We have three other distinct species of *Oceanodroma* in Japanese waters, as follows :—

Oceanodroma leucorrhoa leucorrhoa (Vieillot).

Hab. Kurile Is. and Hokkaidō.

Oceanodroma monorhis monorhis (Swinhoe).

Hab. Prov. Mutsu, N. Hondo ; Loo-Choo Is. ; ?North-eastern Formosa.

Oceanodroma furcata (Gmelin).

Hab. Kurile Is. ; Hokkaidō ; Hondo (Sagami, Suruga, Kobe).

XVIII.—*Modern Nomenclature and Subspecies.*

By H. J. Elwes, F.R.S., M.B.O.U.

I have long had it on my mind to write something on this subject, which in Botany and Entomology, as well as Ornithology, is becoming one of the greatest difficulties which any student has to cope with. If I wanted a good proof that our branch of biology, which has been raised, largely by British ornithologists in the pages of 'The Ibis,' to a higher standard of knowledge than any other kindred study, I cannot find a better one than the last numbers of this Journal. Comparing it with a volume of the time when I first joined the B. O. U. in 1866, I find that the whole scope of our work is changed, and that some of the most active and enthusiastic workers of the present time are devoting themselves to the study of the minute variations of birds, or to the attempt, in which there seems no prospect of finality or agreement, to discover what are the oldest names of many of our long known species.

As a proof that my opinion is not without support from ornithologists of knowledge and repute, I will refer first to a paper by our late President, Dr. Eagle Clarke, in the 'Scottish Naturalist' for September, 1912, on "The New Nomenclature of British Birds." He recites briefly the

history of the question, and states that the 'Handlist of British Birds,' published in 1912, for the nomenclature of which Dr. E. Hartert was mainly responsible (*cf.* Introduction, p. xii), has changed the scientific name of no fewer than 226 out of 417 species there recognized as belonging to the British avifauna, from those that were adopted in the last edition (1907) of Saunders's list.

These changes were largely due to the adoption of a rule for which the International Committee were responsible, but which was never agreed to by many of our best ornithologists, viz., that the 10th edition of Linnæus should be taken as the starting-point for priority instead of the 12th. The lamentable results of this change cannot be better shown than by the well-known and often cited cases of the common Wild Duck and the Song-Thrush. That a man who must have known both these birds as well as any living species, could have made such careless changes in their names in his own books, seems to me an excellent reason for saying that, however great a systematist Linnæus might have been, neither he nor his works deserve to be treated as a fetish, or to be worshipped by his successors for ever. I think we have made a great deal too much of Linnæus's claim to be the founder of binomial nomenclature; and now that we have necessarily adopted trinomialism, whatever reasons there may have been for this rule, seem to me to have more or less disappeared. But there are a number of old authors who have not, and never had, any claim to real knowledge of the species to which they gave names, who deserve even less recognition; and it is just such cases as those pointed out by Dr. Ticehurst in our last number (Ibis, 1922, p. 147) which will, for years to come, cause names founded in obscure and forgotten publications of no scientific value whatever, to be used by the strict worshippers of priority.

Let us now consider the opinions of the most recent writers on these questions. I will take the letter of Mr. Loomis in 'The Ibis,' 1920, p. 964, "On the last phase of the subspecies,"

as my starting-point. In it he expressed what is in my opinion a sound idea, when he stated at the end of his letter that "the foundation of the subspecies is an unstable variation and in consequence the structure is collapsing." In a letter by Dr. Lowe and Mr. Mackworth-Praed (Ibis, 1921, p. 344) they say that Mr. Loomis's letter will be welcomed by many ornithologists on this side of the Atlantic, and not least by some of those who might be termed subspecies men. They go on to say that trinomialism supplies a handy (not invariably handy) adjective which is internationally understood and which designates birds from a certain locality (may I add, and often very ill-defined or uncertain locality or separate localities) in a short and concise way (may I qualify this by saying that, in the hands of some followers of the subspecies mania, the differential characters are anything but short or concise); and that "in some cases the recognition of subspecies enables us to map out migration routes of birds from any given locality, and to note the effect of environment on any given species throughout its range." They further say that they do not believe that natural selection as defined by Darwin can have any practical effect on the actual formation of species, nor do they believe in the direct action of environment on the formation of new species. They then point out that there are at least two forms of variation, one known as "mutational" and the other as "environmental," and that many of our present-day subspecific forms would probably quickly disappear if the organism were transferred from its normal environment to some other of a different nature. On this there is plenty of evidence among mammals and plants if not among birds. They conclude a very valuable and thoughtful letter by saying that it behoves us to set our subspecific house in order with a view to defining more accurately the exact rank of our subspecies. They do not suggest how this is to be done. Neither can I do so under the existing rules.

These letters were reviewed by Col. Meinertzhagen in a most valuable and careful paper (Ibis, 1921, p. 528), "Some thoughts on Subspecies and Evolution," which discusses

very ably the ideas of the previous letters referred to, and deals with five points on which the opinions of many of us seem to be very undecided at present. A careful study of this paper is essential, because most of the points which Col. Meinertzhagen raises, are illustrated by examples from his own knowledge of birds, and he particularly emphasizes the fact that the truth of Mendelian theory rests largely upon artificial experiments on plants, animals, and birds under artificial conditions controlled by man, and not on conditions which exist in wild nature. He says that he can call to mind no geographical race which can be ascribed to mutation. He believes "that all such are due to environment or isolation, both being geographical factors. A mutation has nothing whatever to do with geography." As to the value of a subspecies he gives excellent reasons for accepting them, as the shortest, most scientific, and convenient way of referring to geographical variation, and concludes by saying, "But there are still a few (and I am afraid the number is growing) who regard the trinomial system as a simple and quick way of gaining notoriety, while others look on the method as a confusing and unnecessary invention of the devil." The former class I desire to suppress by some form of boycott; of the latter class I have no personal knowledge.

Col. Meinertzhagen has devoted an immense deal of time and energy, not only in collecting and making field-notes on his collections (*cf.* Ibis, 1921, pp. 621–671 and 1922, pp. 1–74), but has taken unusual pains to name his collection with Dr. Hartert's help in Lord Rothschild's museum. It is evident that in very many cases he was unable to come to any satisfactory conclusion as to how far his specimens could be identified with the numerous subspecies recognised in Hartert's great catalogue. I should not wonder if these cases were little-known African birds; but it is just those common and wide-ranging birds, such as the Ravens, Crows, Sparrows, and Larks, in which the worst confusion occurs. With regard to the Crested Larks, Col. Meinertzhagen's remarks on the influence, or want of influence, which

environment has on the colour of plumage, require careful investigation in the light of Mr. Bonhote's remarks on subspecies and their part in evolution (Ibis, 1921, p. 270); and of Dr. Lowe's still more enlightening paper on "Species and Subspecies" (Ibis, 1922, p. 179). Much better brains than mine, and an amount of study which I cannot give to this very difficult question, are necessary to decide whether it is possible to formulate rules, which can be adhered to by men whose opinions vary, and must always vary, according to the amount of knowledge they acquire, and are capable of using to this end. But we have accumulated evidence that we cannot stop where we are, as, for instance, in the following cases :—Mr. Kuroda has, in his recent paper on "The Birds of Tshusima," followed the example of Hartert and his supporters, and has described, on differences of measurement in bill and wing, a new subspecies of the Blue Rock-Thrush which already has, according to Kuroda, in the Japanese Empire alone, three described forms, only one of which was recognized by Hartert, on p. 675 of his Catalogue, as *Monticola solitarius philippensis*, P. S. L. Müll. ; a name for which he claims priority over *manilla* Boddaert, though the identification is evidently doubtful. Who is to decide between Hartert and Kuroda? The latter may well say that he has a better claim to know Japanese birds than Hartert or any European can have, and when Kuroda has exhausted his ambition for subdivision, some younger Japanese ornithologist may spring up desirous for fame, and adopt another view of the position, either by making several more subspecies, or by uniting them all with our old friend *Turdus cyanus*, or *cyaneus*, of Linnæus in Ed. xii = *T. solitarius* of Linnæus in Ed. x. He may upset and unite under one name the nine subspecies of what Kuroda calls *Sittiparus varius*, into which the *Parus varius* Temminck = *sieboldi* Seebohm = *rubidus* Blakiston *fide* Hartert, has been subdivided mainly by Kuroda, on specimens from various islands of the Japanese archipelago ; of these specimens cannot exist in any sufficient number to enable European ornithologists to form an opinion.

After all, the Japanese may say that the little island of Yakushima has just as good a right to its own peculiar form of Tit as St. Kilda has to a peculiar form of Wren, or the Outer Hebrides to a peculiar Thrush.

These papers were followed up by Mr. Bonhote (Ibis, 1921, p. 720) in a paper on "Subspecies and their part in Evolution," in which he agrees with Col. Meinertzhagen that "a mutation cannot establish a subspecies, since to my mind a subspecies is entirely an environmental or geographic form," and ends by asking, "Have we any definite knowledge of a new species originating as a mutation?" He mentions *Pavo nigripennis* and *Athene chiaradiæ* Giglioli, Ibis, 1903, p. 1, as possible exceptions.

Dr. Lowe in his letter on Species and Subspecies (Ibis, 1922, p. 179) gives us, however, three concrete examples of discontinuous or mutational subspecies as follows:—*Pluvialis apricarius oreophilos* Meinertz. (Bull. B. O. C. xlii. 1921, p. 6), *Podiceps cristatus infuscatus* Salvad., and *Querquedula discors albinucha* Kennard, which latter case he considers specially interesting because the character which distinguishes it has apparently not as yet been completely and permanently established. He goes on to indicate briefly the fundamental difference between these discontinuous or mutational variations, and the superficial somatic changes induced by mere environment. He points out how the former owe their origin to deep-seated gametic factors, indicating by way of proof, in a footnote, how in the twinkling of an eye, as it were, a form which is to all intents and purposes nothing else than *Gennæus lineatus* can be produced by the crossing of *G. horsfieldi* and *G. nycthemerus*. It is to this new conception of the origin of species, a conception we owe to the work done by the followers of Mendel, that ornithologists must in the future turn their attention, or so at least he seems to suggest. Dr. Lowe concludes by saying that though it is impossible as yet to deal with the question of subspecies and their classification, he hopes that the subject will be more carefully and thoughtfully studied. And I particularly recommend the perusal of all these papers to

the men described by Col. Meinertzhagen who regard trinomials as an easy way of gaining notoriety.

I will now allude to another paper on "Modern Zoological Nomenclature" by Mr. Robert Gurney, published in the Transactions of the Norfolk and Norwich Naturalists' Society, 1918, p. 335, with most of which I heartily agree. In this he points out that the differences of names between the 'Handlist' and the B. O. U. List, which, at the time the latter was published, I found to amount to nearly 100, either of generic or specific names, are due to four causes: —

(1) Differences of opinion regarding method of naming and validity of subspecies. On this point I will enlarge later on.

(2) Differences of specific names due to different interpretations of authors' descriptions.

(3) Differences due to the retention of names by the B. O. U. Committee as *nomina conservanda*.

(4) Differences of generic names due to disagreement as to types and authority of genera.

I may point out that in this last case the rules followed by botanists are different, in that they allow any author who on better knowledge, or different opinions as to the value of generic characters, transfers a species to a new genus, to attach his name to the species. For instance, if I thought that *Turdus merula* L. was not a true *Turdus*, and called it *Merula merula* or *Merula vulgaris*, it would become *M. vulgaris* Elwes, or if I called it *Neoturdus* it would be *Neoturdus merula* Elwes. The effect of this practice in horticulture has been to my mind disastrous, and experience has proved that when a name has become well-established in garden and commercial use, it is impossible to effect a change however good the authority for such change may be. I venture to predict that there will be a similar strike amongst English bird-lovers if we continue our present practice. It may be asked, and should be asked, what is your remedy? My remedy is drastic: namely, that we shall give a lead to other countries by adopting a new rule, by which all questions of priority

shall for ever be decided. This is that the starting-point for priority shall not be Linnæus 10th, 12th or any other edition; but a list, catalogue, or book of comparatively recent date, approved and sanctioned by a strong committee representing all shades of opinion in the country, as the best and most reliable starting-point for the nomenclature of the birds, mammals, fishes, and other orders respectively. And that a new rule should be made to this effect:—That no new names of species or subspecies should be recognized as binding and properly published, until they have been accepted and passed by the committee appointed for that purpose by a constituted authority such as the B. O. U. Reasonable men, who alone have a right to be considered, would, I believe, be willing to sink their differences of opinion which must always exist in such cases as are specified by Mr. Gurney, or in other cases which might arise as our knowledge increases. New names or changes in names given by unreasonable men, or men who were considered by the authority as cranks, or whose position in the world of science does not justify them in giving names at all, would simply be ignored and boycotted by their fellows.

Now it may be objected, and rightly so, that such rules could be applied only in countries where the knowledge of the particular class of objects concerned had reached a point which has not yet been reached in many new and distant countries, or in many branches of biology; that such rules would have no international authority, and would not deter naturalists from describing new or supposed new forms in languages such as Polish, Hungarian, Bulgarian, or in Asiatic languages generally. That is, I think, a very desirable object to aim at, for as the rule stands there is nothing to prevent the publication of new names in daily newspapers, or by obscure local Societies in languages which cannot be understood generally. I would insist on some international language for such cases as these. Latin used to be the language of science; English, French, and German are possibly preferable if the greatest good of the greatest

number is considered. Another objection will be made, namely, that though this rule could be followed in the case of species, yet it could not be made to apply to such minute subdivisions of species as are being adopted by modern naturalists.

To this latter objection I can suggest no remedy until there is more general agreement as to the limit which may be allowed to this practice. In Botany it has reached a point of folly which cannot be imagined by an ornithologist. In proof of this I may say that in the fourth Supplement to the 'Index Kewensis,' which contains the names of all plants described as new in the five years 1906–1910, I find over forty columns, each containing about forty names, in the one genus *Hieracium*; of these 1600 names three men are responsible for by far the greater number. I could cite cases among the Lepidoptera which, if not so outrageous as this, have led to many complaints amongst butterfly-collectors. Lt.-Col. Evans, perhaps the best authority in India, writing in the 'Journal of the Bombay Natural History Society,' xxviii. 1921, p. 32, says:—"Nothing annoys the amateur student so much as the apparently useless changes in nomenclature." He goes on to say: "It is a great pity that we have no International authority empowered to issue an authoritative list of known families, genera, and species. Any alterations or additions might be proposed by individuals, but should not come into force until approved by the central authority after due discussion in scientific journals; all delving into the past should be vetoed as far as nomenclature is concerned; the result would be that the energies of many excellent naturalists would be diverted to useful progressive work from what might be termed useless retrospective labour."

I had hoped to conclude this paper by an examination of the results arrived at by Beebe in his account of *Phasianus colchicus* and its numerous subspecies, but this must be deferred to our next number.