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Cognitive-Behavioral School-Based Interventions for Anxious and Depressed Youth: A Meta-Analysis of Outcomes

Matthew P. Mychailyszyn, Douglas M. Brodman, Kendra L. Read, and Philip C. Kendall,
Temple University

A meta-analysis of school-based interventions for anxious and depressed youth using QUORUM guidelines was conducted. Studies were located by searching electronic databases, manual effort, and contact with expert researchers. Analyses examined 63 studies with 8,225 participants receiving cognitive-behavioral therapy (CBT) and 6,986 in comparison conditions. Mean pre–post effect sizes indicate that anxiety-focused school-based CBT was moderately effective in reducing anxiety (Hedge's $g = 0.501$) and depression-focused school-based CBT was mildly effective in reducing depression (Hedge's $g = 0.298$) for youth receiving interventions as compared to those in anxiety intervention control conditions (Hedge's $g = 0.193$) and depression intervention controls (Hedge's $g = 0.091$). Predictors of outcome were explored. School-based CBT interventions for youth anxiety and for youth depression hold considerable promise, although investigation is still needed to identify features that optimize service delivery and outcome.

Key words: **anxiety, anxiety treatment, cognitive-behavioral therapy, depression, depression treatment, meta-analysis, school-based intervention, youth.** ***[Clin Psychol Sci Prac 19: 129–153, 2012]***

Prevalence rates for anxiety disorders and depressive disorders in children and adolescents have been found to range from 2% to 27% (Costello, Mustillo, Erkanli, Keller, & Angold, 2003). Research suggests that these disorders are among the most prevalent categories of psychological problems in youth (Albano, Chorpita, Barlow, Mash, & Barkley, 2003; Costello et al., 2003). Although anxious and depressed youth may be misperceived as less troubled than those exhibiting hyperactive or oppositional behavior, they are nevertheless distressed and impaired (Ialongo, Edelsohn, Werthamer-Larsson, Crockett, & Kellam, 1995; Strauss, Frame, & Forehand, 1987; Wood, 2006). Anxiety disorders increase vulnerability to the development of comorbid conditions and, if left untreated, may persist into adulthood and lead to the development of substance abuse problems (Kendall, Safford, Flannery-Schroeder, & Webb, 2004). Depression is similarly associated with a range of negative outcomes for youth (Collins & Dozois, 2008), with evidence suggesting that youth with even only subclinical levels of depressive symptoms experience a wide range of impairment (Georgiades, Lewinsohn, Monroe, & Seeley, 2006) and are at greater risk of experiencing the onset of an episode of dysthymia and/or major depression (Arnarson & Craighead, 2011).

Although as many as 40% of youth with mental health diagnoses may be accessing services across different sectors, only about one in five receives care from a specialty mental health provider (Burns et al., 1995). This disparity may be magnified for anxiety and depression because such problems are often less visible to parents and teachers as compared to externalizing conditions (e.g., ADHD), and the majority of youth struggling with these disorders have never received

Address correspondence to Philip C. Kendall, Temple University, 478 Weiss Hall, 1701 North 13th Street, Philadelphia, PA 19122. E-mail: pkendall@temple.edu.

treatment (Chavira, Stein, Bailey, & Stein, 2004; Logan & King, 2002). Of great concern is that little ground has been gained, as the estimated percentage of those with unmet needs has remained unchanged for two decades (United States Congress, Office of Technology Assessment, 1986).

Despite strong support in favor of evidence-based practice (EBP) from the American Psychological Association (APA) and the American Academy of Child and Adolescent Psychiatry (AACAP), few individuals accessing services actually receive empirically supported treatments (ESTs; U.S. Public Health Service, 2000). Although the importance of relying on an evidence base is beginning to lead to a greater focus on ESTs in the training of psychologists (Cukrowicz et al., 2005), a considerable gap remains. Indeed, McCabe (2004) claims that "practical guidelines for professional psychologists who may be interested in incorporating EBPs into their own work setting are not available" (p. 571), and Schoenwald et al. (2008) state that "little is known about the nation's infrastructure for children's mental health services (CMHS), the capacity of that infrastructure to support the implementation of (ESTs), and factors affecting that capacity" (p. 85).

One potential solution is to more fully incorporate mental health services into school systems. The need for schools to play a larger role in the maintenance of socio-emotional well-being for youth is widely noted (Weist et al., 2003). The former Surgeon General's Report on Children's Mental Health in 2000 promoted a strengthening of schools' capacity to be "a key link to a comprehensive, seamless system of school- and community-based identification, assessment and treatment services" (U.S. Public Health Service, 2000). The President's New Freedom Commission on Mental Health (2003) supported these sentiments, emphasizing the dynamic interplay between emotional well-being and academic success. In establishing its national action plan for mental health from 2006 to 2011, the Council of Australian Governments (COAG, 2009) pledged support to mental health-care reform and building partnerships, allowing a more effective institution of school-based prevention and early intervention programs for youth in need of care.

Treatment of Anxiety and Depression in Youth

What services have been found to be helpful for children and adolescents? One of the primary modes of intervention for youth anxiety and depression is cognitive-behavioral therapy (CBT; James, Soler, & Weatherall, 2005). CBT is a collaborative, problem-focused approach that seeks to address the underlying and maintaining factors of a child's distress (Kendall, 2006).

Research has found that CBT for anxiety disorders in youth is efficacious. A randomized clinical trial (RCT; Kendall, 1994) found that anxious youth receiving CBT (the *Coping Cat Program*; Kendall & Hedtke, 2006a, 2006b) experienced significant improvement by posttreatment compared to controls. These results were replicated (Kendall et al., 1997), with treatment gains maintained up to 7 years later (Kendall et al., 2004). Using an Australian adaptation of the *Coping Cat*, Barrett, Dadds, and Rapee (1996) again reported beneficial gains and found added benefits derived from the addition of a family anxiety management component; support for family cognitive-behavioral therapy (FCBT) was also reported by Kendall, Hudson, Gosch, Flannery-Schroeder, and Suveg (2008). Silverman et al. (1999) as well as Flannery-Schroeder and Kendall (2000) found support for group cognitive-behavioral therapy (GCBT), with gains maintained at 1-year follow-up (Flannery-Schroeder, Choudhury, & Kendall, 2005). A recent multi-site evaluation of treatments for youth anxiety (Walkup et al., 2008) found that CBT (60%) and medication (sertraline, 55%) yielded significant treatment response, with the combination of CBT and medications (80%) producing the best outcomes.

Empirical support has also been found for the efficacy of CBT for depression in youth. Clarke et al. (2001) reduced subclinical levels of depression to prevent later onset of depressive episodes for youth with a depressed parent. A recent evaluation of long-term efficacy of various treatments for youth depression (The TADS Team, 2007) found increasing response rates over time for adolescents receiving CBT, such that by 36 weeks, CBT produced response rates equivalent to those of medication and medication combined with therapy. Moreover, adding CBT appeared to enhance the safety of the medication, as youth receiving combination treatment experienced significantly fewer

suicidal events than those receiving medication alone. Research has found support for family-based approaches, such as Systemic Behavioral Family Therapy, to treat depression in youth (Kolko, Brent, Baugher, Bridge, & Birmaher, 2000) by reducing family conflict as well as parent–child relationship problems. Group-based CBT has also been demonstrated to be a robust approach to treatment for depressed youth (Lockwood, Page, & Conroy-Hiller, 2004; Rossello, Bernal, & Rivera-Medina, 2008).

Transportability of Interventions

Cognitive-behavioral therapy meets established standards to be deemed efficacious for anxious and for depressive disorders in youth and is recommended as the first-line treatment of choice (APA Task Force on Promotion & Dissemination of Psychological Procedures, 1995; Chambless & Hollon, 1998). However, questions remain regarding the potential for empirically supported interventions to be successful in schools (Owens & Murphy, 2004).

Answers revolve primarily around issues of transportability—the degree to which evidence-based treatments work when implemented in community contexts (Schoenwald & Hoagwood, 2001). This topic is not new: Over 15 years ago, the *Journal of Consulting and Clinical Psychology* devoted a special issue to an examination of "how findings from carefully controlled studies of efficacious psychosocial interventions for children can be transported into naturalistic studies of the effectiveness of services" (Hoagwood, Hibbs, Brent, & Jensen, 1995, p. 683). More recently, Ginsburg, Becker, Kingery, and Nichols (2008) pointed out that the challenges continuing to confront psychology today are with regard to successful dissemination of empirically supported intervention strategies to community treatment settings.

The achievement of transportability requires "bridging the gap" (Weisz, Donenberg, Han, & Weiss, 1995), which has also been referred to as "smoothing the trail" (Kendall & Beidas, 2007) and "translating science into practice" (Chorpita, 2003). Success on this front would yield what Schoenwald and Hoagwood (2001) refer to as "street-ready" interventions—ones that can be applied in representative settings and systems.

The School Context

The link between mental health and academic success provides a natural avenue for collaborative efforts between professionals in psychology and education (Mufson, Dorta, Olfson, Weissman, & Hoagwood, 2004), as research has demonstrated the deleterious effects of psychopathology on school functioning (Ialongo et al., 1995; Mychailyszyn, Mendez, & Kendall, 2010). However, challenges exist regarding schools' acceptance of a greater role in children's mental health (Pincus & Friedman, 2004), and difficult questions remain unanswered. Schoenwald and Hoagwood (2001) inquire: What is the intervention? Who can implement it, under what circumstances, and to what effect? Further, Owens and Murphy (2004) ask: How effective are treatments when delivered to diverse populations by mental health professionals in community settings, who struggle with the burden of higher caseloads and fewer resources?

Numerous advantages make schools a preferred setting for addressing mental health needs. First and foremost, schools are the most youth-accessible location because this is where they spend most of each day (New Freedom Commission on Mental Health, 2003). From an ecological contextual perspective (Bronfenbrenner, 1979), schools constitute an integral part of the microsystem, serving as one of the most proximal influences in a youth's contextual environment. As such, the school setting maximizes access to youth by offering interventions "where they are" (Weist et al., 2003), which can help to eliminate obstacles that prevent youth from receiving care (Flaherty, Weist, & Warner, 1996).

Another benefit is that schools are a primary setting in which youth display impairment (Ginsburg et al., 2008; McLoone, Hudson, & Rapee, 2006), and thus, school-based interventions are uniquely suited to enhance generalizability by fostering growth in the very situations that lead to difficulty. Additionally, schools are often comprised of large, diverse populations with a heterogeneous collection of presenting difficulties. All of these factors contribute to schools demonstrating the type of "ecological validity" (Owens & Murphy, 2004) that allows treatment benefits to be realized in a context that is both clinically and practically meaningful. Further, the naturalistic setting of

schools may reduce the stigma that often accompanies mental health treatment in the greater community (Storch & Crisp, 2004); indeed, research suggests that youth are more likely to utilize school-based services than those offered through traditional mental health clinics (Anglin, Naylor, & Kaplan, 1996).

Providing mental health training to school personnel is another valuable feature of implementing school-based services and one that confers a number of benefits (Ginsburg et al., 2008). For instance, their presence in schools allows them to intervene with youth and process problematic situations on a real-time basis. Of particular importance to school systems located in less economically advantaged areas, school-based clinicians can offer programs that are much more affordable as compared to traditional private practice outpatient or hospital-based services.

Intervention Classification System

Is the goal of school-based interventions to prevent the onset of problems or treat existing problems? The initial conceptualization of disease prevention was developed approximately a half-century ago within a public health framework. The Commission on Chronic Illness (1957) proposed "primary," "secondary," and "tertiary" as three levels of prevention. However, only the primary level seemed to reflect prevention in the truest sense of the word, and the insufficiency of this system led to alternative frameworks (Gordon, 1983; Gordon, Steinberg, & Silverman, 1987; Kendall & Norton-Ford, 1982). Gordon developed a similar three-tier system that adopted a "(cost) risk–benefit" perspective; however, criticism about confusion between prevention and treatment endured.

In 1994, the Institute of Medicine (IOM) commissioned the Committee on Prevention of Mental Disorders to develop a classification system for defining interventions. Incorporating some of Gordon's (1983, 1987) terminology, Mrazek and Haggerty (1994) suggested: "Universal preventive interventions" are for entire population groups and not based on identified risk status; "selective preventive interventions" are for those with a higher than average risk for developing a mental disorder on the basis of various risk factors associated with its onset; finally, "indicated preventive interventions" seek to help high-risk individuals exhibiting measurable symptoms signaling the impending onset of mental disorder, but who do not meet sufficient criteria for a clinical diagnosis. Despite these clarifications, the boundaries between levels are blurred in real-world implementation, such as when disorder onset is not known, making distinction between indicated prevention and early treatment impossible (Albee & Gullotta, 1986).

Previous Reviews and Meta-Analyses

To what extent have interventions met the mental health needs of anxious and depressed youth, and what is the status of the collective set of outcomes? The answer requires a synthesis of available information: To date, this objective has not been fully achieved.

Ishikawa, Okajima, Matsuoka, and Sakano (2007) conducted a meta-analysis of 20 randomized controlled trials of CBT for youth anxiety disorders. Results suggested essentially no difference between treatments employing 10 or fewer sessions as compared to those utilizing 11 or more sessions. The authors noted that, due to the small number of studies included in their review, more work is needed to identify the influence of CBT for anxiety disorders on comorbid depression. Finally, effect sizes of studies conducted in university clinics or hospitals were found to be larger than for the studies conducted at "other settings." However, in moving toward the expansion of dissemination efforts where efficacious programs are implemented in the community at large, it is exactly these types of *other settings* (e.g., schools) that need to be examined in greater detail.

In some cases, efforts have been made to narrow investigation to only studies of school-based interventions. Rones and Hoagwood (2003) reviewed school-based program evaluations published between 1985 and 1999 that targeted multiple mental health concerns and found there to be a lack of treatment effects for even the most prevalent disorders of childhood. They noted that, "Surprisingly, we did not find any school-based anxiety prevention or intervention programs that met the criteria for entry into this review" (p. 238). Neil and Christensen (2007) reviewed school-based interventions for anxiety, but the review included only studies examining programs developed or evaluated in Australia and did not apply meta-analytic procedures.

Most studies reviewed in meta-analyses are those conducted in research clinics (Weisz & Jensen, 2001) and thus do not offer crucial details about which aspects of school-based interventions contribute to success. Indeed, in a recent review of evidence-based treatments for anxious youth, Silverman, Pina, and Viswesvaran (2008) asserted that while there are existing studies to suggest the feasibility of group-based CBT (GCBT) in school settings, "the efficacy of school-based GCBT warrants further research attention" (p. 118).

Two recent efforts sought to meet the need of an exclusive focus on school-based interventions. Neil and Christensen (2009) and Calear and Christensen (2010) reviewed randomized controlled trials of school-based prevention and early intervention for youth anxiety and depression, respectively. Of note, while the former determined that significant effects for anxiety interventions did not depend on type of comparison control group or program implementer, the latter review found that interventions using attention-control conditions and those led by teachers were associated with fewer significant outcomes. Effect sizes were reported, but the authors of both reviews stated that a formal meta-analysis was not conducted. To our knowledge, no meta-analysis devoted entirely to school-based studies of anxiety and depression in youth has been reported.

The Present Study

Meta-analysis is a preferred method of data synthesis (Rosenthal & DiMatteo, 2001), and the Task Force on Statistical Inference of the American Psychological Association emphasizes "that reporting and interpreting effect sizes in the context of previously reported effects is essential to good research" (Wilkinson & the APA Task Force on Statistical Inference, 1999, p. 599).

The current review included school-based interventions for anxiety and depression, the rationale being that, as internalizing disorders, anxiety and depression share similarities in their cognitive and behavioral symptoms. Anxiety and depression overlap, with substantial correlations between scales assessing them, a high degree of comorbidity, and the likelihood that the two disorders share a common diathesis (Watson & Kendall, 1989). Research has demonstrated that anxiety often predates depression (Brady & Kendall, 1992; Cole, Peeke, Martin, Truglio, & Seroczynsky, 1998), and contemporary conceptualizations have begun to focus on "common elements" (Chorpita, Daleiden, & Weisz, 2005) and/or "transdiagnostic" (McLaughlin & Nolen-Hoeksema, 2011) features and "unified protocols" (Ellard, Fairholme, Boisseau, Farchione, & Barlow, 2010). Given the meaningful relationship between anxiety and depression, their joint exploration is worthwhile.

The present project focused on the following: How effective are school-based interventions in reducing anxious and depressive symptoms among school-age youth? What are the relationships between characteristics of the programs that are associated with more or less positive outcomes? Further, do these interventions yield positive effects on associated outcomes such as self-esteem and comorbid psychopathology symptoms? The primary hypothesis was that combining all studies of school-based interventions would reveal statistically significant summary effect sizes, which would be significantly greater than those obtained for youth in control conditions. Additional hypotheses included the following: (a) the effect size found for treatment studies would be greater than that found for prevention studies; (b) the effect sizes for selective and indicated prevention studies would be greater than that found for universal prevention studies; and (c) increasing duration of intervention would be associated with larger magnitude of effects. The process and findings were guided by the standards developed at the Quality of Reporting of Meta-analyses conference (QUORUM; Moher et al., 1999).

METHOD

For this review, an *intervention* was defined as a program of content delivered to students with the purpose of either (a) preventing the onset or exacerbation of anxiety or depressive symptoms or disorders or (b) alleviating the symptoms and/or severity of already existing anxious and depressive disorders and associated symptomatology.

Searching

Studies were identified through several search strategies. First, the PsycINFO and PubMed online databases were searched using the following terms: *anxiety, depression, prevention, treatment, intervention, school,*

school-based, children, adolescents, and *youth*. To provide a comprehensive review of the most recent work in the area while ensuring inclusion of an adequate number of investigations to yield meaningful results, search parameters were limited to empirical studies between the 20-year time frame of January 1, 1990, and December 31, 2009. The final online search date was 06/01/2010. Second, the reference sections of included articles as well as those of previously conducted meta-analyses were reviewed. Third, the tables of contents for journals published between 2005 and 2009 that typically include studies on child psychopathology as it pertains to schools were reviewed. These journals included *School Psychology Review, Journal of School Psychology, School Psychology Quarterly, Journal of Consulting and Clinical Psychology, Journal of the American Academy of Child and Adolescent Psychiatry*, *Archives of General Psychiatry*, *Behaviour Research and Therapy*, *Development and Psychopathology*, *Journal of Abnormal Child Psychology*, *Journal of Abnormal Psychology*, *Journal of Child Psychology and Psychiatry*, *Journal of Clinical Child and Adolescent Psychology*, and *Psychological Bulletin*. Finally, several prominent researchers were consulted to ensure inclusion of relevant published and unpublished studies that had not yet been identified.

Selection

Studies were selected using the following inclusion criteria: (a) the study evaluated a cognitive-behavioral intervention for anxiety or depression implemented within a school; because some studies used alternative descriptions for associated symptomatology (e.g., "emotional resilience"), we required that studies used a standardized measure of anxiety (e.g., RCMAS, MASC) or depression (e.g., CDI, CES-D); (b) participants were grades K through 12; (c) the study provided the statistical information needed for the calculation of effect sizes, or author contact information to obtain the data needed; and (d) although the intervention may have been delivered in any language, the study was written in English. To preserve independence, studies were excluded if the sample under investigation overlapped with the sample from another included study. When this was suspected, authors were contacted to confirm overlap. In such instances, the study conducted first was included.

Validity Assessment

The standards of peer review have led to the suspicion that published studies are more likely to have statistically significant findings than those that are not published (Sterling, 1959). This "File Drawer Problem" (Rosenthal, 1979) refers to the notion that studies capable of being retrieved and included in a meta-analysis are not likely to be a random sample of all studies conducted (Rosenthal, 1991). This may result in a publication bias, whereby unpublished studies are not included in the calculations, and the resultant mean effect size may not reflect an accurate representation of the findings. Of note, despite research suggesting that a majority of meta-analysts believe that unpublished material should be included, only about a third of published meta-analyses have been found to include such unpublished work (Cook, Guyatt, & Ryan, 1993). The present meta-analysis addressed the file drawer problem in two ways. First, unpublished studies were not excluded; efforts were made to obtain data from relevant unpublished investigations discovered at conference presentations, in article reference lists, via communication with experts in the field, and through a request for relevant unpublished studies distributed to the email listserv of the Association for Behavioral and Cognitive Therapies (ABCT).

Second, Orwin's (1983) adaptation of "Fail-Safe N" (Rosenthal, 1979) was calculated: This allows for the determination of the number of studies with a small magnitude effect size that would be needed to reduce the mean effect size to a specified criterion level. Such a value offers an estimation of how resistant to null effects the calculated mean effect size is.

Data Abstraction

All studies were coded by doctoral students in clinical psychology; two served as independent coders following training that included (a) instruction, (b) practice coding, and (c) training to criterion. Quantitative data from measures used in the reported studies to examine constructs of interest were entered into a Microsoft Excel database, with algorithms programmed to calculate effect sizes (ESs). Inter-rater reliability, calculated using intra-class correlation (ICC) for continuous variables and Cohen's kappa (κ) for categorical variables, was acceptable for all coded variables included in the current project's analyses (see Results section).

Study Characteristics

Information related to research design (e.g., level of intervention, participant demographics, study year) and intervention delivery (e.g., number of sessions, session length, program implementer) was coded—either as continuous variables or as categorical dummy variables—so as to determine whether ESs fluctuated as a function of any of these variables.

In terms of selecting variables to examine as predictors, we elected to focus only on those that were related to participant characteristics (e.g., gender) or features of the intervention (e.g., intervention level). Our motivating intention for this study was to be able to make recommendations about which participants and which modes of intervention delivery yield the most optimal results. It was our hope that in this way, future efforts could be tailored in terms of program development to maximize efficiency.

Quantitative Data Synthesis

Individual studies often used more than one measure of a construct. However, treating multiple measures of a unitary construct as distinct entities would violate assumptions of independence that underlie meta-analysis (Rosenthal, 1984). Following the recommendations of Lipsey and Wilson (2000) and consistent with recent high-quality meta-analyses (Stewart & Chambless, 2009), multiple effect sizes for a particular construct within individual studies were averaged. This was performed prior to synthesis with effect sizes from other studies so as to ensure that each study would only contribute one single effect size per construct.

Effect Size. The present meta-analysis used the standardized mean gain (SMG; Becker, 1988) effect size, which is used to explore changes in continuous measures of constructs over time (e.g., baseline to posttreatment). The SMG allows for taking control groups into account by comparing a summary effect size for intervention groups with one for control groups to determine whether one is statistically significantly greater than the other. In this way, findings are not limited to knowing only if intervention groups have lower scores than control groups at a singular posttreatment timepoint—an effect that could potentially be affected by nonequivalent group scores at baseline—as with the standardized mean difference (SMD; Lipsey & Wilson, 2000). Rather, the use of SMG allows for the determination of whether an intervention group truly yields statistically significantly greater *change* in scores over time as compared to a control group, which takes baseline scores into account. Additionally, because the SMD can only be calculated for studies using control groups, reliance on this measure of effect size would require the exclusion of studies lacking control groups and thus a loss of available data.

To calculate the SMG, the correlation between baseline and posttreatment scores is needed; however, most studies do not provide this value or the raw data necessary to calculate it. As such, following Rosenthal's (1993) recommendation used in recently published meta-analyses (Hoffman, Sawyer, Witt, & Oh, 2010), a conservative estimate of $r = 0.7$ was utilized.

All calculated effect sizes are represented in the form of Hedge's g, as the distribution of Cohen's d effect sizes may be upwardly biased (Lipsey & Wilson, 2000) if based on a number of studies with small sample sizes (e.g., $N < 20$). Hedge's g is obtained by multiplying Cohen's d by an adjustment factor:

$$\text{Hedge's } g = (d) \times \left(1 - \frac{3}{4\text{df} - 1}\right)$$

and the magnitude may be interpreted according to the convention established by Cohen (1988), in which effect sizes are considered small (0.2), medium (0.5), and large (0.8). For most of the measures of anxiety and depression utilized, higher scores indicate greater levels of symptomatology. SMG effect sizes were calculated by subtracting postintervention scores from baseline scores, such that positive values would reflect effects in the expected direction (e.g., reduced over the course of the intervention). Correspondingly, a negative SMG effect size indicates that symptomatology worsened over time.

From the distribution of Hedge's g values produced, a summary effect was produced by pooling across studies and calculating an average effect size statistic. Analyses were completed following the procedures outlined by Borenstein, Hedges, Higgins, and Rothstein (2009), developers of the software program Comprehensive Meta-Analysis, which has been utilized in other recent meta-analytical investigations (Brunwasser, Gillham, &

Kim, 2009; Hoffman et al., 2010). Additional analyses were conducted using the SPSS Version 18.0 for Windows statistical software package (SPSS, Inc., Chicago, IL).

Testing for Heterogeneity of Effect Sizes. An important issue is whether the distribution of effect sizes is homogeneous (Hedges, 1982). In other words, do effect sizes in the set used to obtain the mean effect size all estimate the same population effect size? In a homogeneous distribution, each individual effect size would be expected to differ from the population mean effect size only as a function of sampling error. However, when the null hypothesis of homogeneity is rejected, it must be assumed that the variation in effect sizes is owing to a source other than only participant-based sampling error, namely, random differences that cannot be identified among the distribution of studies. The equation used to test homogeneity is:

$$Q = \sum_{i=1}^{k} W_i Y_i^2 - \frac{\left(\sum_{i=1}^{k} W_i Y_i\right)^2}{\sum_{i=1}^{k} W_i}$$

The primary goal of meta-analysis is the synthesis and summarization of existing data. Two statistical models—fixed effects and random effects—differ in their approach to describing the specific universe to which conclusions can be applied. The "universe" refers to the hypothetical assortment of studies that, in principle, could possibly be conducted and about which we wish to generalize (Cooper & Hedges, 1994). The universe to which generalizations are made from a fixed effects model consists of studies that differ from those analyzed only as a function of having different samples of participants. On the other hand, using a random effects model allows inferences not to be restricted only to cases with predictor variables already represented in the sample (Cooper & Hedges, 1994), and comparisons can be generalized to a universe of studies that are not identical to those in the sample (Rosenthal & DiMatteo, 2001).

Owing to differences between models, the choice should depend on the type of inferences the analyst wishes to make (Hedges & Vevea, 1998). Given the underlying theory, Borenstein et al. (2009) suggest that a fixed effects model should only be used if it is believed that all studies are functionally identical, and the goal is not to generalize to other populations. As the included studies were not functionally identical and as generalizing to studies beyond those under investigation was a principal goal of this project, neither of these tenets apply, and so a random effects model approach was adopted (Moses, Mostellar, & Buehler, 2002).

RESULTS

Study Characteristics

A total of 63 studies conducted in 11 countries (the majority in Australia and the United States) were included. A total of 15,211 youth were involved; as some studies jointly targeted anxiety and depression, there were 7,885 youth in anxiety intervention studies and 9,727 involved in studies of interventions for depression. The majority of studies (n = 44) used random allocation at either the level of individual student, class, or school. Fifteen studies conducted assessments only at baseline and postintervention, whereas the remainder evaluated outcomes across a range of follow-up periods (1 month to 4 years). In some instances, evaluations of long-term outcome later published separately from the initial article were used to obtain follow-up data. In terms of intervention classification level, 31 studies were universal prevention, 8 were selective prevention, 6 were indicated prevention, 5 were targeted prevention, 3 were early intervention, and 11 were treatment studies. Of the studies included, 57 were published in peer-reviewed journals, and 6 were unpublished manuscripts that were most often doctoral dissertations.

Effect Size Distribution

Homogeneity Analysis. The distributions of effect sizes were evaluated to determine whether existing variation can be entirely explained by random sampling error within studies, or whether it reflects true and meaningful differences between studies. Statistical tests utilizing the Q-statistic (Borenstein et al., 2009) and I^2 statistic (Higgins, Thompson, Deeks, & Altman, 2003) were conducted. The Q-statistic evaluates the null hypothesis that all studies share a common effect size, whereas the I^2 statistic estimates the proportion of

observed variance that reflects real differences in effect size. With regard to the latter, 25%, 50%, and 75% are suggested standards against which to compare an obtained I^2 statistic, reflecting "low," "moderate," and "high" amounts, respectively, of how much variance is accounted for by real differences.

For the collection of studies exploring anxiety interventions, the null hypothesis that all studies share a common effect size was rejected, and it was concluded that the true effects vary ($Q = 228.73$, $p < .0001$). Further, the I^2 statistic indicates that nearly 90% of the observed variance is accounted for by real differences. For the collection of depression intervention studies, the null hypothesis was similarly rejected, concluding that there is sufficient evidence that true effects vary ($Q = 128.11$, $p < .001$). In this case, the I^2 statistic indicates that 75% of the observed variance in effect sizes is accounted for by real differences.

The homogeneity results support the a priori decision to conduct the meta-analysis according to a random effects model. Statistics show that the studies do not share one common (true) effect size and that the factors that could influence effect size are, indeed, not the same in all the studies included. These findings direct us to explore the sources of the variance, with such efforts described below using subgroup analysis.

Coder Agreement. Inter-rater reliability between coders was high (ICC > 0.90) for continuous ES outcome data and acceptable ($0.70 > \kappa \geq 1.0$) for categorical variables.

Quantitative Data Synthesis

Standardized Mean Gain Effect Size. For the 27 studies evaluating school-based interventions for anxiety that had baseline and posttreatment data, the summary pre–post effect size estimate (Hedge's *g*) for those receiving the intervention was 0.50 (95% CI [0.40, 0.60], $p < .001$) for reducing anxious symptomatology. Of those studies, 22 implemented control conditions, for which the summary pre–post effect size estimate was 0.22 (95% CI [.09, .34], $p < .001$) in terms of a decrease in anxiety symptoms over time. A comparison of these summary effect size estimates finds a significant difference between the two ($Z = 3.50$, $p < .001$), with intervention participants demonstrating greater reductions in anxious symptomatology from baseline to postintervention than controls (Figure 1).

For the 39 studies evaluating school-based interventions for depression that had baseline and posttreatment data, the summary pre–post effect size estimate for those receiving an intervention was 0.30 (95% CI [0.21, 0.40], $p < .001$) for reducing symptoms of depression. Of those studies, 35 included control conditions, for which the summary pre–post effect size was 0.09 (95% CI [0.01, 0.16], $p < .05$) for decreases in depressive symptoms over time. A comparison of these summary effect size estimates finds a significant difference between the two ($Z = 3.56$, $p < .001$), with intervention participants demonstrating greater reductions in depressive symptomatology than controls (Figure 2).

For the fail-safe *N*, a criterion effect size of 0.10 was selected as the level at which results would no longer be considered meaningful, as this represents half of what Cohen's standards for effect size interpretation suggest are small effect sizes (Cohen, 1988). Using this value, results indicate that 108 anxiety intervention studies with an effect size of zero would have to remain unidentified ("in the file drawer") to reduce the summary effect size for anxiety interventions from 0.50 to 0.10. With regard to depression, 78 conducted and unobtained studies with an effect size of zero would have to exist to reduce the mean effect size for depression interventions from 0.30 to 0.10. Although the criterion effect size of 0.10 is acknowledged to be quite small, from the public health perspective, such effects can be meaningful, as moving the distribution of symptoms in the population by even a small amount will often correspond to a reduction in the number of

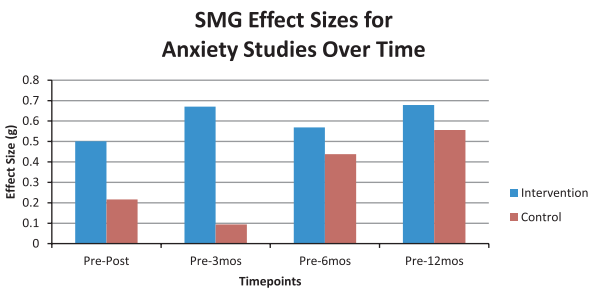

Figure 1. Effect sizes for anxiety studies over time.

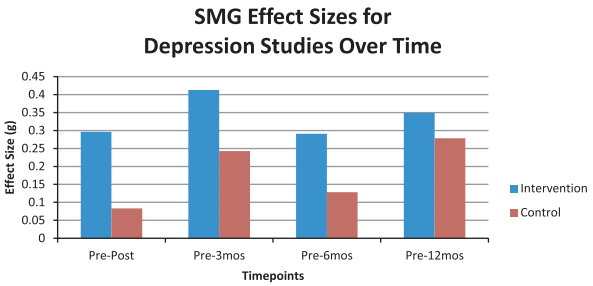

Figure 2. Effect sizes for depression studies over time.

overall cases of disorder (Andrews, Szabo, & Burns, 2002). These fail-safe *N* findings indicate that the obtained summary effect sizes are relatively robust, would not be altered by the presence of a few unidentified studies reaching null effects, and are a fairly accurate representation.

Outcomes Over Time

Three-Month Follow-Up. There were five anxiety intervention studies that assessed outcomes 3 months after the end of the intervention. For these trials, there was a significant mean SMG effect size from baseline to 3-month follow-up for those who had received the intervention of 0.67 (95% CI [0.43, 0.91], $p < .001$). One of these five trials did not make use of a control condition, and in another two, the controls received the intervention after the main study period. This left two studies with baseline and 3-month follow-up data for youth in control conditions, for which there was a nonsignificant mean SMG effect size of 0.09 (95% CI [−0.10, 0.29], $p > .05$). Direct comparison demonstrates that youth receiving interventions experienced significantly greater reductions ($Z = 0.68$, $p < .001$) in anxious symptomatology from baseline to 3-month follow-up than youth in controls (Figure 1).

There were 13 depression intervention studies that assessed outcomes 3 months after the end of the intervention. For these trials, there was a significant mean SMG effect size from baseline to 3-month follow-up for those who had received the intervention of 0.44 (95% CI [0.27, 0.61], $p < .001$). In three of these trials, the control group received the intervention after the main study period. This left 10 studies with baseline and 3-month follow-up data for youth in control groups, for which there was a significant mean SMG effect size of 0.24 (95% CI [0.07, 0.42], $p < .05$). Direct comparison revealed that there were no significant differences ($Z = 1.61$, $p > .05$) in reduction of depressive symptomatology by 3-month follow-up for youth receiving active interventions compared to controls (Figure 2).

Six-Month Follow-Up. There were seven anxiety intervention studies that assessed outcomes 6 months after the intervention. For these, there was a significant mean SMG effect size from baseline to 6-month follow-up for those who had received the intervention of 0.57 (95% CI [0.39, 0.75], $p < .001$). In one of these seven trials, the controls received the intervention after the 3-month follow-up period. This left six studies with baseline and 6-month follow-up data for youth in control conditions, for which there was a significant mean SMG effect size of 0.44 (95% CI [0.15, 0.72], $p < .01$). Direct comparison, however, indicated that there was no significant difference ($Z = 0.77$, $p > .05$) in anxiety reduction from baseline to 6-month follow-up between youth receiving interventions and those assigned to control conditions (Figure 1).

There were 20 depression intervention studies that assessed outcomes 6 months after the end of the intervention. For these trials, there was a significant mean SMG effect size from baseline to 6-month follow-up for those who had received the intervention of 0.31 (95% CI [0.21, 0.40], $p < .001$). One of these studies did not employ a control group, and in another study, the control group received the intervention after the main study period. This left 18 studies with baseline and 6-month follow-up data for youth in control groups, for which there was a significant mean SMG effect size of 0.12 (95% CI [0.03, 0.20], $p < .01$). Direct comparison revealed that youth receiving active interventions experienced significantly greater reductions ($Z = 2.96$, $p < .01$) in depressive symptomatology from baseline to 6-month follow-up compared to controls (Figure 2).

Twelve-Month Follow-Up. Four anxiety intervention studies assessed outcomes 12 months post-intervention. For these trials, there was a significant mean SMG effect size from baseline to 12-month follow-

up for those who had received the intervention of 0.68 (95% CI [0.47, 0.89], $p < .001$). Each of these trials had controls that were followed over the 12-month follow-up period, at which point there was a significant mean SMG effect size of 0.56 (95% CI [0.30, 0.81], $p < .001$). Direct comparison indicated that there was no significant difference ($Z = 0.73$, $p > .05$) in anxiety reduction from baseline to 12-month follow-up between youth receiving interventions and those assigned to control conditions (Figure 1).

There were seven depression intervention studies that assessed outcomes one year after the end of the intervention. For these trials, there was a significant mean SMG effect size from baseline to 12-month follow-up for those who had been assigned to intervention groups of 0.36 (95% CI [0.14, 0.57], $p < .01$). Each of these trials had control groups, which were assessed at the 12-month follow-up period, at which point there was a significant mean SMG effect size of 0.27 (95% CI [0.02, 0.51], $p < .05$). Although youth in both the intervention groups and the control groups demonstrated a significant SMG effect size, direct comparison revealed that the former did not experience significantly greater reductions ($Z = 0.55$, $p > .05$) in depressive symptomatology than the latter from baseline to 12-month follow-up (Figure 2).

Intervention Classification Level

Universal Interventions. Twelve of the 14 studies evaluating universal school-based interventions for anxiety had baseline and postintervention data. For these, the summary pre–post SMG effect size estimate (Hedge's *g*) for those receiving the intervention was 0.32 (95% CI [0.22, 0.43], $p < .001$) for reducing anxious symptomatology. Eleven of those 14 studies had controls with baseline and postintervention data, for which the summary pre–post SMG effect size estimate on continuous measures of anxiety was 0.23 (95% CI [0.06, 0.39], $p < .01$). Direct comparison highlights that both youth receiving interventions and controls experienced significant reductions in anxious symptomatology from baseline to postintervention: There was no significant difference between them (Figure 1) in the amount of change ($Z = 0.99$, $p >$

There were 21 studies evaluating universal school-based interventions for depression that had baseline and postintervention data. These trials yielded a significant summary pre–post SMG effect size estimate for those receiving the intervention of 0.16 (95% CI [0.09, 0.22], $p < .001$) for reducing depressive symptomatology. All of these studies had control groups with baseline and postintervention data, which yielded a nonsignificant summary pre–post SMG effect size estimate on continuous measures of depression of 0.03 (95% CI [−0.03, 0.09], $p > .05$). Direct comparison demonstrated that, while baseline to postintervention effects for youth receiving universal interventions were small, they were significantly (Figure 2) greater than controls ($Z = 2.77$, $p < .01$).

Selective Interventions. All ($n = 4$) of the studies evaluating selective school-based interventions for anxiety had baseline and postintervention data. For these trials, the summary pre–post SMG effect size estimate for those receiving the intervention was 0.53 (95% CI [0.32, 0.74], $p < .001$) for reducing anxious symptomatology. All four studies had control conditions with baseline and postintervention data, for which the summary pre–post SMG effect size estimate on continuous measures of anxiety was 0.04 (95% CI [−0.13, 0.21], $p > .05$). Direct comparison finds that youth receiving selective interventions experienced significantly greater reductions in anxious symptomatology (Figure 1) than did controls ($Z = 3.54$, $p < .001$).

All ($n = 4$) of the studies evaluating selective school-based prevention efforts for depression had baseline and postintervention data for both intervention and control groups. For these trials, the summary pre–post SMG effect size estimate for those receiving the intervention was 0.38 (95% CI [0.23, 0.54], $p < .001$) for reducing depressive symptomatology. Control group studies yielded a summary pre–post SMG effect size estimate for continuous measures of depression of 0.11 (95% CI [−0.11, 0.32], $p > .05$). Direct comparison revealed significant differences in depressive symptomatology between selective interventions and controls ($Z = 2.08$, $p < .05$).

Interventions for Youth With Elevated Symptom Levels. There were four studies evaluating school-based interventions for youth with elevated (though subclinical) levels of anxiety (e.g., indicated and targeted approaches) that had baseline and postintervention data. For these trials, the summary pre–post SMG effect size estimate for participants receiving the intervention was 0.79 (95% CI [0.44, 1.15], $p < .001$) for reducing anxious symptomatology. Only two of these studies had control conditions with available baseline and postintervention data, for which the summary pre–post SMG effect size estimate on continuous measures of anxiety was −0.02 (95% CI [−0.37, 0.32], $p > .05$). Direct comparison finds that youth with elevated levels of anxiety receiving active interventions experienced significantly greater reductions (Figure 1) in symptomatology than did controls ($Z = 3.23$, $p < .01$).

There were seven studies evaluating school-based interventions for youth with elevated levels of depression (e.g., indicated and targeted approaches). For these trials, the summary pre–post SMG effect size estimate for those receiving the intervention was 0.46 (95% CI [0.23, 0.69], $p < .001$) for reducing depressive symptomatology. One of these studies did not have a control group, and thus, there were six studies with available baseline and postintervention data for controls. The summary pre–post SMG effect size estimate on continuous measures of depression for these controls was 0.26 (95% CI [0.04, 0.48], $p < .05$). Direct comparison revealed that youth with elevated levels of depression at baseline receiving active interventions did not experience significantly greater reductions (Figure 2) in symptomatology by postintervention than did controls ($Z = 1.19$, $p > .05$).

Treatment Interventions. Baseline and postintervention data were available for all intervention group youth in the five studies evaluating school-based treatment for youth meeting diagnostic criteria for an anxiety disorder. For these trials, the summary pre–post SMG effect size estimate for those receiving the intervention was 1.10 (95% CI [0.71, 1.50], $p < .001$) for reducing anxious symptomatology. Three of these studies had control conditions with available baseline and postintervention data, for which the summary pre–post SMG effect size estimate on continuous measures of anxiety was 0.38 (95% CI [−0.06, 0.81], $p > .05$). Youth receiving treatment interventions demonstrated significantly greater reductions in symptomatology (Figure 1) than did controls ($Z = 2.43$, $p < .05$).

Baseline and postintervention data were available for all intervention group youth in the five studies evaluating school-based treatment for youth meeting diagnostic criteria for a depressive disorder. For these trials, the summary pre–post SMG effect size estimate for those receiving the intervention was 1.06 (95% CI [0.41, 1.71], $p < .01$) for reducing depressive symptomatology. Four of those five studies had a control group with available baseline and postintervention data, for which the summary pre–post SMG effect size estimate on continuous measures of depression was 0.26 (95% CI [0.02, 0.49], $p < .05$). Youth receiving treatment interventions demonstrated significantly greater reductions in depressive symptomatology (Figure 2) than did controls ($Z = 2.29$, $p < .05$).

Secondary Analyses

Anxiety Interventions' Effect on Depression. From the studies targeting anxiety, there were seven trials that explored the program's effects on depressive symptoms. For these studies, there was a significant mean SMG effect size from baseline to postintervention for those who had received the intervention of 0.24 (95% CI [0.06, 0.42], $p < .01$). One of these studies did not employ a control condition, and another did not present data for the controls at postintervention. This left five studies with baseline and postintervention data for youth in control conditions, for which there was a significant mean SMG effect size of 0.20 (95% CI [0.08, 0.32], $p < .001$). Direct comparison revealed youth in anxiety intervention did not demonstrate significantly greater reductions in depressive symptoms than controls ($Z = 0.36$, $p > .05$).

Intervention Effects on Related Constructs. Some authors of the included school-based intervention studies investigated whether significant reductions in disorder severity were mediated by changes in related variables. In an effort to explore outcomes that may have mediated relationships found in our comprehensive collection of studies, we sought to examine the aggregated effects that interventions had on variables

related to the primary issue of disorder severity. However, the following analyses were limited insofar as anxiety intervention studies seldom assessed additional constructs other than depression, and so our analyses focused only on those related constructs reported on by authors of depression intervention studies.

Self-Esteem. There were nine depression intervention studies that assessed the impact of a program on youth self-esteem. For these trials, there was a nonsignificant mean SMG effect size from baseline to postintervention for those who had received the intervention of 0.09 (95% CI [−0.19, 0.36], $p > .05$). All nine of these studies used a control condition, for which there was a nonsignificant mean SMG effect size of −0.08 (95% CI [−0.34, 0.19], $p > .05$). Direct comparison revealed that youth receiving active interventions did not experience significantly greater improvements in self-esteem than controls ($Z = 0.83$, $p > .05$).

Hopelessness. Six of the studies examining depression incorporated measures of hopelessness and provided data at baseline and postintervention. The mean pre–post effect size estimate for those receiving the intervention was significant at 0.16 (95% CI [0.06, 0.26], $p < .01$) for improving hopelessness (e.g., becoming less hopeless about the future). All six of these studies implemented control conditions, for which the summary pre–post effect size estimate was 0.16 (95% CI [−0.07, 0.40], $p > .05$). A comparison of these summary effect size estimates ($Z = -0.02$, $p > .05$) indicates that although intervention participants demonstrated significant improvements in hopelessness from baseline to postintervention and controls did not, the former did not demonstrate significantly greater changes in hopelessness as compared to the latter.

Attributional Style. Thirteen studies examining interventions for depression incorporated measures of attributional style (e.g., explanatory style) and provided data at baseline and postintervention. The mean pre–post effect size estimate for those receiving the intervention was 0.19 (95% CI [0.06, 0.33], $p < .01$) for improving explanatory style (e.g., making it either more optimistic or less pessimistic). Of those studies, all 13 implemented control conditions, for which the summary pre–post effect size estimate was 0.04 (95% CI [−0.07, 0.15], $p > .05$). A comparison of these summary effect size estimates ($Z = 1.74$, $p > .05$) indicates that intervention participants did not demonstrate significantly greater improvements in explanatory style from baseline to postintervention than did controls.

Predictor Analyses. Are there variables that impact the direction and/or strength of the relationship between intervention and outcomes of anxiety and depression symptomatology? In meta-analysis, examining the significance of differences in effect size between categories of variable tests whether that variable is a moderator (Shadish & Sweeney, 1991).

Intervention Implementer. The comparison of mean effect sizes produced when interventions were implemented by school staff versus research staff was limited to evaluation of universal prevention studies for two reasons: First, studies evaluating universal prevention interventions were more likely than other intervention levels to have school staff implement a protocol, thus creating the needed sample size. Second, including indicated prevention and treatment studies where research staff were implementers would artificially inflate the summary effect size produced for this group, as these intervention levels were generally associated with larger effect sizes.

There were six universal anxiety prevention evaluations that were led by school staff (e.g., classroom teachers in five studies and school nurses in one). There were six universal preventive interventions led by research staff (e.g., psychologists and graduate students in training). Youth receiving universal prevention programs implemented by teachers had a significant SMG effect size of 0.33 (95% CI [0.17, 0.48], $p < .001$), and those in universal prevention programs led by research staff had a significant SMG of 0.41 (95% CI [0.26, 0.55], $p < .001$). A direct comparison of universal prevention interventions implemented by teachers and those led by research staff revealed no significant differences in mean effect size ($Z = 0.77$, $p > .05$).

Among the universal depression prevention evaluations, there were six that were led by school staff, and in all of these studies, the implementers were teachers.

There were eight universal preventive interventions led by research staff, who were psychologists and graduate students in training. Youth receiving universal prevention programs implemented by teachers had a significant mean SMG effect size of 0.19 (95% CI [0.08, 0.30], $p < .01$), whereas those in universal prevention programs led by research staff yielded a nonsignificant SMG of 0.14 (95% CI [−0.06, 0.34], $p > .05$). A direct comparison between universal prevention interventions implemented by teachers and those led by research staff revealed no significant differences in mean effect size ($Z = 0.38$, $p > .05$).

Intervention Dose. Interventions varied in terms of both the number and duration of sessions. Intervention "dose" was defined as the total number of minutes involved in the intervention (i.e., number of sessions multiplied by session duration). For the same reasons described earlier, the comparison of mean effect sizes produced by differing levels of intervention dose was limited to evaluation of universal prevention studies. Studies of universal interventions were ordered according to intervention dose. The median dose was used to divide the studies into "low-dose" and "high-dose" interventions, which were then directly compared to determine whether dosage was a predictor of intervention effects.

Twelve studies of universal interventions for anxiety had available baseline and postintervention data. The average low dose was 354.87 min, and the average high dose of intervention was 682.50 min. Youth receiving low-dose universal interventions had a significant mean SMG effect size of 0.32 (95% CI [0.14, 0.49], $p < .001$), and those receiving high-dose universal interventions had a significant SMG of 0.32 (95% CI [0.19, 0.46], $p < .001$). Direct comparison between low-dose and high-dose interventions, however, revealed no significant differences in mean effect size ($Z = 0.08$, $p > .05$).

Age. To test whether interventions were differentially effective according to participants' age (developmental level), mean effect sizes were compared based on the age of the samples that yielded them. Studies containing the necessary data were ranked according to average age of the participant sample, and the median average age was used to divide studies into "younger" and "older" participants. The mean ages of the study samples were then averaged across studies. For equality of comparison, the mean effect sizes produced by differing levels of intervention dose were once again limited to those obtained from evaluations of universal prevention studies.

There were eight studies of universal interventions for anxiety for which the average age of the sample was reported or could be obtained from study authors, and that had baseline and postintervention data. The average age of "younger" participants was 10.16, whereas with "older" participants the average age was 14.43. Younger youth receiving universal interventions had a significant mean SMG effect size of 0.32 (95% CI [0.14, 0.51], $p < .001$), and older youth receiving universal interventions had a significant SMG of 0.22 (95% CI [0.04, 0.40], $p < .05$). Direct comparison between younger and older youth receiving universal interventions, however, revealed no significant differences in mean effect size ($Z = 0.79$, $p > .05$).

Gender. To test whether participant gender moderated the effect of school-based CBT on reduction in disorder-related symptomatology, Q tests were used to evaluate whether sex accounted for systematic variance in the effects of interventions. Within meta-analysis, Q tests—which are interpreted along the chi-square distribution—are statistical tests that perform a similar function as analyses of variance (ANOVAs) in that they allow for comparison of within- and between-group variance to determine whether variability between groups exceeds the amount that would be expected to occur by chance alone (Borenstein et al., 2009). Only studies that presented data (e.g., means, standard deviations, and sample sizes) separately for boys and girls could be analyzed.

Two studies of school-based anxiety interventions reported data separately by gender of participants. For these studies, gender was not found to moderate changes in anxiety symptomatology from baseline to postintervention ($Q = .08$, $p > .05$).

Mediation Analyses. Are changes in the primary outcomes of anxiety and depression explained by changes in other constructs (e.g., automatic thoughts,

self-esteem, hopelessness, explanatory style)? According to Baron and Kenny (1986; see also Hopwood, 2007; Shrout & Bolger, 2002), a construct acts as a mediating variable when it meets the following four criteria:

1. Variations in levels of the independent variable significantly account for variation in the dependent variable.
2. Variations in levels of the independent variable account for a significant amount of the variation in the hypothesized mediating variable.
3. Variations in the hypothesized mediating variable significantly account for variations in the dependent variable.
4. After controlling for the relationships in conditions 2 and 3, a previously significant relationship between the independent and dependent variables is no longer significant.

In the present meta-analytic review, however, neither self-esteem, hopelessness, nor explanatory/attributional style were found to have improved significantly more from baseline to postintervention among youth receiving active interventions for depression as compared to controls. As such, the second necessary condition for mediation is not satisfied, thus precluding mediation analyses.

DISCUSSION

This meta-analysis yielded support for the primary hypothesis: From 63 identified studies, mean effect size estimates for anxiety and depression interventions were statistically significant and were significantly greater than those obtained for the collection of comparison controls. Specifically, summary effect sizes suggested that anxiety interventions were moderately effective, with a medium-sized effect of 0.50, and that depression interventions were mildly effective, with a small-to-medium overall effect of 0.30. Fail-safe *N* calculations indicated that 108 anxiety and 78 depression intervention studies, all with effect sizes of zero, would have to remain unidentified "in file drawers" to reduce summary effect sizes to the criterion level of 0.10. As such, the present summary effects can be interpreted as relatively robust insofar as it is unlikely that such a number of additional unpublished studies, all with an effect size of zero, exist.

Consistent with hypotheses, Figures 3 and 4 illustrate a stepwise pattern wherein treatment interventions yielded larger summary effect sizes than those produced by prevention programs, and prevention protocols based on some degree of participant risk (e.g., selective or indicated) yielded larger effect sizes than ones delivered to all individuals regardless of risk (e.g., universal). This pattern held for both anxiety and depression interventions. These results are consistent with what would be expected given the populations targeted by each level of intervention. For instance, the small effect sizes yielded by universal interventions may simply reflect the presence of "floor effects" caused by samples that are often largely comprised of nonsymptomatic youth. While understandable given the circumstances of their delivery, the small effect sizes produced by universal studies when combined with results about maintenance of gains (to be discussed) have raised important questions about the justifiability of such efforts (discussed below).

Contrary to hypothesis, increasing duration of intervention was not associated with larger magnitude effect sizes. Regardless of whether they were categorized as "low dose" or "high dose," each had significant (albeit small) mean effect sizes. In terms of cost-effectiveness, such results may suggest that protocols of a more compact nature can fare as well as longer ones in terms of reducing symptomatology of anxiety and depression in youth.

Are intervention effects maintained over time? The present meta-analysis suggests that the answer is "no." By 12-month follow-up, neither youth receiving anxiety interventions nor those in depression interventions exhibited significantly greater reductions in symptomatology from baseline than controls. These results are consistent with those in a Cochrane review of inter-

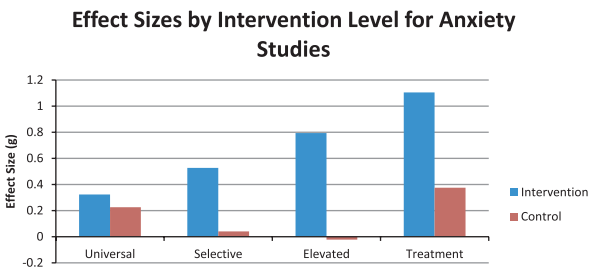

Figure 3. Effect sizes across intervention levels for anxiety studies.

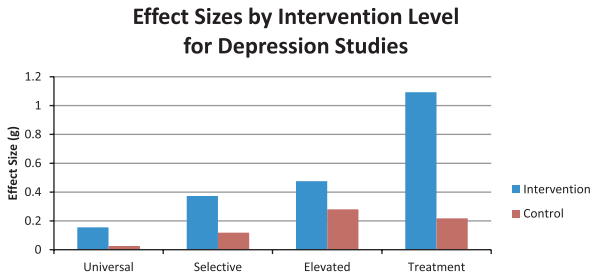

Figure 4. Effect sizes across intervention level for depression studies.

ventions for youth depression (Merry, McDowell, Hetrick, Bir, & Muller, 2004; Merry, McDowell, Wild, Bir, & Cunliffe, 2004), which found that "the only evidence of effectiveness was seen at immediate follow-up" (p. 8), and although this was true for targeted and not universal interventions, "…a significant effect remained when data from both targeted and universal programs was pooled" (p. 8). Thus, our findings are also consistent with prevailing attitudes (Spence & Shortt, 2007), which suggest that relatively brief universal prevention programs may be insufficient to yield lasting effects in the prevention of youth depression, and concluded that endorsement of widespread school-based dissemination of such programs could be premature. The present results seem to indicate that although anxiety and depression programs lead to short-term symptom reduction, they do not confer the type of benefits that are necessary for yielding long-term gains.

Are the intervention effects different when a protocol is implemented by school staff as compared to members of the research team? Mean summary effect size findings indicated that there was no difference between these implementers in terms of the amount of reduction in symptomatology from baseline to postintervention—for those studies exploring programs for either anxiety or depression. However, this review found the description of program leader training to be inconsistently reported and vaguely described across studies. Thus, it is difficult to determine whether the amount or quality of initial instruction on the program (e.g., extensive training versus only being given a manual to follow) or level of ongoing support (e.g., availability of supervision) is what actually delineates successful outcomes from those that do not yield results. Efforts to measure individuals' implementation skill and fidelity is also worthy of exploration to determine whether the "quality of the implementer" is more important than their status as a school versus research staff. A greater knowledge of these variables may help to explain differences that have formerly been ascribed to various types of program implementers.

Such results fall within the context of largely mixed findings throughout the literature regarding the general effectiveness of interventions when implemented by individuals other than research staff. For instance, one evaluation (Harnett & Dadds, 2004) devoted to evaluating a universal depression prevention program under real-world conditions with teachers as implementers found there to be no evidence for the effectiveness for those receiving the intervention as compared to a no-intervention control group. On the other hand, in a trial (Barrett & Turner, 2001) that directly compared a teacher-led and a psychologist-led universal prevention intervention for anxiety, the authors found that both groups outperformed a monitoring control condition, and reductions in anxious symptomatology were equitable.

The present findings suggest that anxiety and depression interventions implemented by school staff yield outcomes equitable to those delivered by research staff. Such findings can be interpreted as good news: The sustainability of school-based programs can only be achieved if interventions can be implemented effectively by individuals who have a consistent presence that spans multiple school years. However, this analysis focused on universal prevention efforts, and thus, the conclusion may vary for other levels of intervention. For instance, Hunt, Andrews, Crino, Erskine, and Sakashita (2009) concluded that although the school counselors and teachers implementing a prevention protocol achieved acceptable program fidelity, "for an indicated intervention…to be effective specialist mental health staff are needed to run it" (p. 303). CBT interventions delivered to youth who evidence some degree of risk or are already manifesting significant symptoms may require implementation by facilitators with specialized training. While this may exclude individuals such as teachers, other school staff (e.g., counselors, social workers, and school psychologists) could potentially deliver selective or indicated prevention, although further research is necessary to determine whether such efforts are feasible and effective.

Investigations of depression interventions often sought to determine whether change in associated variables mediated the relationship between intervention status and reductions in depressive symptomatology. Results were mixed. In the present analysis, the mean effect size for youth receiving interventions was not significantly different from controls in terms of improving explanatory style, and thus, no evidence for mediation relationships was obtained. The same was true for self-esteem and hopelessness within evaluations of depression interventions.

An important issue for school systems is how to balance delivery of what is considered to be the most appropriate level of intervention with the potential risk for stigmatization. For instance, Offord, Kraemer, Kazdin, Jensen, and Harrington (1998) list labeling and stigmatization as a disadvantage of targeted interventions while describing an absence of labeling and stigmatization as an advantage of universal interventions. In describing universal prevention programs, Barrett, Lock, and Farrell (2005) suggest that "potential advantages involve reducing stigmatization" (p. 541), and Quayle, Dziurawiec, Roberts, Kane, and Ebsworthy (2001) claim that they "prevent the labeling effects of selective programs provided only to children 'at risk'" (p. 195). Although these perspectives are valuable, weight must also be given to the magnitude of the effects of the different levels of intervention.

Rapee et al. (2006) noted that adolescents receiving indicated prevention reported significantly more satisfaction with the program they received than those in the universal intervention. Rapee and colleagues acknowledged more stigma, though they conclude that "this may be a small price to pay for a mode of delivery that is potentially more satisfying for consumers and hence more sustainable" (p. 175). Perhaps schools need not shy away from interventions based on fear of stigmatization, but implement the type of selective and indicated preventive efforts that yield the greatest effect sizes.

The present meta-analysis has potential limitations. First, to maximize comprehensiveness, the studies included were not limited to RCTs, an approach that is sometimes taken when conducting reviews such as those published in the Cochrane Database (Higgins & Green, 2005). Although RCTs yield reliable estimates of effects, there are challenges inherent to conducting them in schools, which reduces the number of studies that meet RCT criteria. Another limitation had to do with the application of statistics for analyzing and interpreting results: While our findings for studies of depression interventions were robust, the significance level of what were relatively small mean effect sizes may have been due largely to the very large sample size derived from our comprehensive collection of studies rather than to considerable clinical meaningfulness. Another statistical limitation was reduced power (small samples) for identifying effects in secondary analyses.

In terms of recommendations for further research, we suggest that future school-based investigations more systematically report rates of attendance and program adherence. To examine such variables in future meta-analyses, it will be necessary for these data to be reported as part of standard practice. Additionally, school-based intervention research should broaden its scope of identified risk factors beyond those typically investigated (e.g., elevated symptoms, having divorced parents) to focus on other less-studied factors such as personality pathology, which has been shown to be a powerful predictor of the recurrence of major depressive disorder (Craighead, Sheets, Craighead, & Madsen, 2011; Hart, Craighead, & Craighead, 2001).

Of central importance is consideration of how "success" is measured, and whether current methods fall short of assessing whether programs truly lead to meaningful change in the lives of youth. Many interventions are aimed at modifying dysfunctional thought patterns in the hopes of leading to improved mood. It is recommended that future studies go beyond paper-and-pencil questionnaires to determine whether interventions are leading to change in *functional* outcomes and quality of life (Chambless & Hollon, 1998). For instance, are anxious youth actually engaging in less-avoidant behaviors, and are depressed youth actually participating in a higher rate of pleasurable activities? There remains a critical need to evaluate whether improvements in internalizing symptomatology translate into less of the interference that ultimately underlies anxious and depressive disorders.

Another important issue to consider is the distinction between prevention and treatment effects. Horo-

witz and Garber (2006) emphasize that "prevention effects" are found when control group participants worsen over time as compared to no worsening or diminished worsening of disorder by those in an intervention group; "treatment effects," on the other hand, are reflected by improvement of the intervention group as compared to significantly less or no improvement experienced by control group participants. Similar to conclusions made by Horowitz and Garber, the present review found that the majority of studies claiming to "prevent" anxiety or depression actually obtained treatment effects, as very few studies found control group youth to worsen in symptomatology over time. Future research is encouraged to be clear about the nature of results, regardless of the initial purpose of the intervention.

Finally, schools would be wise to consider a more complete integration of mental health education and coping strategies into the curriculum. A hope of prevention efforts is that youth learn skills that can be used later in life at a time of need. However, the short-term (e.g., approximately 10-week) intervention may not be sufficient. Might it not be efficient and sage to shift to a developmental approach for youth anxiety and depression that includes interventions woven into the fabric of the regular curriculum? Such efforts could be led by school staff, thus facilitating sustainability, and achieving a meaningful step in schools' effort to address mental health-care needs of youth.

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