

CORRESPONDENCE

EDITOR OF 'THE AUK':

It is disappointing to find (Auk, April, 1919, pp. 317-318) that even so careful a reader and reasoner as yourself has failed to grasp my meaning, and the principles that I have attempted to put in practice. If you have failed in understanding me it can be expected that there is further misinformation abroad regarding my aims and methods. Whether this is my fault or that of others, a further statement seems necessary.

You are quite correct when you state that I do not agree to the slightest abandonment of the subspecific principle. Neither am I a binomialist in the common sense of the term, as I hold that the trinomial is the only logical form of name for subspecific races. That I differ with some as to the exact *degree of differentiation* it is *expedient* to recognize in this manner and think that in the past the subspecific fact has been given undue prominence over the specific one, are matters of detail and do not interfere with any generally accepted fundamental principles.

When however you say,—“Mr. Taverner would use this binomial for *some one race* (seen but not positively determined) of *M. melodia*” you are attributing to me sentiments that I do not hold, and I have expressed myself but poorly if you can base them on anything I have said or on examples I have furnished. On the contrary I have consistently applied the binomial *collectively to all the races* of a species, lumping them under the specific title and using the trinomial for *each* of the constituent subspecies. In this I have followed to the letter the principles of the A. O. U. as exemplified in the Check List of 1910 and am in harmony with all who believe in representing the first described race by a trinomial name. The use of the binomial specific heading is an old one, sanctioned by the occasional practice of writers of repute, notably yourself. I have therefore only used a recognized form in a recognized manner, departing from current practice only in its freer use.

You suggest that where the exact subspecific status is doubtful, the abbreviation *subsp?* can be used as indicative of the fact. I grant it, but submit that it is redundant. According to A. O. U. practice there is absolutely no difference in meaning between the forms *M. melodia* and *M. melodia subsp?* except, perhaps, that a little greater emphasis is placed upon the question in the latter case. Of course to those who still cling to the nearly obsolete practice of giving the type race the specific binomial, as if the attendant subspecies were subordinate instead of coördinate divisions, there is a great difference, but this reflects a concept that the A. O. U. has already rejected.

It may be asserted with some justice that the listing of such forms as Magpie, Black Tern, Rough-leg and others as binomials savors of pedantry, as the possibility of American specimens being other than American forms

is, in the majority of cases, very small. I would say that whilst the probability is small it is not negligible. The persistence of *Larus argentatus smithsonianus* in our lists through acceptance of unverified authority, is evidence that this is a practical as well a theoretical source of error. The use of the specific term where the subspecific differentiation is not actually verified is a precaution against perpetuated error that is very cheaply applied. By its very nature it can cause no confusion, for the greater the subspecific certainty, the less *necessity* there is for naming it. When the probability becomes certainty, the subspecific title may be a convenience but it ceases to be a scientific necessity. The further the probability departs from certainty the more advisable subspecific designation becomes but the more cautious we should be in applying it. It would be interesting to know how often since their original description these "American" races have been examined and compared with adequate extralimital material. It would not be surprising if many of them failed to stand modern tests and were found to rest on faith rather than fact.

Your parallel between these subspecific cases and the New-world Titlark is another question. Doubt unquestionably exists with species as with subspecies and if there were as easy a way of generalizing upon them I would advocate its adoption. As there is no such neutral course I do not see that we can do other than get along as well as we can with an imperfect system. Specific difficulties of this kind are comparatively few whilst subspecific ones are legion and that we cannot correct the few former is no argument against progress with the many latter when it can be obtained by simple methods, already to hand, which have received the sanction of leading authorities and the A. O. U.

An objection that has been generally advanced against records made in the manner under discussion is that they are "unquotable." I cannot see that a definite subspecific identification made in a concrete statement in small print is any less quotable than when made by inference in a heading in heavy type. Where such identifications are not definite they certainly can not be quoted as definite. The fault, if fault there is here, lies in the writer, not in the method of presentation, for the latter certainly allows a freedom of action that has great advantages.

If there is no room in scientific literature for other than final results; if no interest lies in specific facts and distributions unaccompanied by subspecific identities, such identifications, definite specifically but indefinite subspecifically, as I have published lately are to be condemned. If however, we admit that a species is worthy of study as a species, and that statements of evidence uninfluenced by preconceptions can be presented as the basis for future generalizations rather than as finalities such lists fill a valuable place in scientific investigation. In attempting an orderly arrangement of our material we have had a tendency towards forcing of facts into pigeon holes prepared for them. The attempt to make every specimen agree with a preconceived scheme is not for the advancement of science. To call intergrading, worn, undeveloped, mutating or wandering

plumages known instead of unknown quantities will not solve zoölogical equations and may produce astonishing results. Our *ab*'s and *xy*'s should be recognized as such and kept separate, the former only being used in final solutions and the latter put aside for future consideration and incorporation in the problem when increased knowledge justifies. Against this there is the constant cry for exact information on the grounds that he who examines material is best qualified to pronounce upon it. Exact information is most desirable but we do not want to obtain an appearance of it by disguising a guess as a verified fact. Of course when our own knowledge or material fails there is always the alternative of submitting the problem to "authority," but the question then is, what authority and how far it should be accepted without verification. It may be admitted that some specialists, through wide experience and specially developed faculties, at times attain an almost uncanny intuition as to the identity of specimens, and their opinions even where they fail to support them by evidence convincing to others carry considerable weight, but we cannot admit that the mere dicta of even such gifted mortals should be accepted without reservation nor can their findings relieve the rest of us from the responsibility. When such determinations are to be included in our presentations they certainly should be given for just what they are, quotations of others, and their source plainly indicated, not only that due credit be given but to protect the writer and that the personality, experience and viewpoint of the authority may be estimated by the reader. For this purpose it seems to me the binomial heading and subspecific discussion in accompanying text offers the most ample opportunities without violating any of the vital principles of modern practice.

P. A. TAVERNER,

Geological Survey,
Ottawa, Canada, April 26, 1919.

[Our "misunderstanding" of Mr. Taverner's practice is we think more imaginary than real. When one sees a single Song Sparrow and being in doubt as to its subspecific identity, uses the term *Melospiza melodia*, it seems that he is using the term for "some one race" just as stated in our previous note. In many cases too he uses the binomial heading with no subspecific discussion whatever in the text, which is the practice that we particularly criticised. However, this is a trivial matter. We understand and are in sympathy with Mr. Taverner's desire to record facts and not guesses, but we still consider that his method is confusing and the use of "subsp?" is necessary if his meaning is to be made clear to the great bulk of readers. The number who do not yet understand the A. O. U. plan as practised in the last edition of the 'Check List' is vastly greater than Mr. Taverner imagines. They still think that *every* binomial indicates one sort of bird!

By introducing a system of this sort, which only a few are likely to follow,

we only cause confusion. Presently some one else will suggest another scheme and before we know it we shall have hopeless chaos and our indexes will lead us nowhere.

To those who have struggled long with the maze of published names and who by the aid of well framed codes are beginning to see the solution of that side of the nomenclatorial problem, it is discouraging to encounter well intentioned innovations such as Mr. Taverner's, and the endless activities of the genus splitter who has forgotten that a name is a name and tries to make of it a phylogenetic expression which changes with every user. There can be no rules to govern such phases of nomenclature which will always be matters of personal opinion. Why not let well enough alone?— W. S.]

NOTES AND NEWS

DR. LOUIS BRASIL, a Foreign Member of the B. O. U., who was elected a Corresponding Fellow of the A. O. U. at the last meeting, died at Caen, France, October 15, 1918, but the news of his death has only recently been received. From 'The Ibis' we learn that Dr. Brasil was born in Paris in 1865 and at the time of his death was only 53 years of age. He was brought up at Caen where he received his education and where he became Lecturer and later Professor of Zoölogy in the University. He also served as President of the Linnaean Society of Normandy.

His work included invertebrate zoölogy and geology as well as ornithology, but on the latter subject he published several important papers. He contributed the sections on Apteryges, Cassowaries, Cranes and Emus to Wytsmann's 'Genera Avium,' 1905, and in 1914 published a little work on the 'Shore- and Water-Birds of France, Belgium and the British Islands.' Two years before his death he published a paper on the Birds of New Caledonia, containing descriptions of several new forms based on two collections made by French officers in 1865-69 and deposited in the Caen Museum. Dr. Brasil contributed several papers and short notes to the 'Revue Française d'Ornithologie' and also to 'The Ibis.' His writings and his work in general were characterized by care and accuracy.— T. S. P.

FREDERICK BRIDGHAM MCKECHNIE, an Associate of the A. O. U. from 1900 to 1911, was born in Dorchester, Mass., Sept. 19, 1882, and lived there until about 1900 when his family moved to Ponkapog, Mass. He seems always to have been interested in birds, and this move from a suburban district to Ponkapog, a small country town west of the Milton Hills, and in a setting of as wild country as there is in eastern Massachusetts, was distinctly congenial.