When a Comment Is Much Ado About Little:  
A Reply to Spence

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We read with interest Spence’s (1999) comments directed toward our meta-analysis (Hausenblas, Carron, & Mack, 1997). That meta-analysis was undertaken to summarize research in which the theories of reasoned action (TRA; Ajzen & Fishbein, 1980; Fishbein & Ajzen, 1975) and planned behavior (TPB; Ajzen, 1985, 1988, 1991) had been used by researchers to better understand exercise intention and behavior. Although a number of minor points of contention were also raised by Spence, the three main criticisms he leveled were that

- One of the terms used was inappropriate.
- An essential statistical analysis was not carried out.
- The size of the sample was not adequate for meta-analytic purposes.

The purpose of our response is to address these three criticisms, as well as some auxiliary concerns.

Terminology

Spence was correct in his assertion that our use of the term zero-order correlation to describe correlation coefficients that are at or near zero was not appropriate. This is an example of careless writing on our part. We stand corrected and regret any confusion we might have produced for the readers of our article and the Journal of Sport & Exercise Psychology.

Clarity of expression is at the heart of communication. Nonetheless, we doubt that valuable space in a journal that boasts a rejection rate of 85% was provided by the editor in order to highlight careless writing. Thus, we have to assume (an assumption acknowledged by Spence) that the issues considered more substantive

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emanate from Spence's argument that "a meta-analysis, or a primary study for that matter, requires the application of some type of regression techniques in which interrelationships between competing constructs are statistically controlled" (p. 377).

**Statistical Analysis**

In our original work, we acknowledged that a multivariate statistic such as regression would have been useful for our meta-analysis:

A note of caution concerning the interpretation of the results is warranted. The constructs within TRA and TPB are interrelated (and ordered in a specific manner in the respective models). The ESs reported here undoubtedly overestimate the magnitude of the overall relationships within these models. It would have been beneficial to examine the predictive utility of both TRA and TPB using hierarchical regression or path analysis. However, . . . only 14 ESs were available to examine the complete TRA model, and 8 ESs were available to examine the complete TPB model. Consequently, due to insufficient power, neither a hierarchical regression nor a path analysis were computed. (p. 43)

As the title to Spence's commentary illustrates, he did not consider this note of caution sufficient. In fact, he subsequently concluded (i.e., his third point) that "if enough studies were not available, one must question the legitimacy of testing the predictive utility of these theories in a meta-analysis" (p. 378).

We have two reactions to Spence's concern with what he considers the lack of an appropriate statistical analysis. The first reaction pertainsto what we didn't do (i.e., conduct a regression analysis). Although we again acknowledge that it would have been preferable to do so, we again must emphasize that we did not have sufficient power to undertake some type of regression analysis. Available to us were 14 ESs to examine the complete TRA model and 8 ESs to examine the full TPB model.

How many ESs would have been required to undertake some type of regression analysis? A power analysis shows that proceeding from (a) an assumption of large effects among the various TRA and/or TPB constructs, (b) an alpha level of $p < .05$, and (c) power of .80, the minimum sample (i.e., number of ESs) needed for regression analyses would be either 38 or 42, depending on your preferred theorist (see Cohen, 1988, 1992; Green, 1991). A more reasonable a priori decision, however, would have been for us to assume medium effects among the constructs. In that case, the minimum sample necessary would be either 89 or 91, again depending on your preferred theorist. In short, we would have needed 4 to 10 times the number of effect sizes available to us to ensure sufficient power to justify a regression analysis.

How important is the issue of power? Based on his commentary, Spence's answer to this question seems to be "not very important"—our lack of power did not seem to him to be a significant inhibitor. He is incorrect; it is a significant inhibitor. We suspect that Spence is familiar with the work of Paul Meehl (1990) because he cited Meehl in his critique of our research. He may wish to reread that work, particularly the section in which Meehl commented on the issue of power: