

matter would be crowded out for some time to come if we attempted to print them. Every contributor seems to have different ideas upon the subject and as there is no definite basis to work from the discussion while interesting fails to reach any result.

As a comment upon Mr Lloyd's remarks on the legal side of the question we might say that in a case in the Philadelphia courts some years ago the judge would accept only Warren's 'Birds of Pennsylvania' as authority for names, and the accidental use of "Cardinal Grosbeak" on a plate in that work was all that prevented his agreement with the lawyer's contention that a "Cardinal" was not a Grosbeak nor a Finch and therefore not protected by law. Legal Ornithology seems to be a science in itself!—Ed.]

Species and Subspecies.

EDITOR OF 'THE AUK':

In the July number of 'The Auk' (1921, p. 482), some criticisms are made upon a letter entitled "The Last Phase of the Subspecies" written by Mr. C. Mackworth Praed and myself in the April number of 'The Ibis', (1921, pp. 344-347), in answer to a letter emanating from Mr. Loomis.

In writing this letter Mr Praed and I rather set ourselves to provoke criticism and a discussion of this vexed question. It was not, however, our desire to be misquoted and I, personally, hold the opinion that however desirable criticism may be, it, at least, behoves the critic to read carefully what he sets out to criticise.

Your reviewer states, for instance, "These gentlemen seem to regard the species and subspecies as two different things." As this is precisely and essentially the reverse of what, at any rate, are my own views, I may perhaps be excused for quoting what we did say, especially as it was deliberately said in order to make the matter quite definitely plain.

"On the other hand, to those who accept *in toto* the Darwinian theory or what is commonly accepted as the Darwinian theory, and all that it implies, all subspecies will appear of great value as "incipient species." Now, for our part, it has always been a matter of the greatest difficulty to imagine how a geographical form *which in fact is already a species* can be termed an incipient one" (*italics mine*).

We then went on, after a slight digression, to repeat the generally accepted view that there would appear to be two main forms of variation, one known as a discontinuous or mutational and heritable form and the other as a fluctuating environmental and non-heritable form; and finally we expressed the opinion that subspecies could be referred to one or the other of these two forms of variation, incidentally leaving the reader to infer that subspecies as nowadays recognised by ornithologists could, in reality, be classified under two categories possessing different values or rank.

Subspecies are, in other words, nothing more than local varieties of species manifested under the guise of, at least, two distinct kinds of variation.

Perhaps I might be allowed to give two concrete examples. In the Bermudas, the Goldfinch (*Carduelis parva*) would appear to have established itself somewhere about the year 1875, cage-birds having apparently been introduced either from the Canaries, Azores or Europe. It would now appear to be of a darker coloration on the upper parts than typical examples and for this reason has been separated by Mr. Kennedy under the name *C. parva bermudianus*.

In point of fact it is "as good a subspecies" as scores of others now recognised; but my point is that this darker coloration is almost certainly due to a purely somatic quantitative or superficial chemical change in the feathers the result of the direct influence of environment and would not be inherited. To put it crudely, it is probably a little richer owing to the increased vigour of the bird in its new congenial surroundings. If the bird were to be re-transported to its original habitat it is safe to say that with the advent of its next crop of contour feathers the coloration would revert to its former tone.

In these respects, this type of subspecies differs fundamentally from, let us say, the Great-crested Grebe of Africa in which we get a qualitative colour-pattern character which differentiates it from the European Great Crested Grebe.

In the latter (*P. c. cristatus*) we have the white eye-stripe continued well backwards beyond the eye while in the former (*P. c. infuscatus*) it abruptly stops as it reaches the eye (cf. illustration Ibis 1915, p. 50).

Could anyone, I venture to ask, familiar with the elementary principles and facts of heredity imagine for a moment that the two cases I have quoted represent comparable examples of variation? Would anyone familiar with the trend of modern research in the direction of the secrets of heredity maintain that the difference in colour pattern between the two forms of grebe just quoted was anything else than a discontinuous variation due to some factor, present or absent, in the gamete, or, on the other hand, would they maintain that it was due to the direct action of environmental circumstances or that it was not a heritable character which owed its origin rather to the mere fact of the isolation of the African and European grebe populations, which isolation permitted, within limits, a free and independent career to any harmless variations which might arise in the fertilised ovum. Assuredly no, I think. Yet both the Bermudan Goldfinch and the African form of Great Crested Grebe are writ down by ornithologists as subspecies with nothing to indicate that as variants from the species they are fundamentally different in character.

In connection with our opinion expressed in regard to the superficial characters of a large proportion of present day subspecies (environmental as opposed to germinal) your reviewer quotes Professor Sumner's ex-

periments with mice in California, where dark coloured forms have bred true ever since they were transferred to a desert environment. What does this prove? surely nothing more than that the dark coloration of these mice was due to a factorial character resident in the chromosomes and was of a fundamentally different character to the superimposed darkness in the case of the Bermudan Goldfinch. I would quote another comparable case, which indirectly serves to illustrate this point.

The normally coloured form of Honey Creeper (*Coereba saccharina* of Lawrence) which used to be common on the islands of St. Vincent and Grenada in the West Indies has within very recent years been practically replaced by its melanistic variation which has received two distinct names, *C. striata* and *C. wellsii*, corresponding to the two islands in which it is found (cf. Austin Clarke 'Auk' XXII p. 392). That this melanistic variation of *C. saccharina* does not owe its origin to any form of direct environmental influence would I think be certainly allowed by authorities far better qualified to speak of it than I am and an additional indication is I think found in the fact that I discovered similar black forms of *C. luteola* on the islands of the Testigos Group where the conditions are very dry and arid and exactly the reverse of those met with in St. Vincent and Grenada.

I should be extremely surprised if Prof. Sumner were to introduce these black forms of *Coereba* into his desert Californian regions and find them assuming isabelline tones and moreover I am not in the least surprised that his mice failed to do so although your reviewer evidently expected us to be.

Finally our critic states that Dr. Joseph Grinnell on a previous page of the same number of 'The Auk' sums up the matter concisely when he says "There is no phylogenetic difference between the species and the subspecies." How he could conceivably have said that there was any difference is difficult to imagine, but I should hardly have thought the statement, true as it is, was worth quoting or that it sums up (as your critic states) the very difficult problem with which ornithologists are dealing.

Yours very faithfully

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[As we are responsible for all of the unsigned reviews of periodical literature in 'The Auk' we hasten to assure Mr. Lowe that we most certainly had no intention of misquoting him. We did, it seems, misunderstand him since he assures us that he does not regard species and subspecies as two different things. Our own contention is that the several subspecies (geographic races) collectively form the species and that when the intergradations between them die out each one thereby becomes a species. Intergradation has always been our criterion in systematic ornithology

for deciding whether a certain form should be given a trinomial or binomial appellation, and the only "difference" between the species and subspecies has been the existence or absence of intergradation.

Mr. Lowe in making a claim for two *kinds* of subspecies which apparently cannot be told apart until extensive breeding experiments have been carried on, is carrying the matter much farther, but it is obvious that until such experiments are carried on and until a new standard is established, intergradation must needs be our only practical criterion for the use of a trinomial name. I should, personally, however, be loath to think that there was any difference in the character of the differentiation that he cites in the case of the Bermudan Goldfinch and in Prof. Sumner's mice except one of degree.—Ed.]

NOTES AND NEWS

DR. THEOBALD JOHANNES KRÜPER, a Corresponding Fellow of the American Ornithologists' Union since 1884, died in Athens, Greece, Mar. 23, 1921. He was born at Uckermünde in Pomerania, Germany, June 30, 1829, and at the time of his death was within a few months of his 92nd birthday. For nearly half a century, since 1852, he had been Conservator and Director of the University Museum at Athens. In his younger days Dr. Krüper was an energetic traveller and field naturalist visiting many places in northern and southern Europe in quest of natural history material. Nearly every year from 1855 to 1877 he visited some interesting region as shown by the following brief summary of his trips: 1855 Lapland, 1856 Iceland, 1857 Gottland, 1858 Iowian Islands and Acarnania, Greece, 1859, 1865-66, 1873, 1877 Mt. Parnassus, 1860 the Taygetus Mountains in Peloponnesus, 1861 Mt. Velukhi in Eurytania, 1862 the Cyclades, 1863-64 Asia Minor, 1867 Attica, 1868 Germany, 1869 Macedonia, 1870 Mt. Olympus, 1871-72 Smyrna, 1875 the Balkans, 1876 northern Peloponnesus, and 1877 Aegina.

He published a number of short papers on the birds observed during his travels chiefly in 'Naumannia' and the 'Journal für Ornithologie.' His doctorate thesis, in Latin, was on the subject of 'The Geographic Distribution of the Falconidae of Europe', and his first published note in 'Naumannia', in 1852, was entitled 'Die Adler Pommerns.' Among his more important papers were his 'Ornithologische Notizen über Griechenland,' 1862 and 'Beitrag zur Ornithologie Klein Asiens, 1875, both of which appeared in the 'J. f. O.', and 'Zeiten des Gehens und Kommens und des Brutens der Vogel in Griechenland und Iowen', 1875. His memory is recalled by the specific designation of a Nuthatch (*Sitta krueperi*) from Smyrna, named in his honor by Pelzeln in 1863 in recognition of his explorations in Asia Minor. Dr. Krüper's work in Greece