

CORRESPONDENCE

Sir:

The confusing leading article of the 1961 *Auk* calls for comment. Despite his title, "Evolutionary Relationships among the North American Mallards," the author discusses primarily geographic distributions, population sizes, egg-white proteins(!), and certain behavioral patterns. Maps are based on ". . . the literature, personal communications, and the major United States collections," ". . . and from additional sight and specimen records available to me"; yet only two museums other than Cornell University are mentioned in the acknowledgments. These maps show many records of *platyrhynchos* far to the south; sometimes (Figure 1) the reader must search hard for any hint that these are not breeding localities. The section "Estimation of Gene Pools and Hybridization Incidence" discusses only a part of the area of present overlap of breeding *Anas p. platyrhynchos* and *rubripes*; these population estimates are worthless from most stand-points, since all were made in fall and winter, thus consisting of birds from very diverse areas, some of them outside of the zone of overlap. The section "Materials and Methods" tells us neither the source of the specimens examined nor what measures, if any, were taken to assure the purity of their strain. Instead, we read a long account of the measurement of general plumage darkness, a matter never considered of primary importance in this group by taxonomists. As was therefore predictable, this proves to have little real value. The taxonomically useful characters are mentioned by Johnsgard only in summarizing the literature, after which he ignores all of them except the secondary coverts! He merely states that ". . . supposed differences in speculum coloration . . . and the degree of streaking on the throat . . . were not considered of major importance for study."

Under "Evolutionary Implications" we read that sexually nondimorphic populations arose by the same mutation at three different times and places; whereas actually a consideration of the *entire* mallard group, including the Pacific island forms, points strongly in the *opposite* direction, *i.e.*, the acquiring once in a nondimorphic species of sexual dimorphism, a character that is still spreading out geographically.

The only original taxonomic comments in this lengthy article are: "The later description of the Mottled Duck . . . weakened these distinctions and left no clear-cut difference between these forms [*fulvigula* and *maculosa*] and the Black Duck"; "None of the described plumage or soft-part characters, aside from the sexual dimorphism of *platyrhynchos*, were found to be of absolute diagnostic value in differentiating any population from all other populations"; and "The described plumage differences . . . are scarcely valid characters on which to base species judgments." (Voice, plumage sequences, nests and eggs, and juvenal plumages are not even mentioned.) The conclusion therefore comes as rather a surprise: "I am in firm agreement with Delacour (1956) that *diasi* and *fulvigula* should be considered subspecies of *Anas platyrhynchos*," whereas "An accurate and completely satisfactory disposition of *rubripes* cannot, in my opinion, be made. No modern taxonomist has as yet formally proposed the conspecificity of *rubripes* and *platyrhynchos*," though Baillie suggested this and Trautman calls them "not 'good' species."

As a matter of fact, in a symposium on speciation in 1957 (*J. Ariz. Acad. Sci.*, 1 (1), 1959), I pointed out the Mallard-Black Duck group as an example of the famous "open-ring form," and pointed out how failure to recognize this had led

to erroneous records of northward straying by *diazi novimexicana* and *fulvigula maculosa*, by those who refuse to recognize open-ring forms in North America. Since this new journal may not be available to some readers, I quote excerpts: "Female and eclipse plumages, voice, and ecology, at least, are very similar throughout the group. The main morphological gap is in New Mexico, where the familiar green-headed Mallard drake becomes a hen-feathered male (*diazi*) much like a female Mallard. There is little or no good evidence of reproductive isolation in the Albuquerque region and northward, where a careful study is needed; breeding experiments should also be conducted to expose the genetic basis. Farther southeast and east we have *maculosa*, north of which nests the Black Duck, *ribripes*, partially overlapping the breeding range of *A. p. platyrhynchos*, with only limited hybridization. . . . Differences between this case and the famous one of the Herring and Lesser Black-backed Gulls (*Larus argentatus*) are: (1) the step from *platyrhynchos* to *diazi*, in males, is steep phenotypically; and (2) reproductive isolation of the overlapping ends is incomplete. . . ."

For the benefit of those to whom a simple statement of the facts is too "informal," I hereby list some taxonomic changes in the mallard group which I then stated or implied to be necessary, and which have not been made in subsequent Check-lists:

Anas platyrhynchos diazi Ridgway.

Anas platyrhynchos fulvigula Ridgway.

Anas platyrhynchos rubripes Brewster.

These and other changes in other groups I still consider necessary. As to *Anas*, neither Johnsgard nor anyone else has produced good evidence that any of the four main forms under discussion is more closely related to any other than to the rest of the group. Thus I perceive no factual basis for his uniting *maculosa* with *platyrhynchos* while keeping it distinct from the more similar *rubripes*. Parkes (*Annals Carnegie Mus.*, 35: 120-121, 1958) has already pointed out that none of these half-way measures gives a good picture of the facts; we must either retain the old A.O.U. arrangement of four species, as Parkes tentatively suggests, or recognize nomenclaturally that we have here a simple open-ring form. The ducks themselves, from New Zealand to the Maritime Provinces (see Boyer, *Can. Field-Nat.*, 73: 1-5, 1959), favor this recognition.

As a side comment, nomenclatural recognition of the obviously close relationship of all these ducks would have the *practical* value of discouraging further squandering of funds for the dubiously useful purpose of mongrelizing the breeds any more than they are already mixed. The ornithologist cannot expect the public to heed the admission of the facts when he himself fails to express them plainly.

Johnsgard's paper does, however, have one heartening and commendable feature: the mis-named "Hybrid Index" appears here simply as "Index." We are spared from reading that the Mexican and Mottled Ducks are all "hybrids." A hybrid, properly, is and has always been a cross between two distinct biological species, the classical example being the mule. Real hybrids are rare and often sterile. Geneticists have done biology and our language a grave disservice by using the word "hybrid" (instead of cross, intermediate, or mongrel) for a cross between two more-or-less different-looking organisms *within* a species, and ornithologists were ill-advised to follow suit. Since two animals are rarely exactly alike if carefully analyzed, "hybrid" in the geneticists' sense, "hybrid swarms," "hybrid index," "introgression," etc., etc., are meaningless words having nothing to do with true hybrids. The distinction between such commonplace crosses and true hybrids

was very clearly and carefully pointed out a century ago by Darwin, and it is most regrettable that modern zoologists are so unfamiliar with the basic literature of their science! Ornithologists once were well aware of this distinction; for example, Chapman (*Bull. Am. Mus. Nat. Hist.*, 13: 320, 1900) applied the term "intermediates," rather than "hybrids," to what he thought were specimens intermediate between the Eastern and Western Meadowlarks (*Sturnella magna* and *neglecta*), which he supposed to be conspecific. Others would have done well to follow these good examples. The words we read would then have some meaning. Johnsgard's paper marks a slight step in the direction of clarity; let us continue!—ALLAN R. PHILLIPS, *Instituto de Biología, Universidad Nacional Autónoma de México, México, D.F.*

Sir:

I would like to reply to Dr. Phillips' criticisms of my paper, as they seem to stem mainly from my failure to consider (or cite) his open-ring interpretation of the forms involved. I will consider his arguments in sequence. In Figure 1 breeding Mallard records are indicated by the combination of an upright triangle and a "B," as indicated in the legend. I find no difficulty in locating these symbols on the map. The museums cited were those which I personally visited and where I examined specimens; in the cases of other major waterfowl collections (U.S. National Museum, Chicago Museum, Univ. of Calif.) data on specimens were kindly provided by other persons.

The estimation of gene pools took into account all major wintering areas involving sympatry, with Mallard and Black Duck population estimates being calculated on a state-by-state basis. That the estimations of hybridization incidence did not include all states in which sympatry occurs is regrettable and unavoidable; however, I feel that the numerical estimates, based on nearly 57,000 birds, compare well with estimates of hybridization incidence available for other species (see Miller, 1955, *Recent Advances in Avian Biology*, pp. 1-22). I do not agree that fall and winter population estimates are "worthless," for such estimates indicate degree of sympatry during the period of waterfowl courtship and pair formation when isolating mechanisms must be most effective. In both Mallards and Black Ducks the midwinter period is the time of greatest courtship activity (Johnsgard, *Wils. Bull.*, 72: 133-155; Ramsay, *Wils. Bull.*, 68: 275-281).

I am unaware of any methods which might be used to test the "purity of the strain" of museum specimens other than by measuring the plumage variations in the manner I did; whether such variations are the result of hybridization or individual plumage variations is of course sometimes impossible to determine. To be certain of obtaining only "pure" Black Ducks one would be forced to use only specimens collected north and east of Massachusetts previous to 1900, "pure" Mottled Ducks would be available from only well south of the Mexican border, and scarcely any Florida or Mexican Ducks could be utilized at all.

The "taxonomically useful" characteristics of speculum color and throat streaking may be seen, by the slightest investigation, to be almost valueless. Thus J. Phillips (*Auk*, 29: 295-306) points out: "The speculum color of *diazi* varies, as it does also in *A. platyrhynchos* and *A. tristis*, from a metallic violaceous green to a violaceous purple. This difference has apparently nothing to do with age or sex and is not a character of specific importance, except within wide bounds. It seems to have been used too frequently in describing species differences." Kortright (*Ducks, Geese and Swans of North America*, p. 171) says of the Florida

Duck's speculum: "Variable, from green with strong purplish gloss to almost solid purple. . . ." Cheek and throat streaking varies both in extensiveness of the brownish streaking and the degree of streaking in exactly the same way that the larger body feathers vary in the proportions of dark and light coloration. As the latter was easier to judge quantitatively I used it. Reference to the original descriptions of the Florida Duck (Ridgway, *Amer. Nat.*, 8: 108-111), Mexican Duck (Ridgway, *Auk*, 3: 331-333) and Mottled Duck (Sennett, *Auk*, 6: 263-265) would convince Dr. Phillips that the variations in body mottling and relative amounts of fulvous, ochraceous or dusky body coloration were major bases for the erection of these forms.

There is no evidence whatsoever that sexual dimorphism in the mallard group is spreading out geographically; *A. p. platyrhynchos* and *A. p. conboschas* are the only two of the 20 forms of mallard-like ducks which exhibit strong sexual dimorphism. In common with the other *Anas* species the trend in mallards is towards loss of sexual dimorphism wherever allopatric populations are formed (see Sibley, *Condor*, 59: 166-191).

Dr. Phillips may feel justified in thinking that I did not contribute any major "original taxonomic comments" as a result of my studies, and thus have no basis for my conclusions and suggested taxonomic changes. I would, however, like to point out that the four possible changes in the A.O.U. *Check-list* which I suggested were the result of three years' full-time study. Dr. Phillips has recommended three changes for the mallard group in the *Check-list* without presenting any original evidence and ignoring much of the evidence which is available. In discussing the mallard group, for example, he cites only one reference (Bent's *Life Histories of North American Wild Fowl*). I am not suggesting, however, that I disagree with his proposed changes. We are in fact in essential agreement regarding the conspecificity of the mallard-like ducks. I regret that I was unaware of his 1959 proposals that *rubripes* be considered conspecific with *A. platyrhynchos* and that the whole group of North American forms might be an example of the "open-ring" type of speciation. I considered the open-ring possibility at the onset of my own studies, but soon discarded the idea as unsupported by the evidence. My primary objection to it is that it requires a major geographic barrier around which the "ring" can be formed. Dr. Phillips hypothesizes the Great Plains prairies as such a barrier ("The open ring thus surrounds the unoccupied Great Plains area"), yet how these prairies, which are the most favored of all types of waterfowl habitat, could serve as such a barrier, remains inexplicable to me. In addition, the Florida Duck should, by this explanation, be the Black Duck's closest relative, yet Delacour (*The Waterfowl of the World*, Vol. 2, p. 53) states that if given the opportunity Florida Ducks will mate with Mallards in preference to Black Ducks. Furthermore, if the Florida Duck gave rise to the Black Duck then one would expect that a greater overlap of features should exist between them. Instead, they differ markedly in plumage and ecology. However, as indicated in my paper, I favor only subspecific recognition of all the forms concerned. Dr. Phillips and I therefore differ primarily in the hypothesized mechanism of speciation.

I agree that "hybrid" is a term that strictly speaking should refer to species crosses. However, since there is a continuous genetic gradient between two individuals and two species and thus the point at which complete speciation has been achieved must always be a subjective judgment, I believe that it is not practical to hold to this definition. Since Mexican, Florida and Mottled ducks are

obviously not the result of secondary contact between Mallards and Black Ducks they would in any case not be "hybrids."—PAUL A. JOHNSGARD.

Dear Sir:

After seeing my note (*Auk*, 78: 275) David K. Wetherbee called my attention to his paper (1959 *Bird Banding*, 30: 119-121) entitled "Egg teeth and hatched shells of various bird species," which should have been referenced in my note. He has described a normal second egg tooth on the lower bill of Mourning Doves that has a posteriorly directed point. His description matches my observations on a recent Mourning Dove squab that I examined from the day of hatching. The normal egg tooth on the lower bill has persisted over a week. The structure pictured in my note in *Auk*, however, is larger, did not possess a posteriorly directed point, and is of a different texture (more calcified?). I believe the structure I pictured is an aberrant form of the normal lower egg tooth.—WILMER J. MILLER.