

The Criterion for the Trinomial.—Objections may well be raised to many of the late proposals of Oberholser that closely allied species be reduced to subspecific rank. A case in point is brought up, and disposed of with vigorous show of finality, in 'The Auk' for January, 1921 (pp. 80-82). The two Cranes, the Little Brown and the Sandhill, *Grus canadensis* and *G. mexicana*, are thrown together as subspecies of one species on the ground that "the writer [Oberholser] has examined and measured a large number of these birds, and the results obtained show that while typical specimens, and in fact the majority, are readily assignable to one form or the other, the measurements . . . completely inoscillate . . ." "Therefore," says Oberholser, "specific distinction cannot be maintained."

Granted the criterion, for the employment of the trinomial, of intergradation through individual variation: What *were* the figures assembled? And, furthermore, what relation did the extremes bear to age and sex as well as to geography? In this case of the cranes, where size is the chief or only character, were the "intergrades" simply small first-year birds, or were they really comparable adult individual variants?

In this connection I would call attention to some actual measurements which have been given, by Swarth ('Condor' XXI, 1919, pp. 212-213) and by Mailliard ('Condor', XXIII, 1921, pp. 30-31). There was a distinct hiatus between the largest *canadensis* examined by these men and the smallest *mexicana*. Hence, as the latter author rightly insists, they should still be considered distinct species—until proper evidence, is brought forward to the contrary. This, I contend, has not been done, even now.

A question arises as to whether an obvious "sport", a runt, say, in *mexicana* because size is the special character in the present case, should be counted as a valid intergrade. Such specimens fall outside of the polygon of normal variation in the species and, despite the claims of some mutationists, it is questionable if such aberrancies figure at all in the process of species-evolution in the wild. In other words they may have no phylogenetic significance whatever. It is important, then, that any collection of specimens representing two or more near-related forms, should be looked at critically, from various angles, before drawing conclusions. The results of hasty scanning may be wrong.

Particularly grievous are the cases involving Old World and New World forms, closely similar to be sure, but almost or quite universally, up till now, handled as binomials. I sincerely hope that the A. O. U. Committee on nomenclature will subject each one of these cases to searching inquiry, on the basis of specimens determined to be fully adult, sex as well as age being considered also. The criterion for the trinomial must *not* be closeness in general appearance, but it must be *intergradation*, either by way of geographic blending, or by way of individual variation (if this form of intergradation be insisted upon), *determined strictly as such*. If intergradation through the characters of subadults or of juvenals were generally

adopted as the subspecific criterion, what a lot of changes we would be in for. Think of the opportunities among the Empidonaces, and the gulls!

Oberholser (*loco citato*, p. 79) implies that because a form is clearly a "geographic race," this consideration alone is a reason for employment of the trinomial. Is it necessary for him to be reminded that according to widely held current belief the great majority, if not all, of the lesser differentiated species, among the higher vertebrate animals, are but the results of geographic variation and isolation? There may be species of hybrid origin, but if so, they are relatively rare. Geography, the evolution of habitats through time, has been the *sine qua non* of vertebrate speciation: Very many *good* species are "merely" geographic variants.

The subspecies concept will fall, just as some few people devoutly hope it will (and we will get back to pure binomials for every form recognizable at all), if it fails to be used on a consistently definite basis. Of course there is no real phylogenetic difference between a species and a subspecies. Degree of difference is a subjective matter; and the only criterion left is that of intergradation, *actually known to exist*.—J. GRINNELL, *Museum of Vertebrate Zoology, Berkeley, California*.

RECENT LITERATURE

Beebe's 'A Monograph of the Pheasants.'—In November, 1918, appeared the first volume of Mr. William Beebe's 'Monograph of the Pheasants' which was reviewed in 'The Auk' for January, 1919. Now after a lapse of two and a half years the second volume is before us and we are informed by the New York Zoological Society, under whose auspices the work is being published, that they expect to deliver the two remaining volumes during 1922. Considering the complications in printing and publishing that we have had to face, during the past few years, the progress of this work has been most commendable.

Volume II maintains the same high standard that was set by its pre-

¹ A Monograph of the | Pheasants. | By | William Beebe, Curator of Birds of the New York Zoological Park; Fellow of the New York Zoological | Society and Director of the Tropical Research Station in British Guiana; Fellow | of the American Ornithologists Union and of the New York Academy | of Sciences; Member of the British Ornithologists, Union; | Corresponding Member of the Zoological | Society of London, etc. | In Four Volumes | Volume II. | Published under the auspices of the | New York Zoological Society by | H. F. and G. Witherby. | 326 High Holborn, London, England, | 1921. Royal Quarto. (12 × 16 in.) pp. I-xv + 1-269, 24 colored plates, 24 photogravures and 5 maps. Edition limited to 600 copies: price of each volume \$62.50.