

PROPOSED SHIFTS OF NAMES IN *PASSERCULUS*—A PROTEST

By JOSEPH GRINNELL

Following are some criticisms of the paper by James L. Peters and Ludlow Griscom on "Geographical Variation in the Savannah Sparrow" (Bull. Mus. Comp. Zool., vol. 80, no. 13, January [my copy received July 22], 1938, pp. 445–477 [+3], 1 col. pl.). However, let me say at the outset that no systematic bird paper for a long time has so interested me as this one. It is obviously the result of painstaking study, involving a great amount of material; and the group dealt with was already known to be confusingly variable throughout the extent of its continent-wide occurrence; furthermore, its authors are experienced, competent. The problem was a difficult one, and it is handled in sober fashion; the resulting revision is no slap-dash product. So, the adverse criticisms I make, and the suggestions I am led then to offer, are not to be inferred as discounting my respectful feeling toward the paper and its authors.

The first unfavorable reaction upon the present reader of this paper (and this same reaction was produced by the reading of Griscom's crossbill paper) results from a certain, seemingly unnecessary, positiveness of statement. Here and there the reader is told a thing in a manner of wording that denies him the privilege of entertaining any other possible interpretation or alternate view! To illustrate: On page 447, Peters and Griscom state: "While the Ipswich Sparrow has been maintained as a distinct species ever since it was first described, there are no reasons [*sic!*] (except possibly sentimental ones) that would warrant the continuation of such a course." And again, page 448: "The fact, therefore, that the Ipswich Sparrow is readily distinguishable from *labradorius* and *savanna*, the only races with which it ever associates, has nothing whatever to do [*sic!*] with a final evaluation of its relationship to the group as a whole."

Maybe this, in the authors' minds, is simply a mode of indicating the strength of their own views as to how to use the trinomial versus the binomial form of scientific name for this sparrow; they propose the *trinomial*. But certainly I, as one reader, after double-checking all the authors' collateral statements, do see other reasons, and non-sentimental ones too, for continuing to call the Ipswich Sparrow, *Passerculus princeps*. In the first place, under the authors' treatment, the genus *Passerculus* contains but one species, *sandwichensis*, this therefore with upwards of 18 subspecies. What, then, practically, is the value of the middle term in all these trinomial names—unless we, even arbitrarily, adapt the function of the binomial for indicating recognizable "groups" of subspecies within the genus? This course I think is the justifiable one in the genus *Passerculus*, even if not in certain other fringillid genera of more or less similar make-up. Consistency of treatment is impossible. Nature has not acted with uniformity in the different genera —far from it.

Further along this line, I note that Peters and Griscom in discussion refer to the "Savannah Sparrows proper" or the "typical Savannah Sparrows (in the old or popular sense)" as set off from the "*rostratus* group." Why, then, run the subspecies of the latter "group" into the same binomial series? A usable and useful break comes between *anulus* and *halophilus*, as I have indicated in the revised list of the forms of *Passerculus* at the end of the present critique. (This was first suggested by Oberholser, Cleveland Mus. Nat. Hist., Sci. Publ., vol. 1, 1930, p. 111.) There seems to be a stampede among avian taxonomists toward running long series of forms that are more or less near related, into continuous series of trinomials. The test of demonstrated geographical blending no

longer suffices; now the criterion is intergradation through individual variation irrespective of spatial contiguity, this as exhibited in discrete, uncorrelated characters. That is to say, returning to *princeps*, even though constantly different in aggregate of characters from its geographically, and doubtless also its phylogenetically, nearest relatives, *labradorius* and *savanna*, the authors reduce it to subspecific status *because* as to size it is overlapped by the far-distant *sandwichensis* and as to certain features of coloration it is approximated by the distant *nevadensis* and in pallor "is exceeded by certain races of the *rostratus* group in Lower California." They say, "There is no *absolute* [italics theirs] difference of any kind between this form [*princeps*] and one or more races of *P. sandwichensis*"—another positive statement, the warrant for which does not stand up under close scrutiny. For, this extreme criterion for reduction to subspecies, of gross intergradation in discrete or separate characters, if applied similarly would, it looks to me, tumble into conspecific groups a long suite of warblers, of empidonax flycatchers, chickadees, nuthatches. Especially would this criterion, too much a matter of personal opinion anyway, be highly undesirable when we consider the current opposite trend, that of generic splitting whereby, already, many of the accepted genera contain but one species, with its series of included subspecies. Nomenclature must be kept subservient to practical needs and as far as possible free from perturbation through changeable personal hunch.

Like some other contemporaneous writers, Peters and Griscom are "superior" to using vernacular names formally in headings, associated with the scientific names—which would certainly have made their special applications of the latter more quickly apparent to the present reader. At the same time vernaculars are used here and there in the running text! Why not have furnished help in this way to that reader who does not happen to be wholly familiar with the literature of this particular group? Indeed, some of the vernacular names used by the authors incidentally in the text are obviously more permanent than the associated scientific ones!

Another feature of Peters and Griscom's paper that I think may properly be criticized (and recent systematic papers by other persons are censurable on the same score) is this: That essentially biological fact and discussion all through are confusedly mixed with fact and argument relative to the application of names. Speciation and nomenclature are two totally different realms of thought. Indeed, here and there it appears to me as though matters of naming antedated or dominated in the authors' minds the scientific considerations directly concerned with the race defined. For example, type locality has nothing to do with definition or range of a subspecies; it is purely a matter of accident and has to do only with the fixation of a name. Yet we find the word "typical" used, at least in some places, in connection with the concept of a race as a biological entity. So with "topotypes," such specimens being cited as exemplifying the highest development of the characters of a natural subspecies—which rarely either the type or topotypes do! Running back and forth through the paper, I see that "typical" many times is used really in *both* senses—certainly beclouding the discussion as a whole.

Here is a suggestion at large: Would that some thoroughly qualified student of avian speciation select a geographically variable group, like *Passerculus*, and publish a paper revising that group, in two parts: (1) the main part, exclusively biological, setting forth the facts of characterization and distribution of each race decided to be definable as such, under merely a graphic designation, such as *a*, *b*, *c*, etc., together with full discussion of centers of differentiation, trends of variation, ecology, and the like; and (2) a part, or appendix relegated to subsidiary place, dealing with the names to be applied under the rules of nomenclature—a sort of concordance of the letter-designations

with the names for the races then given, along with purely nomenclatural debate bringing in matters of type-ship, type locality, name-priority, etc. By this scheme, we would be afforded what is really needed, a revision of soundly zoological nature clear through, uncontaminated by nomenclatural artificialities, *followed* by whatever needs to be decided concerning the names. Won't someone *try* this, and see if an improved type of revision does not result?

There will, of course, be outright mistakes in any publication; a modicum of mistakes is normal. But in the paper now reviewed there are too many typographical errors, as if hastily proof-read. Other kinds of mistakes are also too often apparent. On page 460, line 22, "loc. cit." fails to lead back to any appertaining citation; specimens referred to on page 464, line 5 from bottom, and page 465, top line, are in M.V.Z., not "M.C.Z."; the "resident" range of *bryanti* (in the current application of this name) is declared (p. 473) to extend south to "Ventura County, California"; and at the same time individuals from "San Mateo" County, as well as Los Angeles County, are stated to be "obvious intermediates" between *bryanti* and *beldingi*. San Mateo County is, of course, on San Francisco Bay next door to San Francisco. However, there is the chance that these last two "mistakes" (supposed so to be by me on basis of my knowledge of materials at hand) are really matters of differing interpretation.

Here and elsewhere in the paper there is lack of clarity in certain vital concepts. Individual variants within the metropolis of a given race that happen by chance closely to resemble or even exactly to duplicate individuals from within the metropolis of another race, the authors seem inclined to call by the name of the latter race; at least in *some* places they do. My idea, and I am sure this is the idea of many another systematist, is that relationship to the population of which a bird is a member must be given foremost consideration, over any certain resemblance to members of another race. That is to say, probable blood relationship is often more important in making subspecific determinations of individual specimens than some feature of outward appearance, *when* place of occurrence militates accordingly.

General meaning in some other connections is not clear because, I think, the special meanings or applications of words or phrases have not been carefully weighed. Ambiguity results. For example, the word "distinct" in different places shows a sliding scale of meaning; so also "typical," as I have pointed out in another paragraph. The combination of "much" with descriptive words—"much paler," "much heavier," "much broader"—is often an actual exaggeration. "Very faintly" (p. 467), "very few," "very different," "very much darker" (pp. 472, 473), are not too happy; indeed the word "very" might with profit be expunged from our descriptive vocabulary!

Now some commendatory comment: The authors are sharply condemnatory of "opera glass" identifications of subspecies. I completely agree with them in this stricture. The subspecific identity of Savannah sparrows is well-nigh certain when an observer is on the breeding grounds of a race in the height of the nesting season, especially if authentically determined specimens from there be contained in a collection accessible for ready reference. But when it comes to trying to name subspecies of birds seen at migration times or in winter, it is, as a rule, folly to use subspecific names. Let the specific name then suffice! Seasonal bird lists published in reputable journals are full of such unwarrantable namings. Only where use of the gun has been resorted to should subspecific names be given, though I grant there are (rare) exceptions to this dictum.

Another thing I commend: Peters and Griscom in naming their two new races selected simple but appropriate Latin adjectives—*oblitus* and *crassus*—short, easy to spell and remember. They did *not* foist into nomenclature any long combination of classic

roots such as no Greek or Roman ever heard of, no barbarous geographic name, no personal name. Blessings on them—in *this* regard!

With both the biological and nomenclatural findings as regards eastern North America, except as to the status of *princeps*, the present reviewer can find no serious fault. Indeed, he is impressed with the propriety of the authors' conclusions, as consequent upon the evidence and discussion presented. But when it comes to the nomenclature of the Pacific Coast forms, objection arises in his mind, to the point of protestation. What was *bryanti* becomes *alaudinus*; what was *alaudinus* becomes *anthinus*; what was *anthinus* becomes *crassus*! If these shifts be generally adopted, think of the confusion thenceforth in the literature! The *concepts* of the subspecies concerned remain practically the same. Moreover, are these confusing transfers of names really justified under the rules of nomenclature? If, in final analysis, they are, then isn't it time to invoke suspension of the rules, in the interests of *ornithology*, in order to retain the long-used names?

The ground advanced by Peters and Griscom for their so-positive statement that the "local coastal race, better known as *bryanti* to American students, must [*sic!*] now be known as *alaudinus*," is that the "type" specimen of the latter turns out to be identifiable with the San Francisco Bay race, and the name *alaudinus* of Bonaparte has long priority over *bryanti* of Ridgway. The case for such change is really not at all so clear-cut: (1) Bonaparte's description by itself is brief and indefinite as applying to any one race of *Passerculus sandwichensis*; (2) the locality given is merely "Californie," although the indicated collector, P. A. Delattre, can be shown to have been in the vicinity of San Francisco or Monterey from August, 1851, until about the same month in 1852 (see Palmer, Condor, vol. 20, 1918, p. 123), but the exact places and dates of his collecting can now only be guessed at; (3) so early as 1853 there was no such thing as a type specimen in the modern sense—certainly Bonaparte did not indicate one in his description of either *alaudinus* or *anthinus*; (4) a "type," subsequently so marked, in the Paris Museum, is reported to be a mounted bird, worn and faded, furthermore "partially albinistic" (van Rossem, Trans. San Diego Soc. Nat. Hist., vol. 7, 1933, p. 346). This type specimen has been examined by just one person, as far as I know, who is competent to identify it subspecifically—van Rossem as above, who declares it to be an "example of the Bryant's Marsh Sparrow, normal [*sic*] in size and proportions and as regards the unaffected portions of the plumage."

Before he had seen this type, the same author made some comments which may be quoted as significant in the present connection (van Rossem, Trans. San Diego Soc. Nat. Hist., vol. 6, 1931, p. 297): "In the matter of Dr. Oberholser's treatment [Cleveland Mus. Nat. Hist., Sci. Publ., vol. 1, 1930, pp. 109-111] of the races of *Passerculus sandwichensis*, however, such name changing as is involved in the case of *nevadensis* seems to me to be wholly unwarranted. While Bonaparte's description of *alaudinus* is vague in the definition of essential characters, this very vagueness eliminates any necessity for a switching of names. . . . Under these circumstances I cannot see any advantage in adopting the proposed transfer of a long-established name [in this case, *alaudinus* to apply to *nevadensis*, as proposed by Oberholser] with the consequent confusion of literature. Neither can I subscribe to the 'lumping' of the Savannah sparrows of the northwest coast and of the interior [as also had been proposed by Oberholser]. If the coastal race (*anthinus*) is to be merged with any other it must be with *savanna*, but certainly not with *alaudinus*, using the latter name in the sense employed by Swarth and authors in general." Pretty much these same, one-time sentiments of van Rossem

hold with me as regards Peters and Griscom's ruling to supplant the name *bryanti* with that of *alaudinus*, with its train of consequences.

A point in Bonaparte's descriptions (*Comptes Rendus*, vol. 37, 1853, pp. 918, 920) that seems not to have been given weight by any of the recent proponents of name shifting is this: Bonaparte describes his *Passerculus alaudinus* first, comparing it with *P. savanna*, and then follows immediately with *Passerculus anthinus*, which he declares to be still smaller than *alaudinus*, with a more slender bill, and more heavily marked below. How that comparison can be reconciled on the basis of Bonaparte's *alaudinus* having been a specimen of *bryanti*, I cannot see! Indeed, long before Ridgway named *bryanti*, the existence of a small dark bird in the San Francisco Bay region evidently had been known to Baird and Cooper, and for it they used the name *Passerculus anthinus*, probably taking Bonaparte's description at face-value and disregarding the locality "Kodiak." Yet another nomenclatural course might thus be suggested—with much vigor! (For this and other angles, see Ridgway, *Proc. U. S. Nat. Mus.*, vol. 7, 1884, p. 517, footnote; Grinnell, *Condor*, vol. 3, 1901, p. 85.)

And some further considerations bear on the question of typeship: The range of the resident race *bryanti* in the San Francisco Bay region is notably limited—to the salt marshes; additionally, there is an upland, bald-hill, summer-resident form, not yet named though possibly not satisfactorily definable (see Mailliard and Mailliard, *Condor*, vol. 22, 1920, p. 63); there are possibly five winter-visitant races, *brooksi*, *nevadensis*, *sandwichensis*, *anthinus*, and *alaudinus*, using these names in their current application, not in that of Peters and Griscom; of these, *alaudinus* is the most abundant and widespread locally, and it arrives as early as the first week of September and some individuals may tarry until the last week of April; in aggregate numbers *alaudinus* probably exceeds the combined populations of all the other races, both resident and winter-visitant; variation is very great; through the years, I have spent much time on the large series in the Museum of Vertebrate Zoology, trying to discharge the curatorial "duty" to put *some* name on each and every specimen; the simple fact is that many specimens are so equivocal in characters as to defy naming except by fiat (this I have *done*, admittedly, but have no defense to offer except that of meeting the need for *some* designation to use in our system of cataloguing!); van Rossem's measurements of the supposed type of *alaudinus* are not one of them exclusive for the race *bryanti*; axiomatic is the statement that the larger the amount of material of a related group of subspecies available the more extensive the range of variation manifest, and the more individuals of uncertain appearance turn up. What, then, of the genetic significance of any single Savannah sparrow from the San Francisco Bay region? Can it be positively identified as of one race or another, *except* upon clear manifestation of all or of at least the most diagnostic of the characters of one of the races? What of the allocation of a *supposed* type specimen which is stated to be abnormal in some respects, even though "normal" (for *bryanti*) in others? In the face of this concert of doubts, "must" a change of names be made?

Then as to the proposal of supplanting the name *alaudinus* with *anthinus*, made so vigorously by Peters and Griscom, much the same considerations as I have offered above may be urged against this change, irrespective of the item of page-priority of the former name. Bonaparte's description of *anthinus* is likewise too indeterminate for definite application to one subspecies, at least two of which must occur at times on Kodiak Island, the locality whence *anthinus* was ostensibly named. No "type" specimen whatsoever is known. I do not feel it necessary to accept the dictum of Peters and Griscom (p. 465) "that as the type-locality [sic] of *anthinus* is Kodiak Island, the

characters of the subspecies *anthinus* will be those displayed by a definitely breeding [sic] series of Kodiak Island birds." The basis of the original description *might* have been a migrant of *alaudinus* (in the current application).

Incidentally, I have just closely re-examined the basis (type, no. 3476, Stanford Univ. Mus.) of the name *xanthophrys* Grinnell (Condor, vol. 3, January, 1901, p. 21). This specimen was collected at St. Paul, Kodiak Island, July 18, 1897, and is undoubtedly representative of the breeding population, as are two other specimens at hand. I may say that when I bestowed this "new" name, I was blissfully unaware of Bonaparte's *anthinus*; Ridgway's Part I, giving synonymies, was not yet out. Even so, *xanthophrys* is an identifiable name applying definitely to the Savannah Sparrow breeding on Kodiak Island. But here is a vital point: The three Kodiak birds at hand are unequivocally similar in every respect to birds from the Sitkan district of southeastern Alaska. This fact was brought out with his customary clarity by Swarth (Pac. Coast Avif. No. 22, 1934, pp. 48ff.). And yet this exposition by a systematist experienced first-hand with Alaskan biogeography, and with adequate pertinent material before him, was discounted at this vital point by Peters and Griscom!

If, as seems from the above arguments, *Passerculus anthinus* Bonaparte is not sub-specifically identifiable with certainty, I suggest choice of two nomenclatural courses: (1) For the A. O. U. Committee to rule the name *anthinus* as out of consideration, to be regarded thenceforth as a "nomen expurgandum." Thereby the race of southeastern Alaska would become known as *P. s. xanthophrys* Grinnell, and the name *crassus* of Peters and Griscom would fall as a synonym. (2) For the A. O. U. Committee to declare the name *anthinus* usable in the application set forth by Swarth, thus to heed prevalent custom and thereby avoid synonymous confusion. Then both *xanthophrys* and *crassus* would have to ride as synonyms under *anthinus*, and the latter, the best known name for the Sitkan race, would continue in use. I, personally, choose the second course.

Peters and Griscom dwell (pp. 466-467) at length and informatively upon the perplexing kinds and amounts of variation in the Savannah Sparrows they encompass with the name *anthinus*. I merely add my concurrence, save in one point of proffered explanation, wherein they say they "are forced to believe that there is a certain amount of dichromatism." If this should in truth obtain, then the value of the main color character (richer, reddish brown) of their *crassus* is discounted. But I myself cannot see any basis for recognizing a condition of dichromatism in *Passerculus*; I can see wide range of color tone in breeding series from given localities, but nothing bi-modal in nature.

Might I at this point indulge in an animadversion? There seems to me to have been a certain nomenclatural exuberance on the part of several of the contemporary systematists cited in this critique—a sort of over-enthusiasm to seize an opportunity to change a bird's name. I will confess that I myself have experienced the thrill, doubtless also theirs, upon finding a current name (dating from another author!) apparently pre-occupied. I now look back upon certain of my own writings with more or less amused regret. Someone said with a great measure of truth over one hundred years ago: "Opinions are always to be distrusted which are formed during the ardour of novel investigations." *Time*, for calm, impersonal reflection, is required for bringing final, correct appraisal.

Now if, in the mind of my reader, who will also have read searchingly everything in the Peters and Griscom paper to see that I have done those authors no polemic injustice, I have not succeeded in establishing some of the dubiety that exists in my own mind concerning the wisdom of making those proposed name-shifts, then I make

recourse to another suggestion, already hinted at. This is to appeal to the principle of "nomina conservanda," for application to specific and subspecific names, as it is already in force under the activities of the International Commission on Zoological Nomenclature for preserving certain chosen generic names. Under these rulings, each case being carefully considered, the regular Rules of Nomenclature are set aside, and the preferred names are placed on an "Official List," then permanently beyond reach of name-shifters. Was it not to secure permanence and stability that the whole system of nomenclatural rules was set up in the first place? Sometimes it looks as though only instability had been achieved!

I therefore here and now propose that the A. O. U. Committee on Nomenclature put upon its agendum for early serious consideration, and *action* (at least as far as positive recommendation to the International Commission), this matter of *preserving* designated well-known subspecific and specific names of North American birds against replacement on any grounds whatsoever. Such action might be made conditional upon the extent of the use, in ornithological literature, say to the extent of more than 50 percent of all citations, of any one name over against a challenging name. Some cases now gone by might well be given *ex post facto* attention.

It is commonplace to urge that the naming of animal kinds, on the basis of any sort of rules, is merely to help in obtaining and filing knowledge of those kinds. Many people have thought that our present so-cumbersome system of scientific names might well give way to some simpler method of designation, by symbols or formulae, and maybe it will; but no one yet has invented any acceptably workable method, one that will not only provide permanent designations but that will indicate phylogenetic relationships. The present system will survive to subserve its best use throughout the vast breadth of modern ornithology, and will endure, just to the extent that these functions of nomenclature are carried out. It is high time that permanence of names, in fact, is attained.

Let it not be inferred that I belittle the position of systematy (apart from nomenclature) as a most worthy field of ornithological research. That portion of Peters and Griscom's paper which sets forth their findings with regard to geographic, age, seasonal and sexual variations, to determination of truly racial characters, to the correlations of these with environments, in other words, to the evolutionary results at the present time level in the group of birds studied—all this is real contribution to science. Nor do I at all inveigh against further refinements in subspecific splitting, just so thorough descriptions are given of the forms newly defined, adequately justifying their recognition. Indeed, I think these authors were decidedly conservative in having named only two supposedly new forms in a group that is so variable and that had not before been revised as a whole. As they themselves say, additional geographically representative material will warrant another revision and will likely lead to the formal recognition of yet other racial populations.

No, it is not the ornithological content of the Peters and Griscom paper that I would criticise adversely in any serious degree, but the nomenclatural content which unfortunately is so mixed in with the other as to form a barrier to the reader's understanding. On this score my main protest is *against the shifting of names* whereby our present literature concerning the natural history of the forms affected would become everlastingily confused. This can be avoided by the simple expedient, on the part of the established American authority in questions of nomenclature, namely the A. O. U. Committee, of fixing those long-used names in the status of *nomina conservanda*. And eventually the practice should become world-wide.

With all the above reflections induced by Peters and Griscom's paper, along with the extensive factual data they give, and after a re-examination of the collection of Savannah Sparrows in the Museum of Vertebrate Zoology, I venture to offer the following list of names as the ones in my present judgment best to use. Vernaculars are chosen mainly with regard to precedent. The sequence also is chosen with thought.

- Passerculus princeps* Maynard. Ipswich Sparrow.
Passerculus sandwichensis labradorius Howe. Labrador Savannah Sparrow.
Passerculus sandwichensis savanna Wilson. Eastern Savannah Sparrow.
Passerculus sandwichensis oblitus Peters and Griscom. Canadian Savannah Sparrow.
Passerculus sandwichensis alaudinus Bonaparte. Western Savannah Sparrow.
Passerculus sandwichensis nevadensis Grinnell. Nevada Savannah Sparrow.
Passerculus sandwichensis brooksi Bishop. Dwarf Savannah Sparrow.
Passerculus sandwichensis brunnescens Butler. Mexican Savannah Sparrow.
Passerculus sandwichensis sandwichensis Gmelin. Aleutian Savannah Sparrow.
Passerculus sandwichensis antithinus Bonaparte. Kodiak Savannah Sparrow.
Passerculus sandwichensis bryanti Ridgway. Bryant Savannah Sparrow.
Passerculus sandwichensis beldingi Ridgway. Belding Savannah Sparrow.
Passerculus sandwichensis annulus Huey. Scammon Lagoon Savannah Sparrow.
Passerculus rostratus sanctorum Ridgway. San Benito Island Large-billed Sparrow.
Passerculus rostratus halophilus McGregor. Abreojos Large-billed Sparrow.
[*Passerculus rostratus guttatus* Lawrence. San Lucas Large-billed Sparrow. (Status uncertain.)]
Passerculus rostratus rostratus Cassin. San Diego Large-billed Sparrow.
Passerculus rostratus atratus van Rossem. Sonora Large-billed Sparrow.

Museum of Vertebrate Zoology, University of California, Berkeley, March 17, 1939.