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COMPARISON OF SOME STATISTICAL TECHNIQUES FOR ANALYSIS OF RESOURCE SELECTION

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Abstract: Four statistical methods for analysis of habitat selection were compared using over 50,000 computer simulations of field data. Comparisons were based on Type I and Type II error rates, which varied among methods. The methods of habitat analysis gave contradictory results when applied to a given data set. Studies with few observations from few animals should be avoided because of unacceptable Type II error rates, and the number of habitats considered should be limited. Practical considerations for choosing among methods are suggested.

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Analysis of habitat selection is a common and important aspect of wildlife science. Research often attempts to go beyond simple documentation of habitat use to determine if specific habitats are selected; i.e., used more or less than availability. A preference index reported by Ivlev (1961; also see Strauss 1979) has been applied in several wildlife habitat studies (Fritzell 1978, Andelt and Andelt 1981, Whiteside and Guthery 1983, Hargis and McCullough 1984, Sayre and Rundle 1984). This form of analysis is limited because it provides only a ratio of habitat use to habitat availability and does not use a statistical test. The Chi-square goodness-of-fit test is a common statistical approach for testing if observed habitat use is equal to expected use. An associated method reported by Neu et al. (1974) and Byers et al. (1984) has been used to disclose habitat preference (Singer et al. 1981, Ringelman and Longcore 1982, Steventon and Major 1982, Servheen 1983, Jenkins and Starkey 1984, and others). Johnson (1980) described the general problem of determining selection when resource use is compared to availability and suggested a new technique where results do not depend upon the array of habitats considered. Johnson's technique has been used for analysis of wildlife habitat (Haroldson and Fritzell 1984, Sayre and Rundle 1984). The Friedman (1937) test for the randomized complete block design also has been used in recent studies of resource utilization (Pietz and Tester 1982, 1983; Johnson and Montalbano 1984). Other techniques for anal-

ysis of habitat use have been reported by Suring and Vohs (1979) and Talent et al. (1982). To our knowledge, use of the Quade (1979) test for analysis of resource utilization has not been published previously.

Use of several statistical techniques to analyze habitat selection data generated several questions. Do the techniques test the same hypotheses? Do the techniques provide generally similar results from a single data set? And, if the answers to these questions are "no," can we determine which technique provides the most accurate result?

This paper presents hypotheses, assumptions, and data requirements for analysis techniques based on the Neu et al. (1974) method, the Johnson (1980) method, the Friedman (Friedman 1937, Iman and Davenport 1980) test, and the Quade (1979) test. We demonstrate use of these procedures on gray partridge (*Perdix perdix*) data (Smith et al. 1982) and present results from computer simulations of field data. We offer recommendations for the use of these procedures based on computer simulation results. For ease of discussion, we use the terms "animal" and "habitat type," but the methods presented may be useful in other resource utilization analyses such as food preference studies (Ivlev 1961, Strauss 1979).

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METHODS

The Neu et al. (1974) method uses a Chi-square goodness-of-fit analysis to test whether observations of habitat use follow the expected pattern of occurrence based on habitat availability (N_1 below). When the Chi-square test detects a significant difference in usage versus availability, a Bonferroni z -statistic (Miller 1981: 219) is used to determine which habitat types are used more or less frequently than expected (N_2 below).

Formally, the null hypotheses tested by the Neu et al. (1974) method are:

- N_1 . Usage occurs in proportion to availability, considering all habitats simultaneously.
- N_2 . Usage occurs in proportion to availability, considering each habitat separately.

A critical assumption is that all observations are independent. This precludes the analysis of aggregated animals and even makes analysis of individual animals tenuous because locations may not be independent. An assumption required for hypothesis N_1 is that the sample size is sufficiently large to allow a Chi-square approximation for the goodness-of-fit test statistic. Dixon and Massey (1969) recommended that there be ≥ 1 expected observation in each category (i.e., habitat type) and that $\leq 20\%$ of all categories contain < 5 expected observations; these conditions may be conservative (Roscoe and Byars 1971). To test hypothesis N_2 the sample size is assumed to be sufficiently large so a normal distribution approximation to the binomial distribution is valid. A conservative rule of thumb is that the sample size N should be large enough that Np_i and $N(1 - p_i)$ are both ≥ 5 , where p_i is the proportion of observations in the i^{th} habitat (Dixon and Massey 1969). The Neu method allows comparison of usage vs. availability for each habitat across all animals. Identification of individual animals need not be recorded for analysis. It is assumed that habitat availability is the same for all animals. Observation of animals may be obtained by methods such as flushes from systematic transects, survey of random plots, or radio telemetry.

The Johnson (1980), Friedman (1937), and Quade (1979) methods test slightly different hypotheses and require more specific information than the Neu et al. (1974) method; i.e., they require information on the usage of habitat types for each animal considered *individually*. Habitat use vs. availability is determined

for each animal, then these data are combined to form the respective test statistics. These data often are obtained from repeated observations of individual animals that are marked, especially by radio telemetry. The assumption that habitat availability is the same for all animals is unnecessary for these 3 methods but will be made to allow comparisons to the Neu method.

The method suggested by Johnson (1980) yields rankings of items by preference such that significance tests can be made for differences among habitats. An advantage is "the method is relatively insensitive to the inclusion/exclusion of doubtful habitats" (Johnson 1980:70). An example of a doubtful habitat is one that is very common but rarely selected. The decision to include or exclude that component may greatly affect the results for some methods of analysis. Johnson's method compares ranks of habitat selection with ranks of habitat availability for each animal. The differences between selection rank and availability rank for each habitat are averaged across animals to obtain a mean that estimates the relative selection of the i^{th} habitat. The magnitude of the average differences can be used to order the habitats from least to most preferred. The 2 hypotheses tested by Johnson's method are:

- J_1 . The relative selections for all habitats are equal.
- J_2 . The relative selection for habitat i equals that for habitat j .

Hypothesis J_1 states that the rank ordering of habitats according to selection (least to most used) is the same as the rank ordering of the availability of habitats (least to most prevalent). Substantial differences in the rank orders will cause rejection of J_1 , in which case hypothesis J_2 is tested. The 1st hypothesis is tested with Hotelling's T^2 -statistic (Anderson 1958). It is necessary to assume that the sample size is large enough so the differences in the average ranks have a multivariate normal distribution. We are unaware of accepted guidelines for determining minimum sample size. The 2nd hypothesis is tested with the Waller-Duncan multiple comparison procedure (Waller and Duncan 1969). As implemented by Johnson, a critical significant difference is computed for each pair of habitats compared. As with most multiple comparison procedures, it is assumed that the average values being compared are obtained from normal populations having equal variances. A

further assumption is that results from 1 animal do not influence results from other animals; i.e., independence.

The Friedman (1937) test is described by Conover (1980). Friedman's test is an extension of the sign test and allows testing the hypothesis that habitats have identical effects on the difference between selection and availability. Our application of the Friedman test involves computing the difference between percent selection and percent availability of each habitat by each animal. The differences are ranked for each animal, and the ranks are used to compute Friedman's test statistic where habitats represent "treatments" and animals are "blocks." For Johnson's method ranks for selection and availability of habitats are obtained, then differences in ranks are computed. For the Friedman method, as defined here, the difference between percent selected and availability is computed, then differences are ranked.

Formally, the hypothesis tested by Friedman's method is:

F_1 . The ranks of the differences in selection and availability are the same for all habitats.

If the Friedman test results in rejection of this hypothesis, we chose to use Fisher's least significant difference (LSD) procedure based on ranks to determine which habitats are different in terms of selection vs. availability (Conover 1980). The same LSD value is used to compare all pairs of habitats. The hypothesis tested is:

F_2 . The rank of the difference in selection and availability is the same for habitat i and habitat j .

Hypothesis F_1 states that differences between selection and availability for the habitats are the same for all habitats. Hypothesis F_2 states that the difference between selection and availability for 1 habitat (i) equals that difference for some other habitat (j). All possible pairwise comparisons of habitats are tested.

Hypothesis F_1 may be tested by comparing the computed value of the test statistic to an F -distribution with $k - 1$ and $(k - 1)(t - 1)$ degrees of freedom, where k is the number of habitats and t is the number of animals observed (Conover 1980). We assume that results for 1 animal do not influence results for other animals. The number of animals observed is assumed to be large enough for the approximation to the F -distribution to be fairly good

(Iman and Davenport 1980). The exact distribution of the test statistic is available through multiresponse randomized block permutation procedures (Mielke and Iyer 1982). Assumptions for the LSD method for testing F_2 are that ranks for each habitat are normally distributed and have homogeneous variances.

The Quade (1979) test may be considered a 2-way analysis of variance on ranks. This multisample extension of the Wilcoxon signed ranks test may be used to test Q_1 , the same hypothesis as Friedman's 1st hypothesis (F_1). For our application of the Quade test, the assumptions, form of the data, and degrees of freedom for the F -distribution are the same as those used for Friedman's test. If application of the Quade test resulted in rejection of hypothesis Q_1 , we chose to use LSD-type multiple comparisons to determine which habitats are different in terms of selection vs. availability (Conover 1980). This hypothesis (Q_2) is the same as hypothesis F_2 . As with the Friedman method, only 1 LSD value is required to compare all pairs of habitats.

Unless stated otherwise, the acceptable level of statistical significance was $\alpha = 0.05$.

APPLICATION TO REAL DATA

The procedures mentioned above were applied to a portion of data presented by Smith et al. (1982). Data from 10 radio-tagged gray partridge observed in the summer of 1979 indicated that small grain habitats were selected more than expected, whereas row crop and hay habitats were selected less than expected according to the Neu method. Using the Johnson method and these same data, we obtained a significant F -value ($P = 0.012$). We rejected the null hypothesis that all habitat types were selected with equal intensity. The Johnson method produced the following rank ordering of means from least to most preferred (left to right):

Row crop Pasture Small grain Hay Idle

Habitat types underscored by the same line were not different from each other, whereas lack of an underscore indicates that habitat types differed according to the Waller-Duncan procedure. Selection of pasture, small grain, and hay were not significantly different. These conclusions differed from those found by Smith et al. (1982) using the Neu method. With the Friedman test, we obtained a nonsignificant F -value ($P = 0.226$), so no multiple comparison

Table 1. Percentage of Type II errors for 4 statistical methods of habitat selection analysis. One thousand simulations were run for each of 18 habitat-animal-observation conditions.

Condition	N habitats	N animals	N observations/animal	Neu et al. (1974)	Johnson (1980)	Friedman (1937)	Quade (1979)
1	4 ^a	10	15	64.1	98.7	82.6	98.0
2	4	10	50	5.6	99.5	26.3	45.1
3	4	20	15	25.6	30.7	64.2	80.8
4	4	20	50	0.0	98.2	0.8	6.5
5	4	50	15	0.4	0.0	16.9	44.0
6	4	50	50	0.0	88.6	0.0	0.0
\bar{x} error rate				15.9	69.3	31.8	45.7
7	7 ^b	20	15	13.7	0.0	35.8	49.6
8	7	20	50	0.0	0.0	0.0	0.2
9	7	50	15	0.0	0.0	0.8	3.9
10	7	50	50	0.0	0.0	0.0	0.0
\bar{x} error rate				3.4	0.0	9.2	13.4
11	10 ^c	20	15	1.6	0.0	1.9	5.4
12	10	20	50	0.0	0.0	0.0	0.0
13	10	50	15	0.0	0.0	0.0	0.0
14	10	50	50	0.0	0.0	0.0	0.0
\bar{x} error rate				0.4	0.0	0.5	1.4
15	15 ^d	20	15	3.4	0.0	0.9	2.1
16	15	20	50	0.0	0.0	0.0	0.0
17	15	50	15	0.0	0.0	0.0	0.0
18	15	50	50	0.0	0.0	0.0	0.0
\bar{x} error rate				0.8	0.0	0.2	0.5

	Habitat														
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
^a Simulated % selected	5	15	40	40											
% availability	10	15	37.5	37.5											
^b Simulated % selected	9	20	30	8	8	20	5								
% availability	7	17	26	10	10	20	10								
^c Simulated % selected	2	2	10	20	5	30	3	5	20	3					
% availability	1	1	8	15	8	27	5	10	20	5					
^d Simulated % selected	12	12	4	5	5	2	2	3	20	6	2	3	4	15	5
% availability	11	11	5	10	3	4	3	4	20	4	1	2	3	13	6

procedure was used. The Quade procedure yielded a significant *F*-value ($P = 0.012$). The ordering of habitat types and significant differences from the LSD procedure were:

Row crop Hay Pasture Idle Small grain

Here the relative preferences for each habitat type were more similar to those obtained by the Neu method than by either the Johnson or Friedman methods. Small grain habitat was selected more than expected by 7 of the 10 birds, whereas idle habitat was selected more than expected by 6, pasture by 3, row crop by 3, and hay by 2. This information tends to support the Neu and Quade results. Because the results suggested different patterns of habitat selection, it was not possible to identify which method pro-

vided the most accurate indication of habitat selection.

APPLICATION TO SIMULATED DATA

Simulations were designed to study the performance of the 4 methods with respect to Type I and Type II errors for both preliminary tests of significance (N_1, J_1, F_1, Q_1) and the multiple comparison procedures (N_2, J_2, F_2, Q_2). Because the truth for our simulated data was known, error rates for the methods could be determined. A Type I error is made when a significant difference in proportional selection is proclaimed when, in fact, habitats are selected according to their availability. A Type II error is made when a difference in proportional selection is not detected. Most investigators select

Table 2. Percentage of multiple comparison Type I errors for 4 statistical methods of habitat selection analysis. One thousand simulations were run for each of 18 habitat-animal-observation combinations.

Condition	N habitats	N animals	N observations/animal	Neu et al. (1974)	Johnson (1980)	Friedman (1937)	Quade (1979)
1	4 ^a	10	15	1.5	6.1	16.7	6.0
2	4	10	50	1.1	6.2	26.8	4.5
3	4	20	15	1.2	7.5	18.1	5.4
4	4	20	50	1.2	6.8	26.0	4.2
5	4	50	15	1.2	6.8	14.4	5.7
6	4	50	50	1.6	9.1	25.8	5.0
\bar{x} error rate				1.3	7.1	21.3	5.1
7	7 ^b	20	15	0.4	0.4	0.0	0.0
8	7	20	50	0.8	1.1	0.0	0.0
9	7	50	15	0.7	1.0	0.0	0.0
10	7	50	50	1.3	0.8	0.0	0.0
\bar{x} error rate				0.8	0.8	0.0	0.0
11	10 ^c	20	15	0.6	1.1	1.4	0.1
12	10	20	50	0.4	1.9	6.7	0.1
13	10	50	15	0.5	0.3	0.8	0.1
14	10	50	50	0.3	0.4	8.3	0.2
\bar{x} error rate				0.4	0.9	4.3	0.1
15	15 ^d	20	15	0.4	0.7	4.6	2.6
16	15	20	50	0.1	1.1	1.6	0.0
17	15	50	15	0.3	1.0	5.8	3.1
18	15	50	50	0.4	1.0	2.1	1.0
\bar{x} error rate				0.3	1.0	3.5	1.7

	Habitat														
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
^a Simulated % selected	5	15	40	40											
% availability	45	15	20	20											
^b Simulated % selected	9	20	30	8	8	20	5								
% availability	3	1	6	15	15	20	40								
^c Simulated % selected	2	2	10	20	5	30	3	5	20	3					
% availability	1	1	1	1	3	3	10	15	20	45					
^d Simulated % selected	12	12	4	5	5	2	2	3	20	6	2	3	4	15	5
% availability	3	3	1	3	12	8	3	2	20	4	1	4	6	1	29

a Type I error rate of 5%, and a Type II error rate of 10–20% is acceptable (Snedecor and Cochran 1980:102).

The function UNIFORM from the Statistical Analysis System (Helwig and Council 1979) was used to generate pseudorandom habitat selection. To evaluate analysis procedures under a wide variety of conditions, we considered 4, 7, 10, or 15 habitats where the locations of either 20 or 50 animals were generated for each simulation; the number of observations/animal was set at 15 or 50. In addition, locations for 10 animals were generated with 15 or 50 observations for the 4-habitat case. The generated locations of animals were independent of previous locations. The design resulted in 18 conditions, each of which was simulated 1,000 times. Thus, there were 54,000 simulations: 18,000 for examining Type I errors for hypoth-

eses N_1 , J_1 , F_1 , and Q_1 ; 18,000 for examining Type II errors for N_1 , J_1 , F_1 , and Q_1 ; and 18,000 for examining Type I and Type II errors for N_2 , J_2 , F_2 , and Q_2 . This range of conditions encompasses many resource utilization studies.

To examine the Type I error rate for hypotheses N_1 , J_1 , F_1 , and Q_1 , we simulated field studies having equal habitat selection and equal availability (25% for 4 habitats, 10% for 10 habitats, etc.). Thus, in our "population" of values there were no differences in proportional selection although our randomly generated sample allowed Type I errors to occur. The occurrence of Type I errors was controlled by all methods very close to the chosen 5% level of probability. The test statistics used to test N_1 , J_1 , F_1 , and Q_1 resulted in rejection of the null hypothesis in about 5% of the simulations when there actually were no differences in proportional usage.

Table 3. Percentage of multiple comparison Type II errors for 4 statistical methods of habitat selection analysis. One thousand simulations were run for each of 18 habitat-animal-observation conditions.

Condi- tion	N habitats	N animals	N observations/ animal	Neu et al. (1974)	Johnson (1980)	Friedman (1937)	Quade (1979)
1	4 ^a	10	15	0.4	38.5	2.3	6.3
2	4	10	50	0.0	44.5	0.0	0.2
3	4	20	15	0.0	37.3	0.1	0.4
4	4	20	50	0.0	40.4	0.0	0.0
5	4	50	15	0.0	36.1	0.0	0.0
6	4	50	50	0.0	35.9	0.0	0.0
\bar{x} error rate				0.07	38.8	0.4	1.2
7	7 ^b	20	15	3.6	10.4	7.8	13.3
8	7	20	50	0.0	1.9	2.5	8.2
9	7	50	15	0.0	5.7	3.6	6.5
10	7	50	50	0.0	0.0	0.7	2.9
\bar{x} error rate				0.9	4.5	3.7	7.7
11	10 ^c	20	15	31.5	8.4	14.2	18.7
12	10	20	50	22.5	11.1	8.6	15.8
13	10	50	15	26.9	6.2	12.5	13.9
14	10	50	50	0.4	6.4	4.8	10.4
\bar{x} error rate				20.3	8.0	10.0	14.7
15	15 ^d	20	15	51.0	22.6	27.1	34.7
16	15	20	50	34.5	16.8	15.9	24.2
17	15	50	15	39.1	10.1	16.6	21.3
18	15	50	50	12.2	7.0	10.2	15.1
\bar{x} error rate				34.2	14.1	17.4	23.8

	Habitat														
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
^a Simulated % selected	5	15	40	40											
% availability	45	15	20	20											
^b Simulated % selected	9	20	30	8	8	20	5								
% availability	3	1	6	15	15	20	40								
^c Simulated % selected	2	2	10	20	5	30	3	5	20	3					
% availability	1	1	1	1	3	3	10	15	20	45					
^d Simulated % selected	12	12	4	5	5	2	2	3	20	6	2	3	4	15	5
% availability	3	3	1	3	12	8	3	2	20	4	1	4	6	1	29

The frequency of Type II errors for hypotheses N_1 , J_1 , F_1 , and Q_1 was greater for 4 habitats than 7, 10, or 15 habitats (Table 1). In general, as the number of animals or the number of observations/animal increased, the error rate decreased. Exceptions occurred using Johnson's method for conditions 1 through 6 where the number of errors increased as the number of observations/animal increased. This was because the arrays for selection and availability chosen had the same rank ordering across all habitats. For many observations/animal the average rank ordering for simulated selection equaled the rank ordering for availability, so few significant differences were detected. Johnson's method did not detect differences when the rank ordering of selection and availability was the same even if the percentages were quite

disimilar. For instance, in supplemental simulations with selection percentages of 5, 10, 15, and 70 and availability percentages of 1, 20, 30, and 49, Johnson's method detected differences for 32% of the simulations with 20 animals and 50 observations/animal. Each of the other methods detected differences for 100% of the simulations.

To examine Type I and Type II error rates for hypotheses N_2 , J_2 , F_2 , and Q_2 , we simulated another 18,000 field studies with the array of selection and availability shown at the bottom of Table 2. The selection and availability values chosen allowed comparison of the multiple comparison phase of each method because in all cases the preliminary hypotheses (N_1 , J_1 , F_1 , or Q_1) were rejected. There were no Type II errors for the preliminary hypotheses regardless

of the number of animals or observations/animal generated. This was not surprising given the large differences in proportional usage.

In the 4-habitat case, the Neu method should not have found differences between selection and availability for Habitat 2 because they were chosen to be equal (see footnote a, Table 2) but should have found differences for the other 3 habitats. Similarly, the Johnson, Friedman, and Quade methods should not have found differences in the relative uses for habitats 3 and 4 as they were equal, but should have found differences for all other pairs of habitats. The usage and availability values for 7, 10, and 15 habitats were chosen to allow similar comparisons. It was possible to make only 1 Type I error for each of the multiple comparison methods in a simulation, but several Type II errors were possible. For example, in the 4-habitat case, the Neu method could have made 3 Type II errors (habitats 1, 3, and 4) while each of the 3 procedures based on ranks could have made 5 Type II errors/simulation (1 vs. 2, 1 vs. 3, 1 vs. 4, 2 vs. 3, or 2 vs. 4). Comparison error rates were computed as the frequency of errors considering the potential number of errors that might have occurred.

The selected Type I error rate was 5% for all the methods (Table 2). The error rate for the Neu method was <5% for all conditions, indicating the conservative nature of tests based on the Bonferroni *z*-statistic. Johnson's method resulted in an error rate of 7.1% in the 4-habitat conditions, but below the 5% level for all other conditions. The error rate for Friedman's method was quite large for the 4-habitat case (21.3%) but <5% for 7, 10, and 15 habitats. For 4 habitats, the Quade method was very close to the nominal rate of 5% but conservative for 7, 10, and 15 habitats.

The Neu method could have made 3, 6, 9, and 14 Type II errors/simulation for 4, 7, 10, and 15 habitats, respectively (Table 3). The Johnson, Friedman, and Quade methods could have made 5, 20, 44, and 104 Type II errors/simulation for the same respective numbers of habitats.

The frequency of Type II errors appeared to depend on the number of animals observed, the number of observations/animal, the number of habitats, and the magnitude of the differences to be detected (Table 3). As the number of habitats increases, the pairwise differences between habitat usage vs. availability must decrease.

Thus, the magnitude of some differences will tend to be smaller and more difficult to detect. The average error rates for the Neu, Friedman, and Quade methods increased as the number of habitats increased. The average error rate for Johnson's method in the 4-habitat case was 38.8% but was lower for 7, 10, and 15 habitats.

To supplement the simulations presented here, we simulated other field studies with different arrays of usage and availability for 4, 7, and 10 habitats. The results of these supplemental simulations were in agreement with the results shown in Tables 1, 2, and 3.

DISCUSSION

Statistical techniques for the analysis of resource selection may give conflicting results. An investigator should consider the hypotheses tested and the necessary assumptions in choosing a method of analysis. If the data are such that all of the 4 methods compared in this paper are appropriate, then consideration of the performance of the tests based on our simulations may be useful. Our simulation results indicated that all 4 of the methods were effective in controlling the probability of Type I errors for the overall hypotheses; i.e., that all habitats were preferred equally. The occurrence of Type II errors for the overall hypotheses depended on the number of habitats, the number of animals, the number of observations/animal, and the magnitude of the differences to be detected. Although the particular array of selection and availability values chosen had an effect on error rates, these rates generally decreased as the number of animals increased. The Type I error rates for the multiple comparisons were at or below the level of significance chosen for the tests with few exceptions. The multiple comparison Type II error rates were very dependent on the number of habitats.

There was no clear choice of a method that was best in all cases. The Neu and Quade methods performed well when the number of habitats was small if ≥ 20 animals and 50 locations/animal were observed. The Johnson method did not detect differences when the rank ordering for selection and availability was the same even when the percent selected and percent available were quite different. If it is important to detect differences of this type, Johnson's method should not be used. The Johnson and Friedman methods appeared best for the arrays chosen when the number of habitats was large. The

Johnson, Friedman, and Quade methods provided an ordering of habitats from least to most preferred and had the desirable property that inclusion or exclusion of habitats did not substantially alter the results. The Friedman and Quade methods were easier to apply than Johnson's method. Quade's method had smaller Type I and larger Type II multiple comparison error rates than Friedman's method.

Our results demonstrate that regardless of the method of analysis, if the data consist of few (e.g., 15) observations on few animals, there is a likelihood of unacceptably high Type II error rates; thus, we recommend consideration of both Type II and Type I errors while planning resource utilization studies. Also, as the number of habitats increases the Type II multiple comparison error rates increase; therefore, the number of habitats considered should be limited.

LITERATURE CITED

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RELIABILITY OF TREND LINES OBTAINED FROM VARIABLE COUNTS

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Abstract: Standard errors (SE) of trend lines arising from highly variable counts of animals are derived analytically given the variability of the counts, the number of years of trend monitoring, the number of replicate counts each year, and certain assumptions. Examples of the analytical solution's use and graphs of expected trend line variability are presented. When counts are highly variable, performing multiple counts each year is shown to be the only way to achieve precision of a population trend estimate within a short (<12 years) time frame. Computer simulations to test the robustness of the analytical treatment under assumption violations show that it never overestimates and sometimes underestimates the true variability of trend lines. Accordingly, when count variability cannot be reduced, I suggest increasing the number of replicate counts each year above that indicated by the analytical solution to ensure that confidence limits (CL) about the trend line include the population's true rate of growth.

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A common difficulty facing wildlife managers is estimating population size with acceptable precision. Population estimation is especially difficult for rare and/or secretive species whose status may nevertheless be of concern. Counts of these species often are imprecise, and confidence intervals on resulting estimates are correspondingly broad. Eberhardt (1978) treats this topic in detail, suggesting procedures for determining adequate sample sizes and giving rough ideas of the variability to be expected from indirect indices as well as counts.

Where precise counts are difficult or impossible, attempts are often made to monitor general trends over time. A case in point is the current effort to monitor trends of remnant populations of grizzly bears (*Ursus arctos*) in the contiguous United States. Population monitoring is called for in the grizzly bear recovery plan (U.S. Fish and Wildl. Serv., unpubl. rep., Denver, Colo., 1982), but grizzly populations are notoriously difficult to census with reasonable precision (Erickson and Siniff 1963). Little attention has been paid to the precision required of counts used for year-to-year trend monitoring. The implicit assumption often is

that data lacking the quality and quantity needed for population estimation can at least be used for trend monitoring. However, observed trends themselves may be misleading if based on data that are highly variable.

In this paper I use an analytical procedure to calculate the variability of observed trend lines. This procedure requires assumptions that may not be met in field situations. Accordingly, I also present simulation modeling experiments that test the robustness of the calculation when these assumptions are violated. (I define a trend line as the slope of the least squares regression of the logarithm of observed numbers over time [r_t] and quantify trend line reliability by the SE of the slope [SE_{r_t}]).

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