

## THE TYRANNY OF THE TRINOMIAL

By HARRY S. SWARTH\*

It has been impressed upon me of late years that we are beginning to need delivery from the slavery of enforced trinomial nomenclature. It is so short a time since the need was the other way, the struggling trinomial requiring all possible support from its adherents, that it seems like heresy for any modern worker in systematics to raise his voice against the system. Here, as in so many other things, it is the letter of the law that kills. The system is good, it is useful, it expresses great truths, and it is used to excellent advantage to illustrate important facts—but it can also be used, and has been, so that the facts are obscured or entirely hidden.

I am not alone in this conviction. Some years ago Dr. Witmer Stone in a review in the *Auk* suggested the desirability of an arbitrary breaking-up of certain long series of subspecies, but his suggestion has so far met with no recognition or response. His idea seems to me to be reasonable, and such action need not be considered as entirely arbitrary either. W. E. Clyde Todd, in a study of certain South American flycatchers, sounded just the same warning that I am now repeating, against hasty subspecific union of different forms on the basis of external similarities in the prepared specimens. Mr. Ridgway, too, published in the *Auk* (1923) "a plea for caution in the use of trinomials."

Harmful aspects of exaggerated trinomialism are seen in two developments in the systematic treatment of variation: First, in the increasing length of certain series of subspecies, second, in the linking together of Old World and New World series of variable forms into one specific unit. Neither development is necessarily objectionable in itself, and in neither case could strong protest be raised if there were absolute proof forthcoming of the existence of the conditions necessary to subspecific treatment; but such proof is rarely presented. Generally there is a basis of assumption.

My objections to the increasing length of the series of subspecies that I have in mind should not be interpreted as opposition to the naming of additional forms, which is not the point I am making. An example of what I do mean is found in the series of named forms of the Winter Wren and its allies, the genus *Nannus*. The currently accepted treatment of this group labels the several North American forms and the several Aleutian forms all as subspecies of one species, which is, furthermore, conspecific with the several Old World forms. Intergradation of a sort does exist, of course, but even so I contend that trinomial usage applied throughout this series obscures more than it enlightens.

The North American mainland wrens, *hiemalis* and *pacificus*, with the Kodiak Island *helleri*, are closely similar and meet the standards that I would apply in using the trinomial. In the group of subspecies upon the Aleutian and Pribilof islands there is an abrupt difference; gradual change of characters in a given direction, from the mainland westward, has not been demonstrated. *Helleri* is not an important link between the mainland and the Bering Sea groups. Even though variability in one character or another can be picked out here, there and elsewhere throughout the islands, and can be interpreted as a demonstration of variation from one extreme to another, I submit that this is not a standard to which we should adhere. We lose sight thereby of a sudden change in character in this group, and we lose

---

\*Read at the Sixth Annual Meeting of the Cooper Ornithological Club, Berkeley, May 16, 1931.

sight of the fact that variation in this group in the Bering Sea region can be correlated with what is seen there in other bird assemblages. The preferable alternative in this case is, I think, to regard the North American, the Bering Sea, and the Old World group, each as a separate species.

Examples of a slightly different sort may be found in the long series of western American jays of the genus *Aphelocoma*, and brown towhees of the genus *Pipilo*, where in each case rather tortuous application of the intergradation concept has resulted in the linking together of aggregations of geographical variants that to my mind had better be kept in separate assemblages.

Some of our ablest exponents of systematic ornithology have assured us that there is no essential difference between a species and a subspecies. We enthusiastically subscribe to the sentiment without realizing that these same champions do not act in accordance with this belief, for they try to follow a hard and fast rule that is supposed to differentiate absolutely between the two concepts. "Intergradation" is the magic touchstone that is to be applied, and the successful application of this test—the success itself open to various interpretations—can bring into the same category such disputable variants as *Coccyzus americanus americanus* and *C. a. occidentalis* on the one hand, and entities as distinct as *Psaltriparus minimus* and *P. plumbeus* on the other.

I have been working of late on a group of birds, the Geospizidae of the Galapagos Archipelago, where about forty distinguishable forms may be recognized, scattered over many islands. These birds vary in diverse ways, conspicuously so as to bill character. The extremes, from a bill as heavy as that of the clumsiest grosbeak to one as slender as a warbler's, are so different as to have caused their possessors formerly to be classed in different families, the Fringillidae and the Mniotiltidae, yet between those extremes intergradation exists that could be warped into justification of subspecific union of the whole forty odd distinguishable forms. I have no hesitation in ignoring here any such criterion as the intergradation test in favor of an arrangement on another basis that results in a much clearer exposition of conditions than could be obtained from a trinomialized catalogue of names.

More and more of recent years have groups of American (Nearctic) birds been specifically linked with Old World (Palearctic) forms, such as the goshawks, harriers, pipits, creepers, and kinglets; and, conversely, certain European ornithologists have seen in some Old World birds close linkage with species that are perhaps more extensively developed in America. Once the fashion is set in matters of this sort it is easy to find followers who will push the innovation to the farthest extreme, and without any painstaking verification of evidence.

Our American Brown Creeper may truly be a subspecies of the European *Certhia familiaris*. At the same time there are two distinct species of Brown Creepers existing together in parts of Europe,—and who is there familiar with all three forms in life who can give us intelligent observations and deductions on probable relationships? Our American Pipit has been classed of late as a subspecies of the Old World *Anthus spinoletta*. A specimen of *Anthus spinoletta japonica* collected in Alaska from a flock of American Pipits was picked out instantly by the collector through the appearance and actions of the living bird, yet it takes a keen eye quickly to distinguish the prepared skin. I confidently await more testimony from competent witnesses who are familiar with both forms in life and in the museum, in the conviction that they should be regarded as distinct species. In some American flycatchers of the genus *Empidonax* specific characters are such, as

between *E. wrighti* and *E. hammondi*, for example, that if one were European in distribution they would assuredly be regarded as two subspecies of one species. Such is the fashion.

My argument implies nothing so ridiculous as a denial of the existence of intergradation between various forms of birds. It is there, it is to be recognized as a feature to be weighed in adjudicating the relationships of forms. But it is by no means the only important factor in variation, and every sort of variation should not be twisted and forced to fit in with a theory that requires the presence there of intermediate conditions. Nor has my protest against this sort of formulated procedure anything to do with the stabilization of our nomenclature, which is another matter, governed by certain common understandings, "laws" if you wish, that I am anxious to uphold. But I am not at all anxious to be uniformly "consistent" otherwise in the published presentation of observed facts.

It all comes to this, that pursuit of such a study as ornithology is not a game, to be won or lost according to set rules. We are painfully acquiring facts, endeavoring to apply these facts, and trying to convey our ideas to each other. To hold ourselves bound to certain iron-clad conventions, to a fetish such as the concept of intergradation may become, is to risk having American conduct of ornithological research subject to as cynical contempt from the disinterested observer as has been the meed of American legal procedure in the criminal courts, and from just about the same cause—an unworthy regard for the letter of the law as opposed to the spirit thereof.

*California Academy of Sciences, San Francisco, May 16, 1931.*