

tion based upon numbers of syllables per individual are not necessarily less in the Bon Portage birds. Syllable types were not defined/identified similarly in the three studies and, therefore, are noncomparable. There is little justification to assume males in Blacquiere's population had fewer syllables per song than the birds I studied. Not knowing exactly how Naugler and Smith identified and separated syllable types makes uncertain any inter-study comparisons based on values for numbers of syllables per song and syllable types per song. Accepting, however, their assessment as correct (Bon Portage males have fewer syllables and syllable types per song than males in the other populations), their error in following Miller's lead in misconstruing my data invalidates their assessment that Utah/Idaho males have a syllable diversity (1.0 rather than the true 0.74) greater than the Canadian birds. Further, their contention that the number of songs per male and by inference song complexity (songs/song types) were both greater in Utah/Idaho birds is false. Utah/Idaho males did possess a greater number of songs per individual, but their song complexity value (0.91) is less than that for Canadian birds (1.0). These assessments of complexity are central to their argument that Bon Portage birds have been effected by a bottleneck which resulted in depauperate syllable (and song) diversity. Lastly, Naugler and Smith's contention that Blacquiere's and my studies dealt with "resident populations" was false.

There is little to support the conclusions of Naugler and Smith. Their assessment of the selection pressures and historical events which have resulted in the pattern of syllable and song repertoire organization in Fox Sparrows must be rejected. The problems with this publication discussed herein beg comment regarding the failure of our manuscript review process. However, all I shall say is whomever reviewed this manuscript should have been knowledgeable enough about vocal behavior and geographic distribution in the *Passerella-Melospiza-Zonotrichia* complex to know which populations (races) were resident and which were migratory.

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SIMILARITY BREEDS CONFUSION: A REPLY TO MARTIN

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Martin (1993) has recently criticized a short communication describing the song structure of a population of Fox Sparrows *Passerella iliaca* (Naugler and Smith 1991). Contrary to Martin's assertion, the purpose of this paper was not to "test hypotheses about the evolution of the structure of song" but was rather more modest. We presented a descriptive study of song in an insular population and suggested that the observed patterns were consistent with the "founder effect hypothesis." We suggested that such an effect need not occur only when a population is founded, but also in small isolated populations where periodic bottlenecks could occur after seasons of high mortality. It will become apparent that while our original paper did contain several errors, the criticisms raised by Martin are either inaccurate or do not greatly change the trends reported by us, and that Martin's disparagement of the editorial review process is, perhaps, not deserved in this instance.

Martin's first criticism results from an unfortunate misreading of our original paper. He claimed that we incorrectly concluded that the populations of Fox Sparrows studied by him (Martin 1977, 1979) and Blacquiere (1979) were resident. When we stated that "the studies cited at the outset of this paper all deal with resident populations" (Naugler and Smith 1991:1003), we were referring to the papers cited in the opening paragraph of our study (i.e., Nottebohm 1969, Thielcke 1973, Munding 1975, Mirsky 1976, Baptista and Johnson 1982, Lynch and Baker 1986, Baker and Jenkins 1987), not to the work of Martin and Blacquiere which we first cited almost two pages later. Martin (1977) and Blacquiere (1979) both clearly state that the Fox Sparrows they studied were migratory.

Secondly, Martin has objected to our argument that the population described by Naugler and Smith (1991) was more isolated than the populations studied by Martin and Blacquiere. Martin has missed our point. We did not argue that the previously described populations were not "geographically distinct." Indeed "we

considered only published data from populations *with discrete boundaries* and of known population size" (Naugler and Smith 1991:1002). Our point was that unlike Martin's Utah/Idaho birds and Blacquiere's Newfoundland birds, the Bon Portage Fox Sparrows are several hundred kilometers from the nearest population of comparable size, and it is for this reason that we considered Bon Portage to be more isolated (in the biological sense) than the other populations.

Martin's third claim was that we (incorrectly) asserted that high inter-year site fidelity on Bon Portage demonstrated that "significantly more genetic exchange" may have occurred in other populations. As Martin (1993) has stated, the required data does not exist to make this comparison. However, a careful reading of our original work (Naugler and Smith 1991) will show that nowhere do we present such a comparison. Our argument was based merely on the comparative isolation of the Bon Portage population (Naugler and Smith 1991:1002).

Lastly, Martin criticized the parameters of song that we used to compare the different populations. He contended that the various populations are not comparable because different researchers may have used dissimilar criteria for classifying the various song characteristics (e.g., number of song types). It is true that when we compared published data, we made no qualitative judgment as to which authors, for instance, were "lumpers" and which were "splitters." Such inconsistencies in definitions are an insidious source of variation in comparative studies. Does this mean, however, that we should cease comparing the findings of different papers? Martin goes on to discuss "song complexity values" (songs/song types) for the Utah/Idaho and Bon Portage populations. Contrary to Martin, song complexity values were never given in our 1991 paper. Moreover, these values are perhaps not even germane to the original comparison because an average repertoire of three songs (Utah/Idaho) is certainly more complex than a repertoire of one song (Bon Portage), especially if more variation also exists among individual songs in birds possessing multiple song repertoires. Furthermore, Martin has made an error in giving the song complexity value of the Bon Portage birds as 1.0 (Martin 1993). We clearly stated (Naugler and Smith 1991:1002) that some intramale variation did exist and this would, therefore, give a song complexity value different from 1.0. Again, contrary to Martin, nowhere did we state that the Newfoundland Fox Sparrows (Blacquiere 1979) had fewer syllables per song than the Utah/Idaho birds.

Martin did make a valid point by stating that in our 1991 paper, several of the numbers given in Table 1 should be changed. The total number of syllables in the Cub River Canyon population should be 39, not 48.5, and the averages for the Utah/Idaho populations (originally based on Miller 1982) should be as follows: number of syllables per song = 11.97, number of syllable types per song = 8.66, and syllable diversity = 0.73. These changes, however, do not greatly alter the original trends presented in our 1991 paper. The total

number of syllables in each population is still lowest for Bon Portage. Of four measures of song complexity, the comparisons of the number of song types per male and syllable diversity give equivocal results, while the Bon Portage population still has the lowest number of syllables per song and the lowest number of syllable types per song (both opposite to the predictions of the song complexity hypothesis). Thus, while our research did not "test" these hypotheses, our observations were and are consistent with the founder effect hypothesis, and are not consistent with the song complexity hypothesis.

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