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Univariate Analyses of Morphometric Variation Do Not Emulate the Results of Multivariate Analyses

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In a recent paper, Willig et al. (1986) compared the empirical results of univariate (ANOVA) and multivariate (MANOVA) analyses of morphometric data from a large suite of external and cranial characters that were obtained from 21 species of bats from Northeast Brazil. Recognizing that multivariate analysis is the methodological approach of choice when asking systematic questions, we stated that "the degree to which univariate results are coincident with multivariate results will assess the utility of the univariate approach" (p. 196).

The critique of Corruccini (1987) contends that we "assume a priori that the existence of differences shows the multivariate approach superior and preferable" (p. 396). For fundamental theoretical reasons, outlined in our original contribution and reiterated herein, we consider it axiomatic that for a multivariate question, a multivariate analysis is preferable. We pointed this out in the original article, and cited a number of biometric references that hold similar contentions. We reiterate that it is well understood that the MANOVA is *preferable* to a sequence of ANOVAs; the point of our previous work was to evaluate empirically whether the commonly used univariate ANOVAs emulate the multivariate results. Our results indicated that no consistent application of a univariate criterion (a priori number of significant ANOVAs required before declaring a significant multivariate difference between groups) effectively emulated the results of the MANOVAs.

Generally speaking, Willig et al. (1986) tested the null hypothesis that no difference in systematic conclusion exists between multivariate and univariate methodologies in evaluating group differences. Moreover, on a case by case basis, the probability of correctly emulating multivariate conclusions, or making a Type I or Type II error *with respect to the multivariate conclusions* (not to be confused with α or β error in the strict sense), was documented for cranial, external, and combined character suites. The figure in Willig et al. (1986) shows the distribution of those errors based on a variety of univariate criterion values; however, it must be emphasized that these distributions were only known a posteriori (after the MANOVAs were performed for each species) and that they may only apply to these bat taxa from Northeast Brazil. An error rate of more than 15% occurred when the "best" univariate criterion was chosen (see fig. 1B in Willig et al., 1986). We consider this error rate to be unacceptable for systematic conclusions.

Although Corruccini's second paragraph is true, it is incomplete and not germane to the issue that we examined in our original article. A correlation is a measure of association between variates in different distributions; the existence of a correlation does not logically or pragmatically imply a *predictive* relationship. That a significant association between MANOVA and ANOVA results exists is not surprising or interesting; that the correlation coefficient (r) was so low (0.68) for the combined cranial and external character suites attests to the lack of correspondence between univariate and multivariate methodologies. Moreover, given a number of significant ANOVA effects for a previously unanalyzed biological population, we have no idea of the relative distribution of significant univariate and multivariate results. Even if we knew the correlation in advance, which we would not, it would be uninformative for any particular systematic comparison. The extent to which predictive uncertainty is characteristic of our results is shown

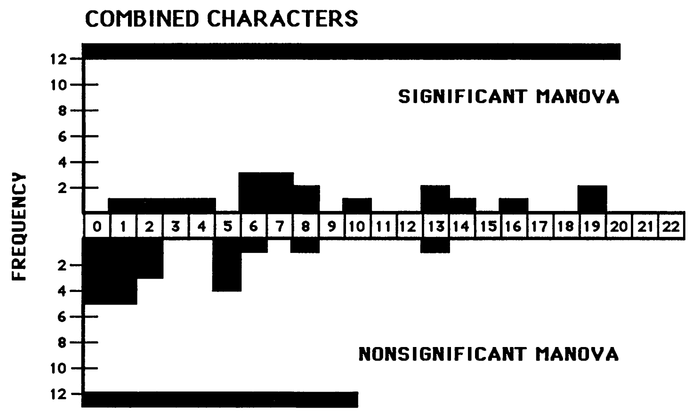

FIG. 1. Paired bar diagrams for combined cranial and external characters in which the number of significant ANOVAs per corresponding MANOVA is displayed for both significant and nonsignificant multivariate analyses. The X-axis represents the number of significant ANOVAs per MANOVA; the Y-axis is the frequency of occurrence; and the horizontal bars located at the top and bottom of the Y-axis indicate the 95% confidence intervals for items (number of significant ANOVA treatment effects per MANOVA). The ranges of data yielding significant and nonsignificant MANOVAs overlap greatly and the 95% confidence interval of items for the data exhibiting nonsignificant MANOVAs are completely contained within the 95% confidence interval of items for the corresponding data exhibiting significant MANOVA treatment effects. Similar results are obtained for cranial and external character suites.

graphically in Figure 1, which illustrates the impracticality of predicting MANOVA significance based upon a single value from the available range of potential univariate criteria (number of significant ANOVAs).

Indeed, the *t*-test and Mann-Whitney *U*-test performed by Corruccini further suggest either a misunderstanding of the systematic dilemma investigated in our previous paper, or a confusion concerning the null hypothesis that is evaluated by those tests. Differences in the central tendencies of the number of significant ANOVA treatments between data sets that yielded significant versus nonsignificant multivariate results are irrelevant to the question addressed in our original paper because they provide no criterion for evaluating how many significant ANOVAs are required to declare overall significance. Rather than compare the degree to which the confidence limits of the *means* overlap, we are more interested in comparing the degree to which the confidence limits of *items* overlap. Again, Figure 1 illustrates the large overlap between the 95% confidence interval for items of data sets yielding significant and nonsignificant MANOVAs. In short, neither the tests of central tendencies nor the correlation provide predictive criteria for evaluating the significance of an effect via the univariate approach.

We do thank Corruccini for pointing out the name (Rao's paradox) of a potential cause of the discrepancy between univariate and multivariate results and citing relevant references to the phenomenon. We recognized the phenomenon when we stated in the conclusion "that an apparent dilemma may arise if a MANOVA is nonsignificant but some characters exhibit significance in the analogous ANOVAs" (p. 201). Rao's paradox may account for this situation; however, in the absence of empirical evidence, such a suggestion must remain a hypothetical explanation. If high intercorrelation exists among characters and leads to Rao's paradox, then the problem lies in the utilized character suite, not in the multivariate statistical procedure. In fact, an appropriate interpretation of Rao's paradox is that the suite of intercorrelated characters does not provide sufficient evidence for group differences (a conclusion entirely coincident with the statistical nonsignificance detected in the multivariate analysis). The disparity between multivariate and univariate results may suggest the existence of this paradox, but it does not logically lead to acceptance of the conclusions derived from a set of univariate analyses.

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On the Naming of Higher Taxa

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Telford and Mooi (1986, Syst. Zool., 35: 254–255) point out that the use of the suffix -oidea in forming certain categories of higher taxa conflicts with the use of that suffix for naming taxa of superfamily rank, a use that has been recommended in the latest edition of the International Code of Zoological Nomenclature. They proposed the changing of the names of 5 classes of the phylum Echinodermata from Echinoidea, Holothuroidea, Crinoidea, Asteroidea, and Ophiuroidea to Echinoides, Holothuroides, Crinoides, Asteroides, and Ophiuroides, i.e., simply changing the final -a to -s.

This is not a solution because -oides is not a plural formant, but singular and therefore forms names suitable for use in the genus group. *Asteroides* in fact appears in Nèave (1940, Nomenclator Zoologicus. Volume 3) as a genus group name in the Coelenterata.

The Code exempts itself from any regulation of names "for taxa above the family group" (Art. 1.b.4). There are many reasons why the "wise men" who in their myriads formed the Code have done this. One reason is that many such names are not formed upon a basonym, that is, a genus name to which a suffix is added. Another is that higher taxa are very numerous and of many ranks and have a most troublesome habit of changing rank when a new classification is proposed. The simplest way out of this is to specify the rank when a name of higher rank is used: Class Asteroidea, subphylum Pelmatozoa, suborder Comatulida, (taxon?) Elasipoda. Perhaps -iformes (used in birds, etc.) could be used (Asteriformes, etc.), and perhaps the "newly erected" Laganidea and Scutellidea have already pointed out an even better way (-idea instead of -oidea).

The same problem is also found by those who wish to name numerous taxa intermediate between superfamily, family, subfamily, etc., now recognized. These in larger groups are very numerous, one at each branching of a phylogeny. Those who restricted the number of named taxa recognized by the Code were perhaps indeed "wise."

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