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Psychosocial Interventions for School Refusal with Primary and Secondary School Students: A Systematic Review

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Executive Summary/Abstract

BACKGROUND

School refusal is a psychosocial problem characterized by a student's difficulty attending school and, in many cases, substantial absence from school (Heyne & Sauter, 2013). It is often distinguished from truancy, in part because of the severe emotional distress associated with having to attend school and the absence of severe antisocial behavior. Truancy, on the other hand, is not typically associated with emotional distress and is commonly associated with severe externalizing behavior. The emotional distress associated with school refusal is often in the form of fear or anxiety, and sometimes in the form of depression. School refusal occurs for about 1-2% of young people, and estimates among clinically referred youth are considerably higher.

There is substantial heterogeneity in both the presentation of school refusal and its associated risk factors. Significant adverse consequences may occur in the short- and long-term, including school dropout and problems with social adjustment. Family members and school staff are also affected by school refusal. The most commonly studied interventions for school refusal are behavioral approaches and cognitive-behavioral therapy (CBT). The overarching aim of these interventions is the reduction of the young person's emotional distress and an increase in school attendance to help the young person follow a normal developmental pathway (Heyne & Sauter, 2013). Behavioral interventions include exposure-based interventions, relaxation training, and/or social skills training with the student, and contingency management procedures with the parents and school staff. CBT manuals additionally focus attention on the identification and modification of maladaptive cognition that may maintain the young person's emotional distress and absenteeism. In some instances parent cognition is also targeted. Other interventions have been used to treat school refusal (e.g., psychodynamic treatment, family therapy, medication) but CBT has been the most studied intervention and most prior reviews have focused on CBT and/or behavioral interventions.

While prior reviews have found some support for CBT and behavioral interventions for reducing anxiety and/or improving attendance, the reviews have been mixed (Maynard et al., 2013). Prior reviews have also been limited to published research, have not adequately assessed the quality of evidence, and have primarily employed

either qualitative or vote-counting methods for synthesizing study outcomes. No prior meta-analysis of interventions targeting school refusal has been located.

OBJECTIVES

The purpose of this review was to inform practice and policy by evaluating the effects of psychosocial interventions for school refusal. The following research questions guided this study:

1) Do psychosocial interventions targeting school refusal reduce anxiety?

2) Do psychosocial interventions targeting school refusal increase attendance?

SEARCH METHODS

Electronic searches were conducted in 15 databases and 4 research registers, and internet searches were conducted for conference proceedings and other grey literature. Searches were conducted using the following keywords: (anxiety OR "school refus*" OR "school phobia") AND (attendance OR absen*) AND (evaluation OR intervention OR treatment OR outcome OR program) AND (student* OR school* OR child* OR adolescen*). Reviews of reference lists of included studies and prior reviews and personal contact with authors of prior studies of school refusal were also conducted to identify potential studies for this review.

SELECTION CRITERIA

Published or unpublished studies assessing effects of psychosocial interventions to improve attendance or reduce anxiety with school-age youth who met criteria for school refusal were included in this review. To be eligible for inclusion in this review, studies must have been conducted or reported between January 1980 and November 2013 and employed an experimental or quasi-experimental design. In addition, studies must have used statistical controls or reported baseline data on outcomes regardless of study design. Studies that assessed effects of medications only or studies conducted in residential treatment centers were excluded from this review.

DATA COLLECTION AND ANALYSIS

Titles and abstracts of the studies found through the search procedures were screened for relevance, and those that were obviously ineligible or irrelevant were screened out. Documents that were not obviously ineligible or irrelevant based on the abstract review were retrieved in full text for final eligibility screening. Two reviewers independently screened the full-text articles for inclusion. Studies that met eligibility criteria were coded independently by two coders. Two review authors also independently assessed the risk of bias in each study using the Cochrane

Collaboration's 'Risk of Bias' tool (Higgins et al., 2011). Coders met to review the coding agreement and any discrepancies were discussed and resolved by consensus.

Effect sizes were calculated in Comprehensive Meta-Analysis (CMA) version 2.0 (Borenstein, Hedges, Higgins, & Rothstein, 2005). We adjusted for differences at baseline by computing the pre-test effect size and subtracting it from the post-test effect size. The standardized mean difference effect size statistic, employing Hedges' *g* to correct for small sample size bias (Hedges, 1981), was used. When an author used more than one measure of an outcome, an effect size was calculated for each measure and a mean ES was calculated so each study contributed only one effect size per study for each outcome. Four meta-analyses were performed; two meta-analyses were performed to synthesize studies assessing effects of psychosocial interventions on anxiety and attendance and two were performed to synthesize effects of studies assessing effects of medication in combination with psychotherapy on anxiety and attendance. A weighted mean effect was calculated by weighting each study by the inverse of its variance using random effects statistical models. We assessed statistical heterogeneity using the *Q*-test and I^2 statistic.

Several moderator and sensitivity analyses were planned, but due to the small number of studies included in this review and lack of heterogeneity, we limited additional analyses performed.

RESULTS

A total of eight studies examining effects of interventions on anxiety or attendance with 435 school-age participants exhibiting school refusal were included in this review. Six studies examining effects of psychosocial interventions and two studies assessing comparative effects of psychosocial interventions with and without medication met inclusion criteria for this review. Six of the included studies were randomized controlled trials (RCT) and two were quasi-experimental design (QED) studies. The majority (75%) of the studies were published in peer-reviewed journals. Five of the interventions took place in a clinic setting, one in the school, one in the school and home and one in an undisclosed setting. All but one of the six psychosocial intervention studies in this review assessed the effects of a variant of cognitive-behavioral therapy (CBT) compared to no treatment control (k = 1), an unspecified control (k = 1) or alternative treatment control group (k = 4). For the two studies assessing effects of medication, the same CBT intervention was applied across treatment and control groups with either Fluoxetine or imipramine as the treatment and placebo or no placebo as the control.

The mean effect of the psychosocial interventions at post-test on anxiety was g = 0.06 (95% CI [-0.63, 0.75], p = .86), demonstrating a non-significant effect. The homogeneity analysis indicated a moderate degree of heterogeneity (Q = 11.13, p = .01, I^2 = 73.05%, τ^2 = .36). Effects on attendance were significant (g = 0.54 (95% CI [0.22, 0.86], p = .00). The homogeneity analysis indicated a small degree of

heterogeneity (Q = 8.82, p = .12, I^2 = 43.32%, τ^2 = .06). Similar results were found for the mean effects of medication + CBT studies, with effects on anxiety being not significant (g= -0.05, 95% CI [-0.40, 0.31], p = .80) and effects on attendance being positive and statistically significant (g = 0.61 (95% CI [0.01, 1.21], p = .046). Studies were homogenous for the medication + CBT studies for both anxiety (Q = .30, p = .58; I^2 = 0.00% and τ^2 = .00) and attendance (Q = 1.93, p = .17, I^2 = 48.23%; τ^2 = .09).

AUTHORS' CONCLUSIONS

The present review found relatively few rigorous studies of interventions for school refusal. Seven of the eight included studies assessed effects of a variant of cognitive behavioral therapy (CBT), thus there appears to be a lack of rigorous evidence of non-CBT interventions for school refusal. Findings of the current review were mixed. While both the CBT only and CBT plus medication interventions found, on average, positive and significant effects on attendance compared to control, effects on anxiety at post-test across both sets of studies were not significantly different from zero. Moreover, the magnitude of treatment effects on anxiety varied across the psychosocial only studies, and thus current estimates of treatment effects should be evaluated with caution.

The current evidence provides tentative support for CBT in the treatment of school refusal, but there is an overall lack of sufficient evidence to draw firm conclusions of the efficacy of CBT as the treatment of choice for school refusal. Most of the studies in this review compared effects against other, and sometimes very similar, interventions that could mask larger effects if compared to wait list control or other disparate interventions. Furthermore, most studies only measured immediate effects of interventions; only one study reported comparative longer-term effects on both attendance and anxiety. Thus, there is insufficient evidence to indicate whether or not treatment effects sustain, and whether or not anxiety might further decrease over time with continued exposure to school.

Several risks of bias were present in most studies included in this review, particularly related to blinding of participants and assessors, which must be considered when interpreting the results of this review. Performance and detection bias resulting from inadequate blinding of participants and assessors to condition could upwardly bias the mean effects. In addition, insufficient details related to random sequence generation and allocation concealment were provided to adequately assess selection bias in most studies, and two studies reported non-random allocation to condition. While most studies in this review reported to use random assignment procedures, it is uncertain whether selection bias is present due to inadequate generation of randomization or concealment of allocation prior to assignment.

The few rigorous studies found for this review and the risks of bias present in most of the included studies indicate a need for better-controlled studies. Moreover, independent replications of the manualized interventions examined in this review are

needed, as are longer-term evaluations of effects of interventions. Assessing long-term effects could provide additional answers and insights as to the mixed findings of the effects of interventions on attendance and anxiety. Future studies should also consider other types of interventions for rigorous evaluation. Furthermore, future studies could benefit from larger sample sizes and attention to mitigating potential biases to improve statistical power and causal inference.

1 Background

1.1 THE PROBLEM, CONDITION, OR ISSUE

School refusal is a psychosocial problem characterized by a student's difficulty attending school and, in many cases, substantial absence from school (Heyne & Sauter, 2013). The literature reveals ongoing discussion about the conceptualization and definition of school refusal. Some authors avoid differentiating school refusal from truancy (e.g., Kearney, 2003) while others argue for the value in doing so (e.g., Elliott, 1999; Heyne, Sauter, & Maynard, in press).

Drawing on the work of Berg and colleagues (Berg, 1997, 2002; Berg, Nichols, & Pritchard, 1969; Bools, Foster, Brown, & Berg, 1990), researchers commonly differentiate school refusal from truancy according to the following features. First, students who exhibit school refusal typically display severe emotional distress associated with school attendance, which is not common among truanting youth (e.g., Bools et al., 1990; Egger et al., 2003). Second, when not at school, school refusers are usually at home with their parents' knowledge. Truanting youth, on the other hand, stay away from school without their parents knowing about the absence (Kearney, 2002) or their whereabouts (Berg, Casswell, Goodwin, Hullin, McGuire, & Tagg, 1985). Third, school refusers seldom exhibit characteristics of conduct disorder. Truancy, on the other hand, is commonly associated with severe antisocial behavior (e.g., Egger et al., 2003; Vaughn, Maynard, Salas-Wright, Perron, & Abdon, 2013). This review will adopt Berg and colleagues' conceptualization of school refusal as a type of school attendance problem, which is different from truancy.

The emotional distress associated with school refusal is often in the form of fear or anxiety. In representative samples of clinic-referred school refusers (i.e., participants are included irrespective of the presence or absence of an anxiety disorder), it is commonly found that around 50% of participants have an anxiety disorder (e.g., Baker & Wills, 1978; Bools et al., 1990; McShane et al., 2001; Prabhuswamy et al., 2007; Walter et al., 2010). A broad range of anxiety disorders has been observed, including separation anxiety disorder, specific phobia, social phobia, generalized anxiety disorder, and panic disorder with agoraphobia. Even when full diagnostic criteria for a particular anxiety disorder are not met, prominent symptoms may lead to a diagnosis of Anxiety Disorder Not Otherwise Specified (e.g., Heyne, King, Tonge et al., 2002; McShane et al., 2001). Other school refusers experience fear or anxiety related to school attendance at a level below the diagnostic threshold. In Egger and colleagues' (2003) community study, many more school refusers reported anxiety-related symptoms than were diagnosed with an anxiety disorder. For example, 36% reported a fear

specific to school, and 27% reported headaches or stomachaches associated with separation or school attendance. Indeed, the physiological symptoms commonly reported among school refusers are headaches and stomach aches (Berg, 1980; Torma & Halsti, 1975). As many as 80% of referred school refusing children and adolescents in Honjo and colleagues' (2001) study expressed somatic complaints.

The relationship between school refusal and depression is not clear. According to Elliott (1999), the depression seen in school refusers may be specific to school refusal or a reflection of the relationship between anxiety and depression. King, Ollendick, and Tonge (1995) noted large variability in the incidence and severity of depression among school refusers. We identified four representative samples of clinic-referred school refusers in which depressive disorders were found to be less common than anxiety disorders (Baker & Wills, 1978; Bools et al., 1990; Walter et al., 2010; Wu et al., 2013), two samples in which the disorders were fairly equally represented (Buitelaar, Van Andel, Duyx, & Van Strien, 1994; McShane et al., 2001), and just one study in which depressive disorders were a little more common (Prabhuswamy et al., 2007)[1]. At the symptom level, Buitelaar et al. (1994) reported that depressive symptoms among school refusers were less common than anxiety-related symptoms. In a study of depressive symptoms and school refusal, Honjo and colleagues (2001) concluded that "the typical case of school refusal appears to exhibit somatic complaints in the foreground rather than depression" (p. 629). If depressive symptoms are present, they are more typically seen in adolescent school refusers relative to children (Baker & Wills, 1978).

Externalizing behavior, such as anger and argumentativeness, may also be seen when parents attempt to get the young person to school (Berg, 2002). In some school refusal cases, the presence of multiple externalizing behaviors over time will lead to a diagnosis of oppositional defiant disorder, with rates ranging between 21% and 44% of referred school refusers (Heyne et al., in press). The school refusal criteria of Berg and colleagues specify the absence of severe antisocial behavior such as stealing and destructiveness, hence the finding that conduct disorder (CD) is usually not observed among samples of school refusers.

Twenty-five years ago the etiology of school refusal was described as "incompletely understood" (King & Ollendick, 1989, p. 213). Even today it is not well understood, probably attributable in part to the substantial heterogeneity in the presentation of school refusal and its associated risk factors (Heyne, 2006). Thambirajah, Grandison, and De-Hayes (2008) describe school refusal as multiply determined by a broad range of risk factors which interact with each other and change over time. Several authors (Heyne, 2006; Heyne & King, 2004; Thambirajah et al., 2008) summarize factors identified in the school refusal literature, specified according to individual factors (e.g., behavioral inhibition; fear of failure; low self-efficacy; physical illness), family factors (e.g., separation and divorce; parent mental health problems; overprotective parenting style; dysfunctional family interactions), school factors (e.g., bullying; physical education lessons; transition to secondary school; structure of the school day), and community factors (e.g., increasing pressure to achieve academically;

[1] The studies by Bernstein et al. (1986), Bernstein (1990), and Bernstein (1991) are not considered here because samples included youth with truancy charges.

inconsistent professional advice; inadequate support services). These may operate as predisposing, precipitating, and/or perpetuating factors. Precipitating factors can be difficult to identify, especially in cases where onset is not recent (Baker & Wills, 1978). A common perpetuating factor is the young person's access to items (e.g., television) and experiences (e.g., attention from a parent) associated with being at home rather than school (Heyne et al., 2001).

In the absence of treatment, most school refusers continue to display problematic school attendance and emotional distress (King, Tonge, Heyne et al., 1998). Significant adverse consequences may occur in the short- and long-term. Research suggests that non-attendance negatively affects learning and achievement (Carroll, 2010) and places youth at risk for early school dropout (Christle, Jolivette, & Nelson, 2007). Problems in social adjustment may also occur. Coolidge, Brodie, and Feeney (1964) described moderate or severe limitations (e.g., few or no friends; primarily home-centered activities) in approximately 70 percent of school refusers followed up in adolescence or early adulthood. Berg, Butler, and Hall (1976) reported that one-third of school refusers treated as inpatients three years earlier continued to be socially impaired and had severe symptoms of emotional disturbance. Valles and Oddy (1984) compared successfully and unsuccessfully treated school refusers and found that those who did not return to school displayed more antisocial behavior and a trend towards poorer social adjustment at 7-year follow-up (Valles & Oddy, 1984). Other follow-up studies indicate a risk for ongoing mental health problems in late adolescence (Buitelaar et al., 1994) and adulthood (Berg & Jackson, 1985; Flakierska-Praquin, Lindstrom, & Gillberg, 1997; McCune & Hynes, 2005).

Family members are also affected by school refusal. Parents are likely to experience distress associated with the crisis-like presentation of school refusal or the challenge of resolving the problem (Heyne & Rollings, 2002; Kearney, 2001). In some cases parents will have to take time off work to supervise a 'home-bound' child. Another consequence of school refusal is family conflict (Kearney & Bensaheb, 2006; McAnanly, 1986; Ollendick & King, 1990). Valles and Oddy (1984) observed more conflict in the families of non-successfully treated cases of school refusal relative to successfully treated cases. One possible explanation is that the ineffective efforts of parents trying to get their child to attend school may exacerbate tension and conflict between the parents themselves and between the parents and their child. School refusal also places a strain upon school staff who may be uncertain about how to deal with the problem and may incur stress displaced onto the school by family members (McAnanly, 1986).

The prevalence of school refusal is difficult to ascertain due to the discrepancy in how school refusal is defined and the lack of national reporting. A qualitative analysis (Heyne & King, 2004) and a large community-based study (Egger et al., 2003) suggest that school refusal occurs for about 1–2% of young people. Estimates among clinic-referred youth range between 5 and 16% (Heyne & King, 2004). Hersov (1985) suggested that school refusal is more prevalent among secondary school students. The age distribution observed in studies, which included children and adolescents, supports this contention (e.g., Last & Strauss, 1990; McShane et al., 2001). According to Heyne and Sauter (2013), the higher referral rates

for adolescents might be explained by the complexity of the problem in adolescence and not by a higher prevalence in this period. School refusal is equally common among males and females (Egger et al., 2003; Heyne & King, 2004) and there appears to be no direct association with socioeconomic status (Heyne, King, Tonge, & Cooper, 2001).

In sum, school refusal is not highly prevalent but it can have serious consequences for a young person's development and well-being. It appears that the emotional distress associated with school refusal is commonly in the form of anxiety and less consistently in the form of depression. From a developmental perspective, efficient and effective treatment is imperative.

1.2 THE INTERVENTION AND HOW THE INTERVENTION MIGHT WORK

There is a long history of psychosocial treatments for school refusal. Blagg (1987) provided a detailed review of studies describing the psychodynamic approach, family therapy, and behavioral approaches. The behavioral approaches were based on classical conditioning, operant conditioning, social skills training, or a combination of these. Blagg's review exposed the extent to which clinicians who intended to operate within a specific behavioral paradigm conducted interventions which can be conceptualized within an alternative behavioral paradigm, and sometimes within another paradigm altogether. Similarly, it showed that clinicians work on personal and family factors not specific to the behavioral paradigm(s) they intentionally draw upon. The complexity of specifying and determining the active ingredients of an intervention was well illuminated. Blagg concluded as follows: "In many ways, the search for a definitive behavioural technique or group of techniques to suit all cases is inappropriate. … every case is unique and will require slightly different emphases in treatment" (p. 91). The behavioral interventions commonly discussed in his review were exposure-based interventions, relaxation training, and/or social skills training with the student, and contingency management procedures with the parents and school staff.

Exposure-based interventions stemming from the classical conditioning paradigm (e.g., imaginal and in-vivo systematic desensitization; emotive imagery) are intended to reduce the young person's anxiety associated with school attendance and thereby make it easier for him or her to attend school. As noted by Chu and Harrison (2007), the specific mechanisms through which exposure facilitates approach behavior in anxious youth are still unclear. Occasionally, with younger children, exposure is implemented in the form of flooding whereby parents block the young person's avoidance and enforce attendance at school (e.g., Kennedy, 1965).

Relaxation can help young people manage the stress (negative emotional responses and physiological arousal) that occurs in situations associated with school attendance (e.g., getting ready to go to school; taking tests; giving a class talk; being around other children at school). Procedures include those to relax the body (breathing retraining; progressive muscle relaxation training), the mind (autogenic relaxation training; guided imagery), or the world around them (soothing activities such as listening to music) (Weersing, Gonzalez, Campo, &

Lucas, 2008). During systematic desensitization, relaxation may be employed as an anxiety-inhibitor.

Social skills training addresses social-related difficulties that may be a cause, consequence, or correlate of school refusal, anxiety, or depression (Heyne & Sauter, 2013). Difficulties might be experienced in forming and strengthening friendships, dealing assertively with bullying, and answering peers' or teachers' questions about absence from school. The young person is helped to generate ideas for responding to difficult social situations, evaluate these ideas, and select a preferred response. The clinician models variations on the response and engages the young person in practice, with attention to micro-skills (e.g., body language to show interest in another), macro-skills (e.g., keeping conversations going; assertiveness), and social-cognitive skills (e.g., perspective-taking; gauging the receptiveness of another) as needed.

Contingency management draws on operant conditioning principles. Interventions derived from this paradigm are indicated when school refusal has become positively reinforced (e.g., Meyer, Hagopian, & Paclawskyj, 1999) or negatively reinforced (e.g., Hagopian & Slifer, 1993). Parents are helped to manage the antecedents and consequences of their child's behavior to increase desirable behaviors (e.g., use of coping skills; school attendance; doing homework) and reduce undesirable behaviors that get in the way of school attendance (e.g., tantrums; arguments; excessive reassurance-seeking). Adaptive changes may also occur in the parent–child relationship (Kearney & Silverman, 1995), such as a reduction in parent over-involvement with a child who ordinarily seeks and receives an excessive amount of reassurance. Procedures include positive reinforcement, planned ignoring, reducing the young person's access to items and experiences at home which might otherwise positively reinforce the refusal to attend school, and improved instruction-giving (Heyne & Sauter, 2013). School staff members are also encouraged to employ contingency management befitting the school setting.

The beginning of the cognitive-behavior therapy (CBT) approach to treating school refusal is clearly evidenced in the case reports of Mansdorf and Lukens (1987). Based on the notion that attitudes and beliefs interfere with the effectiveness of behavioral interventions, Mansdorf and Lukens used self-instruction techniques to help two school-refusing children employ coping self-statements to guide positive behavior. A cognitive restructuring process was used with parents to challenge distorted beliefs about their child's problem and the management of school refusal. Since then, various authors have suggested that unhelpful cognition is linked with the development and maintenance of school refusal (e.g., Kennard, Ginsburg, Feeny, Sweeney, & Zagurski, 2005; McNamara, 1988; Okuyama et al., 1999; Place, Hulsmeier, Davis, & Taylor, 2000), and two systematic investigations showed a relationship between school refusal and maladaptive cognition (Heyne et al., 1998; Maric, Heyne, de Heus, van Widenfelt, & Westenberg, 2012).

Today, treatment for school refusal typically incorporates both cognitive and behavioral interventions. Five CBT manuals have been developed (Heyne & Rollings, 2002; Heyne,

Sauter, & van Hout, 2008; Kearney & Albano, 2007[2]; Last, 1993; Tolin et al., 2009). All manuals involve: treatment delivery per case (as opposed to group treatment) and usually in a clinical setting; some level of involvement with parents (from parents as consultants to parents as co-clients); consultation with school staff; and an emphasis on the completion of between-session tasks. Graded exposure to school attendance is commonly advocated. Most of the manuals incorporate psychoeducation, problem-solving training with the young person as a matter of routine or as required, and family work on communication and problem-solving. Cognitive therapy (CT) interventions are employed with all school refusers or as required, and there is variation across manuals in the type of CT interventions employed. Just two of the five manuals explicitly refer to cognitive interventions with parents. The number of sessions and treatment duration vary considerably (e.g., 15 sessions of 90-120 minutes across 3 weeks; 8 sessions of 50 minutes across 4 to 8 weeks; 12 sessions of 45-60 minutes across 12 weeks). The earliest CBT manual (Last, 1993) was standardized, with all cases receiving the same intervention, but the newer manuals advocate individualized treatment based on the main function(s) served by the young person's behavior (e.g., Kearney & Albano, 2007) and/or a broader case formulation including predisposing, precipitating, perpetuating, and protective factors (e.g., Heyne & Rollings, 2002; Heyne et al., 2008; Tolin et al., 2009).

In a trial conducted by Last and colleagues (1998), CBT was compared with an educational-support therapy (ES) to control for the nonspecific effects of CBT. In short, ES comprised educational presentations and supportive psychotherapy. More specifically, it included handouts with questions for the school refuser to consider; a daily diary to record feared situations and associated thoughts, feelings, and responses; encouragement for the young person to talk about their fears; and instruction in identifying maladaptive thinking. There was no instruction or encouragement for the young person to confront feared situations and no instruction about how to modify maladaptive thinking.

Medications have been trialed in studies of tricyclic anti-depressants (Berney et al., 1981; Bernstein et al., 2000; Gittelman-Klein & Klein, 1971), a tricyclic antidepressant or benzodiazepine (Bernstein, Garfinkel, & Borchardt, 1990), or a selective serotonin reuptake inhibitor (Wu et al., 2013), all combined with psychosocial treatment. The effects of inpatient treatment combined with psychosocial interventions have also been reported (e.g., Blagg & Yule, 1984; McShane et al., 2004; Walter et al., 2010). For this review we were interested in examining intervention types that could be implemented by clinical or counseling professionals in clinics, schools, or community facilities thus we focused on psychosocial interventions.

Research examining mediators and moderators of school refusal can contribute to better understanding of the development, maintenance, and treatment of school refusal. To date, just one study has investigated the role of a purported risk factor in mediating the outcome of treatment for school refusal. Maric, Heyne, MacKinnon, van Widenfelt, and Westenberg (2013) found that increases in self-efficacy at posttreatment significantly mediated

[2] This manual was developed for the treatment of 'child-motivated school refusal behavior' which includes both school refusal and truancy.

posttreatment increases in school attendance and decreases in fear about attending school. The finding corroborated Pina and colleagues' (2009) suggestion that improved school attendance would be mediated by increased self-efficacy for handling school situations. According to Maric et al. (2013), cognitive or behavioral interventions may promote a school refuser's self-efficacy for facing the anxiety-provoking situation of attending school, which in turn may lead to increased school attendance. There have been numerous studies of individual or family factors predicting treatment outcome. The most consistent finding to date is that treatment response is inferior for school-refusing adolescents with social anxiety disorder (Bernstein et al., 2001; Heyne et al., 2011; McShane et al., 2004). Problem severity–in the form of school absenteeism or co-occurring psychopathology–is another negative prognostic indicator (Bernstein et al., 2001; Last et al., 1998; Layne et al., 2003; McShane et al., 2004; Walter et al., 2013). Very little attention has been paid to school factors predicting outcome (Heyne et al., in press).

In sum, a broad range of factors may be associated with the development and maintenance of school refusal. Earlier, therapists purported to operate within a specific theoretical paradigm, but the various interventions they employed can be conceptualized according to various paradigms. Currently, treatment for school refusal commonly integrates behavioral and cognitive interventions conducted with the young person, their parents, and school staff. The overarching aim of these interventions is the reduction of the young person's emotional distress and an increase in school attendance to help the young person follow a normal developmental pathway (Heyne & Sauter, 2013). Some time ago Valles and Oddy (1984) raised the question of whether reductions in emotional distress primarily facilitate increased school attendance or vice versa. Contemporary theorizing echoes the uncertainty associated with school refusal and its treatment. When discussing mediation models of treatment outcome, Heyne et al. (in press) suggested that "school attendance and internalizing problems might best be regarded as separate variables in causal chains, sometimes as a mediating variable and sometimes as an outcome variable, depending on the proposed relationship with the other variables under discussion.[3]"

1.3 WHY IT IS IMPORTANT TO DO THE REVIEW

Although there have been systematic reviews and meta-analyses conducted on the effects of interventions for children and adolescents with anxiety disorders (Brendel & Maynard, 2014; Ishikawa, Okajima, Hirofumi, & Sakano, 2007) and prior narrative reviews of school refusal (see Maynard et al., 2013 for a summary of prior reviews), we have not located a meta-analysis of interventions focused specifically on school refusal. Prior reviews on school refusal focused on what is known about school refusal in terms of etiology, prevalence, assessment, and treatment; however, few reviews have focused specifically on intervention outcomes. Reviews that have focused more specifically on outcomes have included studies with little or no criteria of study design or internal validity. Only one prior review has

[3] Book by Heyne et al. (in press) does not have pagination available yet for direct quote.

employed systematic review methodology (Pina et al., 2009), and no reviews have quantitatively synthesized effects using meta-analytic methods.

King and colleagues (King et al., 2005; King, Tonge, Heyne, & Ollendick, 2000) conducted two narrative reviews of school refusal intervention outcome studies. The 2000 review included eight published studies of CBT interventions using a range of research designs. Their report concluded, "At first glance, our review of research suggests empirical support for cognitive-behavioral therapy in the treatment of school refusal…" (p. 501). "However, since very few controlled studies have been reported at this stage in treatment research, it would be premature to extol the clinical virtues of cognitive-behavior therapy" (p. 506). King et al.'s 2005 review focused on a broader topic of anxiety and phobic disorders and included seven studies examining effects of behavioral or CBT interventions with school refusal. Although the authors used substantially the same studies in both reviews, the two reviews came to different conclusions, likely due to the inclusion of an additional controlled study in the latter review. In the 2005 study, the authors concluded, "Overall, school refusal has responded to CBT programs as demonstrated in a number of controlled studies, with general maintenance of gains" (p. 249). A more recent review by Pina and colleagues (2009) included both single-subject and group-design studies examining effects of psychosocial interventions for school refusal. The authors presented effect sizes for the included group design studies, but did not synthesize effects across studies. Conclusions from this review indicate that behavioral strategies alone and behavioral strategies in combination with cognitive strategies are promising interventions to reduce school refusal.

Prior reviews of school refusal intervention research also have been limited to published research and have primarily employed either qualitative or vote-counting methods for synthesizing study outcomes. Moreover, most of the existing reviews of school refusal have tended to focus on CBT interventions. Taken together, the past reviews are important to our understanding of school refusal and provide some evidence to guide interventions. However, they do not systematically or quantitatively address the question of whether and which interventions are effective for increasing school attendance and decreasing anxiety for children exhibiting school refusal.

This systematic review expands and improves upon prior work in several ways. First, this review applied a systematic and transparent process for searching, retrieving, and coding studies, including the search for unpublished studies. Using a systematic method to conduct the review of outcome research limits bias and reduces chance effects, leading to more reliable results (Cooper, 1998). Searching for unpublished studies could produce additional studies that have not been included in prior reviews. Further, explicitly and transparently describing the review process allows for others to replicate and expand the review to include new studies or criteria.

Second, this review sought to include evaluations of a variety of interventions operating in a broad set of geographical contexts, including programs across the United States and other countries. This broader reach allowed for the possibility of identifying studies that may not have been included in prior reviews.

Lastly, prior reviews have largely been limited to a narrative approach, presenting a description of programs or using a vote-counting method to categorize outcomes of programs as significantly positive, significantly negative, or null. The vote-counting method, however, disregards sample size, relies on statistical significance, and does not take into account measures of the strength of the study findings, thus possibly leading to erroneous conclusions (Glass, McGaw, & Smith 1981). Meta-analysis, on the other hand, represents key findings in terms of effect size, rather than statistical significance. Thus, meta-analysis provides information about the strength and importance of a relationship, the magnitude of the effects of the interventions, and the characteristics of effective interventions. Moreover, because there are a relatively small number of studies in this area employing small sample sizes, quantitatively synthesizing effects across studies can more accurately answer questions of effectiveness than the narrative or vote counting methods employed by prior reviews. Given the small sample sizes in several of the included studies, it is possible that individual primary studies could fail to demonstrate a significant effect because they were underpowered. One of the strengths of meta-analysis over other synthesis methods is the capability to find effects that are obscured or not readily apparent when using less sophisticated approaches (Lipsey & Wilson, 2001). By pooling effect size estimates across studies, we can combine the results of underpowered studies, thus producing a synthesized effect estimate with considerably more statistical power to discover meaningful effects that may otherwise be missed in low-powered individual studies (Card, 2012).

2 Objectives

The purpose of this review is to inform practice and policy by evaluating the effectiveness of interventions designed to decrease anxiety and increase school attendance for students who exhibit school refusal. The following research questions guide this study:

3) Do psychosocial interventions targeting school refusal reduce anxiety?

4) Do psychosocial interventions targeting school refusal increase attendance?

3 Methods

The study protocol that guided this review (Maynard et al., 2013) can be accessed at www.campbellcollaboration.org.

3.1 CRITERIA FOR CONSIDERING STUDIES FOR THIS REVIEW

3.1.1 Types of studies

Published or unpublished studies conducted or reported between January 1980 and November 2013 that used an experimental or quasi-experimental design were eligible for this review. Studies must have included a comparison of treatment and control conditions to which students were randomly or nonrandomly assigned. In addition, studies must have used statistical controls or reported baseline data on outcomes regardless of study design.

3.1.2 Types of participants

This review included school-age youth–defined as attending kindergarten through 12th grade (or equivalent in countries with a different grade structure)–who met criteria for school refusal. The operationalization of school refusal varies from one study to the next, but two of the key criteria from Berg and colleagues (Berg, 1997, 2002; Berg et al., 1969; Bools et al., 1990) needed to be present, namely absence from school and emotional distress (e.g., anxiety; withdrawal; somatic complaints). In some studies, emotional distress was operationalized as the presence of at least one anxiety disorder. We excluded studies in which participants dropped out of school.

3.1.3 Types of interventions

This review included all psychosocial intervention types. Because we were interested in psychosocial interventions that could be implemented by school or mental health professionals, we excluded solely pharmacological interventions from this review. We did, however, decide post-hoc to include two studies that assessed effects of medication in combination with a psychosocial intervention (both groups received the psychosocial intervention with the treatment group also receiving medication) and analyzed these studies separately as to whether psychosocial interventions with or without medication are more effective.

3.1.4 Types of outcome measures

Primary outcomes

To be included, a study must have assessed intervention effects on anxiety or school attendance. For the purpose of this review, a broad view of the underlying constructs was adopted, as we are interested in studies that broadly target the outcomes of interest to schools and parents. Although emotional distress, which characterizes the definition of school refusal, can include depression along with anxiety, anxiety is the primary measure of emotional distress used in most studies of school refusal. Child anxiety must have been measured using a standardized instrument. School attendance/absence could be self, parent, or teacher report or from school records.

Secondary outcomes

No secondary outcomes were considered for this review.

3.1.5 Duration of follow-up

It was anticipated that most studies would report outcomes at post-test and thus post-test outcomes were the primary focus of this review; however, if studies reported follow-up data or a subsequent report was published with outcomes examined at a follow-up time point, data from the follow-up time points were recorded.

3.1.6 Types of settings

The review included interventions conducted in any setting that served primary or secondary school students with the exception of studies conducted in residential facilities, as these settings are highly controlled and not typical of regular school or community settings. This review included studies from any geographical context.

3.2 SEARCH METHODS FOR IDENTIFICATION OF STUDIES

3.2.1 Electronic searches

Electronic searches in library databases, internet websites, and research registers were conducted in November 2013 unless otherwise noted. Specific terms and limiters, host platforms, and dates each database was searched can be found in Table 9.1 in the Appendix. The following library electronic sources were searched:

Databases

1) Academic Search Premier
2) Australian Education Index
3) British Education Index
4) CBCA Education
5) Dissertations and Theses

6) Education Source
7) ERIC
8) FRANCIS
9) MEDLINE
10) PsycINFO
11) Social Policy and Practice
12) Social Science Citation Index
13) Social Service Abstracts
14) Social Work Abstracts
15) Sociological Abstracts

Research Registers

1) Cochrane Collaboration Library
2) Database of Abstracts of Reviews of Effectiveness
3) National Technical Information Service
4) System for Information on Grey Literature in Europe (OpenSIGLE)

Search Terms

We used combinations of the following terms and keywords related to the problem, outcomes, intervention and target population (full search terms and limiters for each database searched can be found in Table 9.1 in the Appendix):

1) Problem: (anxiety OR "school refus*" OR "school phobia")

   AND

2) Outcomes: (attendance OR absen*)

   AND

3) Intervention: (evaluation OR intervention OR treatment OR outcome OR program)

   AND

4) Targeted population: (student* OR school* OR child* OR adolescen*)

Internet Searches

We searched for conference proceedings within electronic databases and by searching the websites of the American Psychological Association, American Psychiatric Association, Society for Social Work Research, and the American Education Research Association.

3.2.2 Searching other resources

Searches for relevant studies were also conducted by contacting authors, reviewing reference lists of prior reviews and included studies, and reviewing material related to school refusal. In addition, the first author's database of studies screened in a prior review of indicated truancy interventions was reviewed for relevant studies.

3.3 DATA COLLECTION AND ANALYSIS

3.3.1 Selection of studies

Titles and abstracts of the studies found through the search procedures were screened for relevance by two reviewers for most electronic databases, with the exception of the Australian Education Index, the British Education Index, CBCA Education and Social Policy and Practice. The four databases noted above were searched by a search specialist contracted to conduct searches in those databases, as the review authors did not have access. For the search results in those four databases, one reviewer screened titles and abstracts for relevance. Titles and abstracts that were obviously ineligible or irrelevant were screened out at the title/abstract stage. For example, studies that were deemed inappropriate at the title/abstract review stage were those that did not involve the target population (e.g., they involved college students or adults), did not involve an intervention, or were theoretical in nature. If there was any question as to the appropriateness of the study at this stage by either of the abstract screeners, the full text document was obtained. Documents that were not obviously ineligible or irrelevant based on the title and abstract review were retrieved in full text for final eligibility screening. Two authors independently reviewed all full text reports using a screening form to determine final inclusion. Any discrepancies between the reviewers were discussed and resolved through consensus.

3.3.2 Data extraction and management

Two coders independently coded studies that met eligibility criteria using a data-coding instrument developed by the first author. The coding instrument used for this review was comprised of five sections: 1) source descriptors and study context, 2) sample descriptors, 3) intervention descriptors, 4) research methods and quality descriptors, and 5) effect size data. All study coding was done on a hard copy coding form by both coders. Coders met to review coding and discrepancies were discussed and resolved through consensus. All data was then entered into Excel and effect size data was entered into Comprehensive Meta-Analysis (version 2.0; Borenstein et al., 2005). The full coding form can be found in the published protocol for this review (Maynard et al., 2013).

3.3.3 Assessment of risk of bias in included studies

Two review authors independently assessed the risk of bias in each study using the Cochrane Collaboration's 'Risk of Bias' tool (Higgins et al., 2011). Coders met to review coding agreement and any discrepancies were discussed and resolved by consensus.

3.3.4 Measures of treatment effect

The intervention outcomes of interest for this review were anxiety and attendance. Anxiety was reported as a continuous variable in all studies. For attendance, studies reported mean percentage of school attendance or absence (whole or half days) in the majority of studies. Two studies reporting the number of students who did and did not achieve a certain rate of attendance, which the primary study authors used as a cut point to define successful attendance. Effect sizes were calculated in Comprehensive Meta-Analysis (CMA) version 2.0 (Borenstein et al., 2005). For continuous variables, the standardized mean difference effect size statistic, employing Hedges' *g* to correct for small sample size bias (Hedges, 1981), was calculated. For studies reporting the number of students who achieved a specified rate of attendance, the number of successful cases was entered into CMA and the effect size was calculated and converted to Hedges' *g* in CMA. For anxiety, most authors used one or more standardized measures to measure anxiety. When an author used more than one measure of anxiety, an effect size was calculated for each measure and a mean ES was calculated so each study contributed only one effect size per study. See Table 9.2 in the Appendix for a list of all relevant measures and sources of those measures reported in each included study.

To control for pre-test differences between the intervention and comparison conditions, we used adjusted means (adjusted for pre-test scores on the relevant outcome) and unadjusted standard deviations reported by primary study authors in two studies (Heyne et al., 2002; King et al., 1998). For all other studies that did not report adjusted means, we calculated both the pre-test effect size and the post-test effect size separately in CMA as described above. We then subtracted the pre-test effect size from the post-test effect size and then input the difference between the mean effects in CMA as the effect size for the relevant study. For the variance, we elected to use the variance of the post-test effect size calculated in CMA[4].

3.3.5 Unit of analysis issues

For all included studies, the unit of assignment and the unit of analysis were at the level of the individual student.

3.3.6 Dealing with missing data

In the event of missing data necessary to calculate an effect size, study authors were contacted and missing data was requested. If study authors did not respond or were not able to provide the necessary data, the study would have been excluded from the meta-analysis; however, no studies were excluded due to missing effect size data.

3.3.7 Assessment of heterogeneity

We assessed statistical heterogeneity using the *Q*-test, I^2 statistic, and τ^2. The *Q* statistic is distributed as a chi-square with *k*-1 degrees of freedom (*k* = the number of effect sizes). The

[4] Ideally, the standard error should correct for the pre-post correlation. Unfortunately, the pre-post correlation was not reported in any of the studies, thus we selected the next best reasonable alternative.

Q statistic is calculated by adding the squared deviations of each study's effect size from the mean effect size, weighting their contribution by its inverse variance. A significant *Q* rejects the null hypothesis, indicating that the variability of effect sizes between studies is greater than what would be expected by sampling error alone. The I^2 statistic describes the percentage of total variation across studies due to the heterogeneity rather than chance. τ^2 is an estimate of the between-study variance; a τ^2 value greater than 1 suggests presence of substantial statistical heterogeneity.

3.3.8 Assessment of reporting biases

Publication bias can occur as a result of decisions on the part of authors as well as editors to publish studies that demonstrate a significant effect and to not publish studies when findings may be insignificant, or run counter to the hypothesis or conventional wisdom (Cooper, 1998). Including only published studies in a meta-analysis could likely introduce an upward bias into the effect sizes (Lipsey & Wilson, 2001). Therefore, it is recommended that meta-analysis include both published and unpublished studies to minimize this bias (Cooper, 1998; Lipsey & Wilson, 2001). This review made every attempt to include both published and unpublished reports to minimize the occurrence of publication bias; however, only two unpublished dissertations were included in this review. Due to the small number of studies in this review, conducting analysis of reporting bias was not warranted.

3.3.9 Data synthesis

Four meta-analyses were performed using Comprehensive Meta-Analysis 2.0 (CMA; Borenstein et al., 2005). Two meta-analyses were performed to synthesize studies assessing effects of psychosocial interventions—one for anxiety outcomes and one for attendance outcomes. Another set of meta-analyses was performed for the studies assessing effects of medication in combination with psychotherapy—one for anxiety outcomes and another for attendance outcomes. As the psychosocial-only studies and the combined psychosocial and medication studies were asking different research questions, but still of interest to this review, we analyzed them separately. A weighted mean effect was calculated by weighting each study by the inverse of its variance using random effects statistical models.

3.3.10 Subgroup and sensitivity analysis and investigation of heterogeneity

Several moderator and sensitivity analyses were planned, but due to the small number of studies included in this review and lack of heterogeneity across most sets of studies, we limited additional analyses performed. In the Richardson (1992) study, the study compared two treatment groups—one group received reframing with positive connotation and the other group received systematic desensitization. Since both interventions are a variant of a behavioral or cognitive behavioral intervention, our choice for which to select as the intervention group for the purposes of the analysis was somewhat arbitrary. We thus wanted to examine the effect our choice had on the mean effect. We conducted a sensitivity analysis to examine whether and how the selection of one group over the other as the treatment group impacted the mean effect. We also examined study design (RCT versus QED) and type

of control condition (wait list versus alternative treatment control group) as moderator variables with the psychosocial-only studies.

3.4 DEVIATIONS FROM THE PROTOCOL

3.4.1 Search Methods

There were some minor deviations from the search methods described in the review protocol (see Maynard et al., 2013). First, two databases were changed (Education Complete and Dissertation Abstracts International to Education Source and Proquest Dissertations and Theses) due to availability of databases at the reviewers' institution, and we added a database (Social Policy and Practice) that was not on the protocol. In addition, Web of Science was listed as a separate database; however, this was the host for Social Sciences Citation Index. We also amended the search terms as the originally proposed set and combination of terms produced an extremely large number of false hits in most databases. After further consultation with the librarian and C2 ECG editor, we developed a database search strategy that was more precise. Google and Bing searches were also planned at the protocol stage; however, we decided to forgo the search in Google and Bing upon an initial search in both search engines due to the large number of results retrieved within each search engine (Google = 895,000; Bing = 885,000) which resulted in all false positive results in the first 10 pages within both search engines. Reviewing almost two million titles and abstracts was beyond the resources of this review team. Moreover, given the extensive database and grey literature search conducted for this review outside of Google and Bing, we deemed it unlikely that further internet searches would result in finding additional studies eligible for this review.

3.4.2 Data Collection and Analysis

Deviations to the data collection and analysis plans from the review include 1) adding a risk of bias assessment, 2) methods used to calculate effect sizes, 3) moderator analyses, and 4) assessment of publication bias. First, we did not specify in the protocol that we would assess risk of bias of included studies. Assessing risk of bias of included studies is important, so we chose to use the Cochrane risk of bias tool to assess risk of bias in all included studies. Second, we had specified in our protocol that studies must have reported pre-test data on all outcomes or adjusted for pre-test differences on outcomes, but we did not specify how we would use that in our calculation of effect sizes in the Statistical Procedures and Conventions section of our protocol. We decided to control for possible pre-test differences between groups within studies by using the adjusted means reported in the primary studies, or if this information was not reported, to subtract the pre-test mean effect size from the post-test mean effect size. This was done after consultation with the ECG editor. Third, due to the small number of studies and lack of data reported by primary study authors or lack of variation between studies on certain characteristics (i.e., theoretical basis of intervention), we did not conduct the following moderator analyses as planned: treatment modality, theoretical basis of intervention, grade level, race or socioeconomic status. We also did not conduct formal publication bias analyses due to the small number of studies included, but

rather conducted sensitivity analyses to examine differences between publication status on both outcomes.

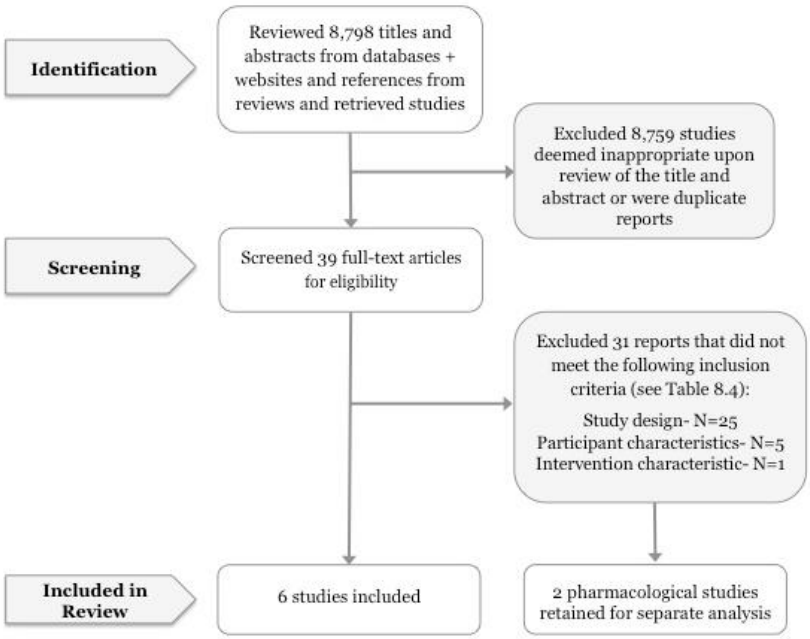
4 Results

4.1 DESCRIPTION OF STUDIES

4.1.1 Results of the search

Electronic searches of bibliographic databases and searches of other sources identified a total of 8,798 citations. Titles and abstracts were screened for relevance, and 8,759 were excluded as duplicates or deemed inappropriate. The full texts of the remaining 39 potential studies were reviewed and screened for eligibility by two independent coders. Six studies passed full-text screening and were included in the primary review. An additional two studies were identified that assessed the comparative effects of psychotherapy with and without medication and met all other criteria. These studies were retained for a separate analysis that was not part of the original protocol. See Figure 4.1 for the flow chart of the study selection process.

FIGURE 4.1: FLOW CHART OF STUDY SELECTION PROCESS



4.1.2 Included studies

Six studies examining effects of psychosocial interventions and two studies assessing comparative effects of psychosocial interventions with and without medication met inclusion criteria for this review. An overview of the characteristics of the included studies is presented in Table 4.1 below, with details of each individual study presented in Table 9.2 in the Appendix.

Six of the included studies were randomized controlled trials and two were quasi-experimental design studies. Two unpublished dissertations were included, with the remaining being published reports in peer-reviewed journals. Two studies were conducted in Australia, two in the United States, one in Canada, one in England, one in China, and one in Kuwait. The majority of the interventions took place in a clinic setting, with one provided in the school (Sahel, 1989), one provided in the school and home settings (Richardson, 1992) and another was unspecified (Blagg & Yule, 1984). With the exception of Sahel (1989), the studies included in this review assessed the effects of a variant of CBT. CBT interventions were conducted with the child alone or with minimal involvement with the parents, while others included the parent and teachers (parent-teacher training; PTT). Interventions were relatively brief, ranging from 4-12 sessions. For those studies that assessed effects of medication, the same CBT intervention was applied across treatment and control groups within each study, with either Fluoxetine (Wu et al., 2013) or imipramine (Bernstein et al., 2000) being tested against no medication or a placebo. Of the six included psychosocial studies, four assessed effects of the treatment group against a group receiving an alternative treatment, one study (King et al., 1998) used a wait list control group and another study (Sahel, 1998) used an unspecified control group. Post-test measurement was taken at the end of treatment or within two to three weeks following treatment for the vast majority of studies. Sahel (1989) measured attendance three months following treatment and Blagg & Yule (1984) measured outcomes one year following treatment. Few authors measured treatment at a follow-up time point. Heyne et al. (2002) measured attendance and anxiety outcomes for the treatment and comparison groups at approximately 4.5 months post-treatment. King et al. (1998) conducted follow-up assessment at approximately 12 weeks post-treatment with only the comparison group as the wait-list control group was offered treatment following post-test.

All eight included studies measured attendance and six studies measured anxiety. As noted in Table 9.2, all studies included in the meta-analyses used standardized measures for anxiety (e.g., *Revised Children's Manifest Anxiety Scale*, *Fear Survey Scheduled for Children*), with all but one study using child-report measures (Bernstein et al. (2000) used a child reported measure and a clinician rating scale to measure anxiety). In terms of measuring attendance, five of the included studies used school records or parent report corroborated by school personnel and three studies did not report the source of attendance data.

Sample sizes of studies included in this review were relatively small, with all studies reporting less than 100 total participants and 37% with samples less than 60 participants. A

total of 435 children meeting school refusal criteria (anxiety/distress related to school attendance/absences) from Australia, the US, Canada, England, Kuwait and China participated in the studies included in this review. The average age of participants was 11.9 (SD = 1.70), with participants in the psychosocial only intervention studies slightly younger in age (M = 11.3, SD = 1.54) than participants in the CBT + medication trials (M = 13.7, SD = 0.35). All studies included eligibility criteria for students to exhibit attendance difficulties, with some having very specific criteria (i.e., at least 20% absence rate) whereas others were less specific (i.e., persistent difficulties in attending school).

TABLE 4.1: CHARACTERISTICS OF INCLUDED STUDIES

Characteristic	RCT/QED N (%)	Characteristic	RCT/QED N (%)
Publication Year (M = 1997, SD = 8.86)		**Country**	
1980-1989	2 (25)	Australia	2 (25)
1990-1999	3 (38)	Canada	1 (13)
2000-2009	2 (25)	China	1 (13)
2010-2014	1 (13)	England	1 (13)
Study Design		Kuwait	1 (13)
RCT	6 (75)	United States	2 (25)
QED	2 (25)	**Treatment (Psychosocial Interventions only)**	
Publication Type		CBT w/ parent training	2 (33)
Journal	6 (75)	Individual CBT	2 (33)
Dissertation or Thesis	2 (25)	Behavioral with child/parent/teacher	1 (17)
Sample Size		Rogerian group therapy	1 (17)
1-29	1 (13)	**Comparison Conditions (Psychosocial Interventions only)**	
30-59	2 (25)	Alternate treatment	4 (67)
60-80	5 (62)	Wait list/nothing	2 (33)
Setting		**Participant Characteristics**	
Clinic	5 (63)	Mean age = 11.9 (SD = 1.7)	
School/home	2 (25)	Sex (≥50% male)	5 (63)
Unknown	1 (13)	Grade level- elementary	1 (12)
		Grade level- mixed grades	7 (88)

Most of the studies assessed effects of a behavioral or CBT intervention. Blagg & Yule (1984) examined effects of *The Behavioral Treatment Approach (BTA)*, a behavioral approach that followed the additive stress model and involved the child, parents, and teacher. This was a brief intervention (mean number of weeks = 2.53) that included the following elements: 1) a detailed clarification of the child's problems; 2) a realistic discussion of child, parental, or teacher worries; 3) contingency plans to ensure maintenance; 4) in vivo flooding; and 5) follow-up (frequent contact with parents and school until child attends school without difficulty for 6 weeks). The study authors did not specify who provided the intervention.

King et al. (1998) and Heyne et al. (2002), both part of the same research group, examined effects of a CBT intervention administered by registered psychologists with minimum of Master's level qualifications. The intervention involved both child therapy and parent/teacher training, although there were some differences in the intervention. Both interventions involved 50 minute individual child therapy sessions, six sessions in the King et al. study and eight sessions in the Heyne et al. study, incorporating relaxation training, social skills training, cognitive therapy, and desensitization. The parent/teacher training involved clinical sessions with the parents (five sessions in King et al. and eight sessions in Heyne et al.) comprised of training in child behavior management skills and cognitive therapy to help parents manage their own anxiety, and consultation with teachers to help facilitate regular school attendance. Wu et al. (2013) reported using the same CBT treatment as described in the Heyne et al. study.

Last et al. (1998) also examined a CBT approach consisting of graduated in vivo exposure and coping self-statement training which occurred with the child once weekly for approximately 60 minutes per week for 12 weeks. The authors reported that "therapists" administered the intervention, but did not provide additional information about the training or type of therapist (e.g., master's or doctoral level or psychologist, social worker, etc.). While the focus of the intervention was the child therapy, parent and school contacts also occurred. Bernstein et al. (2000) reported using an 8 session CBT treatment program adapted from the intervention described in the Last et al. study.

Richardson (1992) and Sahel (1989) each conducted an intervention study as part of their dissertation research. Richardson examined the effects of reframing with positive connotation, a five step intervention consisting of 1) defining the problem; 2) identifying attempted solutions; 3) setting goals in concrete terms; 4) delivering the reframe with positive connotation; and 5) follow up and consolidations. The intervention was delivered by graduate students over four sessions in the family home or school, with a telephone contact between the third and fourth sessions. Sahel examined the effects of the only non-behavioral/non-CBT intervention in this review, a non-directive Rogerian group counseling approach. The intervention consisted of two group counseling sessions per week over seven weeks for a total of 14 sessions. The goal of the first two sessions was to develop relationships using trust games, with the remaining sessions used to "seek to solve the problem of school phobia" and "introduce their experience and feelings about school…to other members of the group" (p. 267). Sahel did not specify who delivered the intervention.

4.1.3 Excluded studies

Thirty studies were excluded during the full-text screening stage of the search and selection process. The majority of studies (24) were excluded due to not meeting criteria for study design (i.e., the article did not report results of an intervention, the study design was not an RCT or QED, or the study did not provide pre-test measures of outcomes of interest, did not match groups, or did not use statistical controls). The remaining studies were excluded due to not meeting criteria related to participant characteristics (i.e., not school refusers; $k = 5$) or intervention criteria ($k = 1$). A list of excluded studies and reasons for exclusion is presented in Table 9.3 in the Appendix.

4.2 RISK OF BIAS IN INCLUDED STUDIES

4.2.1 Selection Bias

Six of the eight included studies reported to be randomized trials; however, none of them provided a clear description of the method used to randomize participants to treatment or control groups or methods of concealing allocation to groups and thus were classified as unclear risk of bias in these areas. The two studies that did not use randomization procedures (Blagg & Yule, 1984; Richardson, 1992) were classified as high risk.

4.2.2 Performance and Detection Bias

Blinding of participants, personnel and outcome assessment was clearly described in only one of the eight studies, which was one of the two medication trials (Bernstein, 2000). No other included studies reported blinding of participants, personnel or outcome assessment.

4.2.3 Attrition Bias

Most studies reported outcomes on participants who completed the treatment. One study (Bernstein et al., 2000) reported including all randomized subjects in analysis based on intent to treat analysis (ITT) but did not report their strategy for missing data. For the remaining studies, overall and differential attrition was assessed to be low in six studies and high in the remaining two studies (Last, 1998; Richardson, 1992).

4.2.4 Reporting Bias

Although no protocols for the included primary studies were located, we classified all eight included studies as low risk of bias with regards to selective outcome reporting as they appear to report expected outcomes and provided adequate data on outcomes of interest to this review.

See Figures 4.2 and 4.3 for a summary of risk of bias within and across included studies.

FIGURE 4.2: RISK OF BIAS ITEMS BY STUDY

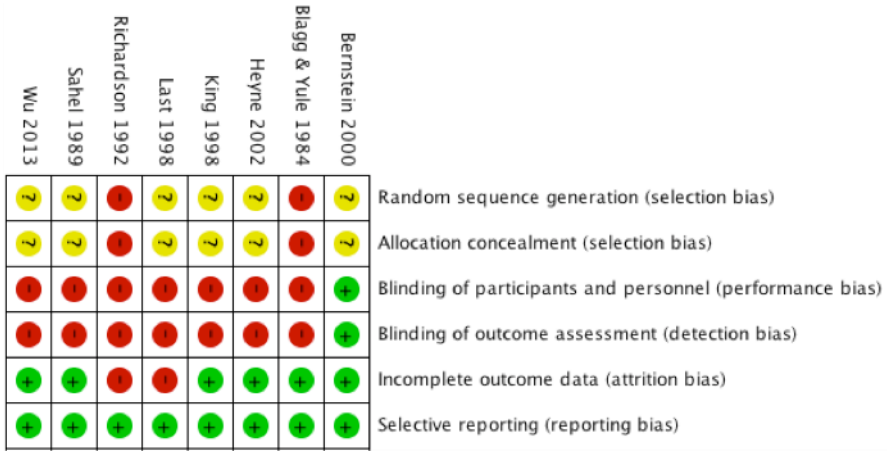
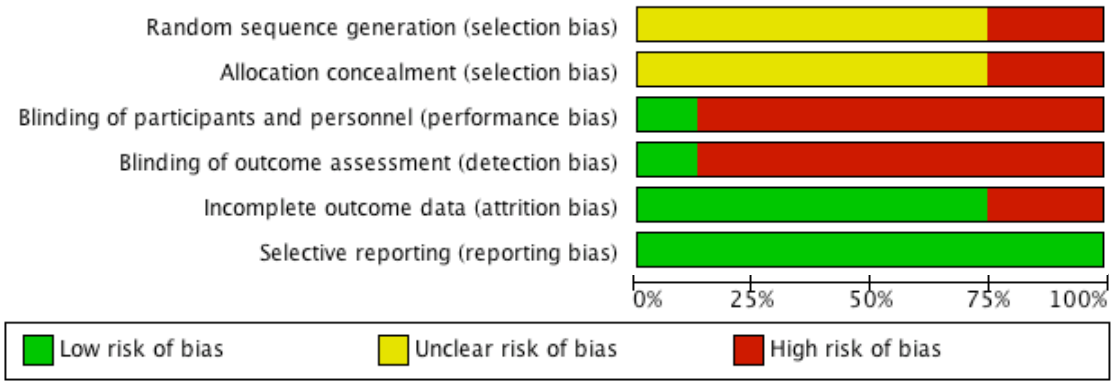


FIGURE 4.3: RISK OF BIAS ACROSS STUDIES

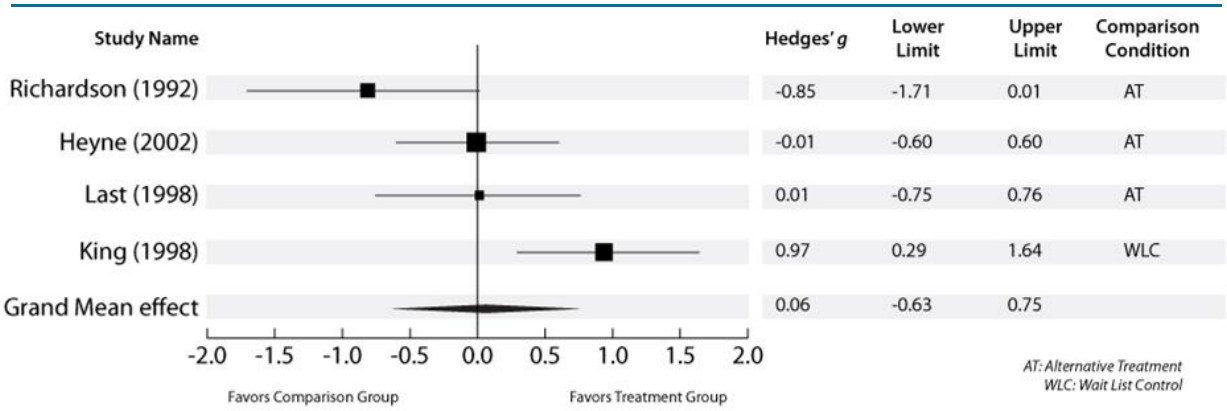


4.3 SYNTHESIS OF RESULTS OF PSYCHOSOCIAL-ONLY INTERVENTIONS

4.3.1 Mean Effects on Anxiety Outcomes

Four of the included psychosocial studies assessed effects on anxiety. Results indicated that the overall mean effect at post-test on anxiety, assuming a random effects model, was 0.06 (95% CI [-0.63, 0.75], p = .86), demonstrating a non-significant effect. The precision of the point estimate should be interpreted with caution, as there was significant heterogeneity between the studies (Q = 11.13, p = .01; I^2 = 73.05; τ^2 = .36). The mean effect size and confidence intervals for each study are shown in the forest plot in Figure 4.4 below.

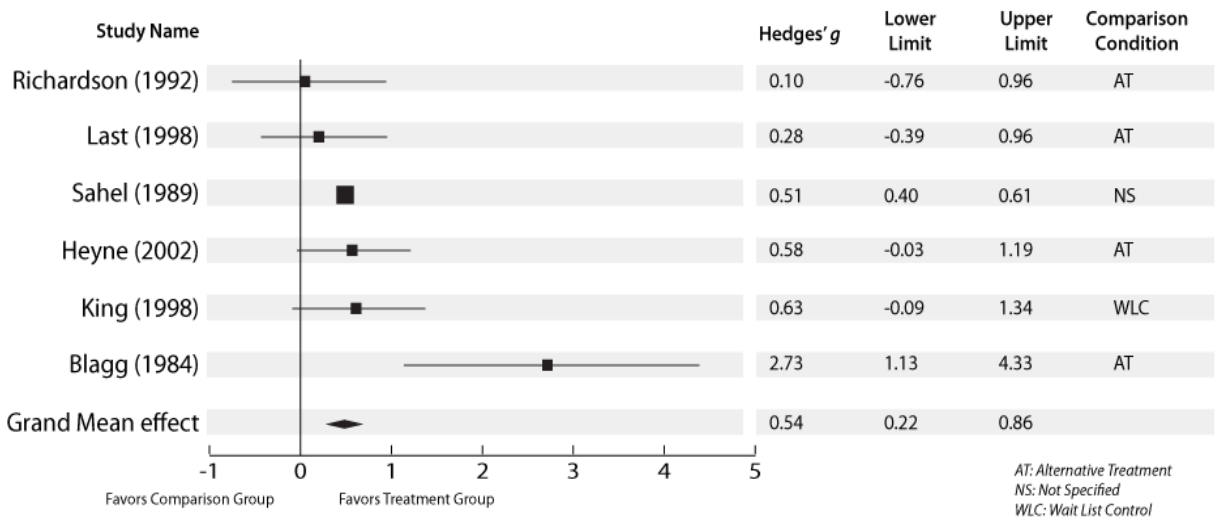
FIGURE 4.4: FOREST PLOT OF MEAN EFFECTS ON ANXIETY



4.3.2 Mean Effects on Attendance Outcomes

The mean effect size at post-test of the six psychosocial studies that assessed effects on attendance, assuming a random effects model, was g = 0.54 (95% CI [0.22, 0.86], p = .00), demonstrating a positive and significant effect. Results of the Q-test were not significant (Q = 8.82, p = .12) and values for I^2 (43.32) and τ^2 (.06) indicate a small amount of heterogeneity. The mean effect size and confidence intervals for each study are shown in the forest plot in Figure 4.5 below.

FIGURE 4.5: FOREST PLOT OF MEAN EFFECTS ON ATTENDANCE

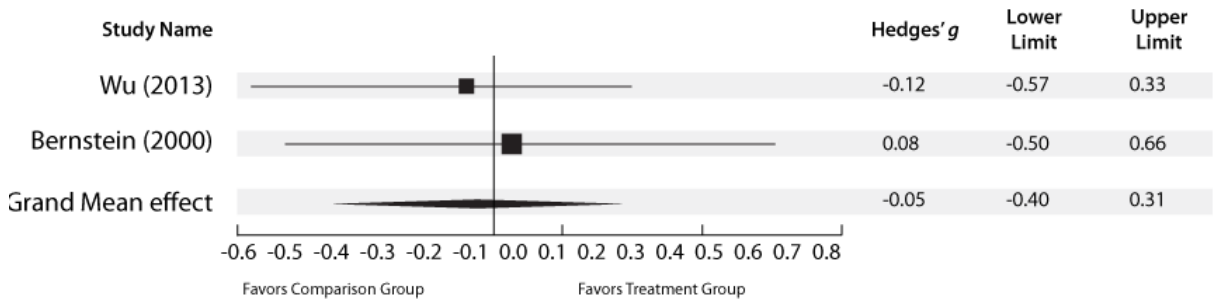


4.4 SYNTHESIS OF RESULTS OF PSYCHOSOCIAL INTERVENTION + MEDICATION

4.4.1 Mean Effects on Anxiety Outcomes

The overall mean effect on anxiety outcomes from the two studies examining effects of CBT with medication versus CBT only, assuming a random effects model, was -0.05 (95% CI [-0.40, 0.31], p = .80), demonstrating negative, but not statistically significant effect on anxiety. Results of the Q-test were not significant (Q = .30, p = .58) and values for I^2 and τ^2 were .00. The mean effect size and confidence intervals for each study are shown in the forest plot in Figure 4.6.

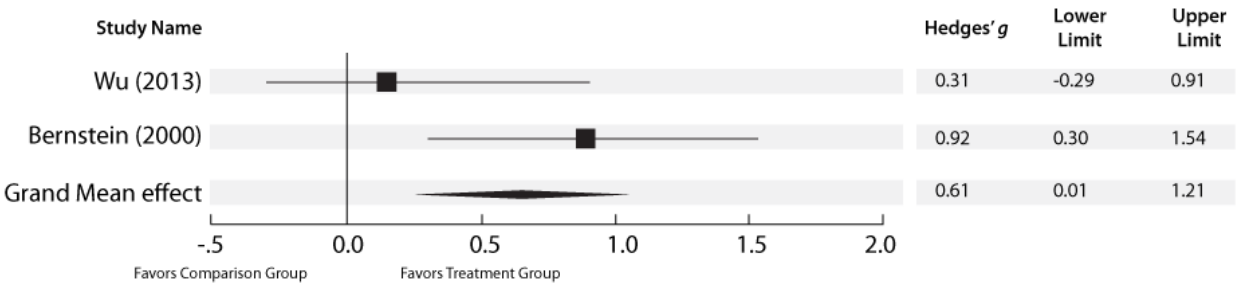
FIGURE 4.6: FOREST PLOT OF MEAN EFFECTS ON ANXIETY



4.4.2 Mean Effects on Attendance Outcomes

The overall mean effect on attendance outcomes of the two studies examining effects of CBT with medication versus CBT only, assuming a random effects model, was 0.61 (95% CI [0.01, 1.21], p = .046), demonstrating a positive and statistically significant effect. Results of the Q-test were not significant (Q = 1.93, p = .17) and values for I^2 (48.23%) and τ^2 (.09) indicate a small amount of heterogeneity. The mean effect size and confidence intervals for each study are shown in the forest plot in Figure 4.7 below.

FIGURE 4.7: FOREST PLOT OF MEAN EFFECTS ON ATTENDANCE



4.5 SENSITIVITY AND MODERATOR ANALYSES

Due to the small number of studies and homogeneity across the medication studies and the psychosocial studies on attendance outcomes, performing a number of sensitivity and moderator analyses was not warranted. However, we did conduct a sensitivity analysis to examine the effects of our selection of the treatment and comparison groups for the

Richardson study on effect size and a sensitivity analysis to assess the impact of the Blagg and Yule (1984) study on attendance. We also conducted sensitivity analysis to compare the effect sizes from published and unpublished studies. In terms of moderator analyses, C2 guidelines require RCTs be separate from QED studies in the meta-analysis or that research design be examined as a moderator; therefore, we examined study design as a moderator variable on attendance and anxiety with the psychosocial intervention studies. Because we found significant heterogeneity between studies on anxiety, we also examined the type of comparison group condition and publication status (i.e., alternative intervention versus a wait list control condition) as a moderator.

4.5.1 Sensitivity Analysis

The Richardson (1992) study compared two interventions–reframing with positive connotation and systematic desensitization. For this review, we selected the reframing with positive connotation group as the intervention group and the systematic desensitization group as the comparison group. However, this decision was somewhat arbitrary and thus we examined whether our choice of treatment and comparison groups had an impact on the mean effect size. Thus, we re-ran the meta-analysis with the systematic desensitization group as the treatment group and reframing positive connotation group as the comparison group. In doing so, the mean effect size was larger for anxiety (Hedges' g = 0.43 [CI -0.09, 0.96] vs. 0.06 [CI -0.63, 0.75]), but remained non-significant. For attendance, the magnitude of the effect size was relatively unchanged and remained significant (g = 0.52, CI [0.18, 0.87] vs. 0.54, CI [.22, .86]).

We also conducted a sensitivity analysis to assess the impact of the Blagg and Yule (1984) study on attendance. The Blagg and Yule study was an outlier, with much larger effects on attendance than the other five studies, thus we ran the meta-analysis with the Blagg and Yule study omitted. With the Blagg and Yule study omitted, the mean effect on attendance was g = .50, CI [.40, .60] compared to g = 0.54, CI [.22, .86] with the Blagg and Yule study included. Thus, while the Blagg and Yule study appears to be an outlier, the impact of including this study is quite small.

Because of the small number of studies included in this review, our ability to conduct publication bias analyses was limited; however, we did conduct sensitivity analyses to examine effect sizes from published and unpublished studies for the psychosocial interventions (both medication studies were published). For attendance, there was no significant difference (Q = .37, p = .54) between published (k = 2, g = .75, CI [.12, 1.39]) and unpublished (k = 4, g = .50, CI [.39, .61] studies. For anxiety, there was only one unpublished study, which also was the only QED and thus publication status is confounded with study design. Results of this analysis indicate significant differences in magnitude of effect between the unpublished study (k = 1, g = -.85, CI [-1.71, .01]) and the published studies (k = 3, g = .32, CI [-.31, .96]).

4.5.2 Moderator Analysis

C2 guidelines require that RCTs be separate from QEDs in the meta-analysis, or that research design be examined as a moderator. We examined study design as a moderator for the psychosocial intervention studies on both attendance and anxiety. When analyzing the QED (k = 2) and RCT (k = 4) studies for attendance, the mean effect of the QED studies (g = 1.32, CI [-1.25, 3.89]) was larger compared to the mean effect of the RCT studies (g = .504, CI [.40, .61]). Results of the moderator analysis (using the analog to the ANOVA framework), however, was not significant (Q_b = .22, p = .64), thus there is no evidence of differences in magnitude of effects between RCT and QED studies on attendance outcomes. In examining study design on anxiety outcomes, however, significant differences were found in the magnitude of the mean effects (Q_b = 5.85, p = .02). The QED study showed a negative effect on anxiety (g = -.85, CI [-1.71, .01]), while the mean effect of the RCT studies was positive (g = .32, CI [-.31, .96]); however, both means were not significant. With the QED study removed, the mean effect was larger (g = 0.32 versus 0.06), yet remained not significantly different from zero. It must be noted, however, that there were very few QED studies (only two QED studies measured attendance and only one measured anxiety), thus caution must be used when interpreting these findings.

Due to the significant heterogeneity found between the psychosocial intervention only studies on anxiety, we also examined whether the type of comparison group condition (nothing/wait list control versus an alternative treatment) could account for the variation between studies. We hypothesized that studies comparing effects of the treatment against a wait list control condition would be significantly larger than those comparing effects against an alternative treatment. We conducted the analog to the ANOVA and found that the effect size of the study using a wait list control comparison condition (0.97, CI [.29, 1.638], King et al., 1998) was statistically significantly greater in magnitude than the other three studies comparing effects of the intervention with an alternative treatment (g = -0.19, CI [-0.61, 0.219]). The Q between (11.13) was statistically significant (p = .01) whereas the Q within each group was not significant, indicating that the type of comparison condition accounted for the variance between studies. However, there was only one study that used a waitlist control comparison group, thus caution should be used when interpreting the results of this moderator analysis.

5 Discussion

5.1 SUMMARY OF MAIN RESULTS

The present review found relatively few rigorous studies of interventions for school refusal. All but one included study assessed effects of a variant of CBT, thus there appears to be a lack of rigorous evidence of other types of interventions for school refusal. Findings of the current review point to different effects on anxiety and attendance. While the psychosocial-only and CBT-plus-medication interventions found, on average, positive and significant effects on attendance compared to control, effects on anxiety at post-test across both sets of studies were not significantly different from zero. Moreover, the magnitude of treatment effects varied across the psychosocial-only studies on anxiety, and thus current estimates of treatment effects on anxiety should be evaluated with caution.

The mixed findings of effects on anxiety and attendance are quite interesting and appear to be somewhat counterintuitive. Anxiety is often viewed as contributing to the development and maintenance of attendance problems exhibited by school refusers, so one would expect that interventions targeting school refusal would decrease anxiety, which would then lead to improved school attendance. The results here, however, demonstrate a more immediate and significant difference in improvement of attendance and no significant difference between treatment and control groups on anxiety outcomes. Although this seems somewhat counterintuitive, the mechanisms of the specific interventions point to a reasonable explanation for these mixed results, particularly at post-test. That is, the increased exposure to school, which is a key component to some of the interventions in this review, may result in more immediate improvement in attendance. However, the increased exposure to school could, at least in the short term, result in an increase in anxiety. A decrease in anxiety may follow from a child's continued attendance at school. In the discussion of Last's (1998) treatment outcome study it was also argued that the emphasis in CBT on increasing school attendance may have heightened anxiety levels. Only Heyne et al. (2002) examined outcomes of both attendance and anxiety at a follow-up time period, so we could not examine longer-term effects of school refusal interventions on anxiety.

There are two main caveats that must be considered when interpreting the findings of this review. First, all of the psychosocial only intervention studies, except for two (King et al., 1998; Sahel, 1989), compared effects of two interventions. Authors examining comparative effects reported improvement across both groups on at least one (Richardson, 1992) or both (Heyne et al., 2002; Last et al., 1998) outcomes of interest to this review. In two of the six psychosocial only intervention studies, the control group was also provided a variant of a

behavioral or CBT intervention (Heyne et al. 2002; Richardson, 1992), which could lead to a downward bias in the overall mean effect. While there is not consistent evidence that CBT is more effective than other types of interventions, CBT does appear to have some evidentiary support as an intervention for school refusal.

Second, there are few rigorous trials available to support any one CBT intervention for school refusal. There was some variability in the CBT interventions examined in this review (e.g., number of sessions; amount of contact with parents) and while some researchers used similar CBT interventions, none of the studies were replication studies assessing the same manualized intervention by independent researchers. Replication studies are an often-cited requirement for classifying specific interventions as empirically supported (Chambless, 1998; Flay, 2005). Although the Heyne et al. (2002), King et al. (1998), and Wu et al. (2013) studies assessed effects of very similar youth CBT plus PTT interventions, there was variation in the number of sessions and duration of treatment, and two of those studies were conducted by the same research group (Heyne et al., 2002; King et al., 1998). The intervention delivered in the Heyne et al. study was comprised of eight child therapy and eight PTT sessions over four weeks whereas the intervention in the King et al. study was comprised of 6 child therapy and five PTT sessions, and Wu offered 12 sessions across 12 weeks. Last et al. (1998) and Bernstein et al. (2000) also implemented similar CBT interventions; however, there was some variation in terms of number of sessions. In sum, CBT may have some evidentiary support, particularly for attendance outcomes; however, it may be premature to classify any specific form of CBT as empirically supported at this time.

5.2 OVERALL COMPLETENESS AND APPLICABILITY OF EVIDENCE

Six studies examining effects of psychosocial interventions on school refusal and two studies examining effects of medication with CBT versus CBT with placebo (Bernstein et al., 2000) or CBT alone (Wu, 2013) were included in this review. All but one study examined a variant of CBT intervention, thus there is an overall lack of evidence of other types of interventions for school refusal. This review was limited to more rigorous study designs, thus there are fewer studies in this review than some prior reviews of school refusal. While having more studies can be beneficial, we believe that having a higher standard of rigor and internal validity for studies included in this review provides more credible evidence of school refusal interventions than prior reviews. Most of the studies did not report information related to socio-economic status, and only half of the studies reported information on race or nationality. Of those four studies that did report race, ethnicity or nationality, participants in all but one were primarily Caucasian, thus there appears to be a lack of studies examining effects of interventions with more diverse participants.

5.3 QUALITY OF THE EVIDENCE

The studies included in this review were comprised of mostly randomized trials, with two quasi-experimental studies. None of the studies provided a description of the method of

random allocation and only one study blinded participants and outcome assessors, thus the studies in this review were assessed as high risk for performance and detection biases. Most of the included studies assessed effects on only the completers, which may result in an overstatement of treatment effects. Many of the studies in this review were older, and thus may explain to some extent the lack of use of ITT analysis, as ITT analysis has become more common in the recent decade. Unfortunately, there were too few studies to conduct sensitivity analyses to provide a systematic evaluation of the impacts of quality indicators or risk of bias.

5.4 LIMITATIONS AND POTENTIAL BIASES IN THE REVIEW PROCESS

The current review is limited to mainly published research, and while efforts were made to search the gray literature, only two unpublished studies were included in this review. Thus, our review may be upwardly biased due to the possible threat of publication and reporting bias, particularly for anxiety outcomes. Our searching of conference abstracts was weighted toward U.S. conferences, and thus we may have missed unpublished conference papers presented in non-U.S. conferences. As described in the search strategy, we decided post-hoc to forgo the searches in Google and Bing, and thus we may have missed some published and unpublished studies. This study is also limited by the small number of studies included in this review and thus low statistical power and limits to the analytic techniques that could be employed.

5.5 AGREEMENTS AND DISAGREEMENTS WITH OTHER STUDIES OR REVIEWS

The current findings are generally consistent with prior narrative reviews in that behavioral and cognitive interventions were found to be the most frequently studied interventions for school refusal. Although several prior reviews describe positive effects of cognitive and/or behavioral interventions for school refusal, many also point to mixed results and the overall lack of evidence for any one treatment modality (Elliot, 1999; King & Bernstein, 2001; King et al., 2000), which is consistent with the results of this review. This review, however, represents a more rigorous extension of existing work in that a more comprehensive and systematic search process was undertaken than in prior reviews and more rigorous inclusion criteria was used to improve the credibility of the review for causal inference. Only one prior review used systematic search procedures (Pina et al., 2009) and none have included unpublished studies. Moreover, prior reviews have used less stringent inclusion criteria and none have employed meta-analytic techniques to quantitatively synthesize results of included studies. This is especially important for this body of studies, as the sample sizes are small. Quantitatively synthesizing results of underpowered studies can produce a pooled effect estimate with considerably more statistical power to discover meaningful effects that can be missed in low-powered individual studies (Card, 2012).

6 Authors' Conclusions

6.1 IMPLICATIONS FOR PRACTICE AND POLICY

The current evidence provides tentative support for CBT in the treatment of school refusal, and for the supplement of medication to CBT, at least for the improvement of attendance. The effects on anxiety are somewhat less clear as we found no differential effects on anxiety outcomes at post-test. However, the current review highlights important caveats to this conclusion. Most of the studies in this review compared effects against other, and sometimes very similar, interventions and the body of studies included in this review also suffer from some significant biases that could result in an upward bias of the effect. Furthermore, the evidence shows only immediate effects of interventions, as there was only one study that reported comparative longer-term effects on both attendance and anxiety, thus there is insufficient evidence to indicate whether or not treatment effects sustain or whether anxiety was indeed reduced with greater exposure to school. While the interventions show promise in the treatment of school refusal, there is an overall lack of sufficient evidence to draw firm conclusions of the efficacy of CBT as the treatment of choice for school refusal.

6.2 IMPLICATIONS FOR RESEARCH

Overall there are few rigorous trials of interventions for school refusal, particularly studies examining effects of interventions other than behavioral interventions or CBT. Moreover, most of the studies that met inclusion criteria for this review were published more than 10 years ago and most were assessed at high risk of bias. The few rigorous studies found for this review, the age of included studies and the risks of bias present in most of the included studies indicate a need for better-controlled studies. Study design and analytic methods have progressed over the past decade, with more rigorous designs being expected and intent-to-treat analysis becoming more common since the time that most studies in this review were conducted. Future research in this area could benefit from more sophisticated and rigorous designs and analytic techniques. Moreover, independent replications of the manualized interventions examined in this review are needed, as are longer-term evaluations of effects of interventions. Assessing long-term effects could provide additional answers and insights as to the mixed findings of the effects of interventions on attendance and anxiety. Future studies should also consider other types of interventions for rigorous evaluation. While the functional treatment of school refusal has been identified as a promising approach (Kearney & Silverman, 1990; 1999), to date, this intervention lacks rigorous evaluation.

To further our theoretical understanding of school refusal and its treatment, studies should include measures of factors held to mediate outcome. Factors currently regarded as important include maladaptive cognition and problem-solving at the individual level, parenting style and family functioning at the family level, and the quality of the student-teacher relationship at the school level (Heyne et al., in press). To understand the temporal precedence of changes in anxiety and changes in school attendance, both should be measured at various points during treatment. Pertinent pre-treatment measures of the severity and chronicity of school refusal would also permit robust predictor and moderator analyses which are especially important given the common notion that the longer a school refuser is away from school, the harder it is for him or her to return. Furthermore, future studies could benefit from larger sample sizes and attention to mitigating potential biases to improve statistical power and causal inference. Larger samples would permit analyses that can account for the heterogeneity so characteristic of school refusal, such as the influence of the presence of different types of anxiety disorders. For example, outcomes may be inferior for school-refusing youth with social anxiety disorder relative to those with alternate anxiety disorders (Blote, Miers, Heyne, & Westenberg, in press).

7 References

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8 Information about This Review

8.1 REVIEW AUTHORS

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8.2 ROLES AND RESPONSIBILITIES

- Content: Maynard, Heyne, Brendel
- Systematic review methods: Maynard, Brendel
- Statistical analysis: Maynard, Pigott
- Information retrieval: Brendel, Bulanda, Maynard

8.3 SOURCES OF SUPPORT

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8.4 DECLARATIONS OF INTEREST

The authors declare no conflicts of interest.

8.5 PLANS FOR UPDATING THE REVIEW

This review will be updated in approximately three to five years by Brandy R. Maynard.

8.6 AUTHOR DECLARATION

Authors' responsibilities

By completing this form, you accept responsibility for maintaining the review in light of new evidence, comments and criticisms, and other developments, and updating the review at least once every five years, or, if requested, transferring responsibility for maintaining the

review to others as agreed with the Coordinating Group. If an update is not submitted according to agreed plans, or if we are unable to contact you for an extended period, the relevant Coordinating Group has the right to propose the update to alternative authors.

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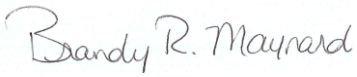
Form completed by:  **Date: 12-17-14**

9 Appendix

9.1 DOCUMENTATION OF SEARCH STRATGIES IN ELECTRONIC DATABASES

Database (host)	Date Searched	Country	Strategy
Academic search premier (EBSCO)	Nov 2013	US	(anxiety OR "school refus*" OR "school phobia") AND (attendance OR absen*) AND (evaluation OR intervention OR treatment OR outcome OR program) AND (student* OR school* OR child* OR adolescen*)
Australian Education Index (EBSCO)	June 2013	Australia	(preschool* OR Lower school* OR primary school* OR kindergarten* OR middle school* OR upper school* OR high school* OR grammar school*) AND (absen* OR refus* OR phobia* OR anxi* OR distress* OR attrition) AND (evaluation* OR intervention* OR psychosocial OR treatment* OR outcome* OR program* OR what works) AND (student* OR child* OR adolescen* OR youth OR young OR teenage*)
British Education Index (EBSCO)	June 2013	UK	(preschool* or Lower school* or primary school* or kindergarten* or middle school* or upper school* or high school* or grammar school*) and (absen* OR refus* OR phobia* OR anxi* or distress* or attrition) AND (evaluation* OR intervention* OR psychosocial OR treatment* OR outcome* OR program* or what works) AND (student* OR child* OR adolescen* or youth or young or teenage*)
CBCA Education (ProQuest)	June 2013	Canada	("Lower school*" or "primary school*" or kindergarten* or "middle school*" or "upper school*" or "high school*" or "grammar school*" or "elementary school*") AND (absen* or refus* or phobia* or anxi* or distress* or attrition) AND (evaluation* or intervention* psychosocial or treatment* or outcome* or program* or what works) AND (student* or child* or adolescen* or youth or young or teen*) Limits applied: Source type: Other Sources, Scholarly Journals; Document type: 9 types searched Article, Conference Paper, Conference Proceeding, Dissertation/Thesis, Instructional Material/Guideline, Market Research, Report, Speech/Lecture, Statistics/Data Report
Dissertation and Theses (ProQuest)	Nov 2013	US	(anxiety OR "school refus*" OR "school phobia") AND (attendance OR absen*) AND (evaluation OR intervention OR treatment OR outcome OR program) AND (student* OR school* OR child* OR adolescen*) [in abstract]
Education Source (EBSCO)	Nov 2013	US	(anxiety OR "school refus*" OR "school phobia") AND (attendance OR absen*) AND (evaluation OR intervention OR treatment OR outcome OR program) AND (student* OR school* OR child* OR adolescen*)

ERIC (EBSCO)	Nov 2013	US	(anxiety OR "school refus*" OR "school phobia") AND (attendance OR absen*) AND (evaluation OR intervention OR treatment OR outcome OR program) AND (student* OR school* OR child* OR adolescen*)
FRANCIS	Feb 2009	France	(attendance OR absen*) AND (evaluation OR intervention OR treatment OR outcome OR program) AND (truancy OR "school refus*" OR absen* OR attendance OR "school phobia" OR "school anxiety" OR dropout OR expulsion OR suspension) AND (student* OR School*) Note: The review team did not have access to FRANCIS at the time of current review, but did review results from search conducted for prior review of indicated truancy interventions
MEDLINE (EBSCO)	Nov 2013	US	(anxiety OR "school refus*" OR "school phobia") AND (attendance OR absen*) AND (evaluation OR intervention OR treatment OR outcome OR program) AND (student* OR school* OR child* OR adolescen*) Limits: humans and children (0-18)
PsychINFO (EBSCO)	Nov 2013	US	(anxiety OR "school refus*" OR "school phobia") AND (attendance OR absen*) AND (evaluation OR intervention OR treatment OR outcome OR program) AND (student* OR school* OR child* OR adolescen*)
Social Science Citation Index (Web of Science)	Nov 2013	US	(anxiety OR "school refus*" OR "school phobia") AND (attendance OR absen*) AND (evaluation OR intervention OR treatment OR outcome OR program) AND (student* OR school* OR child* OR adolescen*) Note: date range 1990-present
Social Services Abstracts (ProQuest)	Nov 2013	US	(anxiety OR "school refus*" OR "school phobia") AND (attendance OR absen*) AND (evaluation OR intervention OR treatment OR outcome OR program) AND (student* OR school* OR child* OR adolescen*)
Social Policy and Practice (Ovid)	Jun 2013	UK	((pre school* or Lower school* or primary school* or kindergarten* or middle school* or upper school* or high school* or grammar school*) and (absen* or refus* or phobia* or anxi* or distress* or attrition) and (evaluation* or intervention* psychosocial or treatment* or outcome* or program* or what works) and (student* or child* or adolescen* or youth or young or teenage*)).mp. [mp=abstract, title, publication type, heading word, accession number] (103) 2 limit 1 to yr="1980 -Current" (102)
Social Work Abstracts (EBSCO)	Nov 2013	US	(anxiety OR "school refus*" OR "school phobia") AND (attendance OR absen*) AND (evaluation OR intervention OR treatment OR outcome OR program) AND (student* OR school* OR child* OR adolescen*)
Sociological Abstracts (ProQuest)	Nov 2013	US	(anxiety OR "school refus*" OR "school phobia") AND (attendance OR absen*) AND (evaluation OR intervention OR treatment OR outcome OR program) AND (student* OR school* OR child* OR adolescen*)

Note: search dates for all searches were limited from 1980 to present unless otherwise noted

9.2 CHARACTERISTICS OF INCLUDED STUDIES:

Bernstein, G. A., Borchardt, C. M., Perwien, A. R., Crosby, R. D., Kushner, M. G., Thuras, P. D., & Last, C. G. (2000). Imipramine plus cognitive-behavioral therapy in the treatment of school refusal. *Journal of the American Academy of Child & Adolescent Psychiatry*, *39*, 276-283. doi: 10.1097/00004583-200003000-00008	Methods	Randomized controlled trial
	Publication type	Peer-reviewed journal article
	Participants	N= 63 (tx=31; comparison=32) Age inclusion criterion: 12-18 years Mean age: 13.9 Gender: 40% male Race: 90.5% White Average pre-test attendance rate: 31% (across both groups) in the previous 4 weeks
	Intervention	Imipramine + 8, 45-50 minute CBT primarily with the adolescent; a parent joined each session for 10-15 minutes
	Comparison Condition	Placebo + 8, 45-50 minute CBT primarily with the adolescent
	Outcomes	Attendance= mean weekly attendance (pre-test- school attendance records corroborated by parents'; post-test- recorded by parents, corroborated by school)- measured mean weekly attendance rates during 8 weeks of treatment Anxiety= R-CMAS (child reported); ARC-R (clinician rated)- measured at end of treatment
	Follow-up	No follow-up reported in this study. A 1-year naturalistic follow-up was reported in Bernstein, Hektner, Borchardt, & McMillan (2001); school attendance data was not collected at follow-up.
	Notes	Effect size calculated by subtracting the pre-test ES from the Post-test ES for both outcomes
Blagg, N. R., & Yule, W. (1984). The behavioural treatment of school	Methods	Quasi-experimental design
	Publication type	Peer-reviewed journal article

refusal—A comparative study. *Behaviour Research & Therapy*, *22*, 119-127. doi: 10.1016/0005-7967(84)90100-1	Participants	N= 50 (tx group = 30; comparison group = 20; also included alternative tx group = 16 not included in meta-analyses) Age inclusion criterion: not specified; most were 11 to 16 years; 5 in the tx group and 0 in the comparison group were younger than 11 Mean age: tx group = 12.95; comparison group = 14.28; alternative tx group = 13.52 Gender: 50.0% male Race: Not reported Average pre-test attendance rate: absent 11-14 weeks (depending on group)
	Intervention	Behavioural Treatment Approach (BTA) involving 1) A detailed clarification of the child's problems; 2) Realistic discussion of child, parental, and teacher worries; 3) Contingency plans to ensure maintenance; 4) In vivo flooding; 5) Follow-up. Actively involves parents, child, and school personnel. Mean total treatment time = 2.53 weeks.
	Comparison Condition	Home tuition and psychotherapy (HT)- Children remained home and received home tuition/home-tutoring and also psychotherapy every two weeks at a child guidance clinic. Mean treatment time = 72.1 weeks.
	Outcomes	Attendance (source not reported)= Pre-test- time off school (# of weeks); post-test- number of students that achieved > 80% attendance at follow-up (1year post-treatment) Anxiety (child reported)= reported at post-test only and thus not included in the analysis. Authors measured whether separation anxiety was present or absent at follow-up (1 year after treatment).
	Follow-up	No follow-up was reported beyond post-treatment functioning which had been measured across a 1 year period (on average) following the end of treatment.
	Notes	Authors included a third group that received hospital in-patient treatment, but is not included in the meta-analysis to maintain data independence and because in-patient interventions were an exclusion criteria. Data to calculate attendance effect size at pretest was reported as time off school (number of weeks) and at post-test (1-year following the end of treatment) as the proportion of students who were attending 0-80% of the time and 81-100% of the time. Effect size calculated for attendance outcomes by subtracting the pre-test ES from the post-test ES.
Heyne, D., King, N. J., Tonge, B., Rollings, S., Young, D., Pritchard, M., et al. (2002). Evaluation of child therapy and caregiver training in the treatment of school refusal. *Journal*	Methods	Randomized controlled trial (low total/differential attrition)
	Publication type	Peer-reviewed journal article
	Participants	N= 41 (tx group = 20; comparison group = 21; also included an alternative tx group = 20 not included in meta-analyses)

of the American Academy of Child and Adolescent Psychiatry, 41, 687–695.		Age inclusion criterion: 7-14 years Mean age: 11.5 years Gender: 54.1% male Race: 91.8 % Australian born Average pre-test attendance rate: 16%-19% (depending on group) in the previous 2 weeks
	Intervention	8, 50 minute individual child CBT sessions + 8, 50 minute parent/teacher training sessions over an approximate 4 week period
	Comparison Condition	8, 50 minute individual child CBT sessions over an approximate 4 week period
	Outcomes	Attendance (school records)= % half days present for the assessment period- post-test measured 2 weeks after end of treatment Anxiety (all child reported)= mean of FT, FSSC-II total score, and RCMAS total score- post-test measured in the 2 weeks after end of treatment
	Follow-up	Follow-up for the treatment group and comparison group, 4.5 months after treatment ended (range: 1.5 months to 6.5 months)
	Notes	Authors included a third group that received parent/teacher training only, but is not included in the meta-analysis to maintain data independence. Adjusted means (adjusted for pretreatment performance for each respective dependent variable) and unadjusted SDs used to calculate ES for attendance outcome. Effect size calculated for anxiety outcome by subtracting the pre-test ES from the Post-test ES.
King, N. J., Tonge, B. J., Heyne, D., Pritchard, M., Rollings, S., Young, D., et al. (1998). Cognitive-behavioral treatment of school-refusing children: A controlled evaluation. *Journal of the American Academy of Child and Adolescent Psychiatry, 37,* 395–403.	Methods	Randomized controlled trial (no attrition)
	Publication type	Peer-reviewed journal article
	Participants	N = 34 (tx = 17; control = 17) Age inclusion criterion: 5-15 years Mean age: 11.03 years Gender: 53% male Race: NR Average pre-test attendance rate: 40%-62% (depending on group) in the previous 2 weeks
	Intervention	6, 50 minute individual child CBT and 5, 50 minute parent/teacher training sessions over 4 weeks
	Comparison condition	Waiting list control group

	Outcomes	Attendance (school attendance record)= % of days present at school for the assessment period- post-test measured 2 weeks after termination of treatment Anxiety (all child reported)= mean of FT, FSSC-II total score, and RCMAS total score- post-test measured 2 weeks after termination of treatment
	Follow-up	Follow-up for the treatment group only, average of 12 weeks after post-treatment assessment
	Notes	Adjusted post-test means (adjusted using pretreatment scores of the dependent variable) and unadjusted SDs used to calculate ES
Last, C. G., Hansen, C., & Franco, N. (1998). Cognitive-behavioral treatment of school phobia. *Journal of the American Academy of Child and Adolescent Psychiatry, 37,* 404–411.	Methods	Randomized controlled trial (with high total/differential attrition)
	Publication type	Peer-reviewed journal article
	Participants	N= 41 (tx = 20; comparison = 21) Age inclusion criterion: children and adolescents Mean age: tx group = 11.67 years; comparison group = 12.40 years Gender: 33% male Race: 90% White Average pre-test attendance rate: 26%-30% (depending on group) in the previous week
	Intervention	Individual CBT- 60 minute sessions once weekly for 12 weeks- comprised of two main components: graduated in-vivo exposure and coping self-statement training. Unspecified amount of contact with parents (mother)
	Comparison Condition	Educational-Support Therapy- 60 minute weekly sessions for 12 weeks- combination of educational presentations and supportive psychotherapy
	Outcomes	Attendance (reported by child and parent, verified by school personnel) = pre-test: % hours child attended school; post-test: # of participants that reached 95% attendance at posttest (converted to Hedges' *g*)- post-test measured at 4 weeks after completion of treatment program Anxiety (all child reported)= mean of FSSC-R (total score) and STAIC-M (total score)- post-test measured at last treatment session
	Follow-up	Follow-up for treatment group and comparison group; conducted 2 weeks into the new school year after treatment completion; assessed severity of school refusal; no data on % school attendance
	Notes	Effect size calculated by subtracting the pre-test ES from the Post-test ES for both outcomes
	Methods	Quasi-experimental design

Richardson, G. (1992). School refusal: Two counselling interventions (Masters thesis, Simon Fraser University, 1992). *Masters Abstracts International, 32*(3), 775.	Publication type	Unpublished dissertation
	Participants	N= 19 (tx = 10, comparison = 9) Age inclusion criterion: Kindergarten to Nine (5-15 years) Mean age: 10.6 Gender: 68% male Race: NR Average pre-test attendance rate: 51%-52% (depending on group) in the previous 19 days
	Intervention	Reframing with Positive Connotation (4 sessions + telephone contact); at least one parent took part in the counseling session
	Comparison Condition	Systematic Desensitization (4 sessions + telephone contact); at least one parent took part in the counseling session
	Outcomes	Attendance (school records)= % of school days attended- post-test measured 15 days post-treatment Anxiety (child reported)= RCMAS- post-test measured at end of treatment
	Follow-up	No follow-up data was reported
	Notes	Effect size calculated by subtracting the pre-test ES from the Post-test ES for both outcomes
Sahel, R. A. (1989). Group counselling/therapy as a technique to modify the undesirable school behaviour (school phobia) of children at elementary school level in the state of Kuwait (Doctoral thesis, University College of North Wales, Bangor, Wales, United Kingdom). Retrieved from http://ethos.bl.uk/OrderDetails.do?uin=uk.bl.ethos.329628	Methods	Randomized controlled trial (low overall and differential attrition)
	Publication type	Doctoral dissertation
	Participants	N= 76 (tx = 37; comparison = 39) Age inclusion criterion: 7-11 years Mean age: tx group = 8.7 years; comparison group = 9.0 years Gender: 39% male Nationality: 76% Kuwaiti Avergae pre-test attendance rate: 74%-75% (depending on group) in the previous 12 weeks
	Intervention	Group counseling using non-directive Rogerian model- 45 minutes twice weekly sessions for 7 weeks (total 14 sessions). Parents were not involved in treatment.
	Comparison Condition	The authors refer to the comparison condition as a “control group”. They did not report that the control group received an alternative intervention.

	Outcomes	Attendance (source not reported)= mean number of days absent per month- post-test measured at three months following end of treatment Anxiety = NR
	Follow-up	No follow-up data was reported.
	Notes	Effect size calculated by subtracting the pre-test ES from the Post-test ES
Wu, X., Liu, F., Cai, H., Huang, L., Li, Y., Mo, Z., & Lin, J. (2013). Cognitive behavior therapy combined fluoxetine treatment superior to cognitive behaviour therapy alone for school refusal. *International Journal of Pharmacology, 9*, 197-203. doi: 10.3923/ijp.2013.197.203	Methods	Randomized controlled trial
	Publication type	Peer-reviewed journal article
	Participants	N=75 (tx=39; comparison=36) Age inclusion criterion: 6-18 years Mean age: 13.44 Gender: 53% male Race: NR Average pre-test attendance rate: absent 19 weeks (same for both groups)
	Intervention	Fluoxetine + 12, 45-50 minute CBT and parent involvement (amount not specified)
	Comparison Condition	Placebo + 12, 45-50 minute CBT and parent involvement (amount not specified)
	Outcomes	Attendance (source not reported)= mean absence from school (pre-test); rate of back to school (post-test)- post-test measured 2 weeks after completion of treatment Anxiety (child reported)- SAS- post-test measured 2 weeks after completion of treatment
	Follow-up	No follow-up reported in this study
	Notes	Effect size calculated by subtracting the pre-test ES from the Post-test ES for both outcomes

Notes: NR = not reported; RCMAS = Revised Children's Manifest Anxiety Scale; FT = Fear Thermometer; FSSC = Fear Survey Scheduled for Children; STAIC-M = Modified State-Trait Anxiety Inventory for Children; ARC-R= Anxiety Rating for Children-Revised; SAS= Self-Rating Anxiety Scale

9.3 EXCLUDED STUDIES

Study	Reason for Exclusion
Aviv (2006)	Study design- No comparison group was used
Baden (1990)	Study design- did not match, use statistical controls or report baseline data on outcome measures
Beidas et al. (2010)	Study design- No comparison group was used (two different CBT models (individual and family) were examined, but treated as one treatment group
Berney et al. (1981)	Study design- Did not provide baseline data on outcomes.
Bernstein et al. (1990)	Intervention- Study examined effects of pharmacotherapy only; pretest scores for attendance were not reported.
Bernstein et al. (2001)	Study design- Follow-up study of Bernstein et al. (2000)- no comparison of treatment and comparison groups (groups combined for analysis)
Bernstein et al. (2005)	Participants- "anxious children" not school refusal specifically
Buitelaar et al. (1994)	Study design- Not an intervention study
Burke (1987)	Study design- Multiple single subject design (n=4) used; all participants received intervention
Chapman (2011)	Participant- Participants did not meet criteria for school refusal (i.e., did not have an anxiety or related clinical symptom)
Chu & Kendall (2004)	Participant and study design- Participants did not meet criteria for school refusal; not a between-group intervention study
Clayton (2012)	Participant- Participants did not meet criteria for school refusal
Doobay (2008)	Study design- Not an intervention study
Gutierrez-Maldonado (2009)	Participant- Participants did not meet criteria for school refusal (attendance was not a criteria for the study and not reported as problematic for participants)
Heyne et al. (2011)	Study design- No comparison group was used
Hughes et al. (2010)	Study design- Not an intervention study
Kearney (2007)	Study design- Not an intervention study
Kearney & Silverman (1990)	Study design- All participants received prescriptive treatment
Kearney & Silverman (1999)	Study design- Study used a multiple single-subject design study (N=8). Authors did present data aggregated by group (i.e., received prescriptive or non-prescriptive tx), but group equivalence was not established and no statistical controls used. The two groups differed > .05 standard deviations on % time out of school and anxiety at pre-test.
King et al. (1999)	Study design- Follow-up report from 1998 study and only included data on the treatment group
King et al. (2001)	Study design- Follow-up of treatment group only (no comparison group)
Lau (2009)	Study design- Not an intervention study

Study	Reason for Exclusion
Layne et al. (2003)	Study design- Not an intervention study; assessed predictors of treatment response using the same sample as Bernstein et al., 2000
Lyon & Cotler (2009)	Study design- Not an intervention study
Maeda et al. (2012)	Study design- Case study design was used
Maric et al. (2013)	Study design- No comparison group was used
McShane et al. (2007)	Study design- No comparison group was used
McCune & Hynes (2005)	Study design- No posttest (this was a 10 year follow-up); retrospective multiple case study design; full sample did not meet criteria for school refusal (comparison group was non-school refusing former psychiatric patients).
Richardson (2013)	Study design- Not an intervention study
Timberlake (1984)	Study design- No comparison group was used
Walter et al. (2013)	Study design- No comparison group was used

9.4 RISK OF BIAS: PSYCHOSOCIAL INTERVENTIONS

Study Name: Blagg & Yule (1984)

Type of Bias	Judgment	Support for Judgment
Selection Bias		
Sequence generation	High	Non-random allocation to groups. Selection not described.
Allocation concealment	High	
Performance Bias		
Blinding of participants and personnel	High	No blinding
Detection Bias		
Blinding of Outcome Assessment	High	No blinding
Attrition Bias		
Incomplete outcome data	Low	No attrition (on outcomes used in meta-analysis)
Reporting Bias		
Selective outcome reporting	Low	Study protocol is not available but the published report appears to report expected outcomes for outcomes of interest to this review.

Study Name: Heyne et al. (2002)

Type of Bias	Judgment	Support for Judgment
Selection Bias		
Sequence generation	Unclear	Authors reported "group assignment occurred by means of randomized block design that accounted for the child's gender and schooling level" p. 689. Authors do not provide details re: the sequence generation or allocation concealment.
Allocation concealment	Unclear	
Performance Bias		
Blinding of participants and personnel	High	No blinding of students, teachers, parents or clinicians were reported- performance (of participants and personnel) could be influenced by lack of blinding.
Detection Bias		
Blinding of Outcome Assessment	High	No blinding of outcome assessment- student, teacher, parent, and clinician reported/assessed outcome measures could be influenced by lack of blinding.
Attrition Bias		
Incomplete outcome data	Low	Attrition was reported: low overall (6%) and differential (3.9%) attrition and reasons for exclusions were reported.
Reporting Bias		
Selective outcome reporting	Low	Study protocol is not available but the published report appears to report expected outcomes. Authors reported outcome data on all reported measures.

Study Name: King et al. (1998)

Type of Bias	Judgment	Support for Judgment
Selection Bias		
Sequence generation	Unclear	Authors reported "children selected for the study were randomly assigned to a 4 week CBT or WLC" and "were randomized to treatment after the completion of pretreatment assessment" (p. 396). Authors do not provide details re: the sequence generation or allocation concealment. While participants were assigned to condition randomly, the assignment to therapists was "a function of convenience in scheduling appointments" p. 397.
Allocation concealment	Unclear	
Performance Bias		
Blinding of participants and personnel	High	No blinding of students, teachers, parents or clinicians were reported- performance (of participants and personnel) could be influenced by lack of blinding.
Detection Bias		
Blinding of Outcome Assessment	High	No blinding- student, teacher, parent, and clinician reported/assessed outcome measures could be influenced by lack of blinding.
Attrition Bias		
Incomplete outcome data	Low	No attrition. Pre- and post-test data reported for all measures on all participants.
Reporting Bias		
Selective outcome reporting	Low	Study protocol is not available but the published report appears to report expected outcomes. Outcomes reported for all measures on all participants.

Study Name: Last et al. (1998)

Type of Bias	Judgment	Support for Judgment
Selection Bias		
Sequence generation	Unclear	Reports "subjects were assigned randomly" to condition, but does not specify the method used to generate the allocation sequence or to conceal the allocation sequence.
Allocation concealment	Unclear	
Performance Bias		
Blinding of participants and personnel	High	No blinding of students, parents or clinicians were reported- performance (of participants and personnel) could be influenced by lack of blinding.
Detection Bias		
Blinding of Outcome Assessment	High	No blinding- student, parent, and clinician reported/assessed outcome measures could be influenced by lack of blinding.
Attrition Bias		
Incomplete outcome data	High	Total (27%) and differential (25%) attrition was high from participants dropping out of study/intervention- outcome data reported only for completers.
Reporting Bias		
Selective outcome reporting	Low	Study protocol is not available but the published report appears to report expected outcomes.

Study Name: Richardson (1992)

Type of Bias	Judgment	Support for Judgment
Selection Bias		
Sequence generation	High	Change in process to assignment during study- first group of participants assigned on an alternating basis and second group were assigned randomly "by drawing a number from a container with 24 slips in it- 8 were labeled RPC and 16 were labeled SD. Author did not provide details re: concealment methods.
Allocation concealment	High	
Performance Bias		
Blinding of participants and personnel	High	No blinding
Detection Bias		
Blinding of Outcome Assessment	High	No blinding
Attrition Bias		
Incomplete outcome data	High	Total attrition was relatively high (24% and 34% depending on outcome) and differential attrition is not known as author did not provide details of the number of participants originally assigned to each condition.
Reporting Bias		
Selective outcome reporting	Low	Study protocol is not available but the published report appears to report expected outcomes.

Study Name: Sahel (1989)

Type of Bias	Judgment	Support for Judgment
Selection Bias		
Sequence generation	Unclear	Reports "subjects were selected randomly to create the experimental and control (A) gorups", but does not specify the method used to generate the allocation sequence or to conceal the allocation sequence.
Allocation concealment	Unclear	
Performance Bias		
Blinding of participants and personnel	High	No blinding
Detection Bias		
Blinding of Outcome Assessment	High	No blinding
Attrition Bias		
Incomplete outcome data	Low	Total (9.6%) and differential (9.3%) attrition were relatively low. Reported results from those who completed the treatment.
Reporting Bias		
Selective outcome reporting	Low	Study protocol is not available but the published report appears to report expected outcomes.

9.5 RISK OF BIAS: PSYCHOSOCIAL INTERVENTIONS + MEDICATION STUDIES

Study Name: Bernstein et al. (2000)

Type of Bias	Judgment	Support for Judgment
Selection Bias		
Sequence generation	Unclear	"subjects were randomly assigned to [condition]…random assignment was blocked on gender and" [length of school vacation during study period]. P. 278 but does not specify the method used to generate the allocation sequence or to conceal the allocation sequence.
Allocation concealment	Unclear	
Performance Bias		
Blinding of participants and personnel	Low	Blinding of participants and "all project personnel with the exception of one child and adolescent psychiatrist were blind to medication assignment". P. 278
Detection Bias		
Blinding of Outcome Assessment	Low	Assessment at endpoint was conducted by "an independent evaluator who had no other contact with the subject or their data. The procedures was designed to minimize bias from rater expectation" p. 278.
Attrition Bias		
Incomplete outcome data	Low	Reported ITT analysis- all participants randomized to condition were included in analyses.
Reporting Bias		
Selective outcome reporting	Low	Study protocol is not available but the published report appears to report expected outcomes.

Study Name: Wu et al. (2013)

Type of Bias	Judgment	Support for Judgment
Selection Bias		
Sequence generation	Unclear	"subjects were randomly divided into two groups " P. 199 but does not specify the method used to generate the allocation sequence or to conceal the allocation sequence.
Allocation concealment	Unclear	
Performance Bias		
Blinding of participants and personnel	High	No blinding reported.
Detection Bias		
Blinding of Outcome Assessment	High	No blinding reported.
Attrition Bias		
Incomplete outcome data	Low	Total (8.5%) attrition relatively low, unable to calculate differential attrition as sample size of tx and control groups at time of assignment were not provided. If assume equal groups, differential attrition (7.1%) moderate.
Reporting Bias		
Selective outcome reporting	Low	Study protocol is not available but the published report appears to report expected outcomes.