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Review

School-based depression and anxiety prevention programs for young people: A systematic review and meta-analysis

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HIGHLIGHTS

- School-based prevention programs have small effects on depression and anxiety.
- Significant prevention effects were detected at 6 and 12 month follow-up.
- Prevention type and personnel delivering the prevention program influenced outcomes.
- For depression, targeted prevention was more effective than universal prevention.
- School-based prevention programs have potential to reduce mental health burden.

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ABSTRACT

Depression and anxiety often emerge for the first time during youth. The school environment provides an ideal context to deliver prevention programs, with potential to offset the trajectory towards disorder. The aim of this review was to provide a comprehensive evaluation of randomised-controlled trials of psychological programs, designed to prevent depression and/or anxiety in children and adolescents delivered in school settings. Medline, PsycINFO and the Cochrane Library were systematically searched for articles published until February 2015. Eighty-one unique studies comprising 31,794 school students met inclusion criteria. Small effect sizes for both depression ($g = 0.23$) and anxiety ($g = 0.20$) prevention programs immediately post-intervention were detected. Small effects were evident after 12-month follow-up for both depression ($g = 0.11$) and anxiety ($g = 0.13$). Overall, the quality of the included studies was poor, and heterogeneity was moderate. Subgroup analyses suggested that universal depression prevention programs had smaller effect sizes at post-test relative to targeted programs. For anxiety, effect sizes were comparable for universal and targeted programs. There was some evidence that externally-delivered interventions were superior to those delivered by school staff for depression, but not anxiety. Meta-regression confirmed that targeted programs predicted larger effect sizes for the prevention of depression. These results suggest that the refinement of school-based prevention programs have the potential to reduce mental health burden and advance public health outcomes.

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1. Introduction

Anxiety and depression are common, debilitating mental health problems that often emerge for the first time during adolescence. Up to 20% of young people will experience a depressive episode or an anxiety disorder by the age of 18 years (Costello, Mustillo, Erkanli, Keeler, and Angold, 2003; Lewinsohn, Rohde, and Seely, 1998; Merry et al., 2011). Both depression and anxiety disorders tend to run a chronic and recurring course, with comorbidity levels of between 10 and 50% (Garber and Weersing, 2010; Kessler, Avenevoli, and Merikangas, 2001; Scholten et al., 2013). This poses a significant public health burden, with depression already the leading cause of disease burden in Australia (Murray et al., 2015).

Earlier onset of depression and anxiety is associated with a worse clinical course over the lifespan, and in youth is associated with drug and alcohol abuse, risky sexual behaviour, suicide risk, poor academic outcomes and physical health problems (Birmaher et al., 1996; Donovan and Spence, 2000; Kessler et al., 2001; Rao et al., 1995). One way in which to address this disease burden is via prevention. Prevention programs can be divided into universal or targeted approaches. Universal prevention is delivered to all individuals within an identified population regardless of risk. For example, universal prevention programs for youth are typically delivered on a large scale in the school environment to every child in the grade (e.g., Neil and Christensen, 2009). Conversely, targeted prevention approaches are directed either towards those who have an increased risk profile for the disorder such as familial risk or poverty (selective prevention), or who have sub-clinical symptoms (indicated prevention; Mrazek and Haggerty, 1994; Muñoz, Cuijpers, Smit, Barrera, and Leykin, 2010).

Prevention programs are associated with a number of advantages. First, prevention, in some cases, will prevent the incidence of a disorder occurring altogether, with research suggesting that it is possible to prevent 22% of new depression cases each year (Cuijpers, van Straten, Smit, Mihalopoulos, and Beekman, 2008). In cases where disorder cannot be

prevented, prevention programs can delay the onset of clinically significant symptoms (Bienvenu and Ginsburg, 2007; Merry et al., 2011), which confers considerable benefit, including reduced disability and service use (Donovan and Spence, 2000; Muñoz et al., 2010). Many young people who have mental health disorders are unable to, or do not access mental health treatment services (Gulliver, Griffiths, and Christensen, 2010; Gulliver, Griffiths, Christensen, and Brewer, 2012). Taking a prevention approach means that this problem may be averted altogether. Second, implementing prevention programs at an early age when behaviour is more amenable to change is likely to produce better outcomes than treatment delivered when rigid patterns of cognition and behaviour have already been established and are engrained (Craske and Zucker, 2001; Gladstone, Beardslee, and O'Connor, 2011). Indeed, the failure to respond to treatment is often a consequence of established patterns of behaviour that are difficult to reverse (Donovan and Spence, 2000).

Utilising the school system as the context for implementation of prevention programs provides a natural and accessible way to reach young people. The school environment is particularly advantageous because it offers unparalleled access to youth (Masia-Warner, Nangle, and Hansen, 2006). Schools are a place of learning and provide tremendous opportunity to provide young people with many of the skills and strategies that may protect against, or delay, the onset of emotional difficulties. School-based programs integrated into the school curriculum can also alleviate many typical barriers to accessing treatment, such as time, location and cost (Barrett and Pahl, 2006).

While there has been an increase in studies examining preventive programs for youth over the past decade, the field has been hampered by a lack of consistent terminology, poor research methods and inadequately powered trials, leading to confusion about whether prevention programs are effective and should be pursued (e.g., Calear and Christensen, 2010; Nehmy and Wade, 2014). On the whole, the evidence suggests that there is a modest but positive effect of prevention programs for depression and anxiety (Merry et al., 2011; Stockings et al., 2016; van Zoonen et al., 2014), particularly those based on Cognitive Behaviour Therapy (CBT; Hetrick, Cox, and Merry, 2015). Larger effects sizes have generally been detected for targeted programs, relative to those delivered universally (Calear and Christensen, 2010; Merry et al., 2011), although most universal trials are likely to have been underpowered (Muñoz et al., 2010). There has also been some indication that potentially larger effect sizes are associated with programs delivered by mental-health professionals, relative to programs delivered by teachers (e.g., Calear and Christensen, 2010; Stallard et al., 2014). Moreover, online prevention programs also require consideration in light of advancing technology and burgeoning interest in the area, as well as the potential for online delivery to reduce burden on teachers and professionals (Calear, Christensen, Mackinnon, Griffiths, and O'Kearney, 2009). Furthermore, variability in the types of control conditions included in previous studies have further complicated the picture, with effect sizes being potentially smaller when an attentional control group serves as the comparison, relative to an inactive comparison, such as a waitlist control (e.g., Calear and Christensen, 2010). Rigorous evaluation of the impact of control group type on effect size is now needed. Finally, there is not yet consensus regarding the optimal temporal window within which to intervene. Given that anxiety typically precedes the onset of depression in youth (Kessler et al., 2005; Merikangas, Nakamura, and Kessler, 2009), earlier prevention might impact anxiety and depressive symptoms differentially.

To date, there have been several systematic reviews and meta-analyses focused on mental health prevention programs in young people. However, most of these reviews do not focus exclusively on the school environment (e.g., Fisak, Richard, and Mann, 2011; Stockings et al., 2016), and there has been a bias towards evaluating depression-prevention studies over anxiety (Horowitz and Garber, 2006; van Zoonen et al., 2014). Most notably, there was a comprehensive Cochrane review of youth-focused depression prevention programs (Merry et al., 2011), which updated an earlier review conducted more than ten years ago (Merry, McDowell, Hetrick, Bir, and Muller, 2004a). The findings of these two reviews reported a modest but genuine prevention effect on symptoms outcomes ($g = 0.26$ in the 2004 review, $g = 0.20$ in the 2011 review). However, together with those reviews listed above, these did not separate school-based from community-delivered programs, which is problematic because the circumstances of young people in schools are likely to be different from those targeted in community settings (e.g., youth offenders, special needs groups). Two previous systematic reviews that have specifically examined school-based prevention programs for anxiety and depression (Calear and Christensen, 2010; Neil and Christensen, 2009), found promising effects (depression: median $d = 0.64$ for children, $d = 0.60$ for adolescents; anxiety: median $d = 0.57$ for children, $d = 0.32$ for adolescents). A comprehensive review is now needed, particularly in light of the advances and substantially increased interest in school-based prevention over recent years.

2. Aims

The aim of the current review was therefore to provide a comprehensive overview and evaluation of randomised-controlled trials of school-based prevention programs for depression and/or anxiety in children and adolescents, and to conduct a meta-analysis of intervention effects. More specifically, the review aims to: (1) identify the overall effect of school-based depression and/or anxiety prevention programs on symptoms of depression and anxiety; (2) identify the time period over which treatment gains are maintained; (3) establish the relative effect of these programs according to (i) control group type (i.e., no intervention (NI) vs. waitlist control (WL) vs. attention control (AC)), (ii) prevention type (i.e., universal vs. targeted), (iii) delivery format (i.e., classroom teacher vs. health professional), (iv) program content (i.e., CBT vs. other), and, (v) age of target sample (i.e., children vs. early adolescence vs. late adolescence); and (4) determine, where possible, fidelity of program delivery, program attendance and completion rates, and the cost-effectiveness of these interventions.

3. Method

3.1. Protocol and registration

In line with Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) recommendations (Moher, Liberati, Tetzlaff, and Altman, 2009), this review was registered with PROSPERO [CRD42015023328].

3.2. Eligibility criteria

3.2.1. Types of participants

Participants were children or adolescents with a mean age between 5 and 19 years. Age was used to categorise participants according to whether they were children (<10 years), early adolescents (10–14 years), or older adolescents (>14 years). Diagnostic status was not used to include or exclude participants, as a majority of school-based studies do not conduct a diagnostic assessment prior to program delivery.

3.2.2. Types of interventions

Included interventions were manualised psychological or psychoeducational programs, including individual, group or computerised interventions such as CBT (including relaxation and progressive muscle relaxation approaches), interpersonal psychotherapy (IPT), mindfulness-based cognitive therapy (MBCT), wellbeing therapy (WBT) and psycho-educational approaches. For multi-component programs, the psychological or educational component was required to constitute >75% of the programs' content. Studies were included if they used a program designed to prevent depressive or anxiety symptoms, and/or

promote wellbeing. Studies evaluating drug and alcohol, physical activity, nutritional or pharmacological interventions were not eligible for inclusion.

Interventions needed to be school-based, which in this context refers to a program that is endorsed by the school and delivered in the classroom during school hours, or before or after school on school premises. The school context could not simply provide the location for private/external programs to be delivered. The program was required to be school-supported with recruitment occurring within and facilitated by the school. For multi-setting studies (e.g., partly at school, partly at a primary health care setting), the school-based component needed to comprise >75% of the overall program. There were no restrictions on whether participants were receiving other forms of therapy or medication.

3.2.3. Types of comparisons

Studies were included in which the effects of the school-based intervention was compared to either a no intervention control group or a school-as-usual control condition (NI), a waitlist control condition (WL), or an attention control condition/alternate educational/psychological condition (e.g., bibliotherapy; AC).

3.2.4. Types of outcomes

Studies were included if they reported symptoms of depression and/or anxiety at both baseline and post-intervention at a minimum.[1] The two primary outcomes were depression and anxiety symptoms. Outcome measures needed to be valid and reliable rating scales suitable for children and adolescents. When more than one continuous measure was described, the primary outcome was used. If the primary outcome was not specified, the data from the measure reported first were extracted. For studies meeting inclusion criteria, means, standard deviations, and sample size of completers at post-intervention and at each follow-up time point thereafter, were extracted. In studies in which outcome data were not reported, AWS contacted the authors of the study to obtain this information.[2]

3.2.5. Length of follow-up

Reporting on follow-up outcomes was not required for inclusion in the current review. However, available follow-up data were extracted and categorised. Each data-point was categorised as being post-intervention, short-term (0–6 months inclusive), medium-term (6–12 months inclusive), or long-term (>12 month) follow-up. Waitlist control groups were required to remain waitlist for the follow-up assessment point for data to be extracted (e.g., waitlist groups could not be delivered the intervention prior to follow-up for follow-up data to be included). The follow-up data categories were based on the time points most frequently reported by authors. When there was more than one follow-up assessment during a particular time-frame in a particular study (e.g., 18 and 24 months), the follow-up period closest to other studies in that category was included.

3.2.6. Types of studies

Studies were eligible for inclusion if they used quantitative randomised controlled trial (RCT) methodology, including cluster RCTs. Studies were included if they were published in English language, peer-reviewed journals.

3.3. Search strategy

The electronic databases PsycINFO, PubMed and the Cochrane Library were systematically searched. The following terms were used as text words and key words: (depress* OR mood OR affect OR anxiety OR anxious) AND (prevent* OR early intervent*) AND (school* OR school-based OR adolescen* OR child* OR youth) AND control. The search was conducted on 12th of February 2015. See Appendix A for search strategy used for PubMED. A separate electronic search (using the same terms and keywords) was also conducted in the peer-reviewed journal *Internet Interventions*, as it is not currently listed in these databases. An additional search in the peer-reviewed journal *Early Intervention in Psychiatry* was carried out, as there is a delay between the online publication of articles in this journal and their inclusion into the databases. Finally, reference lists from studies meeting inclusion criteria, as well as recent reviews in the field, were hand-searched.

3.4. Data extraction process and management

Study characteristics and outcome measures were extracted by AWS, and independently checked by YP. The following data were extracted into a piloted spreadsheet: author, year of publication, program target (depression, anxiety, depression and anxiety), prevention type (universal, targeted), age range, sample size, program name, control group conditions (no intervention, waitlist, active), program format (face-to-face group, individual, computerised), program content (e.g., CBT, IPT), mode of delivery (school staff, external mental health professionals/researchers), and number of sessions. Parental involvement, fidelty and completion information were extracted when reported. Outcome data for depression and anxiety symptoms were extracted for the primary outcome analysis. Studies were distinguished according to whether they targeted depression, anxiety or both, as outlined in the aims and objectives of the study. For studies targeting either depression *or* anxiety, symptom data were only extracted for the target of the program (e.g., depressive symptoms for a depression-prevention program). If a program targeted both depression and anxiety, both outcomes were extracted. In cases where a study targeted either depression or anxiety but included *both* depression and anxiety symptoms as outcomes, symptom data were extracted from both for the purposes of the meta-analysis. This decision was made because it provides a more comprehensive evaluation of both anxiety and depression outcomes, which is warranted given the similarities between internalising disorder symptoms as well as the commonalities between prevention programs for these disorders.

3.5. Risk of bias in individual studies

Quality and risk of bias was assessed using the Cochrane Collaboration 'Risk of Bias' tool (Higgins and Green, 2011). This tool allows possible sources of bias to be assessed. In the current review, included studies were assessed against those criteria deemed to be most relevant to school-based prevention randomised controlled trials. As such, studies were assessed in relation to: a) generation of their condition allocation sequence, b) concealment of this sequence, c) reporting of incomplete outcome data, d) selective reporting of data, and e) protection against contamination.[3] Quality ratings were made independently by AWS and either YP or JN, and disagreements were resolved through discussion and consultation with ALC. Cochrane recommends against using

[1] One study (Stallard et al., 2013) reported 'post-intervention' results at 12-month follow-up. This study was included, but the data were compared to other studies reporting medium term follow-up. Another study (Hunt et al., 2009) also reported 'post-intervention' data only at 24 month follow-up. Again this study was included but compared to other studies reporting long term follow-up.

[2] Several authors did not provide the data requested and so these studies ($n = 4$) could not be included in the meta-analysis. These are noted in Table 1.

[3] Contamination was included as it is relevant to school-based studies and refers to whether the unit of allocation was at the school level or not. When randomisation occurs at the individual or class level, there is potential for risk of contamination across conditions through sharing materials or information, and so a risk of bias is reported. Cluster RCTs (i.e., randomisation at the school level) protect against this source of bias (Craig et al., 2008).

summed scales to make an overall judgement about level of bias because different forms of bias are likely to be more or less relevant depending on the nature of the research (Higgins and Green, 2011). Therefore, risk of bias for each criterion identified above has been reported individually, rather than in aggregated format (see Table 1).

4. Statistical analyses

4.1. Calculation of effect sizes

Comprehensive Meta-Analysis (version 3.0, Biostat Inc.) was used to calculate individual study and pooled effect sizes. For each comparison between a prevention intervention and control group, effect size was calculated using Hedges *g*. This statistic is the standardised mean difference between the two groups at post-treatment, which includes an adjustment to address small sample sizes (Hedges and Olkin, 1985). The 95% confidence interval around effect size is also reported. In cases where studies had multiple comparison conditions, the number of participants in the prevention program group was divided equally over the comparison conditions so that each participant was only represented once in the meta-analysis. One study (Arnarson and Craighead, 2009, 2011) reported dichotomous outcomes only so this data were transformed into Hedges g using Comprehensive Meta-Analysis software. Effect sizes of 0.2, 0.5 and 0.8 refer to small, moderate and large effect sizes respectively (Cohen, 1988). As considerable heterogeneity among studies was expected (as is commonly reported in the field e.g., Merry et al., 2011), a random effects model was used, which assumes that the true effect size varies from one study to the next, and that the studies in the analysis represent a random sample of effect sizes that could have been observed.

4.2. Testing homogeneity

Homogeneity of effect sizes was tested using the I^2 statistic, which indicates heterogeneity in percentages. Zero percent indicates no heterogeneity, while 25%, 50% and 75% indicate low, moderate, and high heterogeneity, respectively.

4.3. Subgroup analyses

A number of subgroup analyses were planned, in which prevention type (targeted vs. universal prevention), personnel delivering the program (classroom teachers/counsellors vs. mental health professional/researcher), control condition type (no intervention vs. waitlist vs. active), age at which the intervention was delivered (childhood vs. early adolescence vs. late adolescence), and content of the programs delivered (CBT vs. other) were examined, using a mixed-effects model.

4.4. Meta-regression

We conducted a multivariate meta-regression with effect size as the dependant variable, using a mixed effects model. This enabled us to simultaneously examine the moderator variables we examined in the subgroup analyses as predictors of outcome.

4.5. Testing for and managing publication bias

The funnel plots of the primary outcome measures (depression, anxiety) were examined to test for publication bias (Egger, Smith, Schneider, and Minder, 1997). In cases where publication bias was indicated, Duval and Tweedie's Trim and Fill procedure (Duval and Tweedie, 2000) was conducted within Comprehensive Meta-Analysis, which yields an adjusted effect size estimate. This procedure corrects for the variance of the effects and provides a best estimate of the unbiased effect size.

5. Results

5.1. Study selection

See Fig. 1 for the PRISMA flowchart illustrating the inclusion of studies. A total of 5917 articles were identified, from which duplicate articles ($n = 486$) were removed. The remaining titles and abstracts ($n = 5431$) were screened by the first author to determine their relevance to this review. These were independently screened by a masters-level research assistant for relevance. Of the abstracts, 5212 were deemed irrelevant according to both raters, and therefore excluded. Two authors (AWS, YP)[4] then independently screened the full text articles of the remaining 219 records, of which 129 were excluded as they did not meet inclusion criteria. Any disagreements were resolved through discussion and consultation with ALC. This resulted in 90 articles being included in the current review.

5.2. Study characteristics

Characteristics of included studies are presented in Table 1. There were 81 unique studies identified in the current review, which included a total of 31,794 participants ($n = 16{,}848$ in prevention program conditions, and $n = 14{,}946$ in control conditions). Sample size of the included studies varied considerably from between 21 participants (Hains and Ellmann, 1994; Hains and Szyjakowski, 1990) to 2512 participants (Araya et al., 2013), with a median of 208 participants. Of the 81 studies, 40 were studies of depression prevention ($n = 15{,}844$), 24 were anxiety prevention studies ($n = 8580$), and 17 were mixed depression and anxiety studies ($n = 7370$).

5.2.1. Prevention type

Overall, more than half of the studies were of a universal program (44 studies), just under one third were of an indicated program (25 studies), nine studies tested a selective prevention program, two studies evaluated a blended indicated/selective program, while the final study involved a blended universal/indicated program. Focusing on the 40 depression studies, 42.5% were universal, 40% indicated, 12.5% selective, 2.5% were blended selective/indicated and 2.5% universal/indicated. Of the 24 anxiety programs, 62.5% were universal, 25% indicated, 8.25% selective, and 4.25% blended selective/indicated. Of the 17 mixed depression/anxiety studies, 71% were universal, 24% were indicated, and 5% selective. The selective interventions defined 'risk' as: a negative attributional style (Arnarson and Craighead, 2009), living in a low-income area (Cardemil, Reivich, and Seligman, 2002; Kindt, Kleinjan, Janssens, and Scholte, 2014), elevated anxiety sensitivity (Balle and Tortella-Feliu, 2010), conduct or behavioural problems (King and Kirschenbaum, 1990), personality risk factors (Castellanos and Conrod, 2006), exposure to community or political violence (Cooley-Strickland, Griffin, Darney, Otte, and Ko, 2011; Tol et al., 2008), or parental divorce (Pedro-Carroll and Cowen, 1985). For the meta-analysis, given the relatively few selective ($n = 9$) and blended trials ($n = 3$), these were combined with indicated trials and collectively labelled 'targeted' trials ($n = 37$; 46%), resulting in a relatively even proportion of universal and targeted programs included within the review.

5.2.2. Participant group

Just over one-fifth (21%; $n = 17$) of the studies identified in the current review delivered the intervention to children with a mean age of less than ten years. A majority (11 of 17) of these programs targeted anxiety only, four were mixed anxiety/depression, and only two studies focused exclusively on depression. Thirty-nine trials (48%) involved

[4] A subset of full-text articles ($n = 89$) were independently screened and selected by AW-S and a doctoral-level research assistant (screening and selection was completed for the other 130 articles by AW-S and YP).

Table 1
School based prevention programs for depression, anxiety and both depression/anxiety.

Study	Country	Prevention type	Age range	N	Program	Control	Program content	Mode of delivery	Number sessions	Outcome measure	Quality ratings				
											a	b	c	d	e
Depression studies															
Araya et al., 2013	Chile	Universal	NR	2512	ITFA	WL	CBT	MHP	11 + 2 booster	BDI-II	?	+	+	+	+
Arnarson and Craighead, 2009; Arnarson and Craighead, 2011	Iceland	Targeted (selective/indicated)	14–15 years	171	–	NI	CBT	School MHP	14	CAS	?	?	−	?	−
Cardemil et al., 2002 (Study 1)	America	Targeted (selective)	NR	49	PRP	NI	CBT	MHP	12	CDI	?	?	?	−	−
Cardemil et al., 2002 (Study 2)	America	Targeted (selective)	NR	103	PRP	NI	CBT	MHP	12	CDI	?	?	?	−	−
Castellanos and Conrod, 2006	UK	Targeted (selective)	13–16 years	423	PM-CBI	NI	CBT	MHP	2	BSI-DEP	?	?	+	−	−
Chaplin et al., 2006	America	Universal	11–14 years	208	PRP	NI	CBT	Grad + Teacher	12	CDI	+	−	?	?	−
Clarke, Hawkins, Murphy, and Sheeber, 1993 (Study 1)	America	Universal	NR	622	Psycho-education	NI	EDU	Teacher	3	CES-D	?	?	?	?	−
Clarke et al., 1993 (Study 2)	America	Universal	NR	380	BI	NI	CBT	Teacher	5	CES-D	?	?	?	?	−
Clarke et al., 1995	America	Targeted (indicated)	14–16 years	150	CWSC	NI	CBT	MHP	5	CES-D	?	?	?	?	−
Gillham et al., 2007	America	Universal	11–14 years	697	PRP	AC + NI	CBT	Teacher + MHP	12	CDI (Anx: RCMAS)	+	?	−	?	−
Gillham et al., 2012	America	Targeted (indicated)	10–15 years	408	PRP-A	NI	CBT + IPT	Teacher + School MHP	10	CDI	+	+	+	?	−
Horowitz et al., 2007	America	Universal	14–15 years	380	CWSC	AC + NI	CBT + IPT	MHP	8	CDI	+	?	+	?	−
Kindt et al., 2014	Holland	Targeted (selective)	11–16 years	1440	OVK	NI	CBT	Teacher	16	CDI	+	+	+	+	−
King and Kirschenbaum, 1990	America	Targeted (selective)	8 years	127	WEI	NI	SS	MHP	24	CDRS-R	?	?	−	−	−
[a]Lamb, Puskar, Sereika, and Corcoran, 1998	America	Targeted (indicated)	14–19 years	46	–	NI	CBT	School MHP	8	RADS	?	?	−	?	−
McCarty, Violette, Duong, Cruz, and McCauley, 2013	America	Targeted (indicated)	11–15 years	120	PTA	AC	CBT	MHP	12	MFQ	+	?	+	?	−
McCarty, Violette, and McCauley, 2010	America	Targeted (indicated)	NR	67	PTA	NI	CBT	MHP	12	MFQ	?	?	−	?	−
Merry, McDowell, Wild, Bir, and Cunliffe, 2004b	New Zealand	Universal	13–15 years	392	RAP-KIWI	AC	CBT + IPT	Teacher	11	BDI-II	+	+	?	?	−
Pattison and Lynd-Stevenson, 2001	Australia	Universal	9–12 years	66	PPP	AC + NI	CBT	MHP	11	CDI (Anx: STAI)	?	?	?	?	−
Pössel, Horn, Groen, and Hautzinger, 2004	Germany	Universal	13–14 years	347	L-T	NI	CBT	MHP	10	CES-D	−	?	+	?	−
Pössel, Seemann, and Hautzinger, 2008; Pössel, Adelson, and Hautzinger, 2011	Germany	Universal	12–13 years	301	L&L	NI	CBT	Grad + MHP	10	SBB-DES	?	+	?	?	−
Pössel, Martin, Garber, and Hautzinger, 2013	America	Universal	14–16 years	518	L&L	AC + NI	CBT	Grad + MHP	10	CDI	?	?	?	?	−
Puskar, Sereika, and Tusaie-Mumford, 2003	America	Targeted (indicated)	14–18 years	89	TKC	NI	CBT	MHP	10 + 1 booster	RADS	+	?	+	?	−
Quayle, Dziurawiec, Roberts, Kane, and Ebsworthy, 2001	Australia	Universal	11–12 years	47	OLP	NI	CBT	MHP	8	CDI	?	?	−	?	−
Raes, Griffith, Van der Gucht, and Williams, 2014	Belgium	Universal	13–20 years	408	MI	NI	MI	MHP	8	DASS-DEP	+	+	+	?	−
Roberts, Kane, Thomson, Bishop, and Hart, 2003; Roberts, Kane, Bishop, Matthews, and Thomson, 2004	Australia	Targeted (indicated)	11–13 years	189	PPP	NI	CBT	MHP	12	CDI (Anx: RCMAS)	?	?	?	?	+
Rivet-Duval, Heriot, and Hunt, 2011	Mauritius	Universal	12–16 years	160	RAP	WL	CBT + IPT	Teacher	11	RADS-2	?	?	−	?	−
Rohde, Stice, Shaw, and Gau, 2014	America	Targeted (indicated)	13–19 years	378	–	AC + NI	CBT	School MHP	6	K-SADS	+	+	+	?	+
Rooney et al., 2006	Australia	Universal	8–9 years	120	PTP	NI	CBT	MHP	8	CDI (Anx: RCMAS)	?	?	−	?	+
Rose, Hawes, and Hunt, 2014	Australia	Universal	9–14 years	210	RAP + PIR	AC + NI	CBT + IPT	Grad	20	RADS-2	?	+	+	?	−
Sheffield et al., 2006	Australia	Targeted (indicated) + Universal	13–15 years	1360	–	NI	CBT	Teacher	8	CDI (Anx: SCAS)	+	+	+	?	+
Spence, Sheffield, and Donovan, 2003, 2005	Australia	Universal	12–14 years	1250	PSFL	NI	CBT	Teacher	8	BDI	?	?	+	?	+
Stallard et al., 2012	England	Targeted (indicated)	12–16 years	1064	RAP-UK	AC + NI	CBT	Grad	9	SMFQ (Anx: SMFQ)	+	+	+	+	−
Stice, Burton, Bearman, and Rohde, 2007	America	Targeted (indicated)	15–22 years	225	BG	AC + WL	CBT	Grad	4	BDI	−	?	?	?	−
Stice, Rohde, Seeley, and Gau, 2008; Stice et al., 2010	America	Targeted (indicated)	14–15 years	341	–	AC + NI	CBT	Grad	6	K-SADS	+	?	+	?	+
Wijnhoven, Creemers, Vermulst, Scholte, and Engels, 2014	Holland	Targeted (indicated)	11–15 years	102	OVK	NI	CBT	MHP	8	CDI	+	+	+	+	+
Woods and Jose, 2011	New Zealand	Targeted (indicated)	NR	56	ACE-Kiwi	NI	CBT	School MHP	8	CDI	?	?	−	?	−
Young, Mufson, and Davies, 2006	America	Targeted (indicated)	11–16 years	41	IPT-AST	AC	IPT	Grad + Researcher	10	CES-D	+	?	+	?	−
Young, Mufson, and Gallop, 2010	America	Targeted (indicated)	13–17 years	57	IPT-AST	NI	IPT	MHP	10	CES-D	+	−	+	+	−
Yu and Seligman, 2002	China	Targeted (indicated)	8–15 years	220	POP	NI	CBT	Teacher	10	CDI	?	?	?	?	−
Anxiety studies															
Anticich, Barrett, Silverman, Lacherez, and Gillies, 2013	Australia	Universal	4–7 years	488	FRIENDS	AC + WL	CBT	Teacher	10	PAS	?	+	+	?	+

(continued on next page)

Table 1 (*continued*)

Study	Country	Prevention type	Age range	N	Program	Control	Program content	Mode of delivery	Number sessions	Outcome measure	Quality ratings				
											a	b	c	d	e
Aune and Stiles, 2009	Norway	Universal	11–15 years	1439	NUPP-SA	NI	CBT	MHP	7	SPAI-C	?	?	?	?	+
Balle and Tortella-Feliu, 2010	Spain	Targeted (selective)	11–17 years	145	FRIENDS	WL	CBT	Grad + MHP	6	SCAS	?	?	−	?	−
Barrett and Turner, 2001	Australia	Universal	10–12 years	489	FRIENDS	AC + NI	CBT	Teacher vs. MHP	10 + 2 booster	SCAS (Dep: CDI)	?	?	−	?	+
[a]Barrett, Lock, and Farrell, 2005	Australia	Universal	9–16 years	692	FRIENDS	NI	CBT	Grad + MHP	10 + 2 booster	SCAS (Dep: CDI)	?	?	−	?	+
Berger, Pat-Horenczyk, and Gelkopf, 2007	Israel	Universal	7–11 years	142	OTT	WL	CBT	Teacher	8	SCARED-GAD	−	?	+	?	−
Bouchard, Gervais, Gagnier, and Loranger, 2013	Canada	Universal	9–12 years	59	DHS	WL	CBT	Grad + MHP	10	MASC	?	?	+	?	−
Cooley-Strickland et al., 2011	America	Targeted (selective/indicated)	8–12 years	93	FRIENDS	WL	CBT	Grad	13	RCMAS	?	?	?	?	−
Dadds, Spence, Holland, Barrett, and Laurens, 1997; Dadds et al., 1999	Australia	Targeted (indicated)	7–14 years	128	CK	NI	CBT	MHP	10 + 2 booster	RCMAS	?	?	?	?	+
Essau, Conradt, Sasagawa, and Ollendick, 2012	Germany	Universal	9–12 years	638	FRIENDS	WL	CBT	Grad	10	SCAS	?	?	?	?	+
Garaigordobil, 2004	Spain	Universal	12–14 years	174	–	NI	SS	Teacher	40	STAI	−	?	?	?	−
Hiebert, Kirby, and Jaknavorian, 1989 (Study 2)	Canada	Universal	13–14 years	113	–	AC	CBT	Teacher	11	STAI	?	?	−	?	−
Hunt et al., 2009	Australia	Targeted (indicated)	11–13 years	259	FRIENDS	NI	CBT	School MHP + Teacher	10 + 2 booster	SCAS (Dep: CDI)	?	?	+	?	+
Keogh, Bond, and Flaxman, 2006	UK	Universal	15–16 years	80	SMI	NI	CBT	MHP	10	TA	?	?	−	?	−
Kiselica, Baker, Thomas, and Reedy, 1994	America	Targeted (indicated)	NR	48	SIT	NI	CBT	MHP	8	STAI	−	?	?	?	−
Lock and Barrett, 2003; Barrett, Farrell, Ollendick, and Dadds, 2006	Australia	Universal	9–16 years	737	FRIENDS	WL	CBT	Grad + MHP	10 + 2 booster	SCAS (Dep: CDI)	?	?	−	?	+
McLoone and Rapee, 2012	Australia	Targeted (indicated)	7–10 years	152	CK	WL	CBT	School MHP	10	SCAS	+	+	+	?	−
Miller, Short, Garland, and Clark, 2010	Canada	Universal	7–12 years	116	TWD	WL	CBT	Teacher	8	MASC	?	+	−	?	+
Miller et al., 2011a	Canada	Universal	7–13 years	533	FRIENDS-CE	WL	CBT	Teacher + School MHP	9	MASC	?	+	−	?	+
Miller et al., 2011b (Study 1)	Canada	Targeted (indicated)	9–12 years	191	FRIENDS	WL	CBT	Teacher + School MHP	9	MASC	?	+	+	?	+
Miller et al., 2011b (Study 2)	Canada	Universal	9–12 years	253	FRIENDS	WL	CBT	Teacher + School MHP	9	MASC	?	+	+	?	+
Mifsud and Rapee, 2005	Australia	Targeted (indicated)	8–11 years	91	Cool Kids	WL	CBT	MHP + School MHP	8	SCAS	?	?	+	?	+
Pedro-Carroll and Cowen, 1985	America	Targeted (selective)	9–12 years	72	CODIP	WL	CBT	School MHP, MHP, Grad	10	STAI	?	?	?	?	−
Stallard et al., 2014	England	Universal	9–10 years	1448	FRIENDS	AC + NI	CBT	Teacher vs. MHP	9	RCADS	+	+	+	−	+
Depression & anxiety studies															
Calear et al., 2009	Australia	Universal	12–17 years	1477	MG	WL	iCBT	Computer (teacher supported)	5	Dep: CES-D Anx: RCMAS	+	+	+	?	+
Gillham, Hamilton, Freres, Patton, and Gallop, 2006	America	Universal	NR	44	PRP	NI	CBT	MHP	8	Dep: CDI Anx: RCMAS	?	?	+	?	−
Hains, 1992	America	Universal	15–16 years	25	SIT	WL	CBT	Grad + MHP	9	Dep: RADS Anx: STAI	?	?	?	?	−
[a]Hains and Ellmann, 1994	America	Universal	NR	21	SIT	WL	CBT	Grad + MHP	13	Dep: RADS Anx: STAI	?	?	−	−	−
Hains and Szyjakowski, 1990	America	Universal	16–17 years	21	SIT	WL	CBT & AM	MHP	9	Dep: BDI Anx: STAI	?	?	?	?	−
Johnstone et al., 2014; Rooney, Hassan, Kane, Roberts, and Nesa, 2013a; Rooney et al., 2013b	Australia	Universal	9–10 years	910	AOP-PTS	NI	CBT	Teacher	10	Dep: CDI Anx: SCAS	?	?	−	?	−
Jordans et al., 2010	Nepal	Targeted (indicated)	11–14 years	325	–	WL	CBT + CEET	MHP	15	Dep: DSRS Anx: SCARED	+	+	+	?	+
Kraag, Van Breukelen, Kok, and Hosman, 2009	Netherlands	Universal	NR	1467	LYLF	NI	CBT	Teachers	8 + 5 booster	Dep: SDIC Anx: STAI	?	?	+	?	+
Lowry-Webster, Barrett, and Dadds, 2001; Lowry-Webster, Barrett, and Lock, 2003	Australia	Universal	10–13 years	594	FRIENDS	NI	CBT	Teacher	10 + 2 booster	Dep: CDI Anx: SCAS	?	?	−	?	+
Manassis et al., 2010	Canada	Targeted (indicated)	8–12 years	148	FC	AC	CBT	Grad + MHP	12	Dep: CDI Anx: MASD	+	+	−	?	−
Nobel, Manassis, and Wilansky-Traynor, 2012	Canada	Targeted (indicated)	8–11 years	78	FC	AC	CBT	MHP	12	Dep: CDI Anx: MASC	?	?	?	?	−
Roberts et al., 2010	Australia	Universal	11–13 years	496	AOP	NI	CBT	Teacher	20	Dep: CDI Anx: RCMAS	?	?	−	?	+
Ruini, Belaise, Brombin, Caffo, and Fava, 2006	Italy	Universal	NR	111	–	AC	CBT	MHP	4	Dep: SQ (DEP) Anx: SQ (Anx)	?	?	?	?	−
Ruini et al., 2009	Italy	Universal	NR	227	WBT	AC	WBT	MHP	6	Dep: SQ (DEP), Anx: SQ (Anx)	?	?	?	?	−
Siu, 2007	China	Targeted (indicated)	8–10 years	47	FRIENDS	WL	CBT	MHP	8	Dep: RCDS Anx: SCARED	?	?	−	?	−
[a]Tol et al., 2008	Indonesia	Targeted (selective)	NR	403	–	NI	CBT	MHP	15	Dep: DSRS Anx: SCARED	+	?	+	?	+
Wong et al., 2014	Australia	Universal	14–16 years	976	TWU-S	NI	iCBT	Computer (teacher supported)	6	Dep: PHQ-9 Anx: GAD-7	?	?	−	?	+

early adolescents, with the participant mean age between ten and 14 years. There was more variability in the mental health target among this age group, with 11 studies focusing on anxiety, seven mixed anxiety/depression, and 21 depression-only trials. The remaining 25 studies (31%) were delivered to individuals in the later adolescent years, with a mean age of between 14 and 19 years. Most programs directed towards older adolescents included a depression outcome measure, 16 exclusively, six with anxiety outcomes as well, and only three were exclusively anxiety focused. Relatively few studies (16%) screened participants prior to study entry with a diagnostic interview, and excluded participants based on the presence of a clinical disorder.

5.2.3. Randomisation

Studies varied in terms of whether randomisation occurred at the school (33% studies overall), grade (1%), class (16%) or individual level (49%). One study randomised condition by county (1%).

5.2.4. Program content

CBT comprised the basis of 84% of the programs identified in the current review. Other therapeutic approaches included five combined CBT/IPT programs (6%), two pure IPT programs (2.5%), two social skills programs (2.5%), one blended CBT and creative-expressive experiential therapy, one mindfulness-based cognitive therapy program, one wellbeing therapy program, and one psychoeducational program (each making up 1.2%).

5.2.5. Program format and mode of delivery

More than half ($n = 51$, 63%) of the included programs were delivered by personnel external to the school environment, while 35% ($n = 28$) were delivered by school staff (including two that were delivered by computer, but delivery was supported by school staff). Two studies involved both school and external staff, and two studies compared a school-led version of the program to a health-professional delivered version (Barrett and Turner, 2001; Stallard et al., 2014). Of the programs delivered by personnel external to the school, 35 (43% overall) were delivered by mental health professionals/researchers, six (7%) by graduate students, and ten (12%) by a combination of the two. For programs delivered by school staff, five (6% overall) of these involved both teachers and school health staff (counsellors or nurses), 18 (22%) were delivered exclusively by classroom teachers, and five (6%) were delivered exclusively by school health staff. Interventions in all but four studies (95%) were delivered in a group format, most frequently in small groups of 6–10 individuals. Two studies used a combination of group and individual sessions, while the two remaining programs were delivered individually online, although one of these involved a group discussion of the material following each module (Wong, Kady, Mewton, Sunderland, and Andrews, 2014).

5.2.6. Program sessions

The length of the programs identified ranged from two to 40 sessions, with most programs (70%) being delivered in between 8 and 12 sessions (median = 10 sessions), usually on a weekly or fortnightly basis. Some programs delivered the intervention in two to seven sessions (16%), while others were administered in 13–24 sessions (13%). One study delivered the intervention weekly over the academic year (approximately 40 weeks). There was substantial variability in the duration over which these programs were delivered, ranging from between 3 and 40 weeks (median = 10 weeks). A majority of sessions ran for between 45 and 60 min (53%; $n = 43$; median = 60 min), although some went for longer (between 75 and 120 min; 30%; $n = 23$). The remaining studies ($n = 15$) did not report session length. Only a small proportion (11%) of the programs involved the delivery of booster sessions, with seven studies offering two booster sessions, one program providing up to five booster sessions, and one study offering a single booster session.

5.2.7. Control groups

Forty of the included studies compared the prevention program to a NI control group (49%), 21 used a WL (26%), eight studies involved an AC only (10%), and 12 studies (15%) involved more than one comparison condition. Of these twelve studies involving more than one comparison, ten included an NI as well as one AC arm, and two involved a WL arm as well as one AC condition.

5.2.8. Outcome measures

Of the studies that evaluated depression symptoms as a primary outcome, the most frequent symptom measures included the Children's Depression Inventory (CDI; 38%; Kovacs, 1992), followed by the Centre for Epidemiological Studies – Depression Scale (CES-D; 11%; Radloff, 1977), and the Reynolds Adolescent Depression Scale (RADS; 11%; Reynolds, 2010). Anxiety symptoms were measured most frequently with the Spence Children's Anxiety Scale (SCAS; 24%; Spence, 1998), followed by the Multidimensional Anxiety Scale for Children (MASC; 21%; March, Parker, Sullivan, Stallings, and Conners, 1997), and the Revised Children's Manifest Anxiety Scale (RCMAS; 12%; Reynolds and Richmond, 1978).

5.2.9. Parental involvement

Most programs (58%) did not involve parents in any way. In studies that did request parental involvement, parents were asked to complete

Notes to Table 1

Programs – ITFA = I Think Feel Act, PRP = Penn Resiliency Program, PM-CBI = Personality Matched Cognitive Behavioural Intervention, BI = Behavioural Intervention, CWSC = Coping with Stress Course, PRP-A = Penn Resiliency Program for Adolescents, OVK = Op Volle Kracht, WEI = Wisconsin Early Intervention, PTA = Positive Thoughts and Actions, RAP-KIWI = Resourceful Adolescent Program Kiwi adaptation, PPP = Penn Prevention Program, L-T = LISA-T, L&L = LARS&LISA, OLP = Optimism & Lifeskills Program, TKC = Teaching Kids to Cope, MI = Mindfulness, RAP = Resourceful Adolescent Program, PTP = Positive Thinking Program, PIR = Peer Interpersonal Relatedness, PSFL = Problem Solving for Life, RAP-UK = RAP UK adaptation, BG = Blues Group, ACE-Kiwi = Adolescents Coping with Emotions – Kiwi adaptation, IPT-AST = Interpersonal Psychotherapy – Adolescent Skills Training, POP = Penn Optimism Program, FRIENDS = Friends Program, NUPP-SA = Norwegian Universal Preventative Program for Social Anxiety, OTT = Overcoming the Threat of Terrorism, DHS = Dominique's Handy Tricks, CK = Cool Kids, SMI = Stress Management Intervention, SIT = Stress Inoculation Training, TWD = Taming Worry Dragons, FRIENDS-CE = Friends Program Culturally Enriched Version, CODIP = Children of Divorce Intervention Program, MG = MoodGYM, AOP: PTS = Aussie Optimism Program: Positive Thinking Skills Program, LYLF = Learn Young Learn Fair, FC = Feelings Club, AOP = Aussie Optimism Program, WBT = Wellbeing Therapy, TWU-S = Thiswayup Schools: Combating Depression and Overcoming Anxiety. **Control group** – WL = wait-list control, NI = no intervention control, AC = active control. **Program content** – CBT = Cognitive Behaviour Therapy, IPT = Interpersonal Psychotherapy, SS = Social Skills, iCBT = Internet-based Cognitive Behaviour Therapy, AM = Anxiety Management, CEET = Creative expressive experiential therapy, MI = Mindfulness. **Depression outcome measures** – BDI-II = Beck Depression Inventory, CAS = Child Assessment Scale, CDI = Children's Depression Inventory, BSI-DEP = Brief Symptom Inventory - Depression Subscale, CES-D = Centre for Epidemiological Studies – Depression Scale, CDRS-R – Children's Depression Rating Scale – Revised, RADS = Reynolds Adolescent Depression Scale, MFQ = Mood & Feelings Questionnaire, SBB-DES = Self-Report Questionnaire – Depression, DASS-DEP = Depression Anxiety Stress Scale – Depression subscale, K-SADS = Kiddie-Schedule for Affective Disorders and Schizophrenia, SMFQ = Short Mood & Feelings Questionnaire, SQ (DEP) = Kellner's Symptom Questionnaire – Depression, RCDS – Reynolds Child Depression Scale, DSRS = Depression Self-Rating Scale, PHQ-9 = Patient Health Questionnaire-9. **Anxiety outcome measures** – RCMAS = Revised Children's Manifest Anxiety Scale, STAI = State-Trait Anxiety Inventory, SCAS = Spence Children's Anxiety Scale, SMFQ = Short Mood & Feelings Questionnaire, PAS = Preschool Anxiety Scale, SPAI-C = Social Phobia and Anxiety Inventory for Children, SCARED-GAD = Screen for Child Anxiety Related Disorders – Generalised Anxiety Disorder, MASC = Multidimensional Anxiety Scale for Children, TA = Test Anxiety Scale, RCADS - Revised Children's Anxiety & Depression Scale, SCARED = Screen for Child Anxiety Related Disorders, GAD-7 = Generalised Anxiety Disorder – 7. **Program leader** – MHP = Mental health professional, School MHP = School mental health professionals (includes school counsellors, psychologists, nurses), Grad = Graduate student/intern. **Quality ratings** – a = allocation sequence adequately generated, b = allocation adequately concealed, c = incompletely data adequately addressed, d = no evidence of selective reporting, e = adequate protection against contamination. + = low risk (included information protecting against bias), − = high risk (did not protect against source of bias), ? = unclear risk of bias.

[a] Data not included in meta-analysis.

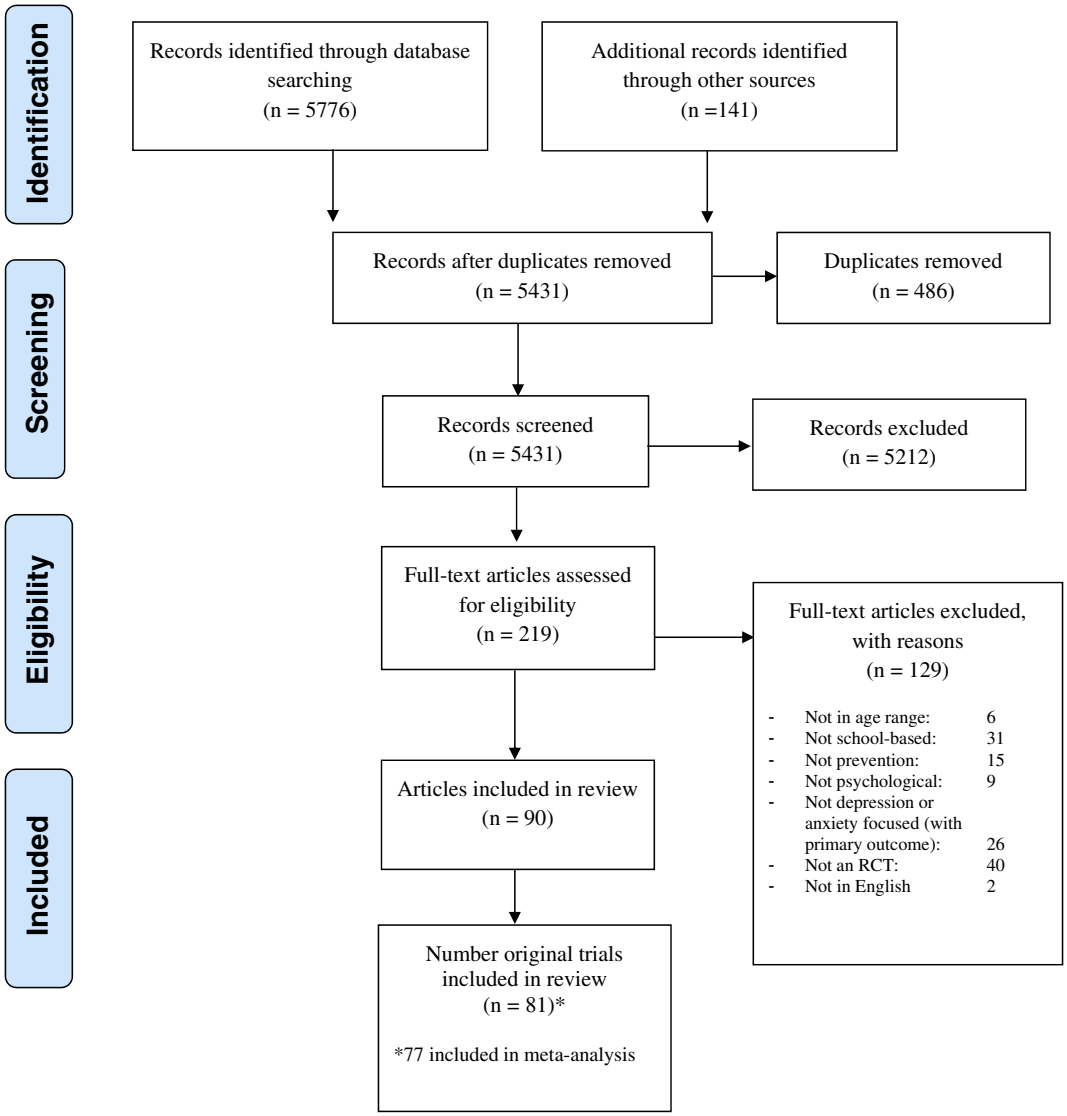


Fig. 1. Study flow chart.

questionnaires about themselves or their children (18%), or were offered information sessions so that they could learn about the program that their child was participating in (4%). However, the attendance level of these information sessions was either low (<32%), or not reported. Some studies (21%) involved parents as partial recipients of the intervention; with between two to seven sessions being provided to parents, in which they were provided psychoeducation about cognitive behavioural skills. Levels of uptake of parental sessions varied considerably, with most studies reporting at least some involvement from one parent in at least one session.

5.2.10. Fidelity to program

Two of the studies were delivered online, protecting the fidelity of the program (Calear et al., 2009; Wong et al., 2014). Of the remaining 79 studies, 33 (42%) did not report on fidelity. The remaining 58% of studies reported some level of program fidelity monitoring, ranging from program leaders completing self-report checklists ($n = 14$), to independent ratings of session audio and visual recordings ($n = 19$).

5.2.11. Program completion rates

A minority of the studies (36%) reported on program completion rates. These varied substantially across studies and the metric in which they were reported (e.g., number attending all sessions, mean number of sessions attended, number of sessions missed), although 22 studies (27%), reported high attendance and completion rates – with at least more than half of their participants completed more than half of the program.

5.2.12. Cost-effectiveness

Only one study evaluated cost-effectiveness (Stallard et al., 2013). It was reported that the targeted depression intervention was not cost effective, relative to regular Personal, Social, Health and Economic (PSHE) classes.

5.2.13. Risk of bias

The methodological quality of the studies reported varied significantly (see Table 1 for individual study quality ratings). There was evidence of selection bias, with only twenty-two studies (27%) reporting that the allocation sequence had been adequately generated. This means that 73% of studies either did not provide sufficient detail to evaluate how groups were randomised (54 studies), or did not use a randomisation procedure that ensured comparability between groups (e.g., did not use a random number table; five studies). Similarly, 29 studies (36%) indicated adequate concealment of allocation, suggesting that a majority of studies did not report enough information to ascertain whether intervention allocations could have been foreseen prior to, or

during enrolment. Risk of attrition bias was lower, with thirty two studies indicating low risk of bias for addressing incomplete outcome data (40%), which indicates that the proportion of missing data was comparable across the study conditions. Bias from selective reporting was difficult to judge, with five studies clearly indicated low risk of bias (6%). Most studies (85%) did not report enough information in order to rule selective reporting out, which is largely contingent on publication of a study protocol to ensure authors report on outcomes they have previously indicated they will. Approximately a third of studies (36%) had adequate protection against contamination by randomising at the school level. This means that there was potential for contamination between conditions in 64% of studies, where participants in the control condition may have had access to material covered in the prevention condition, though contact with participants. Overall, only one study was classified as having a low risk of bias for all five indices.

5.3. Synthesis of results

5.3.1. Primary analysis

Initial meta-analyses were conducted to compare the intervention and control groups on the primary outcomes at post-intervention and follow-up (see Figs. 2 and 3 for forest plots of depression and anxiety symptoms at post-intervention, respectively). The overall effect size at post-intervention for depression was small (n[5] $= 74$, $g = 0.23$, 95%CI: 0.19–0.28), with moderate heterogeneity ($I^2 = 57$, CI: 0–0.97). The effect for depression was small at short-term ($n = 41$, $g = 0.20$, 95%CI: 0.14–0.26); medium-term ($n = 34$, $g = 0.12$, 95%CI: 0.07–0.17); and long-term ($n = 14$, $g = 0.11$, 95%CI: 0.04–0.18) follow-up. For anxiety, the overall effect size at post-intervention was small ($n = 49$, $g = 0.20$, 95%CI: 0.14–0.25), with moderate heterogeneity ($I^2 = 55$, CI: 0.38–0.67). The effect size for anxiety was comparable at the first two follow-up periods, with conventionally small effects at short- ($n = 11$, $g = 0.23$, 95%CI: 0.09–0.37) and medium-term ($n = 20$, $g = 0.23$, 95% CI: 0.13–0.33) follow-up. At long-term follow-up, the effect size for anxiety was also small ($n = 5$, $g = 0.13$, 95%CI: 0.04–0.22).

5.4. Subgroup analyses

5.4.1. Program type

A subgroup meta-analysis (see Table 2 for results) was conducted to investigate if post-intervention effect sizes differed according to prevention type (i.e., universal vs. targeted). For depression, there was a statistically significant difference ($Q = 6.05$, $df = 1$, $p = 0.01$) in the effect size obtained for universal ($n = 39$, $g = 0.19$, 95%CI: 0.14–0.24) compared to targeted ($n = 35$ $g = 0.32$, 95%CI: 0.23–0.41) prevention programs. For anxiety, there was no statistically significant difference between the effect size as a function of prevention type ($Q = 0.12$, $df = 1$, $p = 0.73$; universal: $n = 32$, $g = 0.19$, 95%CI: 0.13–0.26; targeted: $n = 17$, $g = 0.22$, 95%CI: 0.09–0.34). No significant differences were evident at the short, medium or long-term follow up time points between universal and targeted programs.[6]

5.4.2. Program personnel

A second sub-group meta-analysis (see Table 2 for results) was conducted to explore if the personnel involved in delivering the program (classroom teachers/school health staff vs. external providers) influenced the size of the effects obtained. For depression, there was a significant effect, $Q = 7.41$, $df = 1$, $p = 0.006$ with externally-delivered programs having a larger effect ($n = 51$, $g = 0.30$, 95%CI: 0.22–0.37), than programs delivered or supported by school staff ($n = 23$, $g = 0.17$, 95%CI: 0.11–0.22). This difference remained significant at the short-term follow-up period, $Q = 11.56$, $df = 1$, $p = 0.001$, with externally-delivered programs showing stronger effects ($n = 26$, $g = 0.29$, 95%CI: 0.20–0.38), compared to those delivered by school staff ($n = 15$, $g = 0.11$, 95%CI: 0.05–0.16). At medium- and long-term follow-up this difference was no longer significant (medium term: $Q = 0.16$, $df = 1$, $p = 0.69$, external: $n = 24$, $g = 0.11$, 95%CI: 0.06–0.16; school staff: $n = 10$, $g = 0.14$, 95%CI: 0.02–0.25; long-term: $Q = 0.17$, $df = 1$, $p = 0.68$, external: $n = 9$, $g = 0.15$, 95%CI: 0.04–0.26; school staff: $n = 5$, $g = 0.12$, 95%CI: 0.00–0.23). For anxiety symptoms, there was no difference between these subgroups, at post-intervention, $Q = 0.37$, $df = 1$, $p = 0.55$, with similar effect sizes found for externally-delivered programs ($n = 30$, $g = 0.21$, 95%CI: 0.14–0.29), and school staff delivered/supported programs ($n = 19$, $g = 0.18$, 95%CI: 0.10–0.26). No differences were detected at any of the follow-up periods.[7]

5.4.3. Control condition

Further subgroup meta-analyses were conducted to compare if the magnitude of effect sizes differed as a function of the control condition the program was compared to. For depression programs, there was no overall statistically significant difference as a function of control group type ($Q = 1.96$, $df = 2$, $p = 0.38$). The effects for all three control group types were in the small to medium range (NI: $n = 47$, $g = 0.22$, 95%CI: 0.16–0.27; WL: $n = 8$, $g = 0.36$, 95%CI: 0.16–0.56; AC: $n = 19$, $g = 0.24$, 95%CI: 0.13–0.35). No significant differences were apparent at any of the three follow-up time points (all $ps > 0.05$). For anxiety programs, there was a trend-level difference in effect size as a function of control group, $Q = 0.5.78$, $df = 2$, $p = 0.06$, with the WL control comparisons yielding larger effect sizes, $n = 16$, $g = 0.29$, 95%CI: 0.16–0.43, relative to NI, $n = 23$, $g = 0.19$, 95%CI: 0.11–0.26, and AC, $n = 10$, $g = 0.10$, 95% CI: 0.01–0.19 comparison types. However, it should be noted that there was a high level of heterogeneity for the WL control group ($I^2 = 73$). At the short-term follow-up, this trend level difference was no longer evident, $Q = 4.17$, $df = 2$, $p = 0.13$, (WL: $n = 4$, $g = 0.34$, 95%CI: 0.20–0.49; NI: $n = 6$, $g = 0.19$, 95%CI: −0.02–0.40; AC: $n = 1$, $g = 0.05$, 95%CI: −0.22–0.31). There were no differences in the medium term follow-up, $Q = 4.26$, $df = 2$, $p = 0.12$. Only studies including no-intervention control groups ($n = 5$) conducted long-term follow-ups and so no further analyses could be performed.

5.4.4. Program delivery age

Subgroup analyses were conducted to determine if the age at which programs were delivered had an impact on the size of intervention effects (see Table 2). For depression, no significant between-group differences were found, $Q = 3.07$, $df = 2$, $p = 0.22$, (children: $n = 5$, $g = 0.50$, 95%CI: 0.19–0.80, early adolescents: $n = 32$, $g = 0.23$, 95%CI: 0.16–0.30, older adolescents: $n = 37$, $g = 0.22$, 95%CI: 0.15–0.28). For anxiety outcomes, there was no overall effect for age of program delivery, $Q = 3.31$, $df = 2$, $p = 0.19$, (children: $n = 15$, $g = 0.23$, 95%CI: 0.09–0.38, early adolescents: $n = 22$, $g = 0.21$, 95%CI: 0.15–0.28, older adolescents: $n = 12$, $g = 0.12$, 95%CI: 0.02–0.21). No differences emerged over the follow-up periods for depression or anxiety outcomes (all $ps > 0.05$).

[5] n represents number of comparisons included in each analysis.

[6] Short-term follow-up (depression: $Q = 0.82$, $df = 1$, $p = 0.37$; universal: $n = 17$, $g = 0.18$, CI: 0.10–0.26; targeted: $n = 24$, $g = 0.23$, CI: 0.14–0.31; anxiety: $Q = 1.50$, $df = 1$, $p = 0.22$; universal: $n = 5$, $g = 0.17$, CI: −0.01–0.32; targeted: $n = 6$, $g = 0.36$, CI: 0.11–0.61), medium-term (depression: $Q = 0.94.05$, $df = 1$, $p = 0.33$; universal: $n = 18$, $g = 0.09$, CI: 0.04–0.15; targeted: $n = 16$, $g = 0.13$, CI: 0.04–0.23; anxiety: $Q = 1.62$, $df = 1$, $p = 0.20$; universal: $n = 14$, $g = 0.26$, CI: 0.13–0.40; targeted: $n = 6$, $g = 0.14$, CI: 0.00–0.27), or long-term (depression: $Q = 1.01$, $df = 1$, $p = 0.32$; universal: $n = 5$, $g = 0.09$, CI: −0.02–20; targeted: $n = 9$, $g = 0.16$, CI: 0.07–0.27; anxiety: $Q = 0.08$, $df = 1$, $p = 0.78$; universal: $n = 3$, $g = 0.12$, CI: −0.00–01; targeted: $n = 2$, $g = 0.15$, CI: −0.06–0.36).

[7] Anxiety outcomes at short-term ($Q = 2.93$, $df = 1$, $p = 0.09$, externally delivered: $n = 8$, $g = 0.33$, 95%CI: 0.13–0.52; school staff delivered: $n = 3$, $g = 0.11$, 95%CI: −0.06–0.27), medium term ($Q = 0.11$, $df = 1$, $p = 0.75$, externally delivered: $n = 12$, $g = 0.25$, 95%CI: 0.12–0.38; school staff delivered: $n = 8$, $g = 0.21$, 95% CI: 0.04–0.38), long-term ($Q = 0.09$, $df = 1$, $p = 0.76$, externally delivered: $n = 3$, $g = 0.15$, 95% CI: −0.01–0.31; school staff delivered: $n = 2$, $g = 0.12$, 95% CI: 0.01–0.23).

Study name	Comparison	Hedges's g	Standard error	Lower limit	Upper limit
Araya (2013)	CBT vs NI	0.039	0.044	-0.048	0.125
Arnason (2009)	CBT vs NI	0.816	0.855	-0.859	2.491
Barrett (2001)	CBT(h) vs NI	0.175	0.112	-0.045	0.395
Barrett (2001)	CBT(s) vs NI	0.424	0.106	0.216	0.633
Calear (2009)	iCBT vs WL	0.105	0.058	-0.009	0.218
Castellanos (2006)	CBT vs NI	0.070	0.097	-0.120	0.261
Chaplin (2006)	CBT(f) vs NI	0.405	0.284	-0.151	0.961
Chaplin (2006)	CBT(m&f) vs NI	0.377	0.291	-0.193	0.948
Clarke (1993 - Study 1)	EDU vs NI	0.190	0.081	0.030	0.349
Clarke (1993 - Study 2)	CBT vs NI	0.060	0.102	-0.141	0.261
Clarke (1995)	CBT vs NI	0.555	0.187	0.189	0.920
Gillham (2006)	CBT vs NI	0.020	0.310	-0.588	0.627
Gillham (2007)	CBT vs AC	0.209	0.119	-0.024	0.443
Gillham (2007)	CBT vs NI	0.195	0.119	-0.038	0.427
Gillham (2012)	CBT vs AC	0.142	0.126	-0.104	0.388
Gillham (2012)	CBT vs NI	0.156	0.128	-0.094	0.406
Haines (1990)	CBT vs WL	0.604	0.433	-0.245	1.454
Haines (1992 - Study 2)	AM vs NI	0.809	0.589	-0.345	1.963
Haines (1992)	CBT vs WL	0.544	0.569	-0.572	1.659
Horowitz (2007)	IPT vs NI	0.116	0.147	-0.173	0.405
Horowitz (2007)	CBT vs NI	0.215	0.144	-0.067	0.497
Jordans (2010)	CBT/CEET vs WL	0.564	0.113	0.342	0.786
Kindt (2014)	CBT vs NI	0.017	0.059	-0.099	0.132
King (1990)	CBT vs NI	1.013	0.310	0.407	1.620
Kraag (2009)	CBT vs NI	0.098	0.054	-0.007	0.204
Lock (2003)	CBT vs WL	0.057	0.075	-0.090	0.204
Lowry-Webster (2001)	CBT vs WL	0.107	0.099	-0.087	0.300
Manassis (2010)	CBT vs AC	0.373	0.167	0.046	0.700
McCarty (2011)	CBT vs NI	0.205	0.243	-0.270	0.681
McCarty (2013)	CBT vs AC	0.434	0.192	0.058	0.810
Merry (2004)	CBT vs AC	0.209	0.110	-0.007	0.425
Nobel (2012)	CBT vs AC	0.252	0.226	-0.190	0.695
Pattison (2011)	CBTa vs AC	0.024	0.473	-0.903	0.950
Pattison (2011)	CBTa vs NI	0.145	0.462	-0.760	1.051
Pattison (2011)	CBTb vs AC	0.221	0.474	-0.709	1.151
Pattison (2011)	CBTb vs NI	0.130	0.462	-0.775	1.035
Possel (2004)	CBT vs NI	0.489	0.110	0.273	0.704
Possel (2008)	CBT vs NI	0.083	0.119	-0.151	0.316
Possel (2013)	CBT vs AC	0.044	0.117	-0.185	0.273
Possel (2013)	CBT vs NI	0.231	0.117	0.001	0.460
Puskar (2003)	CBT vs NI	0.457	0.213	0.039	0.874
Quayle (2001)	CBT vs NI	-0.304	0.289	-0.870	0.262
Raes (2014)	MBCT vs NI	0.344	0.106	0.136	0.553
Rivet-Duval (2011)	CBT/IPT vs WL	0.441	0.159	0.129	0.753
Roberts (2003)	CBT vs NI	0.122	0.149	-0.170	0.415
Roberts (2010)	CBT vs NI	0.121	0.097	-0.069	0.312
Rohde (2014)	CBT vs AC	0.027	0.128	-0.223	0.277
Rohde (2014)	CBT vs NI	0.245	0.129	-0.008	0.499
Rooney (2006)	CBT vs NI	0.526	0.190	0.153	0.900
Rooney (2013a)	CBT vs NI	0.200	0.067	0.068	0.331
Rose (2014)	CBT/IPT vs AC	0.409	0.180	0.057	0.761
Rose (2014)	CBT/IPT vs WL	0.292	0.169	-0.039	0.624
Ruini (2006)	CBT vs AC	0.195	0.189	-0.175	0.566
Ruini (2009)	WBT vs AC	0.035	0.134	-0.226	0.297
Sheffield (2006)	CBTa vs NI	0.051	0.121	-0.187	0.289
Sheffield (2006)	CBTb vs NI	0.005	0.059	-0.112	0.121
Siu (2007)	CBT vs WL	0.883	0.301	0.293	1.473
Spence (2003)	CBT vs NI	0.343	0.057	0.232	0.454
Stallard (2012)	CBT vs AC	0.042	0.077	-0.109	0.194
Stallard (2012)	CBT vs NI	0.085	0.083	-0.077	0.247
Stice (2006)	CBT vs AC (Biblio)	0.175	0.363	-0.536	0.886
Stice (2006)	CBT vs AC (EW)	0.219	0.354	-0.475	0.913
Stice (2006)	CBT vs AC (Journaling)	0.384	0.337	-0.277	1.045
Stice (2006)	CBT vs AC (SET)	0.186	0.361	-0.522	0.895
Stice (2006)	CBT vs WL	0.862	0.396	0.085	1.638
Stice (2008)	CBT vs AC (Biblio)	0.362	0.153	0.063	0.661
Stice (2008)	CBT vs AC (SET)	0.762	0.161	0.446	1.079
Stice (2008)	CBT vs NI	0.693	0.157	0.385	1.000
Wijnhoven (2014)	CBT vs NI	0.549	0.200	0.156	0.941
Wong (2014)	iCBT(dep) vs NI	0.359	0.155	0.056	0.662
Woods (2011)	CBT vs NI	0.740	0.408	-0.061	1.540
Young (2006)	IPT vs AC	1.061	0.352	0.372	1.750
Young (2010)	CBT vs NI	0.809	0.283	0.255	1.363
Yu (2002 - Study 3)	CBT vs NI	0.321	0.135	0.055	0.586
		0.234	0.024	0.187	0.281

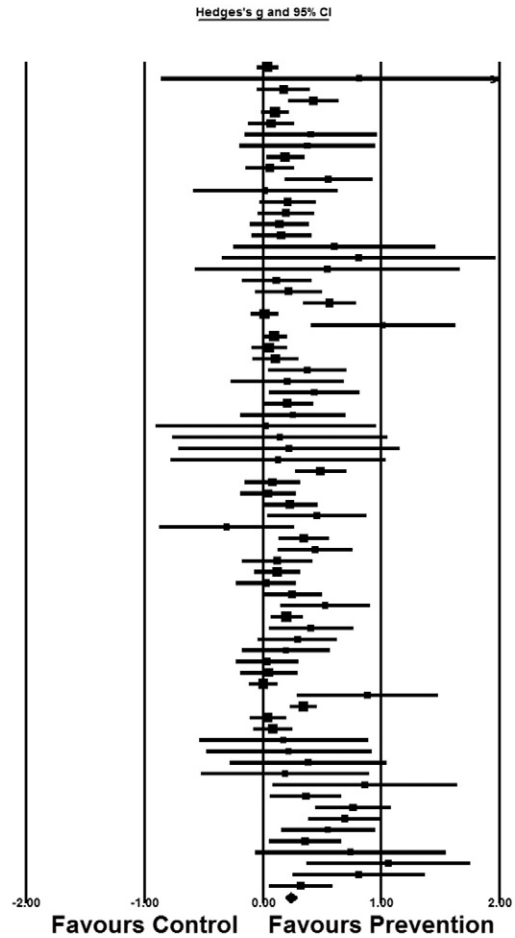


Fig. 2. Forest plot effect sizes for comparisons between prevention programs and control conditions on depressive symptoms at post-intervention.

5.4.5. Program content

The comparison of outcomes as a function of program content indicated that there was no statistically significant difference for CBT vs. other programs for depression, $Q = 1.98$, $df = 1$, $p = 0.16$, (CBT: $n = 66$, $g = 0.22$, 95%CI: 0.18–0.27, other: $n = 8$, $g = 0.38$, 95%CI: 0.17–0.59), or anxiety, $Q = 0.60$, $df = 1$, $p = 0.44$, (CBT: $n = 46$, $g = 0.19$, 95% CI: 0.13–0.25, other: $n = 3$, $g = 0.36$, 95%CI: −0.06–0.78). There were no differences at any of the follow-up intervals (all $ps > 0.05$).

5.5. Meta-regression

We conducted a multivariate meta-regression with effect size at post-intervention as the dependant variable, and study characteristics used in the subgroup analysis entered into the regression model as predictors of outcome. To avoid collinearity among the predictors in the model, we first examined correlations between the variables. All correlations were less than $r = 0.23$, suggesting that none of the variables were highly confounded and were therefore retained in the model. Within each category (prevention type, personnel delivering program, control group comparison, age of delivery, program content), we first defined a reference group. Reference group information and results are presented in Table 3. For depression, only one variable emerged as significant, with targeted studies having larger effect sizes than universal programs. At the trend level ($p < 0.10$), programs delivered by facilitators external to the school had larger effect sizes, and programs delivered to older adolescents has smaller effect sizes than those delivered to children (although it is noted that there were only five depression studies delivered to children). There were no significant predictors of effect size for anxiety prevention studies.

5.6. Publication bias

There was some evidence of publication bias for the depression studies, as demonstrated by inspection of the funnel plot (Appendix B). After adjusting for publication bias using Duval and Tweedie's trim and fill procedure, the estimate of the mean effect size at post-intervention reduced from $g = 0.23$ to $g = 0.15$ (23 studies removed). There was no evidence of publication bias for anxiety studies.

6. Discussion

The aim of this study was to provide a review and evaluation of studies investigating school-based programs designed to prevent depression and/or anxiety in children and adolescents. Across the eighty-one included RCTs involving 31,794 participants, our findings show that school-based prevention programs have a small beneficial effect on depressive and anxiety symptoms when compared to a control condition.

For the studies focused on depression, the effect sizes at post-intervention and short-term follow-up indicated a small effect ($g = 0.23$ and $g = 0.20$ respectively). There was evidence of a very small effect of the depression-prevention programs at the medium-term follow-up (6–12 months; $g = 0.12$), and long term follow-up (>12-months, $g = 0.11$). It is encouraging that 14 studies included follow-up intervals of >12-months, which is important in prevention where there end-goal is to detect prevention effects which necessarily become evident over time. That said, the effect sizes for study follow-up assessment points were modest. Several studies included follow-ups for longer-term periods and suggest that gains can be maintained at 24 month follow-up period (e.g., Stice, Rohde, Gau, and Wade, 2010), but tend to dissipate

Study name	Comparison	Hedges's g	Standard error	Lower limit	Upper limit
Anticich (2013)	CBT vs AC	0.116	0.109	-0.097	0.329
Anticich (2013)	CBT vs WL	0.237	0.119	0.004	0.470
Aune (2009)	CBT vs NI	0.205	0.053	0.101	0.309
Balle (2010)	CBT vs WL	0.133	0.226	-0.311	0.576
Barrett (2001)	CBT(h) vs NI	0.268	0.113	0.048	0.489
Barrett (2001)	CBT(s) vs NI	0.281	0.106	0.074	0.488
Berger (2007)	CBT vs WL	0.987	0.177	0.641	1.334
Bouchard (2013)	CBT vs WL	0.615	0.297	0.032	1.197
Calear (2009)	iCBT vs WL	0.155	0.058	0.042	0.269
Cooley-Strickland (2011)	CBT vs WL	0.017	0.206	-0.386	0.421
Dadds (1997)	CBT vs NI	0.032	0.176	-0.313	0.377
Essau (2012)	CBT vs WL	0.081	0.079	-0.074	0.237
Garaigordobil (2004)	CBT vs NI	0.535	0.170	0.201	0.869
Gillham (2006)	CBT vs NI	0.228	0.311	-0.382	0.837
Haines (1990)	CBT vs WL	0.705	0.437	-0.152	1.562
Haines (1992)	AM vs NI	0.912	0.595	-0.254	2.079
Haines (1992)	CBT vs WL	0.847	0.583	-0.296	1.990
Hiebert (1989)	CBT vs AC	0.179	0.225	-0.261	0.620
Jordans (2010)	CBT/CEET vs WL	0.330	0.112	0.111	0.550
Keogh (2006)	CBT vs NI	0.019	0.221	-0.415	0.453
Kiselica (1994)	CBT vs NI	0.840	0.297	0.259	1.422
Kraag (2009)	CBT vs NI	0.036	0.054	-0.069	0.142
Lock (2003)	CBT vs WL	0.126	0.075	-0.022	0.273
Lowry-Webster (2001)	CBT vs WL	0.432	0.099	0.237	0.627
Manassis (2010)	CBT vs AC	0.085	0.165	-0.239	0.409
McLoone (2012)	CBT vs WL	0.055	0.240	-0.414	0.525
Miller (2010)	CBT vs WL	0.036	0.192	-0.341	0.413
Miller (2011a)	CBT vs WL	0.018	0.094	-0.167	0.204
Miller (2011b - Study 1)	CBT vs AC	0.118	0.157	-0.189	0.426
Miller (2011b - Study 2)	CBT vs AC	0.174	0.131	-0.082	0.431
Misfud (2005)	CBT vs WL	0.471	0.235	0.010	0.933
Nobel (2012)	CBT vs AC	0.152	0.225	-0.289	0.594
Pattison (2011)	CBTa vs AC	0.024	0.473	-0.902	0.951
Pattison (2011)	CBTa vs NI	0.315	0.464	-0.595	1.225
Pattison (2011)	CBTb vs AC	0.108	0.488	-0.847	1.064
Pattison (2011)	CBTb vs NI	0.241	0.478	-0.696	1.179
Pedro-Carroll (1985)	CBT vs WL	0.547	0.239	0.079	1.016
Roberts (2003)	CBT vs NI	0.287	0.150	-0.007	0.581
Roberts (2010)	CBT vs NI	0.101	0.097	-0.089	0.292
Rooney (2006)	CBT vs NI	0.285	0.188	-0.084	0.654
Rooney (2013a)	CBT vs NI	0.038	0.067	-0.093	0.170
Ruini (2006)	CBT vs AC	0.374	0.190	0.001	0.747
Ruini (2009)	WBT vs AC	0.074	0.134	-0.188	0.336
Sheffield (2006)	CBTa vs NI	0.037	0.121	-0.201	0.275
Sheffield (2006)	CBTb vs NI	0.009	0.059	-0.107	0.125
Siu (2007)	CBT vs WL	1.612	0.332	0.962	2.262
Stallard (2012)	CBT vs AC	0.059	0.077	-0.092	0.210
Stallard (2012)	CBT vs NI	0.028	0.082	-0.133	0.190
Wong (2014)	iCBT(anx) vs NI	0.343	0.158	0.034	0.652
		0.197	0.029	0.140	0.254

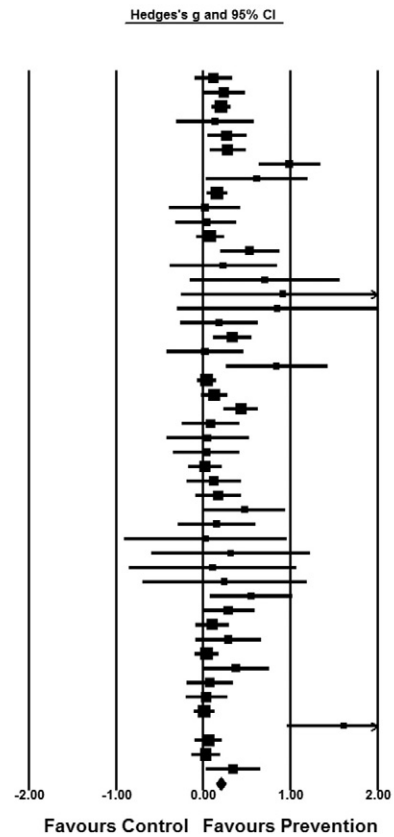


Fig. 3. Forest plot of effect sizes for comparisons between prevention programs and control conditions on anxiety symptoms at post-intervention.

at longer intervals (e.g., Hunt, Andrews, Crino, Erskine, and Sakashita, 2009; Johnstone, Rooney, Hassan, and Kane, 2014).

The pattern of findings for the anxiety prevention programs was similar to that of depression programs, with a small effect at post-intervention ($g = 0.20$), which was maintained at the short-term and medium-term follow-ups ($g = 0.23$ for both). By the long-term follow-up, the effect of the anxiety programs was marginal ($g = 0.13$). Long term follow-ups for anxiety programs were less common, with only five studies including long-term follow-up intervals, again with little evidence of gains being maintained beyond the 24-month follow-up period (e.g., Johnstone et al., 2014). Taken together, these data suggests that the effect of anxiety prevention programs is maintained at 6–12 months after the program is delivered and highlights the need for long-term follow-up assessments to establish whether gains remain at and beyond this point. Indeed, more research is needed to accurately assess if prevention programs in general have the potential for prolonged effects. It is possible that the deterioration in effects is due to natural decay over time, or could reflect reduced power with smaller samples being retained over protracted periods. It will be important for future studies to identify the parameters under which prevention effects are most likely to be maintained. For example, it is possible that prevention effects might be augmented by the delivery of booster sessions. Alternatively, low intensity prevention strategies such as automated, computerised modules could be incorporated into standard classroom activities as intermittent reminders following a more intensive program, if this is likely to maintain preventive effects.

A common criticism of prevention programs is that they are associated with small effect sizes. While this is true, it is important to keep in mind that in *prevention* (as compared to treatment), even small effect sizes are likely to be associated with meaningful improvements particularly at a population level. A small effect size difference in this context is likely to have implications for preventing the onset of these disorders in youth. Analyses that have been conducted on diagnostic outcomes following prevention programs consistently support this assertion, with a recent review showing that preventive programs are associated with a 53% decrease in risk of internalising disorder onset in the 6–9 months following program delivery (Stockings et al., 2016). These studies are difficult to do because many school-based trials often do not include diagnostic outcomes, so symptom levels provide a meaningful proxy for disorder. Emerging online diagnostic screening tools may enable researchers to more often collect diagnostic information in the school environment (Gibbons et al., 2012).

The effect sizes reported in the current review are comparable to those reported in previous reviews of depression (Calear and Christensen, 2010; Merry et al., 2011), and anxiety prevention (Fisak et al., 2011; Stockings et al., 2016). Slight variability in effect size estimates is likely to be due to a number of factors, including the absence of pooled effect sizes in previous reviews (e.g., Calear and Christensen, 2010), and the more conservative method in which effect sizes were calculated in the present review (Hedges *g* rather than Cohen's *d* in order to adjust for small sample sizes).

The quality assessments undertaken in the current review indicated that 80 of the 81 RCTs included some degree of bias. This suggests that there is substantial room for improvement in the rigour and quality of the research conducted in this field, and in the reporting of study methodologies and outcomes. Ways in which the quality of school-based prevention trials may be improved include using random sequence generation methods and adequate allocation concealment. It is possible that these methods actually *were* followed in many of the trials included, but a majority of authors failed to provide sufficient detail around these possible selection biases. Moreover, the increasingly common requirement to publish study protocols will mitigate the risk of selective reporting moving forward, and authors are encouraged to be transparent in their report of outcome data and attrition. Finally, randomisation at the school-level for school-based trials protects against contamination. The potential for contamination in 64% of the included trials would be expected to actually *reduce* outcomes because participants in control conditions may have

Table 2
Subgroup analyses at post-intervention.

Subgroup analyses		Measure	N	*g*	95% CI	I^2
Prevention type	Universal	Depression	39	.19	.14-.24	46
	Targeted		35	.32	.23-.41	64
	Universal	Anxiety	32	.19	.13-.26	54
	Targeted		17	.22	.09-.34	59
Personnel delivering program	External	Depression	51	.30	.22-.37	57
	School-staff		23	.17	.11 -.22	51
	External	Anxiety	30	.21	.14-.29	41
	School-staff		19	.18	.10-.26	67
Control group comparison	No intervention	Depression	47	.22	.16-.27	57
	Waitlist		8	.36	.16-.56	75
	Active		19	.24	.13-.35	46
	No intervention	Anxiety	23	.19	11-.26	53
	Waitlist		16	.29	.16-.43	73
	Active		10	.10	.01-.19	0
Age	Child	Depression	5	.50	.19-.80	69
	Early adolescent		32	.23	.16-.30	46
	Older adolescent		37	.22	.15-.28	62
	Child	Anxiety	15	.23	.09-.38	73
	Early adolescent		22	.21	.15-.28	32
	Older adolescent		12	.12	.23-.21	41
Program content	CBT	Depression	66	.22	.18-.27	55
	Other		8	.38	.17-.59	67
	CBT	Anxiety	46	.19	.13-.25	55
	Other		3	.36	-.06-.78	62

Note: N = number of comparison conditions; I^2 = heterogeneity; 'external' includes mental health professional, researchers, graduates; 'school-staff' includes teachers, counsellors, nurses; and teacher-supported computerised programs.

Table 3
Standardised regression coefficients of study characteristics in relation to the effect size of outcomes at post-test for both depression and anxiety.

Variable	Outcome	*B*	SE	*p* value
Prevention type	Depression	Ref		
Universal		−0.12	0.05	0.03**
Targeted				
Universal	Anxiety	Ref		
Targeted		−0.03	0.08	0.71
Personnel delivering program				
External	Depression	Ref		
School-staff		−0.09	0.05	0.08*
External	Anxiety	Ref		
School staff		−0.03	0.08	0.66
Control group				
No intervention	Depression	Ref		
Waitlist		0.10	0.08	0.24
Active		−0.04	0.07	0.58
No intervention	Anxiety	Ref		
Waitlist		0.05	0.07	0.46
Active		−0.01	0.08	0.90
Age				
Child	Depression	Ref		
Early adolescent		−0.17	0.11	.13
Older adolescent		−0.22	0.11	0.05*
Child	Anxiety	Ref		
Early adolescent		−0.01	0.08	0.91
Older adolescent		−0.09	0.09	0.29
Program content				
CBT	Depression	Ref		
Other		0.15	0.09	.12
CBT	Anxiety	Ref		
Other		0.21	0.02	.18

Note: Ref = reference group.
* Trend level at $\alpha < 0.10$.
** Significant at $\alpha < 0.05$.

accessed material covered in the prevention condition. This means that current effect size estimates may be conservative. Cluster randomisation at the school level in prevention research will protect against this risk. These changes, as well as many journals now requiring submissions follow the CONSORT statement (Schulz, Altman, and Moher, 2010) will help to improve the overall quality of RCTs in the area.

Moderate levels of heterogeneity were also found across the studies included in this review and meta-analysis. This finding was not surprising, given that prevention trials can vary enormously in terms of their sample, the type of program evaluated and how it is delivered. The results from sub-group analyses reveal some possible reasons for this heterogeneity, as discussed below.

6.1. Sub-group and meta-regression analyses outcomes

The analysis comparing universal and targeted prevention programs found that for depression-prevention programs, targeted prevention yielded a significantly greater effect size relative to universal programs at post-test, even when other factors (e.g., facilitator type, age) were controlled for. This was not the case for anxiety, and was not maintained at any of the follow-up periods for either depression or anxiety. The existing literature on this question has produced mixed results, with some previous meta-analyses reporting results consistent with those reported here (e.g., Merry et al., 2011), although null and opposite effects have been reported (e.g., Fisak et al., 2011; Stockings et al., 2016). However, a key difference between the current a previous reviews is that previous reviews did not distinguish between school-based and community settings. The data from the current review suggests for depression programs delivered in the school environment, targeted intervention may be more efficacious. However, several other factors need to be taken into account in the targeted vs. universal debate. First, research evaluations of universal prevention programs require much larger sample sizes to detect effects

than targeted programs, which are often impractical or prohibitively expensive to conduct (Muñoz et al., 2010). Therefore, smaller effect sizes reported in the literature may be due to limits in statistical power to detect effects, not because they do not work. Additionally, universal prevention in the school context confers several advantages over targeted prevention that include: (i) removal of the need for screening; (ii) minimisation of stigma because no student is singled out, and; (iii) capturing youth who may not be at risk yet, but will go on to develop symptoms in the future. It is notable that school administrators often prefer universal programs due to ease of scheduling programs (Horowitz, Garber, Ciesla, Young, and Mufson, 2007). Both universal and targeted prevention programs yielded small effect sizes in the current review, and it is likely that there is value in pursuing both kinds of prevention. Indeed, no differences for anxiety outcomes as a function of prevention type were evident. A suggestion for how future studies may be improved is to take a stepped care approach where a universal program is delivered in the first instance, and followed up with a targeted program for at risk or symptomatic individuals who do not respond to the universal program.

Analyses comparing facilitator type as a predictor of effect size revealed that depression programs were more efficacious when delivered by individuals external to the school environment (e.g., researchers, mental health professionals, graduate students), relative to school-staff at post-intervention and short-term follow-up. This finding is in accordance with a previous review reporting a series of larger effect sizes for programs delivered by individuals external to the school environment (Calear and Christensen, 2010) and another suggesting that there is not yet enough evidence to show that programs delivered by internal providers are effective (Brunwasser and Garber, 2015). It is noteworthy to consider that targeted programs were more likely to be delivered by external facilitators than universal programs were (74% externally delivered for targeted vs. 64% externally delivered for universal), although the meta-regression confirmed this difference was not statistically significant. The differential effect sizes as a consequence of personnel delivering the intervention was not replicated for anxiety programs, with no difference in effect size between programs delivered by school vs. external staff at post-intervention, or any of the follow-up time points. We speculate that this could be due to the fact that anxiety programs tended to be delivered to younger participants, whom may feel more comfortable with familiar school staff than unfamiliar external providers.

Provider type is a particularly important factor to consider because programs delivered by school staff provide information about whether the program is likely to be effective under conditions that are suitable for large-scale implementation. One avenue likely to reduce the resources required to deliver prevention programs in schools are computerised therapies (Richardson, Stallard, and Velleman, 2010), which show promise by reducing the training required for the delivery of both school and mental health staff. Although it was not possible to do a formal analysis on the two computer-delivered programs, symptom differences between the experimental group and controls were detected at post-intervention in both studies ($d = 0.15$ for anxiety, Calear et al., 2009; $d = 0.14$ for depression, $d = 0.18$ for anxiety, Wong et al., 2014). An additional benefit of computer-delivered programs is that they protect the fidelity of the content being delivered. Therefore, outcomes might actually be augmented by having teachers support their delivery, as was the case in one study whereby teachers led a classroom discussion following each treatment module to reinforce the ideas presented by the program (Wong et al., 2014).

A major limitation of the school-based prevention field has been the lack of active control groups (e.g., Merry et al., 2011). The results of the current review suggest that studies are increasingly including active attention control groups, which typically involve a structured program (not including active elements of the prevention program). This is a much stronger comparison than NI and WL groups, which tend to involve regular classes as usual. The present review found that 25% of studies evaluating prevention programs included an attention control condition. This compares more favourably to previous reviews of anxiety and depression programs that reported attention control conditions in between 9% and 15% of trials (Calear and Christensen, 2010; Neil and Christensen, 2009). However, the sub-group analyses suggested that the control group used did not influence the size of the effect. This finding may suggest one of two things: firstly, that non-specific program factors, such as being part of a group or receiving a program outside of regular school classes does not impact program outcomes, or secondly, that there are yet to be a sufficient number of trials with attention control conditions to accurately detect a difference. Further research involving attention control conditions will help to ascertain if the control group utilised in a trial can inflate intervention effects.

Sub-group analyses suggested that the age at which a prevention program was delivered influenced the size of the intervention effects obtained, with the exception of a trend towards depression programs delivered to children being more effective than those delivered to older adolescents. Previous research has suggested the need to deliver prevention programs prior to the onset of a clinical disorder (Cuijpers et al., 2008). Drawing from epidemiological research, it would therefore be expected that depression programs would be best delivered in childhood or early adolescence, as the mean age of onset for depression is between 11 and 14 years (Merikangas et al., 2009), while for anxiety disorders, delivery during childhood would be best given the early age of onset of many anxiety disorders (Kessler et al., 2005; Merikangas et al., 2010). The results of the current review may lend some support for this suggestion, although there were only five studies delivering depression prevention programs to children aged younger than ten years old. It is important to note that studies were included even if they did not screen for the presence of current or past diagnoses, and so the effects may have been obscured by individuals essentially receiving treatment or relapse prevention rather than prevention.

6.2. Other factors

Almost half of the included studies did not measure program fidelity, and among those that did, the use of program leader self-report checklists was pervasive. Less than one third of studies recorded sessions for independent fidelity ratings. Measuring and protecting program fidelity must be a priority for future studies. A promising solution to this problem is the advent of internet-based psychological interventions, which by their very nature protect program fidelity. An additional benefit associated with internet-based programs is that they can automatically collect adherence information. Mapping a dose-response outcome as a function of program adherence will provide important data regarding the parameters needed to achieve preventive effects.

The current review also established that parents vary substantially in rates of participation in school-based prevention programs, with some studies reporting very poor parental attendance levels (e.g., Lock and Barrett, 2003). The impact of involving parents on outcome is not yet clear, although there is some preliminary evidence that their involvement may support the longer term maintenance of intervention effects (Manassis et al., 2014). Therefore, future studies will need to quantify the impact of parental involvement, and explore strategies designed to incentivise and improve parental uptake rates.

6.3. Future research directions

Overall, the findings from the current meta-analysis suggest that there is merit in continuing to develop, deliver and evaluate school-based prevention programs for depression and anxiety. This review establishes that even when an ecologically valid approach is taken, and stringent entry criteria that exclude those who may already be experiencing significant symptoms is *not* imposed, meaningful effects are still detected. A related challenge for the future is improving reach, coverage, and the availability of prevention programs. Advances in technology provide one avenue by which to address reach and availability, although how this might be delivered at a population level has not yet

been established. Finally, enhancing youth engagement in these programs is necessary if program adherence is to be improved. Engaging asymptomatic young people is challenging because the material may be viewed as irrelevant. Consulting with young people and gaining insight into their perspective on the issue may help to elucidate the barriers to engagement.

A priority for the field moving forward is a closer consideration of ways to accelerate efficacy findings into the effectiveness domain. A recent review of depression prevention concluded that there is strong evidence for program efficacy, but evidence for effectiveness under real-world conditions is still accumulating (Brunwasser and Garber, 2015). To ensure efficacious programs get translated into programs ready for wide-scale rollout, efforts now need to be directed towards pragmatic trial designs that enable testing the infrastructure and personnel that will ultimately require for sustainable, long-term programs to be adopted by schools.

7. Limitations

The results of this meta-analysis need to be interpreted in the context of several limitations. First, the meta-analysis was based exclusively on self-report symptom measures and not clinician-rated measures. Self-report symptoms measures were used in the current review as the majority of the studies that met the inclusion criteria did not report clinician-rated diagnostic outcomes and, from a practical perspective, it is unlikely that when delivered under real-world settings, programs will include clinician assessments because they are expensive and time consuming to administer. Self-report measures are acceptable in the field to measure intervention effects, with many researchers arguing that they are as important as clinical diagnoses, as they are associated with comparable degrees of impairment (Gillham, Shatté, and Reivich, 2001; Horowitz et al., 2007).

A second limitation is that studies were included in the review even if they did not exclude participants with significant symptoms, of which >80% of included studies did not. The rationale for this was to emulate procedures that are likely to be employed under real world conditions, as is typically characteristic of universal prevention programs. While this may have impacted effect size estimates as a consequence of including symptomatic individuals, it did allow an overall indication as to the effects of programs as they would be delivered in the real world.

Finally, there was evidence of publication bias in the depression studies and so the effect size estimates may have been inflated. The increasing trend towards publication of study protocols should help to protect against selective publication of studies reporting significant group differences.

8. Conclusion

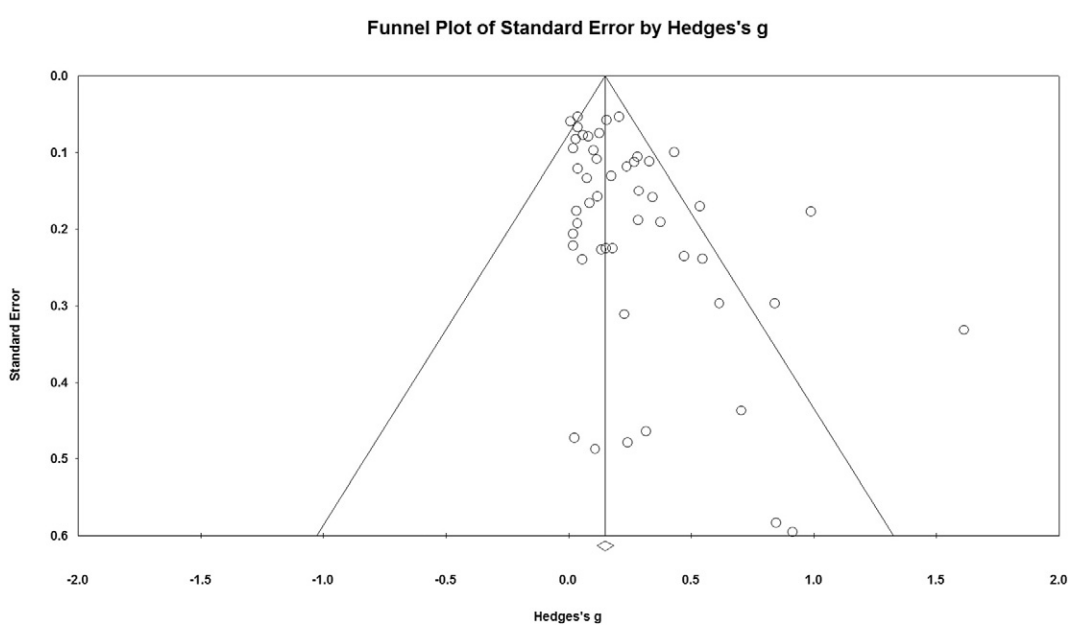
Overall, the findings from the current meta-analysis suggest that there is merit in continuing to evaluate and deliver school-based prevention programs for depression and anxiety. While prevention type and personnel delivering the intervention account for aspects of the heterogeneity observed, more research is needed to identify how program completion and fidelity impacts outcome, at what age program delivery is optimal, and whether there is a need to involve parents. The overall quality of included RCTs was low, and heterogeneity was moderate. Large-scale effectiveness trials that evaluate implementation efforts that are embedded within the school system are now needed in order to identify the most successful ways to roll-out prevention programs on scale.

Appendix A. Search string example

Search String from PubMed:

((depress*[All Fields]) OR mood [All Fields] OR affect [All Fields/MeSH Terms]) OR anxiety [All Fields] OR anxious [All Fields] AND ((school* [All Fields]) OR school-based [All Fields]) OR adolescen* ([All Fields/MeSH Terms]) OR child* (([All Fields]) OR youth ([All Fields])) AND ((prevent* [All Fields]) OR early intervent* ([All Fields])) AND control [All Fields] OR control groups [MeSH Terms] OR control groups [All Fields] AND (Clinical Trial[ptyp] AND English[lang]).

Appendix B. Funnel plot of depression effect size data at post-intervention



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