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## CORRESPONDENCE.

### Scientific Nomenclature.

Editor of 'The Auk':

Ornithological nomenclature is again passing through a crisis, as in the period between 1870 and 1890. The dispute which then broke out between binomialists and trinomialists is now completely settled in favor of the latter. This was the most important step, since Linnaeus' great achieve-

ment of organization, toward bringing our ornithological nomenclature into accord with our knowledge of the genetic relations of the forms with one another. But there came a halt, half-way, because examples became known in ever greater number, of two very similar forms living together in the same district without interbreeding. Thus it was demonstrated that similarity alone must not be wrongly assumed to show that two forms are subspecies of a single species. And at first, to eliminate confusing errors, great caution had to be exercised in the use of trinomial names. J. A. Allen found a very happy formula when, in close dependence on Charles Darwin, he proposed to consider intergradation as the touchstone of trinomialism.

Since then the systematic side of ornithology has not remained stationary. Through its close bond with ecology it has become more and more a biological science, and has reached today a height from which the problem of the origin of species can be surveyed far better than was possible in the time of Charles Darwin. To this progress our nomenclature must also adapt itself, unless we are willing to renounce giving it any scientific significance.

We Europeans are accustomed to regard America as the land of progress, and American ornithologists have given us many proofs for the justification of this view. In the matter of nomenclature, however, according to our view, a certain stagnation has set in over there; and we think that we have meanwhile gained a lead in the development. What Dr. Chapman says in his very interesting and very timely discussion of the 'Criteria for the Determination of Subspecies' may be looked upon in America by many ornithologists as a revolutionary attempt to shake the foundations of classification, for as some utterances of well-known systematists show (I am thinking especially of Dr. J. Grinnell's remarks in 'The Auk,' XXVII, 1921, pp. 469, 470), they still look upon the decisions of the A. O. U. Code of 1886 as a *noli me tangere.*

In Europe, however, and especially in Germany, this article of Dr. Chapman's will not provoke any objections whatsoever. The days when intergradation was required as a criterion of the trinomial, either by way of geographic blending or by way of individual variation, are past over here; and a much freer conception has almost universally taken its place. The thorough work on the birds of the Palaearctic region from the pen of Hartert gives evidence of this on almost every page; and it shows in the most convincing manner that the principles by which the adherents of the new order proceed are sound, and that they advance the cause of science efficiently. In its childhood trinomial nomenclature had need of the leading-strings of intergradation, lest it stumble into the many snares of heterogeneous resemblance; but now that it is full-grown it ought to have the courage to jump over obstacles as Dr. Chapman has recommended. The question is however: How far dare one go without introducing too much uncertainty in nomenclature? To draw hard and fast lines for this would be to force Nature herself into a new Procrustean bed.

One of the most acute ornithologists of our time, Dr O. Kleinschmidt, has been led through his researches on subspecies to the view that the species is a sharply defined entity, which may be highly variable under the influence of external and internal factors, without these variations bearing in themselves the germ of new independent species. He rejects the Darwinian theory of evolution, and takes his stand again on the pre-Darwinian idea of fixity. According to him, even two very similar species, such as *Rhamphastos ambiguus* and *R. abbreviatus*, have not originated from a common stem, but have taken parallel courses of development from the beginning. His conviction allows him to be strictly consistent in nomenclature. The task of the systematic student is then only to list fully the species—or as Kleinschmidt expresses it, the natural groups (Realgattungen)—with all their races. For thirty years all Kleinschmidt's studies have been directed towards erecting and supporting this theory of the "Formenkreis," or group of representative forms. In one comprehensive species (Realgattung) he unites all that ornithologists have customarily regarded as representative species. Thus in the same broad species with the European Goshawk he includes not only all the Eurasian and Nearctic races of *Accipiter gentilis*, but also *Accipiter hensti* of Madagascar, *Accipiter melanoleucos* of Africa, and *Accipiter planes* of New Britain —forms which in part are very unlike *Accipiter gentilis* in their adult plumage.

This method of research has proved to be very fruitful. It necessitates a very careful comparison of all similar forms in morphological as well as biological respects, and results in ascertaining the essential, truly specific characters of a given species. Furthermore, it has become evident from these investigations that as a rule the gaps between natural species are in no wise lessened, even though their conception is made very broad. Specificity is therefore extremely ancient; and if one uses critical judgment, it is rare to find any sure indication of a transition from the race to the species. That such intermediate cases do occur seems however to result from the fact that the sexual affinity between representative forms may be very different in grade. Often it is very pronounced, even in spite of marked morphological divergence (*Colaptes auratus* and *C. cafer l*) or in other cases very slight (*Coracias benghalensis* and *C. garrulus*) Indeed the sexual affinity sometimes varies geographically in degree, as is the case in the relations between *Passer domesticus* and *P. hispaniolensis* or, as I see in Dr. Chapman's article, between *Parus atricapillus* and *P. carolinensis*.

Absolute consistency is thus not possible for the adherents of the Kleinschmidt Formenkreis theory without doing violence to Nature. But they can console themselves with the knowledge that those who claim to follow the A. O. U. Code to the letter also act very inconsistently. If intergradation is the touchstone of trinomialism, why then is the Red-shafted Flicker not called *Colaptes auratus cafer*, and the Golden-winged

Warbler *Vermivora pinus chrysoptera?* Who can prove that racial transition is not also provoked by crossing in many other cases where nearly related forms differ still less from each other? On the contrary, is it not very often the case? I need only recall the races of *Passerella iliaca*, with which we have been made familiar through the excellent monograph of H. S. Swarth. Further, if those who follow the A. O. U. Code wish to be consistent, must they not divide dichromatic forms with strictly alternative inheritance in two species? In the earliest stages *Chrysolophus obscurus*, *Micronisus niger*, and *Clamator serratus* already differ fundamentally from the normal phases, and there can be no question of intergradation through individual variation.

Modern research in heredity, which has led to the search for mutations, has brought into a new light the question of the origin of geographic races. We begin to understand that by a sudden mutation, which changes considerably the color or even the pattern of the plumage, the ornithologists—but not the birds themselves—may be deceived, though in fact the species remains the same. It is quite immaterial to *Buteola brachyura* and *Micronisus gabar* whether its mate is coal-black or not, and *Accipiter novae-hollandiae* pairs just as readily with a snow-white as with a gray consort. What happens with mutations within the same subspecies holds good also in the case where whole populations have been altered mutationally. Here only the ornithologists—but not the birds—recognize two species. The black *Corvus corone* in the border region mates as readily with a gray *Corvus cornix* as with one of its own sort. We may leave therefore to the instinct of the bird, and not to the eye of the skin-ornithologist, the decision as to what belongs to a species and what does not. When one has secured an unequivocal answer it is usually not difficult to fix the bounds of a species, reasoning by comparison, even when the distribution is not continuous. The surprises one experiences when he appeals to the natural instincts of the birds are often very great. What a fundamental difference there seems—to the student of skins—to exist between the tropical *Pericrocotus roseus*, pink below, and the dull gray *P. cantonensis* of southern China. Yet they hybridize where they come together in the province of Kwangtung. So there is only one species! It is not the color that decides, often not even form, but the birds recognize their near affinity by peculiarities which are mostly still unknown to us, but which are much more important than the taxonomic features hitherto recognized.

Of what utility is it to introduce such knowledge into the nomenclature? Dr. Chapman gives an admirable answer when he says: "In the study of both physical and geographic origin of species it is of fundamental importance for us to know whether given forms are species or subspecies." I should like to enlarge and emphasize this thesis. To know whether two forms belong to the same or to two different specific groups (Formenkreise), and to learn how far a natural species has reached in its dispersal is of

fundamental significance to all who concern themselves with the problems of geographic distribution, of the origin of subspecies, of heredity, or of the influence of climate upon size and pigmentation. When by careful investigation we have solved this question, we should see that it is expressed in our nomenclature; otherwise our important result remains hidden in a special work, where the zoogeographers and biologists will easily overlook it, though it is just for them that it would be most stimulating. Again an example: Dr. Chapman himself has very recently investigated the geographic variation within two groups of the genus *Buarremon*, and has shown that in these groups the predisposition has repeatedly been manifested to mutate in a definite direction. His two great groups I and II are however nothing more than comprehensive species, or "Formenkreise," which in my judgment should not be split up in a series of artificial catalog species, but should be called *B. brunneinuchus* and *B. torquatus*. None of the many forms thus reunited under two specific names serves as a bridge to any other "Formenkreis" of the great genus *Buarremon*, and the case is typical for a countless number of others, indeed for nearly all "Formenkreise." I repeat that the gap between natural species (not between the artificial!) is usually very wide.

It will probably be objected that what I should like to bring out with specific names could be stated by the introduction of subgeneric names. I cannot approve such a proposal, since it would be equivalent to quadrinomial nomenclature. May the future preserve us from this! The subgeneric names so far invented would be greatly increased in number, for often they unite just what the Formenkreis theory would keep strictly separate—species closely resembling each other, but physiologically different. What is attained through a broad comprehension of the species is not an overloading, but an unburdening of the memory. One of the most desirable results of this method is the fact that we can again reduce appreciably the number of generic names without the clearness of our nomenclature suffering in the least.

Conclusion.—The surest criterion, in my opinion, or placing two forms together in the same "Formenkreis" (or species) is their physiological affinity, which expresses itself in their inclination to pair. It is an old axiom of systematic zoology that morphological similarity should not induce us to place two forms in one species when experience in nature has established their sexual aversion. Conversely then, morphological difference between two forms ought not to tempt us to count them as two distinct species when experience in nature has given proof that they belong together. The members of a species remain alike in important characters, particularly those relating to ecology, even when they are separated by barriers to dispersal. A knowledge of the life history is thus an important link in the chain of evidence which the systematic worker needs for the correct grouping of forms. Not morphological, but physiological changes evidently bring about the splitting up of bird species. All the morpho-

logical mutations so far known have turned out to be of wholly secondary nature, even though they changed completely the appearance of the species (e. g., *Rhipidura fuliginosa*). What we preserve in our museums, the skin and feathers, cannot be the most essential part of the bird even for the systematic worker.

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## NOTES AND NEWS.

LIEUT. COL. HENRY HAVERSHAM GODWIN-AUSTEN, a Corresponding Fellow of the Union since 1884, and an Extraordinary Member of the British Ornithologists' Union, died at his home in Godalming, Surrey, England, Dec. 2, 1923, in the 90th year of his age. He was born at Teigsmouth, England, July 6, 1834, the eldest son of Robert A. C. Godwin-Austen, F. R. S., a distinguished geologist, and Maria, daughter of General Godwin, C. B.

His education was received at the Royal Military College at Sandhurst and upon his graduation he entered upon a military career which lasted 26 years. In 1851 he was gazetted with H. M. 24th Regiment of Foot, now the South Wales Borderers, and the next year went to India which became the scene of his principal work. He served in the Second Burmese War, was appointed topographical assistant on the Trigonometrical Survey of India, and joined the Kashmir Survey party in 1857. He surveyed a section in Kashmir, discovered the glacier which was named in his honor and determined the position and altitude of the great snow peak known as Mount Godwin-Austen, the second highest mountain in the world. In 1862 he surveyed Ladak, in 1863–64 served with the last mission to Bhutan, mapped the country between Darjeeling and Punakha, and in 1877 retired from active service.

Col. Godwin-Austen was best known as an explorer, geographer and conchologist. He was elected a Fellow of the Royal Society in 1880, served as president of Section E (Geography) of the British Association for the Advancement of Science in 1883, as president of the Malacological Society of Great Britain and Ireland in 1908–09, and in 1910 was awarded the Founders Medal of the Royal Geographic Society for his work in exploration.

Between 1870 and 1878 he published, chiefly in the 'Journal' of the Asiatic Society of Bengal and the 'Proceedings' of the Zoological Society of London, a series of valuable papers on the birds of the regions he had explored and in 1893 disposed of his collection of some 3500 bird skins to the British Museum. During his later years he devoted his attentions chiefly to conchology and contributed the volume on the Testacellidæ and Zonitidæ to the 'Fauna of British India.'