

THE CONDOR

A Magazine of
Western Ornithology

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EDITORIAL NOTES AND NEWS

The Editors of *THE CONDOR* are greatly indebted to Mr. J. R. Pemberton for preparation by him of the annual index appearing with the present issue. The admirably accurate "copy" furnished by Mr. Pemberton is an evidence of the high quality we may look forward to in the ten-year index which he is now compiling to cover volumes XI to XX of our magazine.

Mr. J. Eugene Law has kindly consented to compile for the January issue of *THE CONDOR* a new and complete list of Cooper Club members in military service. He will welcome assistance in this line from anyone who can furnish the latest information concerning the location and rank of Club members.

A specially prepared glossary of words and phrases which can be used in describing birds' voices is something which seems to be badly needed by students of field ornithology. Mr. Richard M. Hunt, of the staff of the Museum of Vertebrate Zoology, Berkeley, has undertaken to prepare such a "nomenclature of sounds" and will appreciate any suggestions along this line.

A question has arisen as to the value of the annual roster of Cooper Club members, such as appeared in the last July issue of *THE CONDOR*, pages 147 to 156, and has appeared almost every year since our magazine began its career. It is claimed by some that, say, ten good pages are thereby appropriated, which might better be used for the recording of *ornithology*. The Editors are quite willing to submit this question to the decision of those Cooper Club members who are sufficiently interested in the matter to respond to a mail vote. Shall we discontinue printing the annual membership roster? Write "yes" or "no" on a postal card addressed to the *Editor of The Condor, Museum of Vertebrate Zoology, Berkeley, California*.

Dr. Charles W. Richmond has recently been advanced to the rank of Associate Curator of Birds in the United States National

Museum. This is a well-deserved recognition of Richmond's scholarly attainments in taxonomic ornithology, as also of his fitness and experience as regards curatorial technique.

We would urge authors in general to exercise greater care in the selection of titles for their articles. It is not unusual to receive for publication in *THE CONDOR* papers, otherwise carefully prepared, lacking any title whatever, with, perhaps, a note "permitting" the Editors to place any heading they see fit above the contribution. In our bibliographic researches we often run across a title which gives no hint as to the nature of the article which it heads. Titles should be brief, but at the same time, should give as clear an indication of the subject matter of the article as is consistent with brevity. Considerable thought can well be expended to attain the best results in this direction.

Our readers will have read with interest Mr. Frank Stephens' "Autobiography" in the September issue of *THE CONDOR*. We are grateful to him for thus being the first to respond to our request. It is a pleasure to announce that three others of our senior ornithologists have now consented to furnish autobiographies; their contributions will appear in early numbers of our magazine.

During the past summer Dr. W. P. Taylor has carried on field work for the United States Biological Survey in parts of eastern Washington.

Dr. Lee R. Dice, recently of the zoological staff of the University of Montana, has become identified with the work of the United States Biological Survey and has removed to Washington, D. C.

Mr. F. C. Lincoln, Curator of Birds at the Colorado Museum of Natural History, is now in the Pigeon Section, U. S. Signal Corps, stationed at the headquarters of the western department at San Francisco.

COMMUNICATION

TRINOMIALS AND CURRENT PRACTICE

Editor of *THE CONDOR*:

Mr. Swarth's criticism (*CONDOR* xx, 1918, pp. 141-142) of my report on the ornithological accessions to the Museum, in the Summary Report of the Geological Survey of Canada, for 1915-16, is welcome, as it gives opportunity for presenting what is perhaps a novel viewpoint, and may be the means of opening up and airing an important subject. As a preliminary, I would state that this bi-

nomial system of list writing was not adopted as a protest against the subspecific or trinomial principle as such, nor in a spirit of arbitrary eccentricity; but, after due consideration, as a corrective to certain current evils that all will acknowledge to exist, and in the full belief that it marks an advance, not a retrogression in scientific record. The fundamental correctness of the method has not been questioned; the expediency of applying it to technical scientific usage alone is under discussion.

It consists in listing the species under binomial headings, reserving subspecific determination for the text following. It has the advantage of allowing the writer as fine definition as his facilities and experience warrant, and permits him to qualify when in doubt, or to suspend judgment where he deems expedient. It has the further advantage of making every subspecific designation a conscious act of judgment and direct assertion, discourages the unconsidered copying of names, and encourages original research and the statement of viewpoint and standards of judgment. It thus is flexible and adapted to all uses, and is a powerful psychological influence in the direction of caution.

The evils that the writer thinks it tends to correct can be seen in any faunal list in which subspecific designations are given without the basis of their determinations being made clear, or a satisfactory authority being evident. A very good example of this use of subspecific names occurs in a list of birds of a section of one of the northern prairie states, in a recent number of an ornithological periodical of the highest standing. I do not wish here to criticize either the author of this special paper, or the editor of the publication in which it occurs, but merely to call attention to a common fault, almost universally current, which could be largely corrected by the system in question.

In this list, trinomials and subspecific nomenclature are used consistently throughout. Among other names appear *Buteo borealis krideri*, *Melospiza melodia melodia* and *Agelaius phoeniceus phoeniceus*. These identifications may be correct, but in this special region, the omission of mention of the Eastern and Western Red-tail, the Dakota Song Sparrow, or the Northern and Thick-billed Red-wing, requires for general acceptance, more authority than is evident. It is not clear from the context whether the author has examined and compared his specimens himself, subjected them to the scru-

tiny of others, or followed the common but reprehensible practice of identification by supposed geographical probability. If the identifications were by acknowledged authority, or followed generalized pronouncements of authority, we would like to know who that authority is. Decisions according to the A. O. U. *Check-List* may differ seriously from those by Mr. Ridgway, and both from other authorities that could be mentioned, and until such information is furnished, we can but withhold judgment upon the statements advanced.

If the subspecific conclusions are based upon generalizations of geographic distribution, they are worse than useless. Purporting to be additions to our knowledge, they add nothing to it either to confirm or correct existing conceptions. If, as Mr. Swarth says, "the value of such a list lies largely in the exact subspecific determination of the various forms at the points at which the specimens are taken", the uselessness of such determination when correct, is as obvious as its danger when incorrect. Geographical presumptions cannot be used as evidence for testing those presumptions upon which they are founded. It is clear that in this case, representative of many others, trinomials are worth no more than binomials, and the latter might well have been substituted for them. We are willing to accept the writer's statement that some form of Red-tailed Hawk, Song Sparrow, or Red-winged Blackbird, occurs in the region treated, but the third terms in the names are so much waste ink and compositing, containing potentialities for perpetuating error, without the possibility of correcting them.

Subspecific designation is only warranted after specimens have been duly compared by competent authority with a suitable series of material, and then draws all its value from the name of the responsible authority. Such work is of extremely technical nature, and is the field of the specialist who alone is competent through experience, and the possession of comparable material, to make pronouncement. It is neither possible nor necessary that all should be specialists, and facilities should be given workers in other branches of ornithology, whereby they can give to the world their undoubtedly valuable results, without exceeding their legitimate limitations. The general public naturally follow the examples set by what they deem the best scientific practice, and when their models use nothing but trinomials, it is natural for them to conclude that they

should do the same, and in accepting hypotheses as established facts, copy authority not wisely but too well. This evil cannot be corrected until the leaders set the example, and by their practice put the seal of approval upon a system that can be followed by all without drawing invidious distinction. The system under discussion meets these requirements.

"The startling innovation in style," of which Mr. Swarth complains, is really no innovation, as it was used by Stone and Cram in their *American Animals* in 1902. Mr. Swarth does not imply that it is fundamentally or scientifically wrong, nor probably would he admit that innovations are to be deprecated, if sufficient reason can be advanced for their adoption. This I have attempted to do.

The English name that accompanies the binomial in the lists under discussion, is the specific one, and not that of the eastern variety. Mr. Swarth is justified perhaps in being a little uncertain on this point, as the A. O. U. *Check-List* and current practice has in many cases applied this name to one of the component parts of the species. Even this sanction, however, does not make the practice correct. Certainly Cassin's Vireo is just as much entitled to the name Solitary Vireo as the eastern race is. The latter can claim title to the term Blue-headed Vireo, or anything else that can be agreed upon. Other species show the incorrect practice of the A. O. U. *Check-List* more clearly than this one does. Thus, the term American Robin obviously applies to all the races of the species, the A. O. U. *Check-List* to the contrary notwithstanding; whilst the race so designated at present requires a qualified term like Eastern Robin to differentiate it from the Western or the Southern forms. The part should not limit to itself the name of the whole. A system like the one under discussion, requires the correction of all of these misapplications of specific terms to racial parts.

While agreeing with Mr. Swarth as to the necessity of recognizing and studying subspecies and their distribution, I cannot but feel that the subject has been given an undue importance in American ornithological presentations. Subspecific differentiation is but a part of the study of ornithology, not its end and object. We are today suffering not only from what Dr. Dwight calls an "indigestion of names" but also, to use another term from the pen of the same apt phrase-maker, from the "exaltation of the subspecies". The subspecies, as a taxono-

mic division, is decidedly secondary to the species in importance. Though through geological time the species is a variable and uncertain quantity, at any one moment or on any given geological horizon, it is practically a fixed quantity. Orders, genera and families are but conventional groupings of lower units merging into each other, with boundaries set by individual and varying opinions of expediency. Subspecies are also hazy in their outlines, and, within their specific limitations, blend together with arbitrary separations. As far as individual human experience is concerned, species are comparatively fixed quantities, and are the only approximately definite and stable units of taxonomic measurement with which the zoologist deals. However, so closely has the lesser hazy division (subspecies) been examined, that it has seemed to occupy most of the horizon, and obscured the greater specific fact. The binomial method of heading corrects this distorted perspective, and by presenting the proper relationships visually, tends to restore them to their proper proportions in public concept. It does this, in spite of Mr. Swarth's complaints to the contrary, without loss of definiteness, for the writer can be as minutely accurate as he cares to be. The vagueness to which he objects is purely personal to the writer and these special papers, and are not inherent in the system. The system and the writer are two separate subjects, and each should be judged upon its own merits.

"In many cases," Mr. Swarth says, "where he has evidently made up his mind as to the subspecies represented, there seems to be no good reason why the proper subspecific name should not be placed plainly as a heading." The contrary method was followed for the sake of consistency, to illustrate the flexibility of the system, and as an example. Liberties may be taken with an established and recognized system, but when under demonstration it should be followed to its logical conclusion, and tested for all cases.

The "atmosphere of vagueness and uncertainty" that Mr. Swarth finds in the papers in question, is understandable. One of the features of current practice is that we treat with finality and definiteness subspecific phenomena that are essentially vague and uncertain. Reading over various lists and descriptions, there is little or no indication given that many of the subspecific generalizations treated with dogmatic assertion, are after all but the expression of

personal opinion upon which no unanimity has been reached, even between acknowledged authorities of similar schools of thought. The discrepancies between the findings of the A. O. U. Committee, Ridgway and some of the Pacific Coast authorities, is demonstration enough of this. Phrases such as "much browner", "distinctly larger", or "comparatively longer", abound; yet when demonstrated by specimens, they are often such as can be seen only by the most careful comparison of a large series under special light conditions, and often are only average characters not shared by a majority of the race, and leaving many individual specimens unrecognizable except by geography. Occasionally even, the most characteristically marked members of one form hail from the headquarters of another. Many of these minute differences undoubtedly exist; some are only recognizable to supernormal perceptions, while others are probably the result of comparing insufficient or uncharacteristic material. The fact that experts of supposedly equal authority arrive at opposed conclusions on viewing similar or even identical material, indicates that subspecific determination is not an exact science. Those who do not claim infallibility either in perception or judgment, may well qualify their decisions as personal opinion rather than indisputable facts.

Mr. Swarth complains of "dribbling protests" and "deprecatory remarks directed against many subspecies now quite universally recognized by bird students". I dare to take exception to the statement regarding the universality of the recognition accorded many or any of the forms in question. In fact I venture to state that there are few racial forms that meet with *quite* universal acceptance. At any rate there have been in the past, and probably still are, many forms that have been generally accepted only because no one seriously questions them. However, the "dribbling protests" and "deprecatory remarks", inasmuch as they are not expressions of mere querulous complaint, but aim to reflect the evidence as it appears to the writer, should add rather than subtract from conclusions, especially when they are contrary to those generally received. Reflecting as they do the basis of the judgment derived from them, they should be preferable to bare dogmatic decisions. In the example quoted by Mr. Swarth, the Goshawk, I stated all that I was justified in assuming, i. e., that young birds are more coarsely vermiculated than

older ones. The facts are, that in a considerable series of these birds, all specimens with any remnants of striped juvenility in their plumage are, irrespective of geography, coarsely marked on the breast. I thought this was suggestive enough to mention, as explanation of my refusal to recognize it as a subspecific character. It will be noted that the western race is not wholly rejected. It is stated that there are indications of differentiation, but they are not deemed constant enough, or marked enough, in British Columbian specimens to warrant subspecific separation, and the possibility of the existence of a well marked race elsewhere, say in Washington or California, is not disputed. Others who regard any perceptible variation as sufficient grounds for racial separation, or have reasons for deciding that age does not explain the difference in vermiculation, are at liberty to form other opinions. They are certainly able to do so more intelligently with the data included, than from a bald dogmatic statement. The whole it seems to me goes to the limit of caution and deference to possibilities and to the opinions of others, without the sacrifice of personal judgment. If this is a fault, I plead guilty.

The charge is made that many records are useless to any student of distribution without a re-examination of the material. This may be true, but I think to a less extent than in the majority of lists that are received without remark, and the very things that make it less true, bring forth the criticism. In few lists will Mr. Swarth accept everything just as it is written, when they disagree with his own conceptions. I recognize this, and give him every opportunity for translating my standards into his, yet he objects. More details might have been given, but the heading on every other page, "Summary Report", is excuse for condensation and brevity. These are summary reports, and preliminary in character, not final studies, and the author feels at liberty to reverse his findings any time additional data warrants it.

In the case of the single trinomial cited by Mr. Swarth, Hybrid Flicker, *Colaptes auratus cafer*, that is the result of a regrettable but obvious typographical error, the omission of the hybrid sign (+) between the specific names, that slipped through the proof reading. Being in the field at the time, I was unable to attend to this important duty myself.

P. A. TAVERNER, *Museum Geological Survey, Ottawa, Ontario, August 19, 1918.*