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SUMMARIZING REMARKS: SPECIES VARIABILITY

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In this summary, a few statistical aspects of each paper will be briefly discussed, followed by some general comments on the need for statistical methodology in ornithological research.

Diehl (1981) was concerned with the IBCC recommendation that a pair observed less than three times per 10 occasions not be counted as a breeding pair when the standard mapping method is used to estimate density. She presented evidence supporting the contention that such inconspicuous pairs can in fact be viable breeding pairs, but that the probability of this occurrence varies with time, density, habitat type, breeding success, and species. The use of statistics is minimal; in fact, only one test of hypothesis is performed and, because data were pooled over years, no variances for the various responses of interest were presented. This lack of quantitative treatment of the data is unfortunate in many instances, e.g., the apparent relationship between percent brood mortality and the percent of least dectectable pairs is not quantified. The reader could have been presented with a more objective, quantitative treatment that would have allowed the inferences suggested by the author to be more easily evaluated and interpreted.

Mayfield (1981) presented a very sobering demonstration of the potential biases involved in conducting censuses using counts of singing males. The most disturbing point arising from the study is not that there are differences in detectability among species, but that the reliability of a single census count is very low. That is, if a count conducted at 07:00 on one day produces very different results than one conducted at 09:00 on the same day or 07:00 the next day, can there be any confidence in inferences made from such counts?

Such diversity in census results can be expected, based on the author's data that show that only 42% of the total population present on the study area is identified on a single average count, and that the average probability of detecting a single member of a given species on a single count is 0.40 with a range of 0.00–0.90. Because no estimates of reliability (variance) can be made from transects on which only a single count is made, multiple counts on the same transect seem necessary. Mayfield (1981) also presented evidence of the danger of assuming a 1:1 sex ratio for songbirds, although he admits that apparent differences in the sex ratio within a season may be due to the fact that the sexes vary in their trappability within a season.

Ekman (1981) was concerned with the effects of violating the assumption of equal observability among individuals, as well as in time, on Jolly-Seber (JS) capture-recapture estimates as well as census counts. Ekman documented such heterogeneity in capture probabilities (observability) among age and sex classes, and lamented the fact that there are no powerful tests for detecting unequal catchability when using the JS model. In this particular capture-recapture experiment, however, I suspect that estimates will not be seriously biased because capture probabilities of all classes are relatively high and do not vary extremely.

In general, however, stratification of the data, as Ekman suggests, into strata containing members with relatively homogeneous catchability is an excellent idea if sufficient data are available. As Ekman points out, sampling design can sometimes be adjusted so that good estimates of stratum parameters can be obtained. Unfortunately, the paper fails to report sampling variances for parameter estimates generated by the JS model; these variances can be easily computed and should have been included so that the reader can appreciate the amount of precision associated with the estimates. Ironically, I am curious as to how the author computed sampling variances for the census counts tested for differences in Fig. 2, because there is no replication in time or space. Finally, Ekman concludes that unequal observability prevents census counts from being accurate indexes of seasonal change in density, but does not preclude the use of such a method for assessing annual changes. I would argue that such between year comparisons are also dangerous, because habitat and environmental conditions in the same area at the same time of year could significantly change between years, and hence the observability of the species could change and therefore bias census count results. I believe it is a general axiom that use of index methods which do not have some kind of theoretical model supporting them is dangerous, and should not automatically be viewed as a logical alternative when density estimation is not practical or possible.

The review by Fuller and Mosher (1981) of the methodology currently available for counting raptors reveals the tremendous difficulties associated with obtaining reliable raptor density

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estimates from relatively small scale censuses or surveys. They emphasize, and I agree, that research on sampling techniques is badly needed, but I am not sure that improvements in such sampling methods will render adequate estimates "affordable." Because of the wide range and low density of these birds, large scale surveys will be necessary. I disagree with them that pooling many small studies is a preferable strategy, for such pooling can often introduce extraneous sources of error and bias into the estimates. With standardized and efficient statistical techniques the cost of such surveys can be minimized, but, depending somewhat on the objectives of a given study, reliable estimates will be expensive.

The papers in this session typify both the growing awareness by ornithologists of the assumptions involved in various census/survey techniques and of the consequences of making inferences from such techniques when assumptions are violated. I believe this awareness brings with it a sense of frustration—we are becoming aware of the deficiencies and inappropriateness of some of our methods but we often have no alternatives! If these frustrations are to be relieved, it is imperative that statisticians and ornithologists begin to work together to develop and improve methodologies for estimating parameters of bird populations. This partnership should involve a joint learning process.

The statistician must have an appreciation for the constraints (monetary, manpower, logistical) under which the researcher is operating, and the biologist must understand the basic concepts (sampling variance, experimental error, bias, robustness) that provide the framework for good statistical practice. For example, a statistician working with researchers using IBCC mapping methods could perhaps develop models and associated methods that use all the data collected in such a census in an efficient manner, and thereby avoid the problems associated with essentially ad hoc recommendations for editing the data, which was the main concern in Diehl's (1981) paper. The same statistician might also assist Ekman (1981) in following up on his excellent documentation of heterogeneous observability by using the computer to simulate a multitude of survey situations so that some general conclusions concerning the practical application of capture-recapture methods in ornithological research could be stated. The raptor biologist could explain, by dragging the statistician out from behind his desk and into the airplane, why it is not possible to take a 10 % sample of the area contained within the range of the species of concern.

These hypothetical examples are designed to emphasize that communication between the two disciplines is the key element necessary for progress. These proceedings make it very clear that ornithologists can no longer use methodologies subject to known, yet not quantified, biases in poorly designed studies whose objectives are not clearly stated. The statistician's help should be solicited from the initial planning stages to the analysis of the collected data, and the statistician must respond to this challenge by working with the researcher to develop practical yet rigorous approaches that will facilitate the performance of such research.