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SUMMARIZING REMARKS: ESTIMATING RELATIVE ABUNDANCE (PART I)

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Ralph Schreiber will be making some general comments on the papers from a biological standpoint; I will simply note a few statistical items I think worthy of consideration.

Bock and Root (1981) present a useful discussion of techniques for analyzing results of Christmas Bird Counts (CBC's). Of statistical interest is their proposal for standardizing CBC data. For common and widespread species, they suggest dividing the number of birds seen by the number of party-hours involved in the count, a measure of effort. For species restricted to special habitats, which are likely to yield the same total regardless of effort, they recommend considering the actual number seen per count. I suggest a more general approach. If E is the number of party-hours effort in a particular CBC, then the total number of birds seen could be standardized by division by $1 - \alpha + \alpha E$, where α is a constant between 0 and 1. Values near 0 would give total birds seen, whereas values near 1 would give the number seen per unit effort.

The merit of this approach lies in the possibility of developing useful values of α for various groups of birds within a CBC area. For example, if a CBC area contained about 90% deciduous forest, 10% open field, and a single pond, we would anticipate counts of forest birds to increase almost linearly with effort, and α for those species might be nearly 1. For birds of open fields, α might be about ½, and for waterbirds, which are likely to show the same total whether there are five observers or 50, α would be near 0.

These values could be estimated from an analysis of a number of years of CBC's in an area. This approach may appear too difficult for routine application, but I suspect it could be worthwhile for detailed analyses of a few CBC areas.

Arbib (1981) offers a good critique of current CBC practices and recommends several improvements. Among other analyses, he shows (his Table 2) that CBC's with more observers tend to identify more species. The implication, no doubt correct, is that more species are likely to be found if more observers are involved. It is true that the 22 observers in the Jamestown (North Dakota) count saw 40 species in 1979, and that 51 observers on the Monterey Peninsula (California) count tallied 194, but I doubt that much of the difference in species totals was due to the number of observers. Although Arbib's table is limited to California, I think a better comparison would involve an examination of the number of species versus number of observers across years for a particular CBC area, rather than across areas in a particular year.

Arbib also suggests that training sessions and examinations be used to develop proficiency in estimating numbers of birds in flocks. We have found that even professional observers tend to underestimate the number of animals in large groups, and the bias increases with the size of the groups. A. R. E. Sinclair (1973) found the same relationship, and provided evidence that training can in fact work. A 20-minute training exercise, which involved showing the observers color slides of various groups of animals, asking them to estimate the number in the group, and providing them with the correct answer, caused a rather pronounced underestimation bias virtually to vanish.

Bystrak (1981) gives an overview of another popular and productive bird survey performed in large part by amateurs, the Breeding Bird Survey (BBS). He points out some of the difficulties in interpretation and analysis; see the paper by Geissler and Noon (1981) for statistical details.

A complaint voiced by participants in the BBS is the length of the survey routes: 50 stops at 0.5 mile intervals, each requiring three minutes of observation. Participants suffer from fatigue, which affects their performance on the last 10 or 20 stops. In addition, the long time span required for 50 stops covers intervals when birds are extremely vocal and conspicuous and intervals when they are relatively retiring and inconspicuous.

From a statistical viewpoint, I suspect that shorter surveys, perhaps 30 stops, would not result in serious loss of information. It is a generally held belief that sample size is more important than the size of the sample unit. I feel confident that three surveys of 30 stops each would be more valuable than two surveys of 50 stops each. Even two of the shorter routes would probably be nearly as good as two longer ones and could actually be better if the longer one happened to straddle a stratum boundary. The potential loss of information could be assessed rather simply by examination of the current data base. The data are tallied by 10-stop

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summaries, so the first 30 stops could be analyzed instead of all 50. If a stratum boundary is crossed, the 30 stops most clearly contained in a stratum should be used.

Järvinen and Väisänen (1981) give a worthwhile review of the methodology used in an extensive survey in Finland and the thinking that went into the selection of the method. I noted that they sampled all relevant habitats in approximately correct proportions. This is called proportional allocation, which results in a self-weighting sample design. Although that choice of allocation may be far different from one that is optimal for a particular purpose, it is easy to use and discuss, and can usually be recommended.

A valuable control in their design is to insist that each region is covered by more than one observer, to minimize the effect that differences in observers may have on results for wide areas.

Dawson (1981) touches on a great many key points when he discusses the factors affecting the detectability of birds. He mentions two ways to account for the effects of those variables, such as season, time of day, and weather, that influence the counts. His first method is to standardize the counts by holding those variables as constant as possible; this can be viewed as controlling them. The second method allows those variables to vary, but their effects are estimated and accounted for; this is more in line with modern methods of experimental design and permits analysis of variance or analysis of covariance to be employed. I would define a third method, which in fact is probably the most commonly used: Ignore those variables, take large samples, and hope their effects "average out." This might appropriately be termed the "Pollyanna approach." Throughout this symposium we will see all three methods used, but with little discussion about their relative merit.