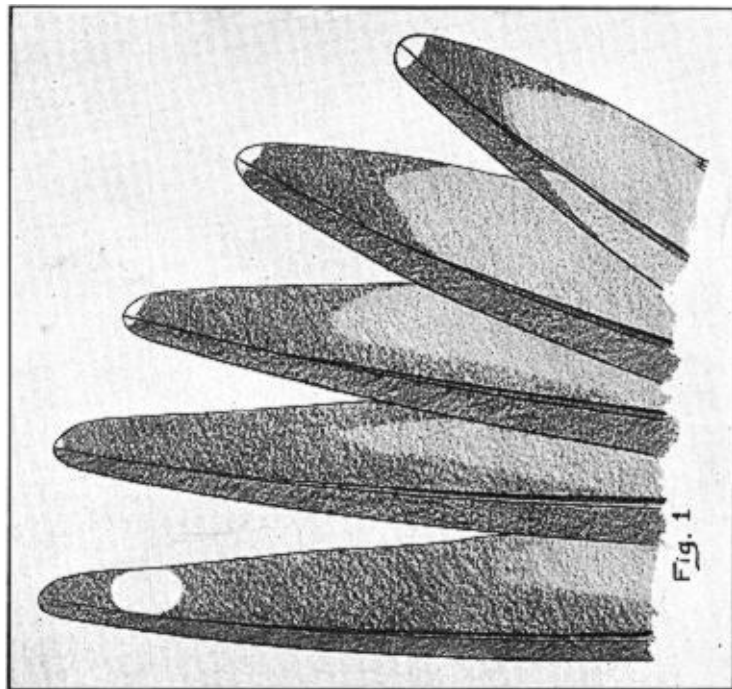
LARUS PHILADELPHIA



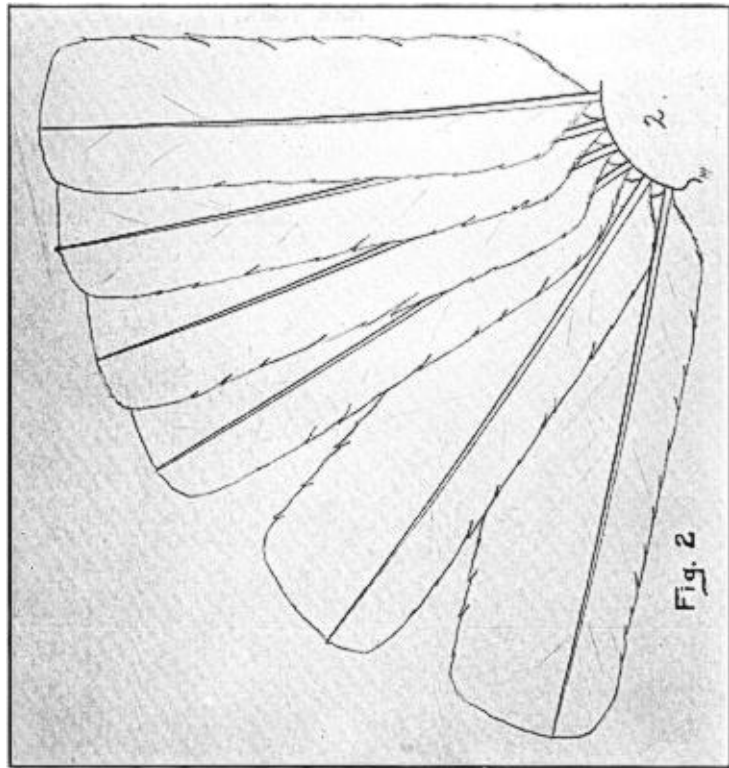
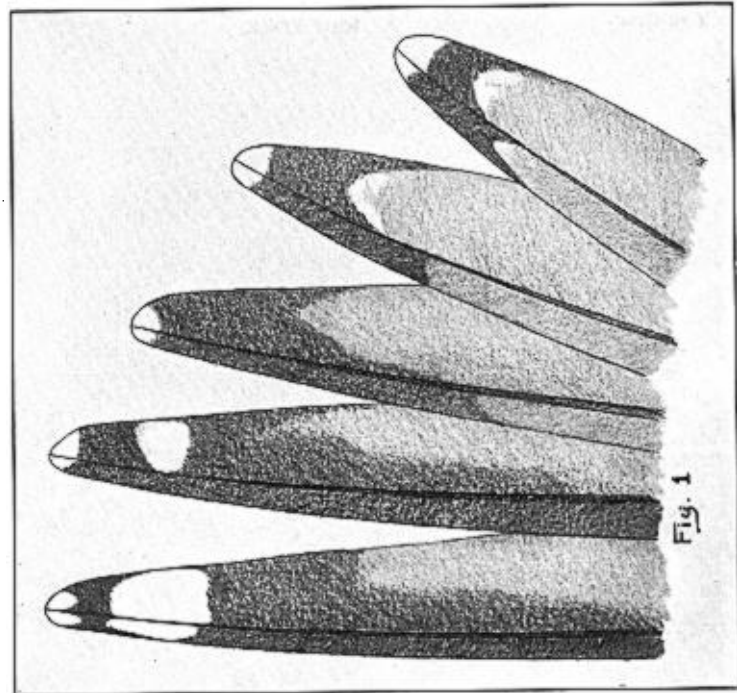
LARUS ARGENTATUS. FIRST YEAR (JUVENAL).



LARUS ARGENTATUS, SECOND YEAR



LARUS ARGENTATUS. (THIRD YEAR).



LARUS ARGENTATUS. (FOURTH YEAR).

ional 'inextricable' examples be recognized as hybrids." Presumably *Branta canadensis* is also to be considered as a species, though he does not say so. At any rate it will be necessary to do so, to supply a second parent for his hybrids.

Now to come to details. "On page three of 'A Study of a Collection of Geese . . .' Swarth states, in a discussion of thirty-six specimens considered as *hutchinsi*, 'twenty-five are males.' Without an explanation of his reasons, he employs but ten of that sex as representative of the differences he describes on page fourteen. It is, therefore, not unreasonable to conclude that the differences he finds in the minimum and maximum measurements of wing, culmen and tarsus, as compared with the findings of other writers, may be due to the elimination of the remaining fifteen males belonging to the series." In plain language, I am accused of juggling the measurements taken to make them accord with my own preconceived ideas.

Mr. Figgins' premise is a false one. The diagnoses of the subspecies given on pages 14-15 are based on *all* the specimens examined. The summaries of measurements (pp. 14-15) are from a limited number (ten in the case of *hutchinsi*), but on pages 16-18 he will find the important measurements of *all* the specimens, all, that is, except a very few that were defective so as not to permit of accurate measurement of one part or another. Furthermore, the extremes as given in the summaries on pages 14-15 are the extremes of all the measurements taken, not from a limited selection. An ordinarily careful reading of my paper would have shown this.

Then, in connection with the subspecies *occidentalis*: "Swarth's contention for a difference in size when compared with *canadensis* is not convincing" (Figgins, l. c., p. 98). My "contention" was that "the maximum of *occidentalis* is below the largest *canadensis*" (Swarth, l. c., p. 7). I did not assert that the size difference between the two was diagnostic. Mr. Figgins has not shown my statement to be erroneous. Then: "The present writer interprets Swarth's description of *occidentalis* as an attempt to justify the continuance of this variation as a subspecies by crediting it as being a more or less resident form inhabiting the Pacific Coast . . .," etc. (Figgins, l. c., p. 98). I think I do not need Mr. Figgins to "interpret" my statements, and I resent an "inter-

pretation" that claims to show an unworthy motive. I stated, in language that seems to me perfectly explicit (Swarth, l. c., p. 10), my belief that *occidentalis* is a recognizable subspecies. I have had no reason since to change my mind.

Another quotation: "Swarth shows that '*hutchinsi*' attains its greatest abundance on the Pacific coast" (Figgins, l. c., p. 101). What I really said was that in California in winter we find "vast numbers of typical *minima*, a lesser number of intergrades, and comparatively few typical *hutchinsi*" (Swarth, l. c., p. 3). One feels rather helpless when he finds his opponent ascribing to him statements exactly the opposite of what he did say. The only assumption permitted me is that Mr. Figgins read my paper too carelessly to judge its contents.

So much for the personal side of the matter, though there are other statements, too, to which I might well take exception. Now, for Mr. Figgins' conclusions, especially as regards the subspecies *occidentalis*.

In the first place, there is no evidence in his paper that he examined a single example of *occidentalis*. If he had any specimens at hand from the coast of southeastern Alaska he does not say so. If he did have, and if he could compare geese from that region with Canada Geese from the interior of the United States and still not appreciate the differences in color, there is nothing more to be said on that score. Others can distinguish these differences without difficulty.

Then, Mr. Figgins confuses two entirely different problems, the characters of the subspecies that inhabits the northwest coast, and the name that should be applied to the race. His argument that some of the characters first ascribed to the subspecies are unreliable is, of course, nothing new and of no importance now that the more stable characters are better understood. The fact that the type specimen of *occidentalis* is not representative of the mode of that subspecies, as now defined, is obviously no reason why the form should not be recognized. I consequently fail to understand why my detailed description of this type specimen "would appear to effectually dispose of *occidentalis* as a subspecific variety" (Figgins, l. c., p. 98).

Mr. Figgins says: "The statement [by Swarth] that 'Of the Alaskan series the Prince William Sound birds are smaller and

darker than those of the Sitkan district . . . ' points rather conclusively to gradation through hybridism." His wording is obscure, but let that pass. Then: "The literature dealing with the distribution of the genus *Branta* fails to take into account the region lying between Prince William Sound and Bering Sea. . . A large part of this territory is ideal breeding ground and to the present writer's personal knowledge, examples of *Branta* are found there in considerable numbers during July, August and September, although no specimens were taken. There are no land barriers that would prohibit these birds crossing from Prince William Sound to Cook Inlet and hence it is not unreasonable to expect that *minima* and *canadensis* and Baird's so-called *occidentalis* interbreed and hence the 'variations' and specimens that intergrade 'inextricably'" (Figgins, l. c., p. 98).

"No specimens were taken!" Yet we are expected to accept as proof of the existence of an extraordinary condition his statement (which I will not deny) of the mere fact that geese are abundant in certain parts of Alaska. It would require the collection of a large series of skins, and the most careful analysis of their peculiarities and of the circumstances under which the birds were taken to carry conviction of the truth of the statement that is made so airily. "Hybridism" has been much used of late to explain things that seem obscure. Mr. Figgins uses the term repeatedly. It is an easy way to wave difficulties aside, but it is an exceedingly difficult thing to prove. Of course on questioning the theory of "hybridism" on a large scale we at once have the Flickers (*Colaptes*) pointed out in triumphant proof, but it may be said that even among these variable woodpeckers there are a great many cases of peculiarities, in color at least, that can not be explained by that theory.

"It appears to be established by several authorities that the breeding range of the representatives of the genus *Branta* overlap, and it is the present writer's belief that *hutchinsi* is a hybrid intergrade between *canadensis* and *minima*" (Figgins, l. c., p. 101). Here again Mr. Figgins' premise is wrong. There are very few explicit statements of the subspecific character of geese found breeding in the far north. Most observers followed the same course as Mr. Figgins—they saw plenty of geese but "no specimens were taken."

Of course there are "intergrades" in collections—many of them. Is that not one of our tests for subspecies? It is my own main reason for regarding the four forms, *canadensis*, *occidentalis*, *hutchinsi* and *minima*, as subspecies of the one species, *Branta canadensis*.

In the foregoing discussion my comments have pertained mainly to the subspecies *occidentalis*, but Mr. Figgins' contentions regarding *hutchinsi* are, I believe, just as much open to criticism. I submit that Mr. Figgins has not proved his points. Furthermore he has not described his Mississippi Valley specimens sufficiently explicitly to enable anyone else to form an opinion regarding them, nor, for that matter, to know just what Mr. Figgins himself thinks of any particular one.

I hope it is not necessary for me to say that I do not regard my previously published paper on the races of *Branta canadensis* as the last word on the subject. In one respect I admit that it would be difficult to make me change my view—in regard to *Branta c. occidentalis*. I have handled enough specimens of that race, in the field and in the museum, to be fully satisfied of its distinctiveness as a subspecies of *Branta canadensis*, whatever name we may eventually apply to the form. Of *hutchinsi* and *minima*, breeding birds from many points and a study of breeding conditions are admittedly necessary to a full understanding of their status.

I may say that my own views upon this subject have been criticised before. Once, at least, in print, by Brooks (Condor, XVI, 1914, p. 123), and in letters to me by others. In each case, however, the suggested correction was the recognition as species of forms that I regard as subspecies. I still think that, in the lack of sufficient breeding birds of certain of the races, my method of treatment, which is the same as that in the A. O. U. 'Check-list,' is the most reasonable course to follow. That is, to regard *Branta canadensis* as a variable species, divided into four recognizable subspecies, *canadensis*, *occidentalis*, *hutchinsi*, and *minima*.

Museum of Vertebrate Zoology, Berkeley, California.