their own analyses. These flaws are not inherent in the statistical techniques themselves and can be avoided by a careful investigator. Although Rexstad et al. (1988:797) are correct when they state, "Sophisticated multivariate techniques cannot be expected to replace careful a priori thinking and design;" it would be unfortunate if careful investigators were to avoid multivariate techniques on the basis of their unreplicated study.

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QUESTIONABLE MULTIVARIATE STATISTICAL INFERENCE IN WILDLIFE HABITAT AND COMMUNITY STUDIES: A REPLY

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Abstract: We substantiate our contention that our multivariate analyses (Rexstad et al. 1988) mimic analyses found in the wildlife literature. We caution users of multivariate techniques that data collected in our discipline are unlikely to meet the restrictive assumptions of these techniques. Even if assumptions are met, we contend interpretation of the results is likely to lead to invalid inferences.

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Given a large set of ecological data from a complex system, scientists attempt to explain the processes underlying those data. "The best way to summarize a mass of multifactor data is by a simple equation or set of equations. . . . Routine use of standard computer programs to fit equations to data does not usually succeed. . . . When computer work is a substitute for hard thinking, it is of little use" (Daniel and Wood 1971:vii). We echo these words in the context of multivariate analysis of habitat and community studies.

We appreciate Taylor's (1990) comments on our use of multivariate statistical analyses (Rexstad et al. 1988); he makes some valuable suggestions concerning appropriate methodology for improving inferences drawn from multivariate techniques. These include the use of the sphericity test (Bartlett 1954) in principal component analysis (PCA), examination of random data sets of the same dimension as the data set of interest (sensu Stauffer et al. 1985), and validation of discriminant function analysis (DFA) with independent data (Capen et al. 1986).

However we disagree with Taylor's (1990: 188) criticism that we ". . . explicitly question the techniques themselves, not uncritical inferences," and that we "flawed [our] study." These objections set the tone for what is, in our opinion, a misrepresentation of our original paper and therefore serve to obfuscate the issues addressed by Rexstad et al. (1988). The objective of our response is to address these 2 points and to substantiate our claim that our analyses were similar to those appearing in *The Journal of Wildlife Management* (JWM).

We acknowledge the comments received from K. P. Burnham and B. K. Williams on an earlier draft of this paper.

Our contention that multivariate techniques can distill patterns in data when no patterns exist is not new. Green (1979:139) notes "descriptive multivariate statistical methods can describe supposed structure that is nothing more than random variation, and it is easy to convince yourself that things you want to see in the data are in the data. A factor analysis will extract factors and a cluster analysis clusters from any data set, even random normal deviates." Similarly, Verbyla (1986:1255) notes that prediction bias (i.e., the inflated predictive ability of a model when based on data used to develop the model) "can lead to erroneous conclusions about the biological significance of the independent variables" in multivariate statistical models. We reinforced these messages when we stated: "Sophisticated multivariate techniques cannot be expected to replace careful a priori thinking and design" (Rexstad et al. 1988:797).

Although statistical inference is not explicitly mentioned in these caveats, clearly inference is the subject of these warnings. We, therefore, cannot explain Taylor's perception that we questioned the techniques themselves when the title of our paper included the word "inference." We acknowledge that multivariate statistics are founded on firm mathematical grounds. Application of these techniques, under certain conditions, yields statistically reliable inferences; however, these conditions (assumptions) are often unmet in biological studies. This pattern of inappropriate application motivated our questioning of the validity of multivariate inferences reported in the wildlife management literature.

The remainder of our reply will focus on what we feel is the main issue of the controversy, namely that we "flawed [our] study." We demonstrate that our study mimics analyses found in the majority of multivariate studies published in JMW. We contend that our original conclusion, that it "seems questionable that multivariate techniques, as currently applied, can consistently identify meaningful relationships in complex, natural systems" (Rexstad et al. 1988:797), is tenable and that our study design was logical given our original objective.

The use of multivariate techniques is sufficiently prevalent in our profession that a review of the past 5 years of JWM revealed 38 papers using discriminant function analysis (DFA), principal components analysis (PCA), or canonical correlation analysis (CC); 28 papers analyzed data with DFA (16 used stepwise procedures), 12 papers used PCA, and 1 paper used CC. The number of analyses exceeded the number of papers because some papers performed multiple analyses. This translates to >1 DFA paper per issue of JWM and a PCA paper nearly every other issue. Our purpose is not to critique past application of multivariate techniques to habitat and community studies, but to compare previous analyses with our study. In particular, we were looking for indicators of parallel data structure, tests of assumptions, and validity of inferences.

Violating DFA assumptions appears to be pervasive in wildlife studies. Equality of variance-covariance matrices is a central assumption of linear DFA (Williams 1983). In our original paper, 35 of 50 tests of equality of variance-covariance matrices were rejected using Box's *M*-test. Taylor (1990:188) presents the unsubstantiated argument that "The *M*-test is marred by the fact that a significant result may indicate only deviation from normality, to which most statistical procedures are quite robust, and many investigators ignore it for this reason. Other investigators assume that DFA, and the formal test of significance used in DFA, are robust to lack of equality of the variance-covariance matrices, although general analysis of the importance of this assumption appears to be lacking." We agree that investigators apparently assume that DFA is robust when variance-covariance matrices are unequal. We found only 1 paper using DFA that acknowledged meeting the assumptions of equality of variance-covariance matrices among groups. Eighteen papers made no mention of the assumptions of DFA, and 6 papers noted that the assumptions were not met, but proceeded with the analysis. Authors often cited Green (1971) who excused application of discriminant analysis in violation of assumptions if the results had "ecological interpretation," or Williams (1983) who differentiated between exploratory and confirmatory analyses, the former being less sensitive to assumption violation. Little published support exists for the belief that DFA is robust when variance-covariance matrices are not equal (see Gilbert 1969, Marks and Dunn 1974, Wahl and Kronmal 1977). This faith in robustness appears to be based on convenience rather than on statistical support. Researchers cannot be complacent about violating the assumption of equality of variance-covariance matrices merely because it has been widely ignored.

Several papers attempted to meet the DFA assumption of normality by transforming some variables to approximate normality. However, transforming individual variables does not ensure multivariate normality (Mood et al. 1974: 141) which is assumed in DFA analyses. Taylor (1990) contends that assumptions of DFA may be relaxed with large data sets, but fails to cite a supportive statistical reference. He suggests that our analyses were conducted on a small data set and thus are vulnerable to violation of assumptions. However, a review of JWM in our original paper indicated that our matrix dimensions (75 observations × 15 variables) were similar to published studies. We conclude that it is difficult to determine the validity of the assumptions of DFA in most cases, and many times when assumptions were tested, they were not met. Consequently, DFA likely yields unverifiable and often invalid inferences.

Taylor (1990) suggests that DFA models should be applied to independent data sets to test validity. We strongly concur. Jackknifing (Efron 1982) is another approach that may help evaluate model validity. However, only 1 paper used jackknifing to assess the "stability" of the canonical coefficients, and only 2 papers withheld data during model construction to use as an independent validation set. Cohen's κ-statistic (Cohen 1968, Titus et al. 1984) measures the improvement of classification over random chance and was used in only 1 instance. Proper approaches to DFA model validation do not appear to be used widely in wildlife habitat studies.

The warnings issued by Williams (1983:1289) about DFA need reiteration: "Under these circumstances [violation of assumptions] it [DFA] may or may not focus the researcher's attention on meaningful relationships. The expectation that it will argues for its use, even when assumptions are clearly violated. The same argument, of course, can be made for any exploratory data procedure. . . . It should be clear from the foregoing that discriminant analysis has frequently been used improperly in ecology."

Taylor (1990) contends that our PCA of the covariance matrix is meaningless because 2 variables explained 99.5% of the variance. We acknowledge that our result was an artifact of variables that were measured on very different scales (e.g., student grade point average vs. used car prices). We concur that interpretation of PCA of the covariance matrix is questionable under such circumstances, but suggest that wildlife habitat variables used in PCA's are frequently measured using different scales (e.g., percent canopy cover vs. canopy height). Results similar to ours are feasible when researchers use PCA of the covariance matrix on unstandardized habitat variables. Our review of the literature indicated that only 2 papers reported whether PCA was conducted on the correlation or covariance matrix.

Taylor (1990) contends that PCA of the correlation matrix did not result in reduced dimensionality of our data set. He further states that in the absence of rigorous hypotheses "this reduction in dimensionality is trivial . . ." (Taylor 1990:187). We note that PCA is not a hypothesis-testing procedure and that criteria to determine whether dimensions are reduced are often not explicitly stated. In our interpretation of meaningful principal components as those with eigenvalues >1, we merely emulated a popular interpretation of PCA results. As indicated by Taylor (1990), a more objective approach to PCA is to use a sphericity test to measure chance correlation among variables. However, of papers in JWM that used PCA, none reported the application of the sphericity test prior to proceeding with interpretation of principal component scores.

Taylor (1990) questions our interpretation of our canonical correlation ($r = 0.601$) as evidence of a strong relationship between biotic and abiotic variables. He also indicates that rejection of the null hypothesis of no relationship at $P = 0.087$ is not valid. We agree that interpretation of CC is arbitrary and that our rejection at $P = 0.087$ is optimistic by statistical standards. However, given the totally arbitrary nature of our data, this seemingly high CC and low likelihood of a Type I error are interesting. Would a researcher looking for relationships within a data set containing biotic and abiotic components not be tempted to draw conclusions based on a similar result? The only example of CC appearing in JWM did not ascribe a significance level to the correlation found in their analysis.

We substantiate our conjecture that our multivariate analyses (Rexstad et al. 1988) did not differ mechanically from the analyses presented in JWM by our review of the recent literature. Violations of assumptions, as Taylor (1990) suggests, have unknown impacts upon results of statistical procedures. In Williams' words (1983:1291), "we are well served by the scientific method, which counsels a healthy skepticism in interpretation as well as inference." Multivariate techniques are most prevalent where manipulative experiments to discover underlying processes and validation of results are often difficult if not impossible.

In our original manuscript we attempted to address 2 major problems with multivariate techniques as applied to habitat and community studies. We explicitly demonstrated that by mimicking the applications of multivariate statistics as practiced by wildlife biologists today—applications that include violations of assumptions, arbitrary rules for exclusion of data, and ignored warning flags—that apparently significant relationships could be found among functionally unrelated factors. The greater part of our paper addressed this issue, which represents the primary focus of Taylor's (1990) comments. The other issue we raised, but did not explicitly address, concerned the biological interpretation of the statistical results. Correctly applied, multivariate techniques may reveal statistically significant relationships, but even then the careful investigator is not free from problems of biological interpretation.

In conclusion, we thank Taylor (1990) for sharing our concern over the use of multivariate techniques. We hope that researchers will heed our cautionary notes. We reiterate our warning that a posteriori analyses of multidimensional data sets are unlikely to lead to understanding of biological systems. Worse, the inferences drawn from multivariate analyses may create misunderstanding by "detecting" patterns that do not exist in biological systems.

We invite others to collect a set of multivariate data on a series of absurd variables, perform multivariate analyses using every precaution, and draw conclusions regarding the validity of inference from multivariate methodology.

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