INDIVIDUAL DIFFERENCES
AND THE DEVELOPMENT
OF PERCEIVED CONTROL

Ellen A. Skinner
Melanie J. Zimmer-Gembeck
James P. Connell

WITH COMMENTARY BY
Jacquelynne S. Eccles

PERCEIVED CONTROL AND THE DEVELOPMENT
OF ACADEMIC MOTIVATION

Jacquelynne S. Eccles

It was with great pleasure that I accepted the charge to write the Commentary for this *Monograph*. I have followed the work of Ellen Skinner, James Connell, and their colleagues for many years with great enthusiasm because it has such strong theoretical and empirical grounding. This *Monograph* certainly reinforced this view. Rarely does one have the opportunity to praise a piece of work as a tour de force—such a characterization is fully warranted in this case. Skinner, Zimmer-Gembeck, and Connell have given the field a great gift—one that will serve as a model of longitudinal research and analysis for years to come.

The authors set out, and fully met, clear theoretical and empirical goals. In so doing, they provided the field with both a comprehensive account of the control-action theory of motivation and its link to attribution theory. The associations among their various constructs were thoroughly discussed, and a wide set of specific hypotheses were generated. Furthermore, the authors were quite clear about when their theoretical framework could lead to specific individual differences hypotheses and when it merely provided hints for exploratory empirical work.

In addition, the authors provided an excellent example of how to aggregate a set of constructs into a higher-order construct on the basis of differentiated patterns of responding rather than simple summary composites. Guided by both their theoretical framework and empirical findings from a variety of motivational perspectives, the authors specified exactly which patterns of beliefs would facilitate, and which would undermine, academic engagement. By and large, their predictions were confirmed, and these composites yielded the strongest relations. For example, the strongest evidence for their general motivational model came from LISREL and cross-time lagged

analyses using these composite variables. In each case, the findings were consistent with their model of influences running from context to self-beliefs to action to outcomes.

Finally, the authors laid out a smaller set of specific developmental predictions based on a variety of other theoretical and empirical work. I found their explication of the launch, ambient, and change-to-change developmental models especially interesting. This distinction should facilitate thinking in the field about the need for proposing mathematically specific developmental models. However, given the current level of theorizing about developmental changes in these types of constructs, the authors were justifiably more cautious in making these predictions than they were in making the individual difference hypotheses derived from their own theoretical framework. Consequently, several of the more interesting patterns of developmental change were not predicted a priori. This was particularly true for the change-to-change model findings. The authors provided quite interesting post hoc explanations for these findings, along with a challenge to the field to replicate the findings and test the validity of their various possible explanations.

Given my theoretical biases, I was especially intrigued by both the data and the theorizing related to systematic grade-related changes in classroom contexts. As has been suggested by several researchers, including those working with me on our stage-environment fit theory of declining academic motivation (see Eccles, Wigfield, & Schiefele, 1998), these authors found evidence of a link between students' declining perceptions of their classroom context and negative changes in achievement-related beliefs (in this case, control beliefs), which, in turn, were linked to declining engagement. They also found that this pattern was particularly marked at the time of the transition into middle grades' educational settings.

I was, however, a bit surprised by the authors reluctance to make change-to-change predictions and by their suggestion that change-to-change models are rare in developmental work. I agree with them that this model is not incorporated into the design of longitudinal studies as much as it should be, given its relevance for all contextual-based developmental theories; and I applaud the authors for both pointing out the importance of such an approach and clearly demonstrating its power. There has, however, been a dramatic increase in the prevalence of these types of studies over the last 10–15 years. Furthermore, work in areas of prevention/intervention, operant conditioning, and time-series analyses have used these types of designs for a very long time (e.g., see Bronfenbrenner & Morris, 1998; Elder, 1998). Finally, prospective longitudinal studies of this type are quite common in studies of life span development (see, e.g., Baltes, Lindenberger, & Staudinger, 1998).

The contribution of this *Monograph* to our appreciation of the statistical methods available for longitudinal analyses is also quite impressive. The au-

thors used several different statistical techniques both to test their various hypotheses and to perform their more exploratory analyses. Given the complexity of both their data set and their hypotheses, they had to make many decisions along the way—ranging from how to aggregate the participants into appropriate groups for the analyses to how best to present their findings. Through the use of multiple summaries, tables, and an extensive methodological appendix, the authors did a masterful job of explaining both the issues they had to confront and the rationale for their particular solution. They have done the field a great service by providing such a detailed discussion of their techniques, particularly their hierarchical linear modeling (HLM) analyses. The HLM method is emerging as a powerful tool in longitudinal analyses. Although it was originally developed to look at contextual influences in nested designs (such as students nested within classrooms), several statistical programs are being developed and refined for use in the longitudinal modeling of stability and change. This *Monograph* makes these new techniques accessible to developmentalists in a very concrete way.

In addition, this *Monograph* will certainly stimulate the debate about how best to deal with missing data and attrition in longitudinal studies. HLM provides one solution to this problem—it models the developmental trajectories of individuals using all available information. Although this approach is mathematically quite legitimate, I am sure that there are those of us who become increasingly uncomfortable with this strategy as the amount of missing data increases. Yet attrition is a fact of life in longitudinal work—at least given the current norms regarding funding levels for developmental research. Studies such as this one are needed to find the optimal mix of "real" data with modeled "data" for understanding development.

The authors have also provided a model of the importance of internal replication. Since their data are correlational in nature, it is impossible to draw firm causal inferences. Instead, correlational studies need to be evaluated on the consistency of the empirical evidence with specific causal models. Providing congruent evidence from several different analytic techniques and from various subpopulations within the sample bolsters our confidence that the findings are not an artifact of either the methods used or the particular sample studied. This *Monograph* includes several examples of this type of internal replication. Given these methodological strengths, it would be quite appropriate to use this *Monograph* in graduate developmental courses as a methodological primer as well as an exemplary developmental study of control theory.

Controversial Issues Raised by This Work

Any study as rich as this one should stimulate debate about the controversial issues in its domain. In this discussion, I focus on three: links to other

motivational theories, difficulties with studying complex, dynamic systems, and where and what is context.

Links to Other Motivational Theories

Allan Wigfield, Uli Schiefele, and I just completed a chapter for new *Handbook of Child Psychology* (Eccles et al., 1998). One thing that was clear in our review was the need to begin directly to compare various motivational theories to each other. There has been a tremendous proliferation over the last 15 years in both the theories and the key constructs linked to a social cognitive perspective on motivation. The control action approach guiding the work presented in this *Monograph* is one of the most important and well developed of these social cognitive perspectives. At the measurement level, it is most similar to Weiner's attribution and Bandura's self-efficacy theories. As the authors note, their approach also shares similarities with expectancy-value approaches and with the work by Dweck and her colleagues (e.g., Dweck & Elliott, 1983) on the meaning of ability. The authors do a very nice job of integrating their approach with Weiner's (1979); they provide a less complete analysis of the overlap between their approach and Bandura's (1996). Finally, like most of the researchers in the field of academic motivation, they provide very little analysis of the relation of their approach to other, more affective-based motivational theories, including the value component of expectancy-value models. Yet all these approaches seek to explain engagement and performance, and most of them include hypotheses regarding the mediating role of self-beliefs between context and engagement/performance. At present there is very little attempt within the broader community of motivational researchers directly to compare these various approaches in order to find out which are the most powerful influences on engagement/performance.

I believe that it is time to pit these various motivational models against each other. As both these authors and many others in the field point out, there are now many similar constructs linked to capacity and control beliefs (e.g., personal efficacy, outcome efficacy, expectancies, locus of control, attribution theory, etc.). And, as noted above, the models from which these constructs are derived make similar predictions about the general influences on engagement and performance. Like so many of the rest of us, Skinner et al. discuss some of these overlaps but then proceed to explore only those predictions derived from their model. Such an approach is quite acceptable if the goal is to test a specific set of hypotheses derived from one theory. It is less useful if the goal is either to move toward a more comprehensive theoretical understanding of academic motivation or to make policy and practice recommendations to school personnel about the best way to improve children's

academic motivation. In my opinion, this field is now ready to move beyond this fractionated approach to a more comparative approach so that we can begin to understand the relative power of all these various constructs.

Effective intervention recommendations also require this type of work because we need to know which are the most economical and powerful points to target for intervention. Skinner et al. do a very nice job of discussing the types of interventions that one might consider on the basis of their findings. But they say little about other powerful variables such as affect/emotions and values/goals precisely because their study has no implications for these types of potential mediators. This will continue to be the state of our ability to make policy recommendations until all of us interested in academic motivation begin to do more direct comparative work. In addition, although the findings presented here are consistent with their theoretical model, these constructs actually explained very little of the variance in either engagement or performance—suggesting that belief in control may be necessary but not sufficient to produce engagement. We need to know which factors are most predictive of engagement in order to design the most effective intervention programs.

There are two additional important issues related to the relation of this report to other work in this field: general beliefs versus domain-specific beliefs and beliefs about the nature of ability. Skinner et al. have chosen quite general measures of control beliefs. Other motivational psychologists (e.g., Bandura, 1996; Marsh, 1984; Eccles et al., 1998) have argued that academic motivation is much more domain specific. Several of us have also designed domain-specific indicators of constructs quite similar to the constructs used in this report. Perhaps the amount of variance accounted for in engagement and performance would have been higher had Skinner et al. used more domain-specific indicators of their constructs.

Second, I have always been intrigued by the relations among theories of ability, perceived control, performance, and engagement. Most social cognitive motivational theorists assume that a strong belief in one's control and a belief that ability is modifiable are optimal—a strong sense of personal efficacy and belief in one's ability to master challenging tasks is good. I basically agree. But we need to consider the possibility that not all things are under our control and some individual differences in aptitudes contain an element of stability that is very likely to influence final possible levels of competence. Is it the case that children are born totally pliable with regard to aptitudes and interests, or are there genetic predispositions? Can any child become an Olympic champion in swimming, or are there innate differences that will make it much easier for some children to achieve this goal than others? And if, as evidence from behavioral genetics suggests (e.g., Loehlin, 1992; Rowe, 1994), the later is the case, then what are the implications for our social cognitive models of achievement motivation?

It seems to me that one important developmental task for each person is to discover which aptitudes and interests are most relevant to oneself. In other words, it is critical to discover either when one has control (that is when effort will matter) and when one does not or exactly how much control one has in each situation or domain. Within this model of development, one optimal adaptation strategy would be to identify those situations and/or domains in which one has maximal control or potential and then to focus much of one's energies on perfecting these potentials or situations.

Individuals have to focus their energies to some degree—they do not have sufficient time or energy to excel at everything. This focusing can be guided either by the individuals themselves through personal selection, or by the individuals' social context, or by some mix of both these influences. It is undoubtedly critical for optimal motivation in any specific context that individuals feel in control of their ability to master the demands of that situation. Work by a wide variety of motivational theorists has demonstrated this fact repeatedly. But it is probably also useful for individuals to be able to select themselves into settings in which they, in fact, have maximal control over their outcomes.

From this prospective, motivational problems are most likely to arise when there is not a good match between the demands of the situation and the individual's unique aptitudes and interests. For example, schools may be a risky setting for some children if there is no provision in that setting for them to demonstrate and develop competence in those areas most closely linked to their personal pattern of aptitudes and interests.

Several motivational psychologists have paid some attention to this dilemma. As noted by the authors, people like Nicholls (e.g., 1984) and Schunk (e.g., Schunk & Cox, 1986) have argued that retraining children's attributions or feelings of control without providing them with the skills necessary to succeed in the school setting is counterproductive. Furthermore, the launching effect of early school achievement on control beliefs demonstrated in this study suggests that children do adjust their control beliefs in response to academic feedback in a manner quite consistent to the adaptive model suggested above. But the full implications of this perspective for maintaining motivation in school settings and for providing children with a greater diversity in the types of skill areas in which they could focus their energies have not been adequately considered by motivational psychologists, particularly those with a social cognitive/control/personal efficacy orientation.

Studying Dynamic Systems of Influence and Behavior

Skinner et al. note that their model is dynamic. They include this dynamic perspective by including early grades as a launching factor in their

longitudinal models and by including feedback loops in their LISREL models. In both instances, performance emerged as an important influence on, as well as an outcome of, control beliefs. But for the most part, and as is done in most developmental research, the analyses performed reflected a linear, unidirectional flow of influence. Developmental psychologists are very good at hypothesizing complex, dynamic models. We are much less adept at testing such models. To simplify our task in such situations, we typically generate linear models of causal influence. Although this is done by everyone, it is not clear that such empirical models are the best way to capture highly interactive, dynamic processes.

Skinner et al. are to be applauded for going beyond simple linear models in some of their analyses. I wish that they had extended these efforts further. For example, with the exception of including previous grades as a launch variable in some of their modeling of slopes and intercepts of control beliefs, their HLM and related regression analyses are quite unidirectional; and, to the extent that they did consider bidirectional effects, they focused only on the potential feedback relation of performance to control beliefs. Other feedback loops and bidirectional influences are also quite probable. For example, I suspect that early grades also have a launch type of influence on individual differences in perceptions of the teacher context. Similarly, I suspect that performance influenced teachers' ratings of their students' engagement. Finally, it is quite likely that control beliefs influenced perceptions of the context. All these bidirectional influences are theoretically feasible within various models of human perception.

Testing such hypotheses would have required more extensive use of either cross-lagged structural equation modeling or HLM analyses with time-varying covariates. The latter strategy would have been an especially good way to assess change-to-change models, particularly if the authors had tested both the predicted and the alternative causal relations using varying lagged patterns. For example, the authors could have used perceived teacher context in one set of analyses as the time-varying covariate to assess whether changes in perceived teacher context predicted changes in perceived control. They could then compare this with a model in which changes in perceived control predicted changes in perceived teacher context. A simplified version of such a comparison could have been done with more extensive cross-lagged structural equation modeling.

Let me reiterate—this concern, like the ones raised earlier, are more a comment on the current best practices in our field than a comment on this specific project. These authors have gone far beyond the level of longitudinal analyses usually used in longitudinal studies. As such, this study will be a strong stimulus to the field to move forward in its analytic sophistication.

Nonetheless, there is still much to be done to better specify and test dynamic psychological models.

Another important methodological issue raised by this study is the need to be very careful about inferences we draw when we compare within- and between-informant measures. As is true in all such studies, the relations between within-informant measures were much stronger than the relations between across-informant measures. For example, in this case, the links between perceived teacher context and perceived control were stronger than the links between perceived control and former grades. Does this mean that teacher context is a stronger influence on perceived control than grades? I do not think that the findings presented in this *Monograph* provide an answer to this question, precisely because the students provided the data for both perceived context and perceived control while the teacher provided the grades. The authors provide a very strong rationale for getting their measures from the specific sources that they selected. But we also need to recognize the methodological limitations that such choices place on our ability to draw strong inferences about differential relations in the data.

This brings me to my last methodological/theoretical concern: How should we operationalize engagement? At the simplest level, this is solely a methodological issue. Can teachers assess engagement? To the extent that engagement includes an emotional and motivational component (which it does in these authors' conceptualization), how good are observers at inferring these internal psychological states? Evidence from personality and social psychology suggests not very good. In addition, my colleagues and I have found that junior high school teachers, in particular, are not very accurate in their ratings of their students' adjustment to junior high school (Lord, Eccles, & McCarthy, 1994). Instead, we found that the teachers used the students' academic performance to assess the students' motivation and adjustment. Given these findings, we need to be cautious in interpreting the causal relation between teachers' ratings of their students' engagement and the grades that these same teachers give the students at the end of the year. Having an independent indicator of performance, such as a score on a standardized test in addition to teachers' grades, would have provided stronger evidence in support of the authors' causal prediction.

At a higher level, the issue of what is engagement is fundamentally theoretical. Skinner et al. operationalize the construct in one way. Other operationalizations are quite feasible. There is an emerging consensus that engagement is likely to be the most powerful mediator of the link between motivation and performance (see Eccles et al., 1998). We now need more extended discussion in the field about what engagement actually is and how it is best operationalized and measured.

Where Is Context?

As our field gets more engaged in research on context, we will need to develop much better theories about what context is and how it is best measured. In this *Monograph,* the authors treat this issue primarily as a methodological problem. They carefully point out, and discuss, the pros and cons of using student reports of classroom context, and, although they acknowledge a need for multiple methods, they argue quite persuasively for both the accuracy and the theoretical validity of student perceptions. I fully agree with their argument. I also think that there is a more fundamental issue here: Where and what is context? Is it outside or inside the individual or both? And, if the latter, as suggested by the classic works in perceptual psychology, what are the critical components of the perceiver and the context, and how do these components interact with each other to influence both perception and behavior? Although this has been a classic problem in perception, it has received relatively little theoretical attention in social and motivational psychology.

A Gibsonian analysis leads to the conclusion that the world external to the individual has affordances that restrict or bound the range of possible internal perceptions. Likewise, the perceiver has certain properties, both mechanical and psychological, that restrict or influence perception. Although some social and personality psychologists have studied the "eye of the beholder," little of this work has informed motivational psychologists' speculations about the influence of context on motivation. Many of us fall back on the assumption that the context is outside the individual and more real than the individuals' perceptions. Such an assumption leads us to focus on prediction regarding the influence of context on beliefs. Consequently, we miss opportunities to study equally compelling hypotheses regarding the influence of beliefs on perceptions of the context. The authors have a perfect data set in which to explore these types of hypotheses.

The problem of where is the context is further highlighted by an increasing number of studies of contexts at a variety of levels, ranging from the family to the neighborhood or school building, that find greater within-context variations in perceptions of the context than average between-context mean differences. For example, studies of classroom effects typically report that less than 10 percent of the variation in perceptions of the classroom context reflects between-classroom effects (Bryk & Raudenbush, 1992). Studies of neighborhood effects are finding similar patterns (e.g., Furstenberg, Cook, Eccles, Elder, & Sameroff, in press). Finally, the findings regarding shared and nonshared family environment influences also suggest a relatively weak influence of the shared properties of the family context on human development (Plomin & Daniels, 1987; Rowe, 1994).

So what are we to conclude about individual differences in perceptions of context? These patterns of results are consistent with several interpretations—ranging from the strength of differential treatment effects within a context to the kinds of individual differences in the perceiver discussed above. The point is that we need to think much harder about these variations for both theoretical and methodological reasons.

In summary, this is a wonderful *Monograph*. As noted earlier, the authors set our their goals very clearly and did an excellent job of carrying them out. The *Monograph* is a model of solid, tight theoretical and methodological rigor. It provides compelling evidence for the authors' control-action theory. But, even more important, both the methods and the results reinforce the importance of several critical issues that motivational psychologists need to consider very carefully if this field is to move forward in substantively important ways. The field can be grateful to these authors for providing such an excellent stimulus for this important future work.

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