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CORRESPONDENCE.

Concerning a Certain Tendency in Systematic Ornithology.

EDITOR OF 'THE AUK':

The more I think of it, the more dangerous appears to me to be the stand of those few who would assign to an extreme of one subspecies or species (an individual from within the breeding range of that form as typically represented by the mean) the name of an essentially different subspecies or species which that individual happens to resemble.

To illustrate, Dr. Dwight in his recent essay on the Genus *Junco* (Bull. Amer. Mus. Nat. Hist., vol. XXXVIII, 1918, pp. 269–309, 5 text-figs. [maps], pls. XI–XIII), cites (p. 293) the case of a series of breeding juncos, one hundred males, all from one locality in the Sierra Nevada of central California. He finds in this series, with regard to one character, color, variations which lead him to refer about seventy-five percent (with pinkish brown backs) to *thurberi*, fifteen percent (with browner backs) to "*couesi*," and a smaller percentage (with deeply ruddy backs) to *oregonus*. Of course, as pointed out by him, there are further variations, and also these categories are not sharply demarked. Dwight says (p. 294): "I do not see how we can escape the necessity of calling a specimen *oregonus* or

thurberi, or any other name, if it shows the characters of the form, no matter where it is taken."

Do not my readers immediately see, with me, the extreme danger into which the spread of this conviction will inevitably lead our science? What will be the value of subspecific determinations by Dwight, Bishop and the others of like mind, in accurate studies of migration and of distribution in general? Can they be used at all, without incurring the risk of making wholly incorrect inductions? If such practice becomes universal, wherein could there be any further use at all for recognizing subspecies and slightly differentiated species? Would we not have to restrict ourselves to dealing with simply black-headed juncos, slate-colored juncos, and gray-headed juncos, or, safer yet, with just *juncos*?

The rational employment of the subspecific concept as different from the specific one requires the exercise of judgment based on experience — just such as is needed in any other advanced field of knowledge. Furthermore, the essential factor involved in the use of trinomials (as designating subspecies as distinguished from species) is variation. After years of study on the part of scores of systematists in ornithology and mammalogy, there are admitted by all, I believe, but two criteria for use of the trinomial: (1) relatively small degree of difference, and (2) the fact of intergradation either through individual variation (as in insular races) or through geographical blending, where the ranges are continuous. Intergradation has always been, among the greatest number of vertebrate systematists, the basis for the use of the subspecies concept, and it should continue so to be. Now, the existence of normal fluctuational variation in two forms means that there *has* to be overlapping where the means are sufficiently close together; in other words, intergradation occurs, and the convergent extremes will be alike. In any case, if we take a considerable number of representatives of an animal which is subject to geographic differentiation, from one locality, and another lot from another locality, in a separate area of differentiation, and plot graphically their different characters separately, which is essentially what Dwight has done with color in the Genus *Junco*, we find that some of the specimens fall together, as demonstrated by him in this particular case; but who, until now, would think of calling such individuals as fall in the small area of coincidence of the polygons by other than the name of the race to which they geographically and *genetically* belong!

I insist, Dwight's repeated assertions to the contrary notwithstanding, that we simply *must* consider locality inhabited as one of the most important characters possessed by a species or subspecies. Otherwise, our efforts to classify specimens as to species and subspecies are liable to be worthless. From time immemorial "habitat" has been included as one of the first and most important diagnostic characters of a species. Why begin to disregard it now!

The main object of classification, from top to bottom, is to express *genetic relationship*, irrespective of superficial resemblances or such as may

obtain in the normal behavior of fluctuational variation. This is, as everyone knows, a formidable problem, one that is likely never to be solved to our complete satisfaction because of some of just those difficulties that Dwight complains of throughout the paper cited. But we are going to approach far closer to the ideal than the present stage—provided the work of the open-minded, painstaking yet optimistic student continues to dominate the field.

JOSEPH GRINNELL.

California Museum of Vertebrate Zoölogy,
July 9, 1918.

A Correction.

EDITOR OF 'THE AUK':

My thanks are due to Mr. Alexander Wetmore, not alone for the pleasure, shared with other readers of 'The Auk,' in perusing his valuable contribution on 'The Birds of Desecheo Island, Porto Rico,' but also for having therein called my attention to a hitherto overlooked slip of the pen in my article 'A Day on De Cicheo Island' (Oölogist, 1900), whereby (page 117, second paragraph), I referred to the "Sooty Tern" instead of to the Noddy, as should have been the case. This error certainly requires correction, even at this late date.

Of course the character of the slip is at once apparent on referring to my paper on the 'Birds of Porto Rico' (Auk, 1902-03), wherein (1902, pages 357-358) the Sooty Tern is correctly recorded as noted only on Mona Island, the Bridled Tern and Noddy, however, having been noted on both Mona and Desecheo Islands.

B. S. BOWDISH.

Newark, N. J., July 11, 1918.

NOTES AND NEWS.

ALL readers of 'The Auk' are familiar with the changes which are continually being proposed in the technical names of our birds and are doubtless reminded of the old saying that 'A rose by any other name will smell as sweet.' Those actively interested in nomenclature know that many of these proposed changes, as well as similar ones in other branches of zoölogy and botany, are necessary in order to conform to the rules adopted to bring about uniformity in scientific nomenclature. What strides have been made toward uniformity and stability in bird names under these rules may be realized by comparing the 'Hand-List' of the B. O. U. and the A. O. U. 'Check-List' (cf. Auk, 1915, p. 243).

Other proposed changes involving the acceptance or rejection of newly described races, subdivisions of genera etc., depend upon individual opinion and can only be decided by an authoritative list prepared by a committee



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