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Letter to the editor

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In a recent article, Carter et al. (1997) have returned to an issue raised by Cressie et al. (1986), namely, the statistical analysis of somatotype data using a methodology that is both statistically valid (i.e., achieves the purported significance level) and statistically powerful (i.e., for a given significance level has a small probability of falsely accepting the null hypothesis). Cressie et al. (1986, pp. 204–207) give a careful mathematical statistical derivation of all their results leading *inter alia* to the following conclusions:

Conclusion 1. The ANOVA (Analysis of variance) methodology outlined by Heath and Carter (1967), Carter (1980), and Carter et al. (1983) uses inappropriate degrees of freedom. Hence, the associated tests have significance levels that are actually smaller than the purported or nominal ones. That is, Heath and Carter's proposed analysis is not statistically valid.

Conclusion 2. A correction to the inappropriate degrees of freedom given by Heath and Carter allows a corrected ANOVA methodology that is (approximately) statistically valid.

Conclusion 3. There is a MANOVA (*Multi-variate* analysis of variance) methodology that has greater statistical power than the (corrected) ANOVA methodology proposed by Heath and Carter.

Carter et al. (1997) recently took a few sentences to respond to Cressie et al.'s (1986) criticisms, in an aside to their analysis of somatotypes of Canadian boys followed from ages 7–16 years. They wrote (pp. 264–265): "In proposing an alternative method of analyzing independent group data, Cressie et al. (1986) claimed that using the SAD prematurely collapsed the three-component somatotype vectors into a scalar SAD value, thereby reducing degrees of freedom for the F-ratio. They suggest increasing the degrees of freedom to include those for the three components as separate variables, thus increasing the likelihood of type I errors when compared with the method of Carter et al. (1983). Their basic premise was that the three somatotype components should be considered together in a one-way MANOVA. As a test of the whole somatotype, their premise is false because it compromises (or denies) the integrity of the whole somatotype and erroneously increases the degrees of freedom. Furthermore, the SAD should be treated as any other derived variable and not be assigned degrees of freedom based on the variables from which it is derived. The procedures of Cressie et al. (1986) are not applicable to analysis of the somatotype as a whole, but could be applied as a secondary analysis to the separate components."

This response is both curious and factually incorrect. It is curious because no attempt has been made to refute the mathematical statistical theory derived on pp. 204–207 of Cressie et al. (1986). Moreover, in the beginning of their 1997 article, Carter et al. gave a very nice introduction to the concept of somatotyping as "an assessment that reduces many physical aspects to the three-digit somatotype rating" (p. 257). What then does the term "whole somatotype" mean when the same authors say that, "As a test of the whole somatotype, [Cressie et al.'s] premise is false, because it compromises (or denies) the integrity of the whole somatotype" (p. 265)? On the contrary, by analyzing the three components *together* (*Multivariate* ANOVA or MANOVA), rather than an arbitrary one-number summary proposed by Heath and Carter (ANOVA), Cressie et al.'s method *preserves* the three-variate nature of somatotyping.

The rest of the phrase goes on to say that "[Cressie et al.'s proposal] erroneously increases the degrees of freedom." Where is the error on pp. 204–207 of Cressie et al. (1986)? A claim such as this requires substantiation. Perhaps the substantiation comes from the next sentence, "Furthermore, the SAD should be treated as any other derived variable and not be assigned degrees of freedom based on the variables from which it is derived." Again, Carter et al. seem to be arguing against themselves: They agree that the variable is derived but then say that one should ignore the basis of that derivation. A close look at the somatotype attitudinal distance (SAD) shows that it is a Euclidean distance between two (three-variate) vectors. Write $a = (a_1, a_2, a_3)$ and $b = (b_1, b_2, b_3)$; then the

squared Euclidean distance between a and b is,

$$\backslash a - b\backslash^2 = (a_1 - b_1)^2 + (a_2 - b_2)^2 + (a_3 - b_3)^2,$$

which is the sum of *three* individual squared differences. Each one of these three squared differences potentially contributes one degree of freedom, leading to each SAD contributing up to *three* degrees of freedom (*not one* as Heath and Carter essentially claim). In fact, the correct degrees of freedom for each SAD is somewhere *between* one and three. (The full three degrees of freedom are obtained when and only when endomorphy, mesomorphy, and ectomorphy vary independently of each other.) The correct distribution and degrees of freedom for Heath and Carter's test statistic are given by (A.5) and (A.6) on p. 206 of Cressie et al. (1986). From their statistical analysis, Cressie et al. are able to characterize when Heath and Carter's proposed degrees of freedom are correct, which is when and only when endomorphy, mesomorphy, and ectomorphy lie along a straight line in the three-dimensional somatotype space. This circumstance is very atypical; Cressie et al. (1986) analyzed somatotype data and reported that Heath and Carter's proposed degrees of freedom should be approximately *doubled!*

In conclusion, Heath and Carter have used a rule for degrees of freedom that is correct for an ANOVA based on the SAD for a *single* component of (or linear combination of) the three-dimensional somatotype score. However, the SAD is a Euclidean distance in the full three-dimensional space and so all the classical ANOVA formulas for degrees of freedom are not correct and have to be modified. This is precisely what Cressie et al. (1986) do in Appendix A. Carter et al.'s statement that the MANOVA ''could be applied as a secondary analysis to separate components'' confirms that they do not understand the issues raised in Cressie et al. (1986). In fact, as discussed above, Cressie et al. show that Heath and Carter's ANOVA, with their proposed, incorrect degrees of freedom, is only valid on *separate* components (or linear combinations of them)!

I fully appreciate that many readers will not follow these technical issues. Nonetheless, any statistical analysis relies on statistical distribution theory as its basis. If Carter et al. (1997) are to offer an informed discussion of Cressie et al. (1986), they should understand the distribution theory underlying both the ANOVA and the MANOVA proposals. Then, I am sure there would be agreement on Conclusions 1., 2., and 3. set out above. Moreover, they would realize that whenever the ANOVA method can be applied, such as for the repeated measures analysis in Carter et al. (1997), so too can the more powerful MANOVA method, after straightforward extension of the distribution theory presented in Cressie et al. (1986), Appendix B.

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