Commentary on Dzewaltowski’s Commentary

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I was indeed surprised when I received a letter from the editor of the Journal of Sport & Exercise Psychology inviting me to react to Dzewaltowski’s commentary on Kimiecik (1992), which examined the utility of the theories of reasoned action and planned behavior for predicting and understanding exercise behavior of corporate employees. (In all honesty, I was quite pleased just to get the study published in a reputable journal rather than have it collect dust in my office along with many of my other papers.) Because this type of interaction/discussion occurs far too infrequently in research journals, I am more than happy to react. The purpose of this paper is to make a few specific comments that address some of Dzewaltowski’s comments, which I found insightful and well thought out.

Although Dzewaltowski made some excellent points, nothing in his commentary changed my view on one thing: Based on two studies (Dzewaltowski, 1989; Dzewaltowski, Nobel, & Shaw, 1990) of college undergraduates, Dzewaltowski concluded that self-efficacy (social cognitive) theory would be more useful for studying exercise behavior than reasoned action or planned behavior; I still disagree with this conclusion and stand by my original statement that his “conclusions, based on only two studies, may be premature” (Kimiecik, 1992, p. 202). In addition, studying college students participating in activity classes does not provide an adequate test of these theories in exercise contexts. Sears (1986) has suggested that overreliance on college students for theory development has led to a flawed view of human behavior. Dzewaltowski’s research is far from flawed. I am only suggesting that his statements concerning the utility of self-efficacy theory versus reasoned action and planned behavior are premature and that studies which include more diverse samples in a variety of contexts are needed before any conclusions can be made about the value of these theories in exercise contexts.

Furthermore, although intention does not predict behavior over and above that accounted for by self-efficacy in two studies, this is no reason to disregard the contributions of reasoned action and planned behavior for studying exercise behavior. I still believe that the lack of predictive power on the part of intentions in both of Dzewaltowski’s studies was due to a restriction in range. In fact, he and his colleagues state that students in their 1990 study were uniformly positive in their intentions to participate in physical activity (as were the students in the

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1989 study). I am convinced that the restriction in range of intentions is the critical factor that contributed to the very low percentage of behavior variance in the 1989 study ($R^2=.05$) and 1990 study ($R^2=.10$) versus the results of my study ($R^2=.46$). Moreover, in Dzewaltowski's (1989) study the time frame between the measurement of intentions and behavior was 7 weeks, whereas my study had a time frame of 4 weeks. It has been suggested by Ajzen (1987) that the longer the time lag between measurement of intention and the criterion behavior, the less likely it is to find a strong relationship.

I would also argue that comparing intentions with self-efficacy may be inappropriate. Dzewaltowski et al. (1990) claim,

Both the theory of planned behavior and social cognitive theory postulate a central variable that mediates behavior. For the theory of planned behavior, intention is identified as a determinant of behavior in all situations. For social cognitive theory, self-efficacy is the central predictor variable. (p. 398)

I disagree. Intention is not the central variable mediating behavior in the theory of planned behavior. It is one in a chain of interrelated variables. I would argue that intentions serve a different theoretical function than self-efficacy. In the theories of reasoned action and planned behavior, intentions have a predictive role, but it is the beliefs and attitudes that are the underlying determinants of behavior. In self-efficacy theory, the self-efficacy construct plays both a predictive and an explanatory role. Since the two constructs serve different roles, should they be compared?

Dzewaltowski also suggests that my study would have made a greater contribution if it had tested self-efficacy versus perceived behavioral control and behavioral intention in the prediction of corporate employees' physical activity. This was not my purpose, but let us say that I had. And let us say the self-efficacy was the most powerful predictor. Would I then conclude that social cognitive theory was the "better" theory and that future work should investigate the utility of this theory for understanding and predicting exercise behavior? No! All I can say is that for this sample, based on this method, these were the results. I get very uneasy when researchers start espousing the benefits of one psychological theory over another. (Although I have been guilty of this myself, I will try to refrain from doing so in the future.) I say this for two reasons. First, different studies utilizing the same theory almost always use different ways of defining and measuring the same variables. For example, in my study I measured intentions with the item "I intend to participate in vigorous physical activity long enough to work up a sweat at least 3 days a week during the next 4 weeks," whereas Dzewaltowski (1989) measured intentions by averaging four different items: "I will try to," "I intend to," "I have decided to," and "I am determined to." The point is that our intention measures are different, and this is the rule rather than the exception in most theoretical work done on exercise behavior up to this point. Few studies even using the same theory as other studies can be compared because most researchers define and measure the same variables in different ways. Add to this that the dependent variable, exercise behavior, is almost always measured differently from study to study, and the result is theoretical chaos.