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Source: *Ecology*, Vol. 67, No. 1 (Feb., 1986), pp. 255-258

Published by: [Ecological Society of America](#)

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¹ Manuscript received 27 July 1984;

revised 1 May 1985; accepted 17 May 1985.

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Ecology, 67(1), 1986, pp. 255–258
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THE IMPORTANCE OF STATISTICAL POWER WHEN TESTING FOR INDEPENDENCE IN ANIMAL MOVEMENTS¹

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The home range (sensu Burt 1943) provides insight into facets of a species' social organization and foraging ecology (Metzgar 1979, Mitani and Rodman 1979, Madison 1980, Damuth 1981, Getty 1981, Mares et al. 1982, Hixon et al. 1983). Considerable effort has been expended deriving models of home range size (e.g., Calhoun and Casby 1958, Jennrich and Turner 1969, Koepl et al. 1975, 1977, Schoener 1981, Anderson 1982, Don and Rennolls 1983). Reliability of home range estimates depends, to varying degrees, on the extent to which assumptions underlying these estimates are valid. For instance, statistical models of home range assume that locational observations are independent of one another (Dunn and Gipson 1977, Anderson 1982, Slade and Swihart 1983), i.e., that an animal's position at time i is not a function of its position at time $i - \delta$. If successive observations are closely spaced in time this assumption probably is not valid, and home range size may be seriously underestimated (Swihart and Slade 1985a, b).

Schoener (1981) developed a statistic for detecting departures from independence of locational observations: t^2/r^2 , the ratio of the mean squared distance between successive observations (t^2) and the mean squared distance from the center of activity (r^2). We empirically

derived the sampling distribution of Schoener's t^2/r^2 ratio and provided a method for testing the null hypothesis of independence between successive observations (Swihart and Slade 1985a). For observations evenly spaced in time, we also suggested a procedure for determining the minimum time interval at which successive observations cease to be significantly correlated. A quasi-independent subset of points separated by this time interval can then be selected for use in analyses.

Recently Toft and Shea (1983) emphasized the utility of ecologists considering the relative costs of type I and type II errors for the statistical tests they use. In testing for independence, too low an α value may result in a biased home range estimate, whereas too high an α value may result in either too many useable data points being eliminated or data being gathered too infrequently. Typically, biologists rely on α levels of .05 or lower because α represents the probability of falsely rejecting H_0 , the null hypothesis (i.e., type I error; Sokal and Rohlf 1981), and the biologist's hypothesis is usually expressed as the alternative hypothesis (H_1). However, in selecting a time interval which will yield quasi-independent locational observations, we ultimately wish to accept the null hypothesis of independence, so we should actually be more concerned with the probability of falsely accepting H_0 (i.e., β or type II error). Because of this, we arbitrarily used an α of .25 with the hope that β would be lowered to a "reasonable" level (Swihart and Slade 1985a). In this paper we evaluate our choice of α by generating power curves for our test of Schoener's ratio.

Methods

The quantity $1 - \beta$, or the probability of rejecting a false H_0 , is the power of a test. Power should increase

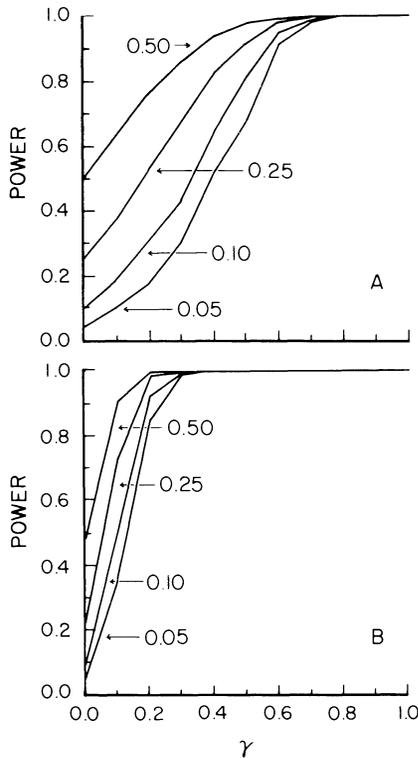


FIG. 1. Selected power curves for the test of independence of locational observations. γ is a scalar measure of bivariate autocorrelation, defined in Methods. Only power curves constructed from sample sizes of 10 (A) and 100 (B) observations are illustrated; intermediate sample sizes ($n = 30, 50$) produced power curves with intermediate characteristics relative to the curves depicted here. For each sample size, four levels of α were used: .05, .10, .25, and .50. Uniform (shown here) and normal error terms produced nearly identical power curves.

as truth (the actual t^2/r^2 ratio in our test) departs from the null hypothesis. The utility of power curves is that they enable comparisons of the effects that various α levels have on β over a wide range of autocorrelations.

In general, first-order bivariate autocorrelation is described using a 2×2 matrix of autocorrelations and cross correlations (see Swihart and Slade 1985b). However, power curves typically are presented as plots of $1 - \beta$ vs. a scalar analog of the statistic in question (t^2/r^2 in this instance). Because we were unable to find any scalar measures of bivariate autocorrelation in the literature, we defined such a measure:

$$\gamma = (\rho_{X_i X_{i-1}} + \rho_{Y_i Y_{i-1}})/2.$$

$\rho_{X_i X_{i-1}}$ and $\rho_{Y_i Y_{i-1}}$ are autocorrelations between X at times i and $i - 1$ and Y at times i and $i - 1$, respectively. No cross correlation terms were included in γ because Schoener's ratio does not involve cross products. In our simulations we restricted our generation of values to cases in which the autocorrelation in each dimension

was identical, i.e., $\rho_{X_i X_{i-1}} = \rho_{Y_i Y_{i-1}} = \gamma$, but with actual data $\rho_{X_i X_{i-1}}$ need not equal $\rho_{Y_i Y_{i-1}}$. Although γ may assume values from -1 to 1 , movement paths characterized by negative autocorrelations, i.e., abrupt shifts from one side of the home range to the other, are difficult to envision. Hence, we focused on values of γ from 0 to 1 .

To construct power curves, a set of autocorrelated observations were required. If X and Y are independently distributed, autocorrelations of strength γ may be generated using the equations

$$X_i = \rho_{X_i X_{i-1}} \times X_{i-1} + \epsilon_X$$

and

$$Y_i = \rho_{Y_i Y_{i-1}} \times Y_{i-1} + \epsilon_Y,$$

where ϵ_X and ϵ_Y are random error terms for X and Y , respectively. Using these equations we generated 1000 sets of locational observations of size n for a variety of γ values. For each γ , Schoener's ratio was calculated for each set, and a tally was made of the number of sets for which the null hypothesis of independence was rejected. Power curves were constructed for four levels of α (.05, .10, .25, .50) at each of four sample sizes ($n = 10, 30, 50, 100$) and two distributions of error terms (a bivariate uniform distribution over the unit square and a bivariate normal distribution with zero mean and unit variance).

Sample locations of an organism within its home range may not reflect the true shape of the home range. Because home range "shape" (i.e., eccentricity; Swihart and Slade 1985a) is important in calculating critical values of Schoener's ratio, we also constructed power curves based on critical values calculated using sample eccentricities rather than the parametric eccentricity of one. This served as a check on the power of the test as used by biologists under ordinary field conditions.

Results and Discussion

Power curves for Schoener's ratio are presented in Fig. 1. For all curves the power of the test increased (or, equivalently, the probability of a type II error decreased) as γ increased. Increases in power were most pronounced for large sample sizes and high values of α (Fig. 1). For example, at $\gamma = 0.1$ the probability of rejecting H_0 was 0.635 for $n = 10$ and $\alpha = .50$ (Fig. 1A), whereas the power of the test for the same α level was 0.906 when $n = 100$ (Fig. 1B). Similarly, the power of the test at $\gamma = 0.1$ and a sample size of 100 was 0.353 for $\alpha = .05$ and 0.906 for $\alpha = .50$ (Fig. 1B).

Using the observed estimate of eccentricity to calculate the critical value (Swihart and Slade 1985a) had little effect on the power of the test when sample sizes were large, but power declined slightly at small sample sizes. At least two factors contributed to underesti-

mation of power at small n . First, small n resulted in larger errors when estimating eccentricity, which in turn produced larger errors in the calculation of critical values for t^2/r^2 . In fact, the standard deviation of sample eccentricities was nearly six times larger for $n = 10$ compared to $n = 100$ when $0 < \gamma < 0.2$. Second, $n = 10$ was the lower limit at which the distributional assumptions necessary to calculate critical values of t^2/r^2 applied (Swihart and Slade 1985a). Nonetheless, the effects of sampling variation on the power of the test of independence appeared to be small. Thus, tests of locational data collected in the field should closely adhere to the power curves shown here.

Any choice of an α level must be tempered by the realization that β will be affected as well (Toft and Shea 1983). When testing the null hypothesis of independence, the level of β is at least as important as the level of α ; in general, then, power curves characterized by large positive slopes at low levels of γ are preferable. High power at low levels of α indicates a small probability of type II error. Curves constructed for high (.50) and moderate (.25) α levels exhibited steep slopes associated with low values of γ (Fig. 1).

We recommend testing for independence with an α level on the order of .25 or .50. Levels of α less than .25 seldom produced powers in excess of 0.50 unless γ was greater than 0.25 (Fig. 1); thus, the probability of falsely accepting the hypothesis of independence is greater than 0.50 for low γ . Clearly, this is an unacceptably high value of β .

Using an α level of .50 was the most conservative and most powerful approach we tried; that is, β was always smallest at this level of significance (Fig. 1). However, use of such a large α is twice as likely to lead to overestimation of the time interval necessary to achieve independence between successive observations, as compared to $\alpha = .25$. As a result, valuable data may be discarded prior to estimation of home range size. Or, if a pilot study is conducted to determine an appropriate sampling interval and $\alpha = .50$ is used, the estimated sampling interval may be longer than necessary to ensure collection of independent observations. Based on our admittedly limited experience with this test of independence, we have noticed that sampling intervals derived for small mammals (<200 g) using α levels ranging from .25 to .50 usually differ by no more than 20–30 min. However, we suspect that the magnitude of this difference may increase as a function of body mass in terrestrial mammals (see Lindstedt and Calder 1981, Calder 1983, 1984).

Acknowledgments: D. John Anderson, Bradley J. Bergstrom, and an anonymous reviewer provided constructive comments on the manuscript. This study was supported by an Honors Fellowship from the Univer-

sity of Kansas Graduate School to R. K. Swihart and by University of Kansas General Research Grant 3509-0038 to N. A. Slade. Computing resources were provided by the University of Kansas Academic Computing Center.

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¹ *Manuscript received 30 November 1984; accepted 28 March 1985; final version received 24 May 1985.*

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Ecology, 67(1), 1986, pp. 258–260
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THE CORRELATION BETWEEN RANGE SIZE AND LOCAL ABUNDANCE OF SOME NORTH AMERICAN BIRDS¹

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Bock and Ricklefs (1983) compared range size and average within-range abundance for 65 taxa of seed-eating songbirds wintering in the continental United States and southern Canada, using data taken from Audubon Society Christmas bird counts (CBCs) for the years 1962–1971. Range size was computed as the number (out of 51) of occupied 5° blocks of latitude and longitude, while abundance was quantified as the log of the mean number of individuals counted per party-hour of count effort for all CBCs in occupied blocks. They reported significant positive correlations between these measures of range size and abundance for all 65 species and for the 22 species >90% restricted in winter to the study area. Splitting the 22 endemics or near-endemics into two groups based on range size, they also found a greater mean within-range abundance in the 11 more widespread taxa than in the 11 more narrowly distributed forms.

The range size–abundance correlations found by Bock and Ricklefs (1983) involve a positive bias, which results from their method of calculating average within-range abundances. Smaller correlations may have been obtained if data were analyzed with a finer scale of geographic resolution. They included all CBCs (each conducted over a 24 km diameter circle) in a given occupied 5° block, even if the species occurred in only a small portion of that block. R. C. Lacy (*personal observation*) found, through a series of computer simulations, that species with small ranges (≤ 10 of the 51 blocks) would appear less abundant by this method, simply because a larger proportion of their occupied

blocks would be on the margin of the species range and only partially occupied. That is, the proportion of zero CBCs within the 5° blocks presumably would be greater among data used to calculate within-range abundances for the species with small ranges than it would for species with large ranges.

This effect can be eliminated by comparing range size with a measure of within-range abundance using only nonzero CBCs for each species. In the present study we used CBC data to compute such fine-scale measures of average within-range abundance for the same species analyzed in the earlier study. We then (1) compared these for the narrowly distributed vs. widely distributed endemic forms, and (2) correlated range size and within-range abundance (as measured using nonzero counts only) for both the endemics and for all 65 species.

Methods

The nature, strengths, and limitations of CBC data, and methods for handling them, have been reviewed elsewhere (Bock and Root 1981). In order to obtain an abundance data set independent of that used in the earlier study, we sampled individual CBCs that occurred in the winter of 1982–1983 and (for rare species) up to three previous winters. From these, we computed mean abundance per party-hour for nonzero counts only, for each of the 65 species (see Bock and Lepthien 1976 for a species list). Three of these taxa recently have been submerged (American Ornithologists' Union 1983), but we have retained them as distinct forms to be consistent with Bock and Ricklefs' data set. (Lumping these forms somewhat increased the correlations found.)

We randomly sampled two 1982–1983 counts published for each province and state, and then one additional count per state and province as many times as was necessary to obtain a sample of at least 25 nonzero counts per species. Certain rare species did not occur on 25 counts in 1982–1983, in which cases counts from up to three preceding years were included