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Letters

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Letters to *The Auk*

Ornithologists have seen opportunities to speculate in print all but disappear in recent years, and there has been a reluctance to publish new ideas, especially controversial ones. I believe that a certain amount of speculation is good for our science, because it stimulates others to think beyond their data and to ask new questions, thus possibly generating new research. Some ideas lead nowhere, but we should be able to take some risks and make mistakes occasionally. Thus, I am introducing a section called "Letters to *The Auk*." In addition to promoting new ideas, readers will be able to comment on recent issues, topics, and methodologies, as with the "Commentaries" currently published, but in a much shorter form. "Letters" also can be used to provide information of historical or taxonomic interest and for commenting briefly on papers published in *The Auk* or responding to such comments. Letters will not replace scientific notes or short communications that were published previously.

This section revives a feature of *The Auk* that ran from 1884 to 1948. The first Editor, Joel Asaph Allen, requested correspondents (Auk 1:100) "…to write briefly and to the point. No attention will be paid to anonymous communications." Montague Chamberlain published the first letter (pp. 100–101), saying he was perplexed by the use of trinomials by American ornithologists. The Editor responded to the contrary and, in the following issue, two more letters addressed this subject: Elliott Coues (pp. 197–198) cited a draft of his "Key" in supporting Allen; Chamberlain (pp. 101–102) thanked Allen for clarification but remained unconvinced. Allen stood his ground and cut off correspondence on the subject after that. This exchange is a good example of the kind of dialogue we hope to encourage.

Letters should be submitted directly to the Editor by mail, fax, or e-mail, but not through Rapid Review. Manuscripts should not be more than 3–4 pages in length and will be reviewed by the Editor and at least one outside reviewer. Letters will be published at the discretion of the Editor and exchanges concerning published papers will be strictly limited.

—S.G. Sealy, Editor

The Auk 122(1):347–348, 2005

Criteria for sexing birds in studies of sexual dimorphism and sex-specific behavior.—Emlen and Wrege (2004) reported interesting data on sexual dimorphism, sex-specific behaviors, sex ratio, and sexual selection in Wattled Jacanas (*Jacana jacana*). However, they did not state anywhere in their paper how they sexed the birds that they studied. They stated (p. 393) that Wattled Jacanas in Panama are sexually monomorphic in plumage and ornamentation. If they sexed them by size, then their conclusions about sexual dimorphism in size would be circular. If they sexed them by behavior, then their conclusions about sex-specific behaviors would be circular. If they sexed them by DNA or some other reliable character, they should have stated this clearly. Although they may well have sexed all the birds reliably, the paper as presented does not exclude the possibility that some large males behaved like females, or that some small females behaved like males. Without a clear statement of what the authors actually did, the reader cannot form an opinion about the validity of the results, and the study cannot be replicated. These comments apply not only to the study by Emlen and Wrege, but more generally to all studies of sexual dimorphism and sex-specific behavior or ecology. Such studies have limited value unless reliable, validated methods are used for sexing, and the criteria used to assign individuals to sex are reported clearly.—Ian C. T. Nisbet, *I.C.T. Nisbet & Company, 150 Alder Lane, North Falmouth, Massachusetts 02556, USA. E-mail: icnisbet@cape.com*

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Emlen, S. T., and P. H. Wrege. 2004. Size dimorphism,

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intrasexual competition, and sexual selection in Wattled Jacana (*Jacana jacana*), a sex-role-reversed shorebird in Panama. Auk 121:391–403.

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Sexing criteria, accuracy, and statistical inference—A reply.—Through his questioning of the validity of various analyses in our paper (Emlen and Wrege 2004), Nisbet (2005) teaches a valuable lesson regarding the importance of clear methods and avoiding circularity in statistical analyses. Nisbet is correct to criticize us for failing to detail our sexing methods, but we show below that the analyses in our paper are both valid and robust. Characteristics of our study species, combined with intensive observation, made our sexing of individuals extremely reliable, but we argue more generally that a low frequency of errors in assigning subjects to groups does not necessarily compromise conclusions drawn from statistical inference.

In our 2004 paper, we erred by not explicitly describing how we sexed our study animals. Resident Wattled Jacanas (*Jacana jacana*) (i.e. adults defending exclusive territories and observed displaying either sexual or reproductive behavior or both) were sexed initially through behavioral observations (764 h of focal behavior samples, plus uncounted thousands of hours of *ad libitum* behavioral observations), confirmed by measures of mass for nearly all individuals. We found no overlap in the mass distributions of resident males and females (Fig. 1A). Similarly, the mass distributions of floater males and floater females did not overlap each other (Fig. 1B). Although we could not use behavioral criteria to sex floaters, the clear difference in mass, as well as an easily perceived "chunkiness" to females in the hand, made sexing decisions seem unambiguous. Nonetheless, our Table 1 (Emlen and Wrege 2004) pools residents and floaters of each sex and, therefore, those data were inappropriate for a statistical test of mass difference between males and females. Nisbet correctly points out that this is circular, because the sexing of floaters was based at least in part on their mass at capture. That circularity, however, does not extend to tests of sexual size dimorphism in other characters (nor, if sexing was based on behavioral criteria, to the testing of all hypotheses about sex-specific behavior). For example, in our Table 1, we examined the sexual dimorphism of morphological characters associated with territorial defense (wing spur) and sexual signaling (shield and wattle size and color), none of which would *a priori* vary with body mass. Even purely structural characters, such as tarsus length, are not necessarily correlated with body mass. In situations where the grouping character is necessarily highly correlated with some other character of interest, statistical methods can be used to examine residual differences, after removing the correlated component.

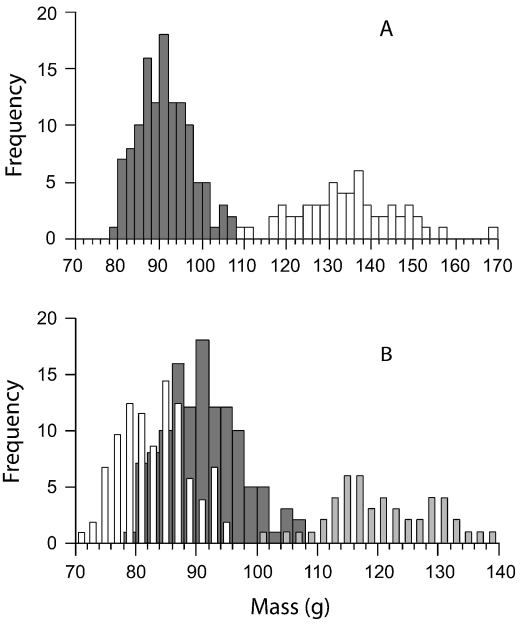


Fig. 1. Mass distribution of male and female Wattled Jacanas, 1991–1995. (A) Resident males (dark bars) and resident females (white bars). (B) Floater males (white bars), resident males (dark bars), and floater females (light gray bars). Note different scales on the abscissae. Data from Table 1 of Emlen and Wrege (2004).

Nisbet's letter raises another point with broad applicability: do errors in assignment of subjects to groups compromise the validity of statistical inference based on those groups? For example, Nisbet suggests that a few large male jacanas acting like females, or small females acting like males, could cast doubt on our conclusions about sex-specific behaviors and sexual selection. We disagree. Any method used to determine the sex of an individual (or determine any other form of group assignment) is subject to human error (e.g. measurement and recording error), including genetic methods for sexing (e.g. through contamination, scoring, and mislabeling error). In addition, errors may arise because of factors intrinsic to the method (e.g. overlapping distributions on the discriminating character[s]). From a statistical inference viewpoint, such errors often are not a problem, unless the errors are both relatively frequent and introduce a bias with respect to the hypotheses being tested. Unbiased errors, even if relatively frequent, increase unexplained variance and thus reduce the probability of type I error (i.e. rejecting a null hypothesis when it is true). However, unbiased

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errors would tend to increase type II error (i.e. accepting a null hypothesis when it is false), and could be misleading if, for example, data were pooled on the basis of failure to reject the null hypothesis. Clearly, researchers must assess the potential frequency of errors, and carefully examine whether such errors could bias tests of hypotheses.

Inspection of Figure 1 shows that the only sexing errors we might have made would have been to classify as male a small female floater. The intensive schedule of behavioral observation essentially eliminated the possibility that any such females would remain incorrectly sexed had they achieved resident status. As part of the male floater class, such individuals would appear as extreme outliers in the statistical analyses of resident versus floater morphology presented in our Table 2 (Emlen and Wrege 2004), and such outliers were not observed. Finally, small females classed as males during censuses to estimate the floater population would have tended to increase our estimates of variance in male lifetime mating success (and decrease that of females), a conservative contaminant that would not affect our conclusions.—Peter H. Wrege, *Department of Ecology and Evolutionary Biology* (e-*mail: phw2@cornell.edu) and* Stephen T. Emlen, *Department of Neurobiology and Behavior (e-mail: ste1@cornell.edu), Cornell University, Ithaca, New York 14853, USA*.

Acknowledgments

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Type locality of *Ammodramus bairdii* (Audubon).—The highlight of Audubon's expedition up the Missouri River was his stay at Fort Union from 12 June to 16 August 1843. Among those accompanying him were John Bell and Edward Harris, who procured two birds near Fort Union that proved to be new to science: Sprague's Pipit (*Anthus spragueii*) and Baird's Sparrow (*Ammodramus bairdii*). The third edition of the *AOU Check-list* gives as the type locality for the latter species "eastern Montana, near Old Fort Union, North Dakota" (American Ornithologists' Union [AOU] 1910). The type locality was changed to "prairie of the Upper Missouri = near Old Fort Union, North Dakota" in the fourth edition of the *Check-list* (AOU 1931). That change reflected the new practice of quoting a type locality verbatim from the author's original description, adding a restricted locality when possible (AOU 1931). Montana and North Dakota were not admitted to statehood until 1889, and Audubon gave the vague type locality of "wet portions of the prairies of the Upper Missouri" (Audubon 1844:359). The *Check-list* committee for the fourth edition presumably was content to assign the type to North Dakota because Fort Union occurred there. Deignan (1961:641) followed AOU (1931) but also added "Williams or McKenzie County, North Dakota." He noted, however, that "the exact locality for birds described from 'Fort Union' is uncertain" (Deignan 1961:474).

Fort Union was active from 1829 to 1867, the original structure being located on the north bank of the Missouri River in what is now North Dakota, about 5 km above the mouth of the Yellowstone River and less than 300 m east of the Montana border (Weist et al. 1980). The journals of Audubon, Bell, and Harris indicate that those men ventured well into Montana several times during the summer of 1843 (Bell 1843, Audubon 1897, McDermott 1951). On the basis of the information in those journals, we argue that the type specimen of Baird's Sparrow was taken in Montana, not North Dakota.

Bell shot the first Baird's Sparrow specimens on 26 July 1843, during a day of bison (*Bos bison*) hunting between the Yellowstone and Missouri rivers (Audubon 1897). On that morning, Audubon's party forded the Missouri River at Fort Union and headed up the Yellowstone Valley in a horse-drawn wagon, Harris stating that they used "the same road we took on our last hunt" (McDermott 1951:154). The road is first mentioned in Harris's journal entry for 20 July: "We mounted the hills by a middle road between the rivers [Missouri and Yellowstone]" (McDermott 1951:145). Harris mentioned the road again on 4 August, noting that it was "midway between the two rivers" (McDermott 1951:165). On 26 July, the party eventually crossed the "Fox River," now known as Fox Creek, which enters the Yellowstone River from the west 7 km above Sidney, Montana, and about 11 km west of the North Dakota border. There is no indication that anyone in Audubon's party crossed the Yellowstone River during the hunt. They spent the night near the "Three Mamelles," which Audubon illustrated in his journal (Audubon 1897). Audubon's drawing clearly depicts the "Three Buttes," which are located in southern Richland County, Montana, about 30 km west-northwest of the mouth of Fox Creek and

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40 km west of the North Dakota border (see Durant and Harwood 1980).

Audubon, Bell, and Harris were caught up in the excitement of the chase during their bison hunt on 26 July. So much so, in fact, that only Bell (1843) mentioned the sparrows in his journal entry for that day ("I killed 3 small finches, or buntings, very similar to the Henslow's Bunting…"). Bell did not say when during the day he shot the birds, or that they were new. The latter task fell to Elliott Coues in a footnote to Audubon's journal, who noted that "Among the 'birds shot yesterday,' July 26, when Audubon was too full of his Buffalo hunt to notice them in his Journal, were two, a male and a female, killed by Mr. Bell, which turned out to be new to science" (Audubon 1897: 116). The discrepancy in the number of sparrows Bell shot apparently results from an error by Audubon, because Harris mentions in his journal entry for 29 July that "Bell killed three finches…which so closely resemble Henslow's Bunting…" during the buffalo hunt of 26 July (McDermott 1951:161).

As they headed southwest toward Fox Creek on 26 July, Audubon's party would have entered what is now Montana very soon after crossing the Missouri River at Fort Union, and they would have remained in Montana until their return to the fort two days later. The Baird's Sparrow specimens could not have been taken in North Dakota unless they were encountered immediately after Audubon crossed the Missouri River that morning. Given that Audubon, Bell, and Harris had crossed the Missouri at Fort Union at least six times before 26 July (Audubon 1897, McDermott 1951), it is unlikely that they would have failed to find Baird's Sparrows previously had the birds occurred so close to the fort. Consequently, we believe that the type locality for this species should be revised to "prairie of the Upper Missouri = eastern Montana near Old Fort Union."—Jeffrey S. Marks, *Montana Cooperative Wildlife Research Unit, University of Montana, Missoula, Montana 59812, USA (e-mail: jeff.marks@umontana.edu) and* Ted Nordhagen, *P.O. Box 44, Westby, Montana 59275, USA.*

Acknowledgments

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