EDUCATIONAL PSYCHOLOGIST, *39*(2), 111–133


# The Relation Between Self-Beliefs and Academic Achievement: A Meta-Analytic Review

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There has been extensive debate among scholars and practitioners concerning whether self-beliefs influence academic achievement. To address this question, findings of longitudinal studies investigating the relation between self-beliefs and achievement were synthesized using meta-analysis. Estimated effects are consistent with a small, favorable influence of positive self-beliefs on academic achievement, with an average standardized path or regression coefficient of .08 for self-beliefs as a predictor of later achievement, controlling for initial levels of achievement. Stronger effects of self-beliefs are evident when assessing self-beliefs specific to the academic domain and when measures of self-beliefs and achievement are matched by domain (e.g., same subject area). Under these conditions, the relation of self-beliefs to later achievement meets or exceeds Cohen's (1988) definition of a small effect size.

There is a long-standing view among many educators that the beliefs and feelings students have about themselves are a key determinant of academic success (Beane, 1994). Juxtaposed against this viewpoint, others have argued that self-beliefs are either irrelevant to academic achievement (Emler, 2001; Seligman, 1993) or, worse, part of the problem of academic underachievement (Stevenson, 1992; Stout, 2000). Those proposing the latter point out that many students seemingly harbor positive beliefs about themselves that lack a substantive basis in actual skills or prior accomplishments, thus creating a false and ultimately damaging foundation for approaching learning situations in school.

The contrasting views regarding the role of self-beliefs in academic achievement have significant implications for both theory and practice. From a theoretical standpoint, efforts to clarify the role of beliefs and feelings about the self in shaping academic achievement outcomes may inform understanding of the degree to which attitudinal and affective variables are important in mediating educational outcomes. From an applied perspective, differing views on the status of self-beliefs as influences on achievement often have a prominent role in arguments offered for or against investing resources in differing types of school reform and intervention programs (DuBois, 2001; Kahne, 1996). Thus, programs designed to promote self-esteem or related self-constructs (e.g., self-efficacy beliefs) often are advocated on the basis of the assumption that self-beliefs are important to achievement. The opposing viewpoint similarly has been used to garner support for competing types of reform, such as those that focus on increasing standards and accountability for student learning (e.g., mandatory promotion and graduation requirements).

The relation between self-beliefs (broadly defined) and student achievement has been examined in a large number of studies, most of which have been cross-sectional in design. Previous reviews of this literature have concluded that self-beliefs and academic achievement are positively and

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moderately correlated (Hansford & Hattie, 1982; Hattie, 1992; West, Fish, & Stevens, 1980; Wylie, 1979; B. J. Zimmerman, 1995). Simply establishing a relation between self-beliefs and achievement, however, does little to help distinguish empirical support for the competing perspectives regarding the role of self-beliefs in student achievement noted previously. Thus, even if self-beliefs and achievement levels are assumed to be causally related, it is impossible to determine the extent to which concurrent associations between them reflect effects of self-beliefs on achievement, contributions of achievement to self-beliefs, or some combination of these two types of processes (Byrne, 1996a). To address this concern, longitudinal studies of the relation between self-beliefs and student achievement have been conducted. The methodologically strongest of these investigations have controlled statistically for baseline levels of achievement when using self-beliefs to predict later achievement, thus allowing for a focus on possible contributions of self-beliefs to changes in achievement over time (Marsh & Yeung, 1997a).

Despite a growing number of studies reporting on the longitudinal relation between self-beliefs and academic achievement, to date there has not been a systematic, quantitative review of their findings (see Byrne, 1996a, and Marsh, 1993, for brief narrative reviews). There is a need for this type of review for several reasons. First, the question of whether self-beliefs affect later achievement deserves a state-of-the-art review because many prominent theories in educational psychology as well as other areas of the social sciences rest on the assumption that beliefs about the self are causal agents in human behavior and learning (e.g., self-determination theory, Deci & Ryan, 1985; social learning theory, Bandura, 1997; self-regulation theory, Carver & Schreier, 1981). Second, we chose a quantitative review because sufficient data are available to support such a review and because there is the potential for significant bias to occur when authors rely on more subjective approaches to describing available findings (Cooper, 1998). In addition, systematic review methods can be useful for identifying important methodological and conceptual moderators of effect size. Finally, through comprehensive coding of study information, "gaps" may be identified in design, analysis, or reporting of findings within existing investigations. Illustratively, it may be found that too few studies are available to address the significance of one or more theoretically important moderating variables.

This article uses meta-analysis to synthesize findings of longitudinal investigations of the relation of self-beliefs to academic achievement. The focus of the article is on investigating (a) the strength and (b) the potential moderators of the contributions of self-beliefs to later achievement. The following sections address several important areas of background for the review. These include terminological, theoretical, and empirical issues pertaining to the definition and structure of self-beliefs, theoretical and empirical evidence suggesting self-beliefs contribute to academic achievement, and conceptual and methodological influences that may moderate any effects of self-beliefs on achievement.

## SELF-BELIEFS

### Self Terms

Theory and research on the relation between self-beliefs and achievement have been hampered by reference to a confusing array of differing self terms (Hattie, 1992; Wylie, 1979). The terms used most frequently, however, are *self-concept*, *self-esteem*, and *self-efficacy* (Byrne, 1996a). *Self-concept* has been defined broadly as "a person's self-perceptions formed through experience with and interpretations of his or her environment" (Marsh & Hattie, 1996, p. 58; see also Shavelson, Hubner, & Stanton, 1976). *Self-esteem* has been viewed as encompassing evaluations of the descriptive components of self-concept (Beane & Lipka, 1980; Brinthaupt & Erwin, 1992; Rosenberg, 1979). As described by Bandura (1997), perceived *self-efficacy* refers to "beliefs in one's capabilities to organize and execute the courses of action required to manage prospective situations" (p. 2).

Theoretically, self-concept, self-esteem, and self-efficacy beliefs share a common emphasis on an individual's beliefs about his or her attributes and abilities as a person. However, these constructs also have been distinguished from one another along dimensions that could lead them to differ in their levels and, hence, relations to academic achievement (Harter, 1983). Self-esteem, for example, may be high or low for a given self-concept depending on the extent to which positive or negative views in the self-concept are concentrated in areas central to the individual's value system (Harter, 1999) and as a function of the personal standards used to evaluate attributes and accomplishments that are reflected in the self-concept (Rosenberg, 1979). Perceived self-efficacy, in turn, has been viewed as (a) more tied to specific areas or domains of functioning than self-concept (Pajares, 1996; B. J. Zimmerman, 1995), (b) being concerned with judgments of personal capability rather than to the judgments of worth associated with self-esteem (Bandura, 1997), and (c) more directly associated with goals than either self-concept or self-esteem (Pajares & Schunk, 2002).

Despite these theoretical distinctions, empirical efforts to distinguish between self-concept, self-esteem, and self-efficacy beliefs have met with only limited success (for a review, see Byrne, 1996a). This is reflected in part in relatively high correlations between proposed measures of the differing constructs (Pajares, 1996). Methodologically, however, such analyses are complicated by the fact that the content of differing measures is rarely unambiguous with respect to the specific aspect of self-beliefs that is being measured (Byrne, 1996b; Pajares, 1996; Wylie, 1979). It is not unusual, for example, for measures of self-concept to include items that refer to both evaluative views and feelings about the self (i.e.,

self-esteem) and perceptions of capability to perform tasks in different domains (i.e., self-efficacy beliefs).

Based on the preceding considerations, it may be premature to assume clear or consistent conceptual distinctions in assessments of self-beliefs across studies that report having examined differing types of beliefs in relation to academic achievement (Byrne, 1996a). Still, any differences that do emerge in association with the type of self-beliefs assessed could be theoretically important and provide an impetus for further work on refining existing measurement approaches.

### Multidimensionality

A second important concern is the well-documented multidimensional structure of self-beliefs (for reviews, see Bandura, 1997; Harter, 1999; Marsh & Hattie, 1996). With respect to self-concept, for example, there is a potential for students to have specific self-concepts related to their abilities in differing areas of school work such as math and English, as well as those occurring in a wide range of domains not pertaining directly to school such as appearance, social skills, and athletic competence (Hattie, 1992; Marsh & Hattie, 1996; Shavelson & Bolus, 1982). These types of self-beliefs may be arranged hierarchically within the self-system according to their varying levels of content specificity, with the highest and most general levels providing the foundation of global beliefs and feelings about the self (see Byrne, 1996a; Harter, 1983; Marsh & Hattie, 1996). Self theorists nonetheless consistently have emphasized that more circumscribed areas of self-beliefs may be influential in shaping adjustment apart from their associations with more generalized self-beliefs (Bandura, 1997; Harter, 1999). It has been suggested in this regard that self-beliefs pertaining to particular domains may in fact be more instrumental in shaping adaptive functioning in related areas than are more generalized beliefs (Harter, 1999). On this basis, several theorists have noted that academic self-beliefs are a potentially stronger source of influence on school achievement than more general self-beliefs (Byrne, 1996a; Wylie, 1979; B. J. Zimmerman, 1995).[1] In accordance with this view, a meta-analysis of cross-sectional studies (Hansford & Hattie, 1982) found that the average correlation between measures of self-concept and achievement was substantially larger when measures of self-concept of academic ability were used ($r = .42$) in comparison to measures of global self-esteem ($r = .22$) or self-concept ($r = .18$). Whether this pattern reflects differential contributions of academic self-beliefs to achievement, however, is not clear in the absence of a similar analysis for longitudinal findings.

[1]The theoretical importance of self-beliefs that pertain to the academic domain has been emphasized consistently in the literature that addresses the role of self-efficacy beliefs in academic achievement (e.g., B. J. Zimmerman, 1995). It should be noted, however, that this type of domain-specificity has also been suggested to be important for self-esteem (DuBois & Tevendale, 1999) and self-concept (Wylie, 1979).

### Theoretical Rationale for Contributions of Self-Beliefs to Achievement

Several theoretical rationales have been suggested for self-beliefs as a causal agent in academic achievement. With respect to self-concept, there is considerable research supporting the idea that people actively seek to maintain consistency in how they view themselves (for reviews, see Brown, 1993; Swann, 1997). It has been suggested in this regard that students with positive views of themselves may strive to behave and perform in ways that are consistent with their self-image and thus be more likely to achieve highly in school on this basis (Rosenberg, 1979). Several specific mechanisms for fulfilling motivation for consistency have been discussed. These include both *self-affirmation* (i.e., taking action with the intent of demonstrating to oneself that one's self-concept is accurate; Steele, 1988) and *self-regulation* (i.e., monitoring current behaviors for discrepancies with the self-concept and acting to reduce any discrepancies by adjusting behavior; Scheier & Carver, 1988). With respect to implications for achievement in school, students with positive views of themselves and their abilities thus may engage in achievement-related behaviors such as studying for tests and completing homework because these help to confirm their self-perceptions (e.g., Pajares, Britner, & Valiante, 2000). These processes may be most relevant in relation to a positive academic self-concept specifically, as opposed to a more general positive self-concept that may not include positive views in this area (Byrne, 1996a; Marsh & Yeung, 1997a).

Students with high self-esteem similarly may strive for academic achievement as a means of maintaining feelings of self-worth (Rosenberg, 1979). Conversely, those with low self-esteem may engage in various self-handicapping behaviors (e.g., procrastination) to protect themselves from esteem-threatening, ability-based attributions for poor school performance, despite the ultimately negative implications of such tendencies for academic achievement (Covington, 1989), or may overgeneralize failure if it occurs in a domain they consider important (Brown & Dutton, 1994). Relatedly, positive self-esteem also has been conceptualized as a resource for coping with failure (Baumeister, 1999), thus suggesting a contribution to adaptive task persistence that could facilitate better school performance. As with self-concept, however, generalized feelings of positive self-regard may be based on success in nonacademic areas. Under these circumstances, high levels of self-esteem theoretically may diminish rather than increase adaptive efforts in the academic realm (Shavelson & Bolus, 1982). High levels of academic self-esteem thus could be expected to be involved more reliably in processes facilitating academic achievement.

According to self-efficacy theory (Bandura, 1997), positive efficacy beliefs promote exertion of effort, selection of adaptive goals, behavioral choices that are congruent with goals, and task persistence. In other words, students with positive self-efficacy beliefs for a given domain may be more

likely to engage in approach behaviors relative to the domain, giving them more opportunity to practice and receive corrective feedback than students avoiding the task. In addition, students with positive self-efficacy beliefs appear to be more likely to use multiple adaptive self-regulatory strategies (e.g., Pintrich & De Groot, 1990). Thus, even among equally able students, self-efficacy theory predicts that students with higher self-efficacy for a given problem will perform better than students with lower levels of efficacy. Consistent with considerations noted previously, the effects of efficacy beliefs are assumed to be highly domain specific, such that benefits (e.g., task persistence) are greatest for activities in the same domain. Thus, whereas self-efficacy for solving algebra problems would be expected to promote algebra achievement, this is not necessarily the case for generalized efficacy beliefs that could be reflective of feelings of confidence in other domains (Schunk, 1994; B. J. Zimmerman, 1995).

Further theoretical considerations are relevant to the potential for self-beliefs as a causal agent in academic achievement. For example, self-determination theory (Deci & Ryan, 1985) suggests that individuals will exert more effort and will demonstrate more persistence when pursuing goals concordant with their own self-descriptions (Sheldon & Elliot, 1999). A related framework for understanding the relation between self-beliefs and motivation comes from Hazel Markus and her colleagues (e.g., Markus & Nurius, 1986; Markus & Ruvolo, 1989; Markus & Wurf, 1986). Markus proposed the construct of possible selves as a mechanism for organizing self-relevant information. *Possible selves* are cognitive representations of what the individual might become in the future and can be positive (honor student) or negative (dropout). Discrepancies between desired future possible selves and self-concept have especially important motivational consequences. Higgins (1987) referred to this as the distinction between the actual self (the self-concept) and the ideal self (positive future self-concept). The ideal self can serve as a standard of reference to which the actual self-concept is compared. Some empirical research supports the premise that possible selves can have motivational properties. For example, experimental research by Ruvolo and Markus (1992) demonstrated in a sample of undergraduate women that imagining future success was associated with better performance than imagining future failure.

In addition to the links between self-beliefs and motivation, several scholars have investigated the intermediate relations between self-beliefs and conation (e.g., Kuhl & Fuhrman, 1998) that link self-beliefs with the mechanisms of self-regulation. For example, Sheldon and Elliot (1998) found that goals that were not self-generated (i.e., controlled goals) were characterized by a high state of commitment at the decision phase that faded when action needed to be carried out. In contrast, self-directed goals (i.e., autonomous goals) were characterized by both high levels of commitment and strong follow-through. Thus, it appears that one reason goal-directed behavior breaks down is that the individual experiences a failure in the volitional processes that connect goals to behavior when goals are not consistent with the self (Corno et al., 2002).

In further important work, Eccles and her colleagues (e.g., Eccles et al., 1983; Wigfield & Eccles, 2002) found support for a model in which valuing an activity serves as a mediator between self-beliefs and achievement, a process implied in other models that address conditions facilitating goal-directed behavior (e.g., self-determination theory) and cognitive processes (e.g., self-regulated learning; Butler & Winne, 1995; Carver & Schreier, 1981). *Value* is conceptualized as a function of four components: (a) the extent to which an activity is viewed as being important (and presumably, why the activity is important; Dweck, 1986), (b) the degree to which an activity is intrinsically interesting, (c) the expected utility of the outcome in meeting goals, and (d) the cost of engaging in the activity. Thus, according to this theory, a key process through which positive self-beliefs may facilitate greater achievement is that such beliefs may tend to contribute to greater valuing of achievement in comparison to students with less favorable self-beliefs (Pokay & Blumenfeld, 1990). These types of processes again, however, have been discussed exclusively with respect to self-beliefs for the academic domain.

## MODERATORS OF THE RELATION OF SELF-BELIEFS TO ACHIEVEMENT

Moderating influences would include any factors that affect the strength and/or direction (i.e., positive or negative) of effects of self-beliefs on achievement. For present purposes, methodological and theory-based moderators are distinguished. *Methodological moderators* are considered to be those factors that may affect observed relations of measures of self-beliefs to achievement without having implications for "true" relations between the underlying constructs. *Theory-based moderators* are considered to be those that may affect not only observed relations, but also the relations between underlying constructs.

### Methodological Moderators

*Participant recruitment.* Most studies of self-beliefs and achievement have relied on convenience samples. Several large-scale longitudinal studies, however, have used random selection procedures to recruit representative samples (e.g., National Educational Longitudinal Survey, High School & Beyond). These studies also have used weighting techniques within data sets in a further effort to approximate nationally representative samples. It thus is possible that studies using convenience samples will generate different effect size estimates than those relying on representative samples. One limitation of national studies, however, is that, perhaps due to their relatively broad aims, they have tended not

to incorporate optimal levels of attention to factors that are of theoretical interest with regard to the relation between self-beliefs and achievement, such as assessment of self-beliefs pertaining specifically to the academic domain. Relying solely on these types of studies thus could limit understanding of theoretical processes underlying the relation of self-beliefs to academic achievement.

*Statistical control of variables other than prior achievement.* In causal modeling approaches, the greatest threat of Type I error comes from model misspecification (Pedhazur, 1997). Thus, as suggested previously, even when examining the relation between a measure of self-beliefs and a measure of later achievement controlling for prior achievement—if some unknown (and hence unmeasured) third variable is related to self-beliefs but not controlled for statistically—its influence on achievement will be misattributed to self-beliefs. Academic ability could be one type of variable that it is useful to control for in this regard, given that it has the potential to affect both initial self-beliefs and changes in achievement over time. Any number of other variables (e.g., socioeconomic status, or SES) might be useful to consider for similar reasons (Wylie, 1979). When one or more such variables is controlled for, estimated relations between self-beliefs and later achievement will reflect effects attributable only to that portion of self-beliefs that is not associated with the control variables (Pedhazur, 1997). It is possible, however, that some of the influence of self-beliefs is reflected in variance shared between measures of self-beliefs that is being controlled. To the extent that this is the case, controlling for additional variables when investigating relations of self-beliefs to later achievement may lead to underestimation of the true magnitude of effects. Thus, although it is of interest to assess whether effects of self-beliefs on achievement remain evident in this type of analysis, such results have the potential to be overly conservative or biased downward (Rogosa, 1979).

*Reliability of scores.* Another methodological factor that may moderate observed relations between measures of self-beliefs and later achievement is the reliability of the scores on the self-beliefs and achievement measures. Unreliability attenuates observed bivariate relations between variables. However, in longitudinal designs such as those used in studies of possible effects of self-beliefs on academic achievement, unreliability of the criterion at Time 1 (T1; e.g., achievement) actually may increase the observed relation between T1 scores on the predictor variable of interest (e.g., self-beliefs) and scores on a more reliably measured criterion variable at Time 2 (T2; Rogosa, 1979). Latent variable techniques have been used to correct for unreliability of scores in several studies of self-beliefs and academic achievement. The preceding considerations suggest that this type of methodology may be useful in yielding less biased, but not necessarily larger, estimates of effects of self-beliefs on academic achievement.

*Stability of measures.* A further potential source of methodological influence is the degree of stability over time that is evident for the outcome measure of achievement. To the extent that stability in the achievement measure is high, there will be less residual variation in scores at the later point in time after taking into account that variation that can be predicted from baseline scores (Pedhazur, 1997). It is this residual variation or evidence of change in the achievement measure over time that is being predicted by initial scores on the predictor measure of self-beliefs. Limited or restricted amounts of this type of variation may serve to reduce the estimated magnitude of possible effects of self-beliefs on achievement.

### Theory-Based Moderators

*Type of self-belief measured.* As discussed earlier, from a theoretical standpoint it is possible that differing types of self-beliefs (i.e., self-concept, self-esteem, self-efficacy beliefs) may vary in the nature and degree of their influence on academic achievement. In their meta-analysis, Hansford and Hattie (1982) found similar associations with achievement for measures that used the terms *self-concept* ($r = .22$) and *self-esteem* ($r = .18$), respectively. B. J. Zimmerman (1995) suggested that measures of self-efficacy beliefs have yielded stronger and more consistent relations with indices of academic achievement than other types of self measures (e.g., self-concept). This conclusion seems to have been based on the assumption that self-efficacy beliefs necessarily reflect a greater level of domain specificity. In practice, however, this is not always the case (Byrne, 1996a). For example, researchers have summed the results of task-specific questions to form a more general scale of academic self-efficacy (e.g., Bandura, Barbaranelli, Caprara, & Pastorelli, 2001). Given that domain specificity itself may be an important moderator, distinguishing clearly between type of self-belief and level of specificity seems desirable.

*Specificity of self-belief measurement.* As noted previously, generalized self-beliefs and those specific to the academic domain may be related differentially to academic achievement, with stronger effects for the latter than the former. The merits of refinement in measurement of self-beliefs within the academic domain, such as separate assessments for differing subject areas, also have been discussed (Marsh & Hattie, 1996; Pajares, 1996; B. J. Zimmerman, 1995). There are thus several degrees of specificity that could represent an important source of influence on the strength of effects linking self-beliefs to achievement (Byrne, 1996a).

*The index of achievement.* Theoretically, self-beliefs may differ in the degree to which they influence differing areas of academic achievement. Because self-beliefs may operate largely by influencing motivational processes, their effect may be larger on aspects of achievement that are potentially more directly related to student motivation, such as

teacher-assigned grades, than on other aspects for which motivational influences may be less important, such as standardized tests (Wylie, 1979). In addition, grades are a more immediate and obvious source of comparison between students than are scores on standardized tests and thus may be a more important source of feedback about the self (Rosenberg, 1979). Consistent with these considerations, Hansford and Hattie (1982) found that, in most cases, grades were correlated more highly with self measures than were scores on standardized tests.

*Match between self-beliefs and achievement for specific subject areas.* When considering self-beliefs pertaining specifically to the academic domain, another influential consideration may be whether measures of self-beliefs and achievement refer to matching or corresponding subject areas. As noted previously, self-beliefs within the academic realm can be distinguished according to differing subject areas involved such as math, English, and so forth. Theoretically, these types of relatively circumscribed self-beliefs would be expected to have the greatest degree of influence on learning and achievement that occurs within the same subject area (Bandura, 1997; Byrne, 1996a; B. J. Zimmerman, 1995). Illustratively, math self-concept should influence achievement in math more so than do other belief domains (e.g., English self-concept). Similarly, achievement in a given subject area may have relatively more pronounced effects on self-beliefs linked to that area compared to others (e.g., Marsh & Yeung, 1998).

*Measurement delay.* A further potentially important consideration is the delay between measurements. It seems clear that, if prior achievement is controlled in a longitudinal study of the relation between self-beliefs and achievement, some minimum delay will be necessary to detect an effect. Theoretically, different processes have been described through which effects of self-beliefs on academic achievement may cumulate during the course of schooling (DuBois, 2001). These include mutually reinforcing patterns of influence between the two constructs over time (Rosenberg, Schooler, & Schoenbach, 1989). It also is conceivable, however, that observed effect sizes may be diminished by relatively long delays that allow for other influences to impinge on the relation between self-beliefs and later achievement.[2]

*Age.* Several considerations suggest that developmental factors also may be important. The ability to think abstractly and to apply abstractions to the self unfolds as cognitive processes mature (Harter, 1999). Thus, whereas the self-beliefs of young children tend to be both uniform (all aspects of the self are of the same valence) and unrealistically positive, such beliefs become increasingly differentiated and more negative with age (Harter, 1999; Jacobs, Lanza, Osgood, Eccles, & Wigfield, 2002). Based on these changes, self-beliefs could have greater implications for achievement during the course of development. Because the relation of self-beliefs to achievement may be mediated in part through academic motivation (e.g., Meece, Wigfield, & Eccles, 1990), effects also could become stronger at older ages as a result of schooling becoming more demanding and requiring greater time and effort. Hansford and Hattie (1982) reported that the association between self and achievement measures varied significantly according to average age of the sample. Consistent with the preceding considerations, the strongest association was found for secondary students ($r$ = .27) and the weakest for preschool students ($r$ = .12). However, the association found for postsecondary students ($r$ = .14) was lower than that found for either primary or secondary school students. Postsecondary students are likely to be a more homogenous population, especially in terms of their self-beliefs pertaining to the academic domain. Accordingly, restriction of range in self measurements (and perhaps achievement indices as well) may have attenuated the effect size for this age group. This possibility illustrates the importance within meta-analysis of taking into account potential methodological moderating influences prior to investigating those that are theory based (Cooper, 1998).

*Academic ability.* Learning disabilities and other academic skill limitations may present challenges to students that increase the importance of positive self-beliefs for learning and achievement (Chapman, 1988). This type of moderating influence for academic ability was not found to be evident among cross-sectional studies (Hansford & Hattie, 1982). Only a minority of studies, however, provided the information necessary to reliably classify student ability levels (Hattie, 1992).

*Gender.* Hansford and Hattie (1982) found that the strength of the association between self and achievement measures did not differ significantly for boys and girls. However, several considerations suggest gender as a possible moderator. Interpersonal relationships and other nonacademic concerns (e.g., appearance) tend to assume a more prominent role in the self processes of girls relative to boys (Harter, 1999; Josephs, Markus, & Tafarodi, 1992), for example, thus potentially detracting from the degree to which they are suited to facilitating gains in achievement. In addition, a substantial body of empirical research has highlighted characteristics of the school environment that may be associated with gender differences in beliefs about the self. Teachers may tend to interact with boys more, for example, and provide them with higher quality feedback (Eccles & Blumefeld, 1985). Theoretically, such differences have the

[2]In addition to its theoretical implications, the length of delay between measurements of achievement clearly also has methodological implications.

potential to contribute to gender-based variation in relations between self-beliefs and achievement (Hattie, 1992).

*Socioeconomic and cultural background.* Low SES constitutes a risk factor for poor academic achievement (Duncan & Brooks-Gunn, 2001). Theoretically, positive self-beliefs thus may function as a protective influence for students from socioeconomic disadvantaged backgrounds (DuBois, 2001). Prior reviews of cross-sectional studies (Hansford & Hattie, 1982; West et al., 1980) have not revealed a consistent finding suggesting the relation between self measures and achievement varies by student socioeconomic background. It is possible, however, for the role of protective factors to become evident only when examining their relations to outcomes in a longitudinal framework (Werner, 1995).

With regard to cultural factors, it has been suggested that feelings of self-worth among African American youth become relatively detached from academics, especially at latter stages of schooling (Steele, 1992). To the extent that this occurs, self-beliefs could be relatively less influential for African American students (and perhaps other students from minority backgrounds) relative to White students (Osbourne, 1995). Consistent with this possibility, Hansford and Hattie (1982) found that the association between measures of self and achievement was higher, on average, for White samples ($r$ = .33) in comparison to samples of African Americans ($r$ = .19) and those belonging to various other ethnic groups (e.g., Chicano). An important concern not addressed, however, is whether such differences were evident independent of the SES backgrounds of students, which may be related to stereotype threat (e.g., Croizet & Claire, 1998).

Broader societal and cultural factors represent a further source of possible influence on the relation between self-beliefs and student achievement (Hattie, 1992). In Western countries such as the United States, in which there is a relatively strong emphasis on individualism and importance of formal schooling, self-beliefs could have a more prominent role in affecting academic achievement.

*School transition.* The potential role of self-beliefs in facilitating the adjustment of youth to change or transitions in their school environments also has received considerable attention (Simmons & Blyth, 1987). School transitions, such as the move from elementary school to middle or junior high school, are marked by a new physical environment, a new social structure, and more difficult academic work and have been linked to at least temporary declines on measures of self-esteem and academic self-concept (Seidman, Allen, Aber, Mitchell, & Feinman, 1994; Wigfield, Eccles, Mac Iver, Reuman, & Midgley, 1991). Positive self-beliefs (e.g., perceived efficacy) thus have the potential to be of particular value in promoting academic achievement during these periods (Simmons & Blyth, 1987).

## METHOD

### Study Inclusion Criteria

To be included in this meta-analysis, a study had to meet several criteria. First, due to the conceptual and operational overlap between different measures of the self, studies were included that measured any self-belief (e.g., self-concept, self-esteem, self-efficacy, self-perception, self-competence). Second, studies were included only if they were longitudinal—that is, measured self-beliefs and achievement at one time (T1) and achievement again at a later time (T2). Although an a priori minimum delay was not established, the shortest delay between T1 and T2 was 6 weeks.[3] Third, studies had to either (a) report the relation between self-beliefs and later achievement controlling for prior achievement and report the result in the form of a standardized regression or path coefficient (i.e., beta, as described following) or (b) report sufficient data to allow for this type of relation to be computed. With regard to the latter possibility, three zero-order correlations were required: T1 self-beliefs with T1 achievement, T1 self-beliefs with T2 achievement, and T1 achievement with T2 achievement (Pedhazur, 1997). If these correlations were not reported by study authors and the study design met all other criteria, attempts were made to contact study authors directly for this information. Fourth, achievement had to be measured directly (e.g., grades, standardized test scores, attainment). Thus, studies that included measures related only indirectly to achievement (e.g., attitudes toward school, time spent on homework) were excluded. Finally, studies had to present results in English.

### Literature Search Procedures

Several search strategies were used to locate studies that met inclusion criteria. First, the following computerized reference databases were searched: PsychInfo, Educational Resources Information Clearinghouse, Medline, and Dissertation Abstracts International. These databases were searched for any records that contained at least one of numerous self-related terms (e.g., *self-concept*, *self-esteem*, *self-efficacy*, *self-description*), at least one of several terms related to the desired research design (e.g., *longitudinal*, *prospective*), and at least one achievement term (e.g., *grade*, *test*, *graduation*). Additional search strategies included reviewing the reference sections of retrieved studies and previous review articles (Byrne, 1996a; Hansford & Hattie, 1982; Harter, 1983; Ma & Kishor, 1997; Marsh, 1993; West et al., 1980; Wylie, 1979; B. J. Zimmerman, 1995) and contacting several

[3]We chose not to set an a priori minimum delay for several reasons: (a) there is no consensus about what that minimum should be; (b) we wanted to retain as much data that met other inclusion criteria as possible; and (c) we believed that measurement delay was a potentially important moderator of effect size, and it is desirable to have as much variation as possible on that variable for conducting the moderator test.

prominent researchers requesting access to any relevant data not publicly available. The search strategies yielded over 3,100 unique references to studies. Abstracts of the studies were read by the first author and judged for potential relevance. At this point, many studies turned out not to be relevant. For example, several abstracts had the term *longitudinal* in them simply because the author recommended that a longitudinal study be conducted. However, over 200 studies were selected and retrieved to determine if they met all inclusion criteria.

## Effect Size Metric

*Beta*—that is, the standardized regression or path coefficient—was used as the primary metric of effect size. Standardized regression coefficients express the amount of expected change in a standardized unit of the criterion variable associated with one standardized unit of change in a predictor variable, holding constant other variables in the regression equation. For example, if a study yielded $\beta = .10$ for the relation between T1 self-beliefs and T2 achievement, this finding would be interpreted to mean that, for every unit change in the measure of self-beliefs, there was an average increase of .10 standard deviation units in the predicted level of the (standardized) achievement measure at T2, holding constant achievement at T1 and any other control variables. As noted previously, this is a conservative approach to estimating effect size because all of the variance in the criterion variable that is shared by the control and predictor variables is assumed to "belong" to the control variable(s).

In general, there were two ways in which effect size estimates were derived in the studies included in this meta-analysis. Most commonly, investigators reported a correlation matrix, and beta was calculated using the commonly available formula (e.g., Pedhazur, 1997). In other instances, investigators reported a relevant beta directly in their results (e.g., path diagram).

*Variance analysis.* Ideally (from a statistical standpoint), a substantial body of studies would exist that all examine the relation between the same measure of self-beliefs and the same measure of achievement. In this ideal case, the unstandardized regression coefficient could be used as the effect size. However, for this meta-analysis, there was relatively little overlap between studies in terms of measures of self-beliefs or achievement. Therefore, the standardized regression coefficient that was used in its place presents a potential problem because it reflects not only (a) the relation between the variables of interest, but also (b) the underlying variances and covariances associated with these measures within the particular sample. As a result, differences in sampling procedures, settings, and populations may cause estimates of beta to be unstable across studies even when the unstandardized regression coefficients are not (Loehlin, 1998; Pedhazur, 1997). Simulation studies suggest that this instability typically has only a trivial effect on the overall estimate of effect size and its associated confidence interval (Kanetkar, Evans, Everell, Irvine, & Millman, 1995).

Nonetheless, to investigate the extent of this possible source of bias for this analysis, information was collected that allowed for comparison of the relative variability in both self and achievement measures across samples. Specifically, when available, the raw means and standard deviations of both predictor and criterion variables were collected, and an index of relative variability—the coefficient of variation (CV)—was calculated for self-beliefs and achievement separately. In samples, the *CV* is defined as the sample standard deviation divided by the sample mean (Snedecor & Cochran, 1989). The CV for both the self and the achievement measures then was examined as a potential moderator of effect sizes (Kling, Hyde, Showers, & Buswell, 1999).

## Coding Frame

For many characteristics of reports and studies, information could be coded directly from the research report with little need for inferences on the part of the coder. Information such as sample size and length of delay between measurements were of this sort. In cases in which some inference was necessary, pre-established definitions were used to code characteristics. In addition, when information was ambiguous or missing and the research report was published in 1990 or later, attempts were made to contact study author(s) via e-mail to obtain the information.[4]

When available, the study characteristics that were coded include (a) *report characteristics* (e.g., author, publication year); (b) *research design*, including convenience versus random selection from a known population and length of delay between initial and subsequent waves of data collection; (c) *participant information*, including the number, average age, percentage of female participants, representation of differing ethnic and racial groups, SES, country, any special population status (e.g., learning disabled, gifted), grade level(s), public versus private school, and whether the sample experienced a normative school transition during the study; (d) characteristics of the self measure, including the specific construct measured (e.g., self-concept, self-esteem, or self-efficacy), the level of measurement specificity (global, academic, subject specific, task specific), subject area when applicable (such as reading or mathematics), internal consistency and/or test–retest reliability of the measure, and whether the measure was assessed as a latent construct; (e) *characteristics of the achievement* measure, including the type of index (standardized test scores, grades, or educational attainment such as high school graduation), subject area, the T1–T2 stability of the achievement measure, and whether the

[4]A copy of the study coding guide is available from the first author on request.

achievement index was treated as a latent variable; and (f) information pertaining to the calculation of the effect size, including the number and type of variables controlled in the calculation of the effect size.

## Meta-Analytic Procedures

Both unweighted and weighted procedures were used to calculate average effect sizes across independent samples (Cooper, 1998). The weighting procedure uses the inverse of the sampling variance of the effect sizes as a weighting factor and thus gives more weight to samples of larger size. It is generally the preferred procedure (Hedges & Olkin, 1985). When computing overall estimates of average effect size, both weighted and unweighted procedures were used. When conducting moderator analyses, only the weighted procedure was used. Ninety-five percent confidence intervals were calculated for the weighted average effect sizes. If the 95% confidence interval did not contain a value of zero, the hypothesis that the association in the population was zero was rejected.

*Unit of analysis.* The independent sample was the primary unit of analysis. Because effect size information was reported for the overall sample in most reports, each report or study generally contributed one independent sample to the analysis. If a study only reported findings separately for different, nonoverlapping subgroups, however, such as boys and girls, it contributed more than one sample to the analysis.

Within this general framework, a shifting unit of analysis approach was used for determining what constituted an independent estimate of effect (Cooper, 1998). For example, a given study might examine the relation between self-beliefs and achievement, with achievement operationalized two different ways—by teacher-assigned grades and by scores on standardized tests. When calculating the overall effect size for the sample, effect sizes for these two different measures would be averaged to arrive at a single effect size estimate. However, when testing whether the type of achievement measure might have moderated effect size (e.g., grades vs. standardized tests), this study would contribute one effect size to each level of the moderating variable.

## Moderator Analysis

Because effect sizes are sample statistics, they will vary somewhat even if all estimate the same underlying population value. Homogeneity analysis (Hedges & Olkin, 1985) is used in meta-analysis to test whether sampling error alone likely accounts for this variation or whether features of studies, such as sample size, statistical design, or outcome measures, also have a role in creating variance in results. Homogeneity analysis compares the amount of variance in an observed set of effect sizes with the amount of variance expected by sampling error alone. The homogeneity statistic is called $Q$ and follows a chi-square distribution (Lipsey & Wilson, 2001). A significant result from a homogeneity analysis suggests that sampling variation alone cannot adequately explain the variability in the effect size estimates. Individual moderator variables then may be tested to investigate possible systematic sources of variability in effect sizes. In this study, tests of homogeneity indicated significant variability in effect size estimates beyond that associated with sampling error (see Results). Accordingly, individual variables were tested as possible moderators of effect size.

Whenever feasible, the significance of a potential moderator was tested with the moderator treated as a continuous variable in the homogeneity analysis. This approach was designed to maximize sensitivity to detect relevant effects. In several instances, however, it was necessary to treat moderators as categorical variables in analyses either because of their inherently categorical nature (e.g., type of achievement measure) or because the degree of variation observed across potential values of the variable was not sufficient to justify treatment as a continuous variable. To facilitate interpretation of results in instances in which moderators were tested as continuous variables, average effect sizes are reported for two or more discrete ranges of values of the variable involved.

## Fixed Versus Random Effects

A final issue involves the decision about whether a fixed effects or random effects model of error should be used to describe the variance in study results (Lipsey & Wilson, 2001). In a fixed effects analysis, the variance of each effect size is assumed to reflect only sampling error. When a random effects model is used, a study-level variance component is assumed to be an additional source of random influence. As noted by Hedges and Vevea (1998), fixed effects models of error are most appropriate when the goal of the research is "to make inferences only about the effect size parameters in the set of studies that are observed (or a set of studies identical to the observed studies except for uncertainty associated with the sampling of subjects)" (p. 3).

When conducting tests for moderators, fixed effects models may substantially underestimate and random effects models may substantially overestimate error variance when their assumptions are violated (Overton, 1998). The approach used in this article is to conduct analyses using both fixed and random assumptions about error. Random effects were tested using the noninterative method of moments technique, the formula for which is presented in Lipsey and Wilson (2001, p. 134).

## Search Outcomes

The literature search procedure described previously uncovered 56 relevant research reports. Of these, 35 were published in journals, 15 were dissertations, one was a master's thesis, three were conference presentations, one was reported in a book devoted to one study, and one was in a book chapter.

One report (Marsh, Byrne, & Yeung, 1999) was a reanalysis of the same data set used in an earlier report (Byrne, 1986). Only the more recent findings included in the latter report were used in this analysis. The resulting total of 55 reports included evaluations of 60 independent samples and contained 282 separate effect sizes. Results obtained for two independent samples (from large national studies conducted in the United States) were reported in multiple articles, whereas 10 articles reported data on multiple independent samples. The studies were published or appeared between 1978 and 2001, whereas the base year researchers began collecting data ranged from 1951 to 1996. The studies included in this synthesis are summarized in Table 1.

## RESULTS

### Measures of Central Tendency

#### *Preliminary Analyses*

Before conducting analyses of effect sizes, both the raw effect sizes and the sample sizes associated with them were inspected for the presence of outliers (Cooper, 1998). Applying Tukey's (1977) definition of *extreme values* as those that are more than three interquartile ranges from either the 25th or 75th percentile revealed no outliers among effect sizes. However, there were six samples large enough to qualify as outliers. These were recoded to $n = 1641$, a value equal to three interquartile ranges above the 75th percentile of sample sizes (Lipsey & Wilson, 2001).

The 60 independent samples that provided estimates of path or regression coefficients representing effects of self-beliefs on achievement (henceforth, SB → ACH) were based on data from over 50,000 students.

#### *Measures of Central Tendency*

*Effects of self-beliefs on achievement.* As shown in the stem-and-leaf display in Table 2, 54 of the 60 SB → ACH effect sizes were positive. The average unweighted effect size for this relation was $\beta = .09$. When effect sizes were weighted by the inverse of their variance, the average effect size was $\beta = .08$. The 95% confidence interval for the weighted fixed effect size estimate was ±.01, meaning that it ranged from a lower value of .07 to an upper value of .09. When tested using random effects assumptions, the point-estimate of the effect size remained the same ($\beta = .08$), with an increase in the 95% confidence interval to ±.02. The confidence interval for the SB → ACH relation thus did not include zero under either fixed or random effects assumptions. Accordingly, the null hypothesis that there is no relation between measures of self-beliefs and later achievement, controlling for prior achievement, can be rejected.

### Moderating Variables

#### *Homogeneity Analyses*

The test for homogeneity of effect sizes estimating the SB → ACH relation was statistically significant, $Q(59, k = 60) = 107.27$, $p < .001$. Thus, the homogeneity test indicated that sampling error alone was not likely the sole explanation for observed variability in these effect sizes. Tests for individual variables moderating SB → ACH relations began with a consideration of methodological characteristics potentially associated with systematic variation in effect size estimates, then proceeded to characteristics based on more conceptual considerations.

#### *Methodological Moderators of Effect Size*

A total of 10 potential methodological moderators were examined: year the study was published/reported, base year of data collection, sample size of the study, stability of the achievement measure (i.e., T1–T2 stability coefficient), reliability of the self measure, whether the analysis was conducted using manifest or latent variables, number of variables controlled statistically in the effect size estimate, whether study participants were a convenience sample or were selected randomly from a known population, CV for the self measure, and CV for the achievement measure. The year of study report and the base year of data collection also were examined because they were viewed as potentially important proxy indicators of methodological changes in study characteristics across time. Results are presented in Table 3.

*Year of publication.* Year of publication was not related to magnitude of effect size in either the fixed or random effects analysis.

*Base year of data collection.* Base year of data collection was not related to effect size in either the fixed or the random effects analysis.

*Sample size.* This analysis was carried out both with and without adjustment for sample-size outliers. Tested either way, sample size was not associated reliably with effect size.

*Participant recruitment.* Three independent samples used random selection from a known population of students. These samples were from three large U.S. studies: the Youth in Transition study (Bachman & O'Malley, 1986; Bynner, O'Malley, & Bachman, 1981; Felson, 1984; Heinen, 1978; Marsh, 1987, 1990), the High School & Beyond study (Marsh, 1991; Pottebaum, Keith, & Ehly, 1986; Van Melis-Wright, 1988), and the National Educational Longitudinal Study (Marsh & Yeung, 1998). The average effect size from these three independent samples was compared to the effect sizes obtained from convenience samples. There was no reliable difference in effect sizes for samples that were se-

TABLE 1
Summary of Studies Included in the Meta-Analysis

| *First Author (Year)* | *Sample Size* | *Average Age of Sample* | *% Female* | *% White* | *Self-Belief* | *Achievement Variable* | *Measurement Delay (in Months)* | *Average Effect Size* |
|---|---|---|---|---|---|---|---|---|
| Anderman (1999) | 312 | 12.5 | 44 | 82 | Academic possible self | Grades | 12 | +.13 |
| B. J. Zimmerman (1992) | 101 | 15.0 | 51 | 24 | Self-efficacy for self-regulated learning and achievement | Grades | 6 | +.08 |
| Bachman (1986), Bynner et al. (1981), Felson (1984), Henein (1978), Marsh (1987), Marsh (1990) | 1,497 | 15.5 | 0 | — | Academic self-concept, global self-esteem | Grades | 8, 12, 24, 48 | +.10 |
| Boehm-Morelli (1999) | 106 | 8.5 | 45 | — | Reading self-concept | Judge-scored reading test | 2 | +.01 |
| Bradley (2000) | 503 | — | — | — | Self-esteem for learning current course content, academic self-esteem, global self-esteem | Grades | 5 | +.10 |
| Brudos (1995) | 206 | 9.5 | 57 | — | Global self-concept | Standardized test | 36 | +.18 |
| Chan (1999) | 33 | 25.0 | 93 | 91 | Academic self-efficacy | Other | 9 | +.08 |
| Chapman (1981) | 166 | 11.0 | 49 | — | Academic self-concept | Grades | 5, 8 | +.13 |
| Chapman (1981) | 208 | 9.0 | 49 | — | Academic self-concept | Standardized test | 5 | –.01 |
| Chapman (1988) | 77 | 11.3 | 39 | — | Academic self-concept | Standardized test | 9, 12, 13, 21, 22 | +.29 |
| Chapman (1988) | 70 | 11.3 | 41 | — | Academic self-concept | Standardized test | 9, 12, 13, 21, 22 | +.34 |
| Chapman (1997) | 117 | 5.1 | — | — | Reading self-concept | Grades | 12, 16, 28 | +.09 |
| Chemers (2000) | 256 | 19.0 | 79 | 56 | Academic self-efficacy | Self- and teacher reports of achievement | 5 | +.36 |
| Coon-Carty (1998) | 73 | 9.5 | 30 | — | Perception of ability | Standardized test | 7 | +.23 |
| Cross (2001) | 123 | 18.5 | 53 | — | Academic self-efficacy, global self-esteem | Persistence in degree program | 20 | +.11 |
| DuBois (1999) | 332 | 11.5 | 53 | 88 | Academic self-esteem, global self-esteem, other self-esteem | Standardized test, grades | 24 | +.08 |
| DuBois (1999), DuBois (2001) | 144 | 13.4 | 52 | 84 | Academic self-description, academic self-esteem | Grades, teacher-rated learning problems | 12 | +.05 |
| Entwisle (1987) | 155 | 6.5 | 100 | 0 | Academic self-image | Standardized test | 18 | +.11 |
| Entwisle (1987) | 130 | 6.5 | 0 | 100 | Academic self-image | Standardized test | 18 | +.08 |
| Entwisle (1987) | 162 | 6.5 | 0 | 0 | Academic self-image | Standardized test | 18 | +.04 |
| Entwisle (1987) | 129 | 6.5 | 100 | 100 | Academic self-image | Standardized test | 18 | +.04 |
| Geroski (1996) | 141 | 10.5 | 47 | 83 | Academic self-perception | Grades | 2 | +.19 |
| Goldberg (1998) | 788 | 7.9 | 53 | 71 | Academic perceived competence | Grades | 12 | +.02 |
| Guay (1999) | 396 | 9.0 | 52 | — | Academic perceived competence | Other | 12 | +.19 |
| Helmke (1995) | 696 | 7.5 | 49 | — | Math self-concept | Standardized test | 12 | +.13 |
| Hemsley (1991) | 217 | 13.5 | 48 | 46 | Math self-concept, verbal self-concept | Standardized test, grades | 24 | +.14 |
| Hemsley (1991) | 98 | 13.5 | 39 | 46 | Math self-concept, verbal self-concept | Grades | 24 | +.01 |
| Hemsley (1991) | 69 | 13.5 | 36 | 63 | Math self-concept, verbal self-concept | Grades | 24 | +.05 |
| Kong (2000) | 5,985 | 13.5 | — | — | Academic self-concept | Standardized test | 12 | +.05 |
| Kurtz-Costes (1994) | 45 | 8.5 | — | — | Reading self-concept, language self-concept | Grades | 24, 48, 72 | +.13 |
| M. A. Zimmerman (1997) | 1,057 | 11.5 | 50 | 83 | Global self-esteem | Grades | 12, 15, 24, 27, 39 | +.08 |
| Marsh (1988a), Newman (1984) | 107 | 7.4 | 48 | 99 | Math self-concept | Standardized test | 6 | +.10 |

(*continued*)

TABLE 1 (Continued)

| First Author (Year) | Sample Size | Average Age of Sample | % Female | % White | Self-Belief | Achievement Variable | Measurement Delay (in Months) | Average Effect Size |
|---|---|---|---|---|---|---|---|---|
| Marsh (1991), Pottebaum (1986), Van Melis-Wright (1988) | 6,777 | 15.8 | — | 78 | Academic self-concept, global self-concept, global self-esteem | Standardized test, grades, graduation | 24, 36, 48, 60 | +.10 |
| Marsh (1997a) | 402 | 12.7 | 0 | — | Academic self-concept | Other | 2, 4, 24 | +.18 |
| Marsh (1998) | 6,002 | 13.5 | — | — | Math self-concept, English self-concept | Standardized test, grades | 4, 24 | +.01 |
| Marsh (1999) | 927 | 15.9 | — | — | Global self-esteem | Standardized test | 2, 4 | +.01 |
| Marsh (2000) | 7,990 | 13.5 | — | — | Academic self-concept | Standardized test | 12 | +.04 |
| Maruyama (1981) | 145 | 12.0 | — | — | Academic self-esteem, global self-esteem, other self-esteem | Standardized test | 6, 24, 36 | +.02 |
| Maruyama (1981) | 159 | 12.0 | — | — | Global self-esteem | Standardized test | 48 | +.06 |
| Mijus (1997) | 889 | 9.5 | — | — | Academic self-perception | Grades | 12 | +.11 |
| Mone (1995) | 214 | — | 47 | — | Other self-efficacy, global self-esteem | Teacher-developed test | 2 | +.02 |
| Mundy (2000) | 37 | — | — | 100 | Global self-concept | Standardized test | 12 | +.10 |
| Sharrow (1993) | 59 | 13.5 | 47 | — | Math self-concept, reading self-concept | Standardized test | 12 | +.02 |
| Shavelson (1982) | 99 | 14.0 | 47 | 96 | English self-concept, academic self-concept, global self-concept | Grades | 4 | +.11 |
| Shoemaker (1980) | 244 | 10.5 | 53 | 83 | Academic self-concept | Standardized test | 36 | −.01 |
| Simmons (1987) | 276 | 11.5 | 0 | 100 | Global self-esteem | Grades | 17 | +.16 |
| Skaalvik (1990) | 363 | 11.5 | — | — | Academic self-concept, global self-esteem | Teacher rating of achievement | 12 | .00 |
| Skaalvik (1990) | 363 | 8.5 | — | — | Academic self-concept, global self-esteem | Teacher rating of achievement | 12 | +.04 |
| Skaalvik (1999) | 493 | 8.5 | — | — | Math self-concept, verbal self-concept | Researcher achievement test, teacher ratings | 12 | +.04 |
| Skaalvik (1999) | 284 | 11.5 | — | — | Math self-concept, verbal self-concept | Researcher achievement test, teacher ratings | 12 | +.07 |
| Skaalvik (1999) | 225 | 13.5 | — | — | Math self-concept, verbal self-concept | Researcher achievement test, teacher ratings | 12 | +.11 |
| Thordardottir (2000) | 106 | 9.5 | 45 | — | Academic self-efficacy | Standardized test | 7 | +.17 |
| Thordardottir (2000) | 107 | 12.5 | 52 | — | Academic self-efficacy | Standardized test | 7 | +.15 |
| Thordardottir (2000) | 121 | 15.5 | 53 | — | Academic self-efficacy | Standardized test | 7 | +.03 |
| Van Damme (2000) | 6,410 | 6.0 | — | — | Academic self-esteem | Standardized test | 12, 24 | +.10 |
| Widlak (1983) | 83 | 7.5 | 54 | — | Global self-concept | Standardized test | 6 | +.07 |
| Williams (1998) | 141 | 6.5 | 60 | 0 | School self-concept, other self-efficacy | Standardized test | 24 | +.17 |
| Yeung (1999) | 485 | 13.5 | — | — | Math self-concept, verbal self-concept, academic self-concept, global self-concept | Standardized test | 3, 6, 9, 223 | .00 |
| Yin (1999) | 542 | 20.0 | — | — | Academic self-concept | Grades | 6 | +.01 |
| Yoon (1996) | 462 | 11.5 | 100 | — | Self-concept of ability | Other | 36 | −.12 |
| Yoon (1996) | 362 | 11.5 | 0 | — | Self-concept of ability | Other | 36 | −.01 |

lected randomly compared to convenience samples in either the fixed or random effects analysis.

*Manifest versus latent variable analysis.* Use of manifest versus latent variable analyses was associated significantly with effect size, $Q(1, k = 67) = 4.17$, $p < .05$, with larger effect sizes for manifest variable analyses ($\beta = .09$) than latent variable analyses ($\beta = .06$). This result was not significant, however, in the random effects analysis.

*Number of control variables.* In most instances, the effect sizes did not include any additional control variables

TABLE 2
Stem-and-Leaf Display of Effect Sizes

| *Stem* | *Leaf* |
|---|---|
| .3 | 46 |
| .2 | 39 |
| .1 | 00000011113345677899 |
| .0 | 01111122223444445567788899 |
| −.0 | 01115 |
| −.1 | 2 |

*Note.* Multiply each leaf by .01 and add that quantity to the stem to recreate effect sizes.

TABLE 3
Methodological Moderators of Effect Sizes

| | | *Fixed* | | | *Random* | | |
|---|---|---|---|---|---|---|---|
| *Moderator* | *k* | *Q* | *β* | *95% CI* | *Q* | *β* | *95% CI* |
| Year of publication[a] | | 0.38 | | | 0.05 | | |
| < 1996 | 27 | | .09 | .02 | | .09 | .03 |
| ≥ 1996 | 33 | | .07 | .02 | | .08 | .03 |
| Base year of data collection[a,b] | | 0.31 | | | 0.31 | | |
| < 1984 | 16 | | .10 | .03 | | .10 | .03 |
| ≥ 1984 | 17 | | .07 | .02 | | .07 | .02 |
| Sample sizea | | 2.08 | | | 0.61 | | |
| < 120 | 17 | | .11 | .05 | | .11 | .06 |
| 120–487 | 29 | | .08 | .02 | | .08 | .03 |
| > 487 | 14 | | .07 | .01 | | .07 | .03 |
| Participant recruitment | | 0.15 | | | 0.11 | | |
| Convenience sample | 57 | | .08 | .01 | | .08 | .02 |
| Random selection | 3 | | .07 | .03 | | .07 | .06 |
| Type of analysis | | 4.17* | | | 2.49 | | |
| Manifest variables | 43 | | .09 | .02 | | .10 | .03 |
| Latent variables | 24 | | .06 | .02 | | .06 | .03 |
| Number of control variables | | 2.02 | | | 1.91 | | |
| 1 | 44 | | .09 | .02 | | .09 | .03 |
| 2–4 | 15 | | .09 | .02 | | .08 | .04 |
| > 4 | 12 | | .07 | .02 | | .06 | .05 |
| Reliability of self measure scores[a] | | 0.03 | | | 0.04 | | |
| < .81 | 14 | | .09 | .03 | | .10 | .04 |
| ≥ .81 | 21 | | .07 | .03 | | .08 | .04 |
| Achievement measure stability[a] | | 11.52*** | | | 7.03** | | |
| < .62 | 28 | | .10 | .02 | | .11 | .03 |
| ≥ .62 | 28 | | .07 | .02 | | .06 | .03 |
| Self measure CV[a] | | 6.04* | | | 0.68 | | |
| < .22 | 18 | | .08 | .02 | | .10 | .04 |
| ≥ .22 | 18 | | .11 | .03 | | .10 | .04 |
| Achievement measure CV[a] | | 0.03 | | | 0.02 | | |
| < .22 | 18 | | .10 | .02 | | .11 | .04 |
| ≥ .22 | 15 | | .07 | .03 | | .10 | .04 |

*Note.* CI = 95% confidence interval; CV = coefficient of variation.
[a]This variable was utilized as a continuous variable in moderator analyses. [b]The random effects variance for this variable was zero.
*$p < .05$. **$p < .01$. ***$p < .001$.

other than T1 level of achievement. There were, however, some studies that included relatively large numbers of control variables, thus resulting in a nonnormal, positively skewed distribution for this moderator when examined as a continuous variable. Based on these considerations, the number of control variables in the analysis was analyzed as a categorical variable, distinguishing between effect sizes based on only one control variable, two to four control variables, and more than four control variables. As shown in Table 3, these varying levels of statistical control were not related significantly to effect size. Of note in the results of these analyses is that the confidence intervals around average effect sizes as-

sociated with differing numbers of control variables did not include zero and thus consistently indicated support for a positive relation between self-beliefs on achievement.

*Internal consistency of self-beliefs scores.* Most studies reported only internal consistency (i.e., coefficient alpha) estimates of reliability of scores for measures of self-beliefs. Thus, other forms of reliability (e.g., test–retest) were not considered when examining reliability as a moderating variable. Also, because latent variable analyses have an effective reliability for all measures of 1.0, only effect sizes based on manifest variable analyses were examined in this moderator analysis. On average, the internal consistency of measures of self-beliefs was good ($M = .82$, $SD = .12$). As shown in Table 3, internal consistency was not related to effect size in either the fixed or the random effects analyses.

*Stability of criterion.* The average stability coefficient of the achievement variable was moderate ($M = .62$, $SD = .20$). Stability of the achievement measure was related to effect size in both the fixed effects analysis, $Q(1, k = 56) = 11.52, p < .001$, and the random effects analysis, $Q(1, k = 56) = 7.03, p < .01$. Measures with relatively high stability (i.e., achievement measures with a stability coefficient of .62 or greater) yielded smaller effect sizes ($\beta = .07$) than did measures with moderate or low stability ($\beta = .10$).

*Relative variability of measures.* The relative variability of the self measure, assessed using the CV, was related to effect size for the fixed effects model, $Q(1, k = 36) = 6.04$, $p < .05$, with greater degrees of variability associated with stronger effect sizes ($\beta = .11$) than relatively lesser degrees ($\beta = .08$). However, this association was not significant in the random effects analysis. The relative variability of the achievement measure was not related significantly to SB → ACH effect size in either the fixed or random effects analysis.

*Summary of methodological moderators.* One methodological variable was a significant moderator in both the fixed and random effects analyses (i.e., stability of the achievement measure). In addition, the use of latent variables in the analysis and the CV for the self measure were related significantly to effect size under fixed effect assumptions only.

### Theoretical Moderators of Effect Size

When investigating possible substantive moderators of effect size in a meta-analysis, it is recommended that the influence of relevant methodological factors be controlled for statistically (Durlak & Lipsey, 1991). In this context, tests for theoretically based moderators included statistical control for all of the potential methodological moderators of effect size examined in the preceding analyses (with the exception of base year of data collection, for which there was too much missing information). Methods variables were included whether or not the variable was a significant moderator of effect sizes. To implement this statistical control, all effect size estimates were residualized on the full set of methodological characteristics shown in Table 3 (again, with the exception of base year of data collection). The resulting adjusted effect sizes then were used in all theoretical moderator analyses.

It will be recalled that for some moderator variables a single independent sample could contribute more than one effect size if it contained data on more than one moderator category. For example, with respect to the type of achievement measure used, some studies included both grades and standardized test scores as outcomes and thus provided separate estimates of effect size for each type of criterion. To take advantage of such information, when feasible an additional analysis was conducted in which effect sizes were compared across levels of the moderating variable on a within-study basis. Illustratively, with respect to the preceding example, the difference between effect sizes based on grades and standardized test scores from the same study would be calculated. Next, the overall effect size and confidence interval for this difference would be calculated using both fixed and random effects assumptions. If the confidence interval for the effect size difference did not include zero, it was possible to reject the null hypothesis of no difference between levels of the moderating variable.

*Type of self-belief and level of self measurement.* The type of self-belief measured (i.e., self-esteem, self-concept, self-efficacy) was related significantly to the level (i.e., global, academic, specific) of measurement, $\chi^2(4, N = 54) = 26.54, p < .001$. Self-esteem measures were more likely to be assessed at the global level of measurement, whereas measures of self-concept and self-efficacy were more likely to be assessed at the academic or subject-specific level. As a result, our approach to the analysis of this variable is somewhat different from the general approaches presented in this article. Specifically, we examined the relation between effect size simultaneously, distinguishing measures on both (a) type of self-belief and (b) level of self measure independent of one another (see Wang & Bushman, 1999). In this analysis, effect sizes for measures of self-concept, self-esteem, and self-efficacy beliefs controlling for level of measurement did not differ from one another under either fixed or random effects assumptions (see Table 4). Effect sizes associated with level of measurement controlling for type of self-belief measured did, however, exhibit significant differences. Under fixed effects assumptions, effect sizes were larger ($\beta = .13$) for academic measures of the self, $Q(2, k = 63) = 24.40, p < .001$, than for either subject-specific ($\beta = .06$) or global ($\beta = .07$) measures of the self. A similar pattern was found for the random effects analysis, $Q(2, k = 63) = 5.44, p < .07$.

As a secondary analysis of the relation of the level of measurement to effect size, we examined 12 independent samples that allowed for a calculation of separate effect sizes for

TABLE 4
Theoretical Moderators of Effect Sizes

| | | *Fixed Effects* | | | *Random Effects* | | |
|---|---|---|---|---|---|---|---|
| *Moderator* | *k* | *Q* | *β* | *95% CI* | *Q* | *β* | *95% CI* |
| Type of self-belief measured[a] | | 1.56 | | | 0.52 | | |
| Self-concept | 35 | | .08 | .02 | | .07 | .03 |
| Self-esteem | 19 | | .07 | .02 | | .06 | .04 |
| Self-efficacy | 9 | | .11 | .06 | | .10 | .08 |
| Level of self measurement[b] | | 24.40**** | | | 5.44* | | |
| Subject specific | 19 | | .06 | .03 | | .05 | .05 |
| Academic | 25 | | .13 | .02 | | .12 | .03 |
| Global | 19 | | .07 | .03 | | .06 | .04 |
| Type of achievement measure | | 0.44 | | | 0.05 | | |
| Standardized test | 33 | | .10 | .02 | | .08 | .03 |
| Grades | 30 | | .08 | .02 | | .08 | .03 |
| Attainment | 6 | | .10 | .03 | | .11 | .06 |
| Self-achievement match | | 19.36**** | | | 4.23** | | |
| Not matched | 40 | | .06 | .02 | | .05 | .03 |
| Matched | 41 | | .11 | .01 | | .10 | .03 |
| Delay between waves of data collection | | 9.30** | | | 1.60 | | |
| < 6 months | 19 | | .05 | .03 | | .08 | .04 |
| 6–12 months | 35 | | .08 | .02 | | .08 | .03 |
| 12–18 months | 7 | | .09 | .04 | | .11 | .07 |
| > 18 months | 24 | | .09 | .02 | | .08 | .04 |
| Age | | 3.14 | | | 0.93 | | |
| < 11 | 23 | | .11 | .02 | | .09 | .03 |
| 11–15 | 26 | | .09 | .02 | | .08 | .03 |
| 15—18 | 4 | | .09 | .03 | | .08 | .06 |
| > 18 | 4 | | .09 | .06 | | .10 | .09 |
| Gender | | 2.50 | | | 0.21 | | |
| < 45% female | 11 | | .11 | .04 | | .11 | .05 |
| 45–55% female | 20 | | .08 | .03 | | .08 | .04 |
| Ethnicity | | 2.50 | | | 0.21 | | |
| < 40% White | 3 | | .08 | .09 | | .08 | .11 |
| 40–90% White | 12 | | .11 | .03 | | .10 | .04 |
| > 90% White | 7 | | .07 | .05 | | .08 | .07 |
| Country sampled | | 6.59** | | | 1.45 | | |
| United States | 36 | | .08 | .02 | | .08 | .03 |
| Non-United States | 24 | | .11 | .02 | | .10 | .03 |
| School transition | | 11.56*** | | | 3.84** | | |
| No | 37 | | .11 | .02 | | .10 | .03 |
| Yes | 19 | | .06 | .02 | | .06 | .04 |

*Note.* CI = 95% confidence interval.

[a]The Q statistic refers to a test of the type of self-belief as a moderator holding level of measurement constant. [b]The Q statistic refers to a test of the level of measurement as a moderator holding type of self-belief measured constant.

*$p < .10$. **$p < .05$. ***$p < .01$. ****$p < .001$.

measures of academic and global self-beliefs. Given the lack of evidence for differences in effect size for type of self-belief, for purposes of this analysis we ignored the type of self-belief and then calculated a difference between effect sizes for global versus academic measures as described previously. All of the 12 effect sizes were based on measures of self-concept and self-esteem, as no studies yielded effect sizes for both academic and global self-efficacy beliefs. In the fixed effects analysis, the average effect size difference was $\beta = .13$ favoring academic measures of self-beliefs, with an associated 95% confidence interval of ±.03. In the random effects analysis, the average effect size difference was $\beta = .14$ with an associated confidence interval of ±.12. Thus, under both fixed and random effects assumptions, we can reject the null hypothesis that effect sizes for academic and global measures of the self do not differ.

*Type of achievement measure.* As shown in Table 4, there was no relation between the type of achievement measure and effect size under either fixed or random effects assumptions.

*Match between self and achievement domains.* Analyses also examined if effect sizes varied according to whether measures of academic self-beliefs and achievement were matched by domain. For example, an effect

size based on a measure of math self-concept and grades in math was considered to be a match. An effect size based on a measure of math self-concept and a measure of grades in another subject area (e.g., language) was considered not to be a match. As a specific example, Kurtz-Costes and Schneider (1994) obtained measures of reading, math, and language self-concept and grades from a group of primary school students. For this study, several effect sizes representing matching and nonmatching domains could be computed. Among these were an effect size for the relation of math self-concept to later math grades (matched) and an effect size for the relation of math self-concept to later reading grades (not matched). Furthermore, effect sizes based on a measure of self-beliefs for the entire academic domain and a corresponding measure of overall academic achievement (e.g., overall grade point average) were considered a match, whereas those based on overall academic self-beliefs and indices for achievement for specific subjects (e.g., math) were considered not to be a match.

Matching self and achievement domains was associated with larger effect sizes than not matching, $Q(1, k = 72) = 19.36$, $p < .001$, with larger effects found for studies that matched ($\beta = .11$) than studies that did not match ($\beta = .06$) these domains. Under random effects assumptions, this analysis also was significant, $Q(1, k = 72) = 4.23$, $p < .05$. In addition, there were 10 independent samples that provided an effect size for both matched and not matched self and achievement measures. In a fixed effects analysis of the matched and nonmatched effects for those samples, the average effect size difference was significantly different from zero in a direction favoring matched effect sizes for both the fixed effects analysis ($\beta = .10$, 95% confidence interval of $\pm.02$) and the random effects analysis ($\beta = .122$, 95% confidence interval of $\pm.120$).

*Delay between waves of data collection.* Under fixed effects assumptions, the delay between waves of data collection was related significantly to effect size, $Q(3, k = 85) = 9.30$, $p < .05$. A post-hoc contrast revealed that effect sizes based on relatively short measurement delays (i.e., $\leq 6$ months) were associated with smaller effect sizes ($\beta = .07$) than were effect sizes associated with longer delays ($\beta = .11$), $Q(1, k = 85) = 2.71$, $p < .10$. However, under random effects assumptions, measurement delay was not associated with effect size.

*Average age of sample.* Participants in the studies included in this meta-analysis averaged 11.7 years of age the start of the study. Age was not associated significantly with effect size in either the fixed effects or random effects analysis.

*Student ability level.* Only 4 studies reported sufficient information to code study ability level (e.g., learning disability status, gifted). Consequently, it was not feasible to investigate whether effect sizes varied according to ability levels of students.

*Gender.* Gender was tested by examining the relation between the average percentage of females in samples and effect sizes. Gender was not related to effect size.

*Student SES.* Unfortunately, only a minority of studies reported SES information ($n = 14$) for their samples (with most samples being from mixed SES populations), and very few ($n = 3$) provided information to derive effect sizes separately for varying levels of SES. Therefore, no analysis of the relation between SES and effect size was possible.

*Student ethnicity.* Ethnicity information was available for less than 50% of the samples. In addition, very few studies ($n = 3$) reported sufficient information to generate separate effect sizes by ethnicity. Because of these limitations, samples were characterized simply by the percentage of White students. On average, samples were composed of about 67% White students. Defined in this manner, ethnicity was not related to effect size (see Table 4).

It also would have been desirable to conduct an analysis of ethnicity as a moderator, controlling for its association with SES level. As noted, however, adequate information concerning SES was not reported to support analyses involving this variable.

*Country sampled.* A majority of studies were conducted in the United States, and relatively few were conducted in any other single country. As a result, it was feasible only to categorize samples as having come from the United States. It should be noted, furthermore, that the preponderance of non-U.S. samples were from Western countries (e.g., Canada, Australia, etc.; $n = 20$), such that there were only 4 from non-Western countries (e.g., Hong Kong). There was a significant difference between effect sizes for U.S. and non-U.S. samples under the fixed effects model, $Q(1, k = 60) = 6.59$, $p < .05$, with larger effect sizes obtained from non-U.S. samples ($\beta = .11$) compared to U.S. samples ($\beta = .08$). However, this result did not hold under random effects assumptions.

*School transition.* There was a significant relation between the presence of a normative school transition and effect size in the fixed effects analysis, $Q(1, k = 56) = 11.56$, $p < .001$, and in the random effects analysis, $Q(1, k = 56) = 3.84$, $p = .05$. Effect sizes were smaller when the sample experienced a school transition ($\beta = .06$ for fixed and random effects) than when it did not ($\beta = .10$ for fixed effects and $\beta = .11$ for random effects).

## DISCUSSION

Overall, available findings are consistent with the view that self-beliefs can influence academic achievement. The magnitude of the overall estimated relation between self-beliefs

and later achievement, controlling for initial achievement, is not large ($\beta = .08$), but does approach Cohen's (1988) threshold of $r = .10$ for a small effect size in the social sciences. In several instances, effect sizes meet or somewhat exceed this threshold, including when there is relatively less stability in levels of achievement over time, and, from a substantive perspective, there is a focus on self-beliefs that (a) pertain specifically to the academic domain and/or (b) are matched to achievement measures. It is also important to note that Cohen's benchmarks were based on "typical" bivariate effect sizes for social and behavioral sciences and are thus rough guidelines in the context of this study. For example, the effect sizes in this study were based on features that are not typical. The studies were prospective, and statistical control was exercised to control for the most plausible rival hypothesis (i.e., that prior achievement causes both later achievement and later self-beliefs). In addition, a relation of self-beliefs to later achievement is evident not only when controlling for initial levels of achievement, but also when controlling for other factors that could be confounded with self-beliefs. Overall, results suggest that, among equally achieving students, having positive self-beliefs confers a small but noteworthy advantage on subsequent achievement measures relative to students who exhibit less favorable self-beliefs.

From a theoretical perspective, these results offer support for theories of learning and human development that view the self as a causal agent (e.g., Bandura, 1997; Carver & Schreier, 1981; Deci & Ryan, 1985). From an applied standpoint, these results suggest it is untenable to simply reject arguments for school reforms and policies that are intended to address self-beliefs. The claim that self-beliefs are either entirely irrelevant to student achievement or likely to be detrimental in their effects (Kohn, 1993; Seligman, 1993; Stevenson, 1992; Stout, 2000) are not consistent with the cumulative evidence.

These considerations notwithstanding, the findings of the meta-analysis are equally clear in suggesting that effects of student beliefs on achievement typically are small in magnitude. Even allowing for methodological limitations, evidence is lacking to support theoretical or applied perspectives in which self-beliefs are characterized as a strong and pervasive influence on student achievement (Beane, 1994; California Task Force to Promote Self-Esteem and Social Responsibility, 1990; Purkey, 2000).

### Moderators of the Overall Relation

Beyond these overall trends, several aspects of the results offer promising directions (a) for developing a more refined understanding of relations between self-beliefs and achievement and (b) for effective educational interventions oriented toward enhancing self-beliefs. First, the findings indicate that self-beliefs pertaining to the academic domain represent a more important influence on achievement ($\beta = .12$) than global or general beliefs and feelings about the self ($\beta = .06$). In particular, effect sizes based on measures of academic self-concept and academic self-esteem were larger than those based on global measures of these two types of self-beliefs. Similar findings have been reported previously (Byrne, 1996a; Hattie, 1992; West et al., 1980), but these involved primarily cross-sectional studies. A corresponding finding within longitudinal studies represents a significant extension of this earlier work. This finding indicates that the relatively strong pattern of linkages between academic self-beliefs and achievement is not simply a reflection of overlap between the content of such measures.

In sum then, there appears to be the potential for students' beliefs and feelings about their academic abilities to shape their levels of learning and school performance over time (Marsh, 1993; Rosenberg et al., 1995). It will be recalled in this regard that most plausible theoretical mediators of the effects of self-beliefs on achievement would appear to involve mechanisms of action that are specific to beliefs about oneself as a learner and a student. The finding that academic measures of self-beliefs performed better than global measures thus lends support to this aspect of these theoretical mediators.

By contrast, findings provide only equivocal evidence of effects of global or generalized self-beliefs on academic achievement. As a result of their relatively smaller magnitude, for example, the presence of such an effect could not be inferred when conducting analyses under the assumption of random effects. Both methodological and theoretical factors could be influential in accounting for this aspect of results. Methodologically, measures of global beliefs and feelings about the self have been plagued by a variety of concerns that could obscure their predictive utility with respect to academic achievement outcomes (Byrne, 1996a). These include problems with attempting to infer overall beliefs about the self from a summation of reported views and feelings in specific domains. Such an approach may not incorporate attention to all domains that are influential in determining the individuals' overall views and feelings about themselves and also may fail to weight those domains that are considered in a manner that reflects their importance to any particular respondent (Harter, 1983; Wylie, 1979). It is surprising and somewhat disconcerting in this regard that even some of the relatively recent studies included in this review utilized measures that are subject to these types of limitations, such as the Piers-Harris Self-Concept Scale (e.g., Mundy, 2000). Even widely used measures that do ask for direct self-reports of global self-beliefs, such as the Rosenberg Self-Esteem Scale, are not without significant potential limitations. It is by no means assured, for example, that respondents will be either willing or even able to provide accurate information concerning how they truly view and feel about themselves (Byrne, 1996b; Harter, 1999). Recent efforts to address this concern include the use of laboratory measures to tap implicit self-beliefs (Greenwald et al., 2002) as well as reliance on other informants (e.g., teachers) to gauge presented feelings of

self-worth (Harter, 1999). These types of alternative approaches to measurement could potentially yield greater evidence of a contribution of overall or general self-beliefs to academic achievement than is available at present. Theoretically, however, constructs such as general self-concept and global self-esteem simply may be too broad and multifaceted to be of significant predictive utility with respect to adaptive outcomes occurring in a relatively specific realm of functioning such as school (Bandura, 1997; Byrne, 1996a; Marsh & Yeung, 1997b; Rosenberg et al., 1995).

The importance of specificity in measurement is further suggested by the evidence of stronger effects when assessments focus on self-beliefs and achievement for the same domain, such as a particular subject area in school. More generally, this trend suggests the value of efforts to formulate and test theoretically based relations among differing components of self-beliefs and achievement. The relatively straightforward matching hypothesis supported by our findings, for example, could be a useful starting point in developing a more fully elaborated model of processes of linkage between self-beliefs and achievement across differing subject areas. The internal frame of reference model proposed by Marsh and colleagues is illustrative of the potential for this type of theory development (e.g., Marsh, 1986; Marsh & Yeung, 2001). According to the model, higher levels of achievement in any given subject area (e.g., math), when controlling for levels of achievement in other domains (e.g., reading), are expected to predict less, rather than more favorable, self-concepts for the other, noncorresponding domains. This is posited to occur because of a tendency for students to use achievement in any given subject as a standard of comparison in forming evaluations of their skill or performance in other areas. To date, these and other types of processes linking relatively circumscribed facets of self-beliefs and achievement have received relatively little empirical attention. This is a particularly salient limitation of the longitudinal investigations that were the focus of this review. Greater consideration of such processes could nevertheless be instrumental in providing a more comprehensive and dynamic framework within which to investigate linkages between self-beliefs and achievement over time.

Measures intended to assess certain types of self-beliefs did not differ significantly with respect to the indicated magnitude of their possible effects on later achievement. In particular, although measures of academic self-concept, academic self-esteem, and academic self-efficacy each have been included in several studies, effect sizes for these differing types of measures have not differed significantly. In combination with findings noted previously, results thus suggest that the level of specificity at which self-beliefs are measured is a more important consideration than the particular type of self-system component that such beliefs most closely resemble among those that have been investigated most widely as influences on achievement. This conclusion must be regarded as tentative, however, for several reasons. These include the conceptual and empirical overlap in currently available measures of differing types of self-beliefs (Byrne, 1996a) and possible monomethod bias in the extant empirical work due to the almost universal reliance on self-reports. There also has been a failure in much of the theoretical and empirical literature to clearly distinguish issues relating to type of self-belief from those relating to level of specificity. This is reflected in a tendency for assessments of particular types of self-beliefs (e.g., self-esteem) to lack sufficient representation at all levels of measurement, thus potentially obscuring evidence of their differential contributions to achievement. Relatedly, only a small number of studies have sought to directly compare the predictive utility of differing types of self-beliefs in relation to academic achievement within the same sample. However, self-efficacy measures did tend to be associated with larger effect sizes than measures of other self-beliefs. Unfortunately, there simply were too few prospective studies that included measures of self-efficacy for the tests of the type of measure (e.g., self-efficacy vs. self-concept vs. self-esteem) to have much statistical power, nor was it possible to fully test different types of self-beliefs within measurement levels. Pending greater attention to the foregoing concerns, it seems most appropriate to regard each type of self-belief that has received significant consideration in longitudinal studies to date (i.e., self-concept, self-esteem, and self-efficacy beliefs) to be comparable to one another in their capacity to influence levels of student achievement.

## LIMITATIONS AND DIRECTIONS FOR FUTURE RESEARCH

Finally, several limitations and directions for future research merit consideration. These are important to take into account with respect to guiding appropriate interpretation of the theoretical and applied implications of findings and as a basis for highlighting promising directions for future investigation. One issue is that our results should not be taken as proof that self-beliefs have a causal relation to later achievement. Rather, by addressing perhaps the most plausible rival hypothesis to the claim that self-beliefs cause achievement (i.e., that prior achievement causes both self-beliefs and later achievement), findings can be viewed as moving a significant step beyond prior reviews of the literature that have been limited primarily to a focus on the strength of concurrent relations between self and achievement measures. The manner in which relations consistent with effects of self-beliefs on achievement remained evident when controlling for student ability and other potential explanatory factors is of further significance. Ultimately, it is through precisely this type of incremental accumulation of empirical evidence that any hypothesis can be expected to receive support and validation (Berk, 1988).

It is worth noting in this regard that experimental studies do not appear to offer a viable short-cut to arriving at definitive evidence of the causal role of self-beliefs in shaping aca-

demic and educational outcomes. This is largely because self-beliefs, by virtue of their status as mental constructs, are not subject to the same types of relatively focused and unambiguous forms of experimental manipulation that are possible with situational or contextual factors (Haney & Durak, 1998; Harter, 1999; Hattie, 1992). Rather, any intervention intended to modify a particular type of self-belief (e.g., self-concept) has the potential to be successful to varying degrees in achieving not only this goal, but also introducing important changes both in other types of self-processes and in factors outside of the self-system (e.g., academic skills). Efforts to distinguish among these possibilities are subject to the same inherent limitations of any efforts that seek to reliably measure self processes (Byrne, 1996a). This does not imply, however, that experimental research should not receive greater attention. Indeed, intentional efforts to manipulate self-beliefs through intervention, particularly if combined with careful attention to measuring and testing specific theoretical mechanisms of influence using techniques such as structural equation modeling, may offer many important advantages. These include introduction of greater variability in levels of self-beliefs, a result that, based on our findings, would be expected to enhance sensitivity to detecting evidence of effects on achievement.

Findings pertaining to moderating influences also should be regarded as tentative and in need of further investigation. It is particularly important to note in this respect that each of the associations found between a given methodological or theoretical factor and observed variation in effect size across studies has the potential to be attributable instead to other areas of difference between the studies involved (Cooper, 1998). In future research, there should be greater emphasis on investigation of effects of variations in factors of interest within the context of individual studies. The few instances in which these types of analyses were conducted (and reported) with enough frequency to permit incorporation into this meta-analysis were of considerable value in terms of allowing for a more methodologically controlled, within-study approach to synthesizing findings. Thus, in addition to enhancing contributions to knowledge made by any given investigation, such comparisons ultimately can be expected to provide a more compelling base of evidence for research syntheses.

A further concern is the generalizability of findings. Results of tests for moderation in several instances became nonsignificant under the assumptions of a random effects model. This suggests that the differences involved may not be fully robust to all possible variations in study characteristics (Lipsey & Wilson, 2001). Illustratively, the failure to find evidence of relatively strong effects of academic self-beliefs on achievement under the assumption of random effects suggests that this trend may be restricted along one or more dimensions such as the specific measures that are used for this purpose. The manner in which other key findings were largely unaffected by analysis under the assumption of a random effects model is encouraging. Yet, these results still do not fully address concerns of generalizability. This is in part because of limitations in the range of studies that served as the basis for the review. Those relating to types of measures utilized were noted previously. Several others that could be important relate to the nature of the research designs and samples on which most findings are based. With respect to study design, for example, it is of note that there was evidence of greater magnitude of effects of self-beliefs on achievement with a relatively long delay between waves of assessment. Yet, almost one third of studies included delays of less than 6 months. Issues of concern with respect to sample characteristics include a lack of adequate representation of participants from non-Western countries, those from varying ethnic and racial backgrounds, and those in the earliest stages of schooling. The preceding considerations underscore a need for greater diversity in design, instrumentation, and sampling in future research.

The statistical procedures used to evaluate possible effects of self-beliefs on achievement in longitudinal research are a further consideration. To date, these have been limited almost exclusively to multiple regression and structural equation modeling. Relatively little attention thus has been given to more recently developed procedures for analyzing longitudinal relations among variables, such as latent growth modeling (LGM; McArdle & Bell, 2000). LGM and related procedures could be used to fit trajectories of change in academic achievement over multiple time points for individual students. This could potentially serve to enhance sensitivity to effects of self-beliefs on achievement patterns. To capitalize on this type of potential, there should be a priority in future research on applying more recently developed advanced modes of statistical analysis.

Overall, there is encouraging evidence of a contribution of self-beliefs to achievement as well as considerable potential for the magnitude of this contribution to be underestimated due to various methodological limitations of extant studies. Available data thus are inconsistent with arguments to abandon all efforts directed toward strengthening the self-beliefs of students within educational interventions and reform efforts. Yet, at the same time, given the relatively small magnitude of estimated effects of self-beliefs on achievement, it clearly also would not be defensible to attempt to use the available results as justification for interventions that are aimed solely at improving students' views of themselves. This would seem to be the case even for efforts that are concentrated specifically on promoting the types of positive beliefs about academic ability and learning potential that seem most likely to be influential in shaping achievement outcomes. There are, however, alternative strategies that still could permit the opportunity to take advantage of the potential for self-beliefs to contribute to desired academic outcomes. Illustratively, efforts that are focused on improving student achievement via other approaches (e.g., school reform) may be more effective when they are designed so that gains in achievement are likely to also strengthen self-beliefs that, in turn, may be of additional benefit to success

in school (DuBois, 2001). Related theory and research point to several conditions that could be of critical importance in this regard. These include offering ample opportunities for student mastery of course material (Bandura, 1997), incorporating provisions for student choice and meaningful involvement in learning activities (Connell & Wellborn, 1991), and utilizing approaches that support students in making adaptive attributions for success and failure experiences in learning situations (Seligman, 1993). Based on these findings, these types of efforts seem likely to prove most beneficial when there is a focus on promoting positive beliefs and feelings about the self that are tied specifically to the academic domain, and these are well-matched to targeted areas of achievement. This orientation may help to ensure that positive self-beliefs held by students are not weighted disproportionately toward areas outside of school or, relatedly, lack a realistic foundation of actual accomplishment in the school setting. In this manner, it may prove possible to capitalize on the benefits that self-beliefs have to offer students, while avoiding those circumstances highlighted by critics as most responsible for compromising their value as aids to learning and achievement.

## ACKNOWLEDGMENTS

This article is based on Jeffrey C. Valentine's doctoral dissertation from the Department of Psychological Sciences at the University of Missouri–Columbia; Harris Cooper and B. Ann Bettencourt, cochairs. Bruce Biddle, David L. DuBois, and Carol Anne Kardash served as committee members.

We thank Laurel A. Copeland, Dewey G. Cornell, Marc D. Goldberg, William Graziano, and Marc A. Zimmerman for their kind responses to our requests for information.

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