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Review

School-based depression and anxiety prevention programs: An updated systematic review and meta-analysis

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ABSTRACT

Depression and anxiety are often first experienced during childhood and adolescence, and interest in the *prevention* of these disorders is growing. The focus of this review was to assess the effectiveness of psychological prevention programs delivered in schools, and to provide an update to our previous review from five years ago (Werner-Seidler, Perry, Calear, Newby, & Christensen, 2017). Three electronic databases were systematically searched for published articles of randomised controlled trials (RCTs) evaluating the efficacy of school-based prevention programs until October 2020. There were 130 articles that met inclusion criteria, representing 118 unique trials and 45,924 participants. Small between-group effect sizes for depression ($g = 0.21$) and anxiety ($g = 0.18$) were detected immediately post-intervention. Subgroup analyses suggested that targeted prevention programs (for young people with risk factors or symptoms) were associated with significantly greater effect sizes relative to universal programs for depression, which was confirmed by meta-regression. There was also some evidence that external providers conferred some benefit over school-staff delivered programs. Overall, study quality was moderate and no association between risk of bias and effect size was detected. School-delivered psychological prevention programs have small effects in reducing symptoms of depression and anxiety. Refinement of these programs, and knowledge about how they can be sustainably delivered in schools beyond the trial period is now needed for population-level preventive effects.

Systematic Review Registration Number: PROPSERO - CRD42020188323

1. Introduction

Depression and anxiety are common, debilitating, and often lifelong conditions that frequently emerge for the first time early in life. Approximately 50% of mental disorders emerge before the age of 14, and 75% before the age of 24 (Kessler et al., 2007). Despite increased investment in research, growing awareness of mental illness, and the proliferation of treatments, rates of these common mental disorders have not decreased in decades. In fact, the opposite appears to be the case with rising rates of mental ill health, particularly among young people. For example, data from a nationally representative survey of US adolescents showed that the rates of depression in adolescents increased 52%, from 8.7% to 13.2% between the years of 2005–2017 (Bitsko et al., 2018; Twenge, Cooper, Joiner, Duffy, & Binau, 2019), and increases have been found in rates of childhood and adolescent anxiety disorders between 2007 and 2012 (Bitsko et al., 2018). Even at subthreshold levels, general psychological distress has increased, from 19% in 2012 to 24% in 2018 (Hall et al., 2019), painting a picture of worsening mental health and a growing global burden of disease (James et al., 2018).

Globally, the availability and accessibility of quality mental health care for young people simply cannot keep up with the demand (Rocha, Graeff-Martins, Kieling, & Rohde, 2015), something that has been highlighted by the COVID-19 pandemic (Courtney, Watson, Battaglia, Mulsant, & Szatmari, 2020; Young, 2020). Moreover, modelling has shown that treatment alone is insufficient to address the mental health burden, with improved access to, and quality of treatment, only able to alleviate 28% of the disease burden (Andrews, Issakidis, Sanderson, Corry, & Lapsley, 2004). Given the limitations of current treatment

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approaches, there has been increased acknowledgement that upstream prevention approaches must be seriously considered if the burden of mental ill health is to be reduced at a population level (Dopp & Lantz, 2020; Wyman, 2014).

Prevention approaches are ideally delivered before the onset of disorder, which means that delivery to children and adolescents is necessary. Prevention programs are either delivered *universally*, to a whole group or cohort, or are *targeted* towards those with increased risk factors (selective prevention) or who are showing subthreshold symptoms (indicated prevention). The ultimate goal of this approach is to reduce the occurrence of new cases of depression and anxiety, which often involves the targeting and lowering of symptoms such that the risk of disorder onset is reduced.

Educational settings have been identified as a suitable context in which prevention programs can be delivered to young people. This is because most young people attend school, and in many schools, there are often established referral pathways and ways to manage student mental health issues (Rickwood, Deane, & Wilson, 2007). For example, in Australia, more than a third of young people first receive care for a mental health problem within their school (e.g., Lawrence et al., 2015). Furthermore, many schools are increasingly taking a proactive and preventative approach to addressing student wellbeing, and with young people spending more of their waking time at school than anywhere else, the reach of school-based delivery is significant. At the same time, there has been increased acceptance from school administrators and teachers that schools are not merely a place of academic learning and achievement, but also a place where student wellbeing and mental health are key concerns (Beames et al., 2020; Fazel, Hoagwood, Stephan, & Ford, 2014; Patalay et al., 2016). For example, governments and policy makers have called for wellbeing teams and mental health professionals to be placed in all Australian schools to address the growing levels of mental illness in young people (Productivity Commission, 2020; Victorian State Government, 2020), and similar government initiatives have been called for around in other developed countries (e.g., in the UK, Department of Health and Social Care & Department for Education, 2017). Despite the increased focus on schools as a setting in which to embed prevention and early intervention programs, there are circumstances where school-based delivery may be contraindicated, such as when there is not sufficient resourcing to deliver the program, with unintended consequences such as increased to teacher stress (e.g., Gugglberger, Flaschberger, & Teutsch, 2014) and decreased program effectiveness (e.g., Barrett & Turner, 2001). Nonetheless, schools have in effect become a frontline setting for the prevention and treatment of mental health problems in children and adolescents. This has become even more pronounced during the COVID-19 pandemic where schools have needed to address increased rates of student distress, simultaneously raising important discussion about how schools can optimally support young people's mental health (Singh et al., 2020; Thakur, 2020).

In 2017, we published a systematic review and meta-analysis of the school-based prevention literature (search carried out in 2015), which involved more than 31,000 school students (Werner-Seidler et al., 2017). We found small effect sizes for the prevention of depression and anxiety immediately post-intervention ($g = 0.23$ and $g = 0.20$ respectively), with effects still detected at 12-month follow-up. Our review identified several limitations of research in the field, including an overall low quality of studies, and limited follow-up periods, which are critical in prevention research to identify a differential rate of increase in disorder over time. Since the publication of our previous meta-analysis, there has been a transformational shift in the thinking about prevention and early intervention approaches as a core way to reduce adolescent mental illness (e.g., McGorry & Mei, 2018). Specifically, there has been an increased focus the accessibility and scalability of evidence-based prevention programs, both in the research literature (e.g., Fagan et al., 2019) and from policy makers around the world (e.g., Productivity Commission, 2020; Public Health England, 2019). For example, in our previous review, only two computerised prevention programs were identified for inclusion. In the five years since this review, there has been a proliferation of evaluations of digital approaches to prevent youth mental health problems, with the goal of increasing access and scalability of effective programs globally (Naslund et al., 2017; Uhlhaas & Torous, 2019). This updated review is needed now in order to guide the field, both in terms of research and those delivering and implementing prevention programs in schools, to select programs that work and fit the needs and requirements of the contexts in which they will be delivered.

There has been one notable systematic review and meta-analysis published since our previous review, which warrants mention (Caldwell et al., 2019). In this review, the authors concluded that there was insufficient evidence to support school-based prevention programs for depression and anxiety. However, this review used a different methodological and analytic approach, employing a network meta-analysis, which essentially is a technique for comparing three or more interventions in a single analysis. This approach rests on the assumption that all included studies are similar across important factors that impact intervention effects (Chaimani, Caldwell, Li, Higgins, & Salanti, 2019). We identified a moderate level of heterogeneity in the studies conducted in this field previously (Werner-Seidler et al., 2017), and therefore have not adopted a network analysis approach for the current review.

Given the rapid growth in the development of, and interest in prevention programs, we have updated our review of the school-based prevention field, to include all studies that have been conducted to date, including recently published randomised-controlled trials (RCTs). In addition to reviewing the literature on school-based prevention programs for depression and/or anxiety in children and adolescents, we also explored whether the quality of studies improved since the last review, whether longer follow-up periods have been included in more recent studies, and whether there has been greater evaluation of digital prevention approaches.

2. Method

2.1. Protocol and registration

This review was prospectively registered with PROSPERO CRD42020188323 [Registered 25th May 2020].

2.2. Search strategy

The electronic databases PsycINFO, PubMed and the Cochrane Library were systematically searched. See Appendix A for our electronic search strategy for PsycINFO as an example. The search strategy included a combination of four key blocks of terms related to: i) mental health, ii) prevention and early intervention, iii) schools and young people, and iv) studies involving a control group. The updated search was limited to studies published in English, listed as clinical trials and published between 13th February 2015 (the day after the search from the earlier review) and the date the search was conducted, the 8th October 2020. Reference lists from recent reviews and meta-analyses in the field were hand-searched to assess whether any additional studies were relevant and assessed for inclusion. The group of studies identified in the updated search were then added to the study group identified in the previous review (Werner-Seidler et al., 2017).

2.3. Eligibility criteria

All inclusion criteria were identical to those used in the original review and outlined below.

2.3.1. Types of participants

Children or adolescents with a mean age between 5 and 19 years met inclusion criteria. As per our previous review, age was used to categorise participants according to whether they were children (<10 years), early

adolescents (10–14 years), or older adolescents (>14 years).

2.3.2. Types of interventions

Included interventions were psychological or psycho-educational programs, including individual, group and face-to-face or digital interventions. For multi-component programs, the psychological or educational component was required to constitute >75% of the program content and the young person had to be the primary recipient of the intervention (i.e., programs targeted at parents or teachers were excluded). Studies were included if they used a program designed to prevent depressive or anxiety symptoms, and/or promote mental well-being. Studies evaluating drug and alcohol, physical activity, nutritional or pharmacological interventions were not eligible for inclusion.

2.3.3. Setting

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 program was required to be school-supported with recruitment occurring within and facilitated by the school. That is, the school context could not simply provide the location for private/external programs to be delivered. 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.

2.3.4. Types of comparisons

Studies were included in which the effect of the school-based intervention was compared to either a no-intervention control group or a school-as-usual control condition, a waitlist control condition, or an attention control condition/alternate educational/psychological condition.

2.3.5. Types of outcomes

Studies were included if they reported symptoms of depression and/or anxiety at both baseline and post-intervention at a minimum. The two primary outcomes were depression and anxiety symptoms. Outcome measures needed to use 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 was 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 was not reported, AWS or SS contacted the authors of the study to obtain this information ($N = 11$ studies; 9% of included trials).

2.3.6. Length of follow-up

Reporting on follow up data was not required for inclusion in the current review. However, available follow-up data was 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. When there was more than one follow-up assessment during a particular timeframe in a particular study (e.g., 18 and 24 months), the follow-up period closest to other studies in that category was included.

2.3.7. Study design

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.

2.4. Data extraction process and management

Study characteristics and outcome measures were extracted by AWS and SS. Data entry was independently checked by AWS after it was entered into a Microsoft Excel spreadsheet. The following data was extracted: author, year of publication, program target (depression, anxiety, both depression and anxiety), prevention type (universal, selective, indicated), age range, sample size, program name, control group conditions, program format, program content, mode of delivery, number of sessions and outcome data. 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. If a program targeted both depression and anxiety, both outcomes were extracted. In cases where a study targeted either depression or anxiety and included only depression or anxiety outcomes, these outcomes were extracted. In cases where a study targeted 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. All extracted data from the updated search were combined with that from the original review.

2.5. Risk of bias in individual studies

Quality and risk of bias was assessed using the Cochrane Collaboration 'Risk of Bias' tool (Higgins & Green, 2011). We used the same tool in this updated review as we did in the original to allow for comparability across the full set of included studies. The 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: i) generation of their condition allocation sequence, ii) concealment of this sequence, iii) reporting of incomplete outcome data, iv) selective reporting of data, and v) protection against contamination. 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 (ie., randomisation at the school level) protect against this source of bias (Craig et al., 2008). Quality ratings were made independently by AWS, ALC, YP, MT and JN.[1] Risk of bias for each criterion identified above has been reported individually, rather than in an aggregated format. Studies with a low risk of bias on three or more of the five variables were classified as being high quality studies, while those rated high risk or unclear risk of bias in three or more categories were classified collectively as low quality studies, and this classification system was used to compare high quality vs low quality studies in a subgroup meta-analysis.

2.6. Statistical analyses

2.6.1. 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-intervention, which includes an adjustment to address small sample sizes (Hedges & Olkin, 1985). The 95% confidence interval around effect size was 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. 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 (e.g., Werner-Seidler

[1] Studies that were conducted by the quality raters (N=3; Calear et al., 2009, 2016; Perry et al., 2017) were independently rated by MT and JN who were not involved in any of the included studies.

et al., 2017), 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.

2.6.2. Testing homogeneity

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

2.6.3. Subgroup analyses

As in the original review, the same subgroup analyses were planned, in which prevention type, personnel delivering the program, control condition type, age at which the intervention was delivered, and content of the programs delivered was examined. Additional planned subgroup analyses were used to compare face-to-face delivered vs digitally delivered programs, and to compare high vs low quality studies across the whole set. We used the mixed-effects model and a random effects analysis to estimate effect size and compare subgroups.

2.6.4. Meta-regression

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

2.7. 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, & Minder, 1997). In cases where publication bias was indicated, Duval and Tweedie's Trim and Fill procedure (Duval & 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.

3. Results

3.1. Study selection

See Fig. 1 for the PRISMA flowchart illustrating the inclusion of studies (see Appendix B for PRISMA of newly identified studies in the updated search between 2015 and 2020). A total of 8568 articles were identified, from which duplicate articles ($n = 692$) were removed. The remaining titles and abstracts were screened by the first two authors to determine their suitability. Of the abstracts, 7573 were excluded. Two authors (SS, AWS or AWS, YP) then independently screened the full text articles of the remaining 303 records. Disagreements were resolved through discussion, and consultation with ALC and HC if needed. This resulted in 130 articles being included in the current review, of which 118 were unique trials, with 12 articles reporting follow-up data only.

3.2. Study characteristics

See Table 1 for characteristics of included studies. There were 118 unique studies identified in the current review, which included a total of 45,924 participants ($n = 24,409$ in the prevention intervention conditions, and $n = 21,533$ in the control conditions). Of the 118 studies, 54 were studies focused on preventing depression ($n = 21,760$), 34 were focused on preventing anxiety ($n = 12,547$), and 30 were of mixed depression/anxiety prevention programs ($n = 11,635$). Sample sizes of studies were variable, ranging from 21 participants (Hains & Szyjakowski, 1990) to 2512 participants (Araya et al., 2013), with a median of 209 and a mean of 386 participants per study.

3.3. Sample type

Overall, just over half (57%) of the studies were of a universal prevention program ($n = 67$), one third (33%) of studies were of indicated prevention programs ($n = 39$ studies; programs for those with symptoms), eight studies (7%) evaluated a selective prevention program (programs for those with risk factors), three studies were blended selective/indicated programs, and one was a blended universal/indicated program. Of the 54 depression prevention programs, 25 were delivered universally (46%), 22 of the studies were indicated programs (41%), five were of selective programs (9%), and two (4%) were mixed (1 selective/indicated; 1 universal/indicated). For the anxiety prevention programs, 21 of the 34 programs were universally delivered (62%), 10 were indicated programs (29%), two were selective (6%), and one was mixed selective/indicated (3%). Of the 30 mixed depression and anxiety prevention programs, 21 were of universal programs (70%), seven were of indicated programs (23.3%), one was selective (3.3%) and one was mixed selective/indicated (3.3%).

'Risk' was defined in the 12 studies that involved a selective sample as: parental psychopathology (Rasing et al., 2018), parental divorce (Pedro-Carroll & Cowen, 1985), exposure to community or political violence (Cooley-Strickland et al., 2011; Tol et al., 2008), living in a low income area (both studies reported in Cardemil et al., 2002; Kindt et al., 2014), personality risk factors including anxiety sensitivity and a negative attributional style (Arnarson & Craighead, 2009; Balle & Tortella-Feliu, 2010; Castellanos & Conrod, 2006), and conduct or behavioural problems (King & Kirschenbaum, 1990). Given the relatively small number ($n = 11$) and proportion (9%) of studies taking a selective prevention approach, these were combined with indicated trials and collectively referred to as 'targeted' trials for the meta-analysis. This resulted in 51 studies being classified as 'targeted' (43%), and 67 studies as 'universal' (57%), resulting in a relatively even number of trials for each prevention type in the meta-analysis.[2]

3.4. Participant group

Almost a quarter ($n = 28$; 24%) of the studies in the review delivered the intervention to children aged ten years or younger. Eighteen of these 28 programs delivered to young children (64%) were targeted to anxiety only, seven (25%) were focused on mixed anxiety and depression, and three (11%) were focused exclusively on depression prevention. Thirty-seven studies (31%) overall involved early adolescents, with an age range of 10–14 years. The focus of the prevention programs for early adolescents was more variable relative to children, with nine studies (24%) focusing on anxiety, eight (21%) focusing on mixed anxiety and depression, and just over half ($n = 20$ studies, 54%) focusing on preventing depression. The largest proportion of included studies ($n = 53$ studies, 45%) were focused on preventing mental illness in adolescents aged between 14 and 19 years. A majority of these ($n = 31$ studies, 59%) were focused on preventing depression, seven (13%) were anxiety prevention studies, and the remaining 15 (28%) were mixed anxiety and depression studies. A small number of studies ($n = 7$ studies, 6%) screened and assessed participants using diagnostic interviews prior to study entry and excluded those who met criteria for a clinical disorder. This indicates that most studies took a pragmatic approach to participant inclusion, rather than one of true prevention involving the exclusion of those who were already experiencing disorder.

[2] For the meta-analysis we repeated the primary analyses for depression and anxiety at post-intervention excluding the selective interventions from the targeted category and found that the effect sizes were virtually unchanged (e.g., depression - $n = 39$ $g = 0.30$, 95%CI: 0.22–0.38; anxiety - $n = 20$, $g = 0.24$, 95% CI: 0.13–0.34).

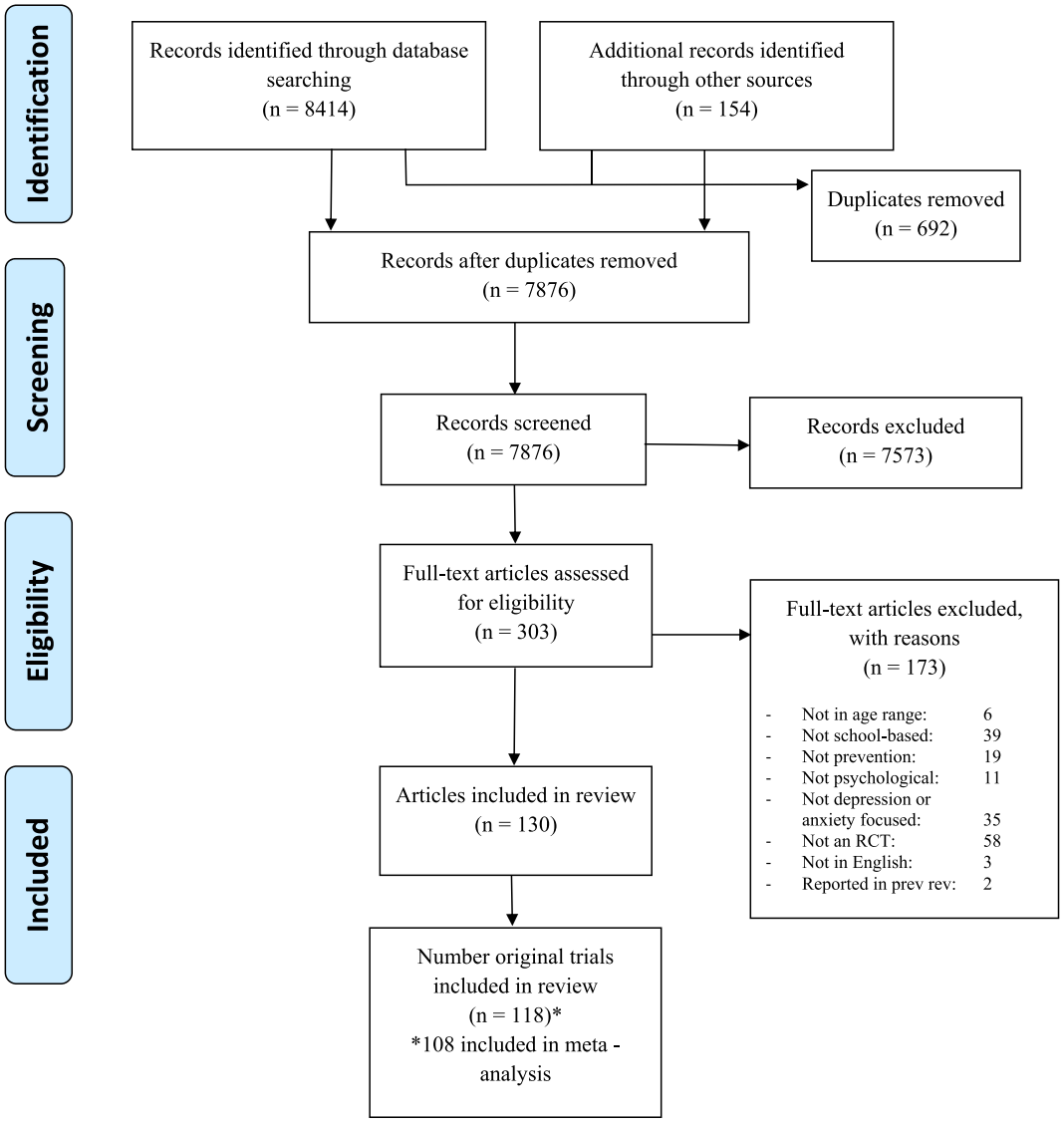

Fig. 1. PRISMA flow chart of randomised controlled trials.

3.5. Randomisation

The largest proportion of studies randomised students at the individual level (n = 51 studies, 43%), followed by randomisation performed at the school level (n = 41 studies, 35%). Twenty-three studies (19%) randomised at the class level and two studies (2%) randomised at the grade level, while the final study randomised by county (1%).

3.6. Program content

Most studies (n = 91, 77%) evaluated a prevention program based on cognitive behavioural therapy (CBT). Three studies involved a CBT arm or an approach that involved some elements of CBT; specifically, one study included a CBT condition and a Cognitive Bias Modification condition (CBM), one study involved a CBT condition and Positive Search Training (PST; involves training individuals to attend to positive stimuli and to ignore threatening stimuli) and one involved a blended CBT and creative-expressive experiential therapy. Five studies (4%) tested a blended interpersonal psychotherapy (IPT)/CBT program and three studies (3%) involved standalone IPT programs. One study tested two separate components of CBT programs – behavioural activation and emotion regulation training. Three studies (3%) tested mindfulness-based approaches and two studies evaluated a personality-focused intervention (2%). Other therapeutic approaches included a dialectical behavior therapy (DBT) program, a wellbeing program, an information-motivational-behavioural (IMB) skills-based program, a psychoeducational program, a coping and support program, and a positive psychology program.

3.7. Mode of delivery

More than half of the studies (n = 74 studies, 63%) included programs that were delivered by personnel external to the school environment (e.g., researchers, graduate students, psychologists), while about a third of studies (n = 37, 31%) were delivered by school staff (e.g., teachers, school counsellors). Notably, four of the programs delivered by school staff were digital programs which were supported by the school staff (e.g., classroom teachers facilitated programs which were accessed on school computers, often involved school counselling staff). Similarly, for those external to the school environment, three programs were delivered digitally, usually involving personnel such as research assistants or psychologists in the classroom who provide instructions while teachers remained present but uninvolved (e.g., Waters et al., 2019). The final seven studies (6%) involved both internal and external

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

Study	Country	Sample 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 & Craighead, 2009, 2011	Iceland	Targeted (selective/ indicated)	14–15 years	171	–	NI	CBT	School MHP	14	CAS	?	?	–	?	–
Briere, Reigner, Yale-Souliere, & Turgeon, 2019^^	Canada	Targeted (indicated)	14–18 years	74	Blues Program	AC	CBT	Grad + School MHP	6	SCID-IV	?	+	?	?	–
Calvete et al., 2019^^	Spain	Universal	12–17 years	456	–	AC	ITPI	MHP	1	CES-D	?	+	+	?	–
Cardemil, Reivich, & Seligman, 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 & 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, & Sheeber, 1993 (Study 1)	America	Universal	NR	622	PE	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	?	?	?	?	–
David, Cardos, & Matu, 2019^^	Romania	Universal	10–16 years	165	REThink	WL	CBT + EDU	Digital (MHP supported)	7	EATQ-R	?	?	+	+	–
de Jong-Heesen et al., 2020^^	The Netherlands	Targeted (indicated)	12–16 years	130	OVK 2.0	AC	CBT	School MHP + MHP	8 + 1 booster	CDI-2	+	+	+	–	–
Gaete et al., 2016*	Chile	Targeted (indicated)	14–19 years	342	YPSA-R	NI	CBT	MHP	8	BDI-II (Anx: RCADS)	+	+	?	?	–
Garaigordobil, Jaureguizar, & Bernaras, 2019^^	Spain	Universal	7–10 years	420	Pozik-Bizi	AC	CBT	Teacher	18	CDS	?	?	?	?	–
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, Garber, Ciesla, Young, & Mufson, 2007	America	Universal	14–15 years	380	CWSC	AC + NI	CBT + IPT	MHP	8	CDI	+	?	+	?	–
Kindt, Kleinjan, Janssens, & Scholte, 2014	Holland	Targeted (selective)	11–16 years	1440	OVK	NI	CBT	Teacher	16	CDI	+	+	+	+	–
King & Kirschenbaum, 1990	America	Targeted (selective)	8 years	127	WEI	NI	SS	MHP	24	CDRS-R	?	?	–	–	–
Lamb, Puskar, Sereika, & Corcoran, 1998*	America	Targeted (indicated)	14–19 years	46	–	NI	CBT	School MHP	8	RADS	?	?	–	?	–
Livheim et al., 2015 (Study 1)	Australia	Targeted (indicated)	12–17 years	58	–	NI	ACT	Grad or School MHP + MHP	8	RADS-2	+	?	?	?	–
McCarty, Violette, Duong, Cruz, & McCauley, 2013; Duong, Cruz, King, Violette, & McCarty, 2016^^	America	Targeted (indicated)	11–15 years	120	PTA	AC	CBT	MHP	12	MFQ	+	?	+	?	–
*McCarty et al., 2011	America	Targeted (indicated)	NR	67	PTA	NI	CBT	MHP + Grad	12	MFQ	?	?	–	?	–
Miu & Yeager, 2015*	America	Universal	14–15 years	599	–	NI	ITPI	Grad or Digital	1	CDI-S	+	+	?	?	–
Merry, McDowell, Wild, Bir, & Cunliffe, 2004	New Zealand	Universal		392	RAP-KIWI	AC	CBT + IPT	Teacher	11	BDI-II	+	+	?	?	–

(*continued on next page*)

Table 1 (*continued*)

Study	Country	Sample type	Age range	N	Program	Control	Program content	Mode of delivery	Number sessions	Outcome measure	Quality Ratings				
											a	b	c	d	e
			13–15 years												
Ooi et al., 2016^^	Australia	Targeted (indicated)	10–17 years	82	TRT	WL	CBT	Grad + School MHP	8	DSRS	+	?	+	?	+
*Pattison and Lynd-Stevenson, 2001	Australia	Universal	9–12 years	66	PPP	AC + NI	CBT	MHP	11	CDI (Anx: STAI)	?	?	?	?	–
Perry et al., 2017^^	Australia	Universal	16–17 years	540	SPARX-R	AC	CBT	Digital (teacher-supported)	7	MDI	?	+	+	+	+
Poppelaars et al., 2016^^	The Netherlands	Universal	11–16 years	208	OVK, SPARX	NI	CBT	Digital vs. MHP	7–8	RADS-2	+	+	+	–	–
Pössel, Horn, Groen, & Hautzinger, 2004	Germany	Universal	13–14 years	347	L-T	NI	CBT	MHP	10	CES-D	–	?	+	?	–
Pössel, Seemann, & Hautzinger, 2008; Pössel, Adelson, & Hautzinger, 2011	Germany	Universal	12–13 years	301	L&L	NI	CBT	Grad + MHP	10	SBB-DES	?	+	?	?	–
Pössel, Martin, Garber, & Hautzinger, 2013	America	Universal	14–16 years	518	L&L	AC + NI	CBT	Grad + MHP	10	CDI	?	?	?	?	–
Puskar, Sereika, & Tusaie-Mumford, 2003	America	Targeted (indicated)	14–18 years	89	TKC	NI	CBT	MHP	10 + 1 booster	RADS	+	?	+	?	–
Quayle, Dziurawiec, Roberts, Kane, & Ebsworthy, 2001	Australia	Universal	11–12 years	47	OLP	NI	CBT	MHP	8	CDI	?	?	–	?	–
Raes, Griffith, Van der Gucht, & Williams, 2014	Belgium	Universal	13–20 years	408	MI	NI	MI	MHP	8	DASS-DEP	+	+	+	?	–
Roberts, Kane, Thomson, Bishop, & Hart, 2003; Roberts, Kane, Bishop, Matthews, & Thomson, 2004	Australia	Targeted (indicated)	11–13 years	189	PPP	NI	CBT	MHP	12	CDI (Anx: RCMAS)	?	?	?	?	+
Rivet-Duval, Heriot, & Hunt, 2011	Mauritius	Universal	12–16 years	160	RAP	WL	CBT + IPT	Teacher	11	RADS-2	?	?	–	?	–
Rohde, Stice, Shaw, & Gau, 2014; 2015	America	Targeted (indicated)	13–19 years	378	–	AC + NI	CBT; Biblio	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, & Hunt, 2014	Australia	Universal	9–14 years	210	RAP + PIR	AC + NI	CBT + IPT	Grad	20	RADS-2	?	+	+	?	–
Sanchez-Hernandez, Mendez, Ato, & Garber, 2019*^^	Spain	Targeted (indicated)	12–14 years	89	Smile	NI	CBT	Grad	11	CDI	+	+	?	?	–
Sheffield et al., 2006	Australia	Targeted (indicated) + Universal	13–15 years	1360	–	NI	CBT	Teacher	8	CDI (Anx: SCAS)	+	+	+	?	+
Spence, Sheffield, & 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, & Rohde, 2007	America	Targeted (indicated)	15–22 years	225	BG	AC + WL	CBT	Grad	4	BDI	–	?	?	?	–
Stice, Rohde, Seeley, & Gau, 2008; Stice, Rohde, Gau, & Wade, 2010	America	Targeted (indicated)	14–15 years	341	–	AC + NI	CBT	Grad	6	K-SADS	+	?	+	?	+
Tak, Lichtwarck-Aschoof, Gillham, Van Zundert, & Engels, 2016^^	The Netherlands	Universal	13–14 years	1341	OVK	NI	CBT	MHP	16 + 1 booster	CDI	?	+	+	–	+
Whittaker et al., 2017^^	New Zealand	Universal	13–17 years	855	MEMO	AC	CBT	Digital (researcher & school MHP supported)	15 text messages	CDRS-R	+	+	–	+	–
Wijhoven et al. 2014	Holland	Targeted (indicated)	11–15 years	102	OVK	NI	CBT	MHP	8	CDI	+	+	+	+	+
Woods & Jose, 2011	New Zealand	Targeted (indicated)	NR	56	ACE-Kiwi	NI	CBT	School MHP	8	CDI	?	?	–	?	–

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Table 1 (*continued*)

Study	Country	Sample type	Age range	N	Program	Control	Program content	Mode of delivery	Number sessions	Outcome measure	Quality Ratings				
											a	b	c	d	e
Young, Mufson, & Davies, 2006	America	Targeted (indicated)	11–16 years	41	IPT-AST	AC	IPT	Grad + Researcher	10	CES-D	+	?	+	?	–
Young, Mufson, & Gallop, 2010	America	Targeted (indicated)	13–17 years	57	IPT-AST	NI	IPT	MHP	10	CES-D	+	–	+	+	–
Young et al. 2016	America	Indicated	12–16 years	186	IPT-AST	AC	IPT	Grad + MHP + School MHP	11 + 4 booster	CES-D	+	?	+	?	–
Yu & Seligman, 2002	China	Targeted (indicated)	8–15 years	220	POP	NI	CBT	Teacher	10	CDI	?	?	?	?	–
Anxiety Studies															
Ab Ghaffar, Sidik, Ibrahim, Awang, & Rampal, 2019^^	Malaysia	Universal	10–11	461	–	WL	SR	Grad	4	RCADS	+	?	?	?	+
Anticich, Barrett, Silverman, Lacherez, & Gillies, 2013	Australia	Universal	4–7 years	488	FRIENDS	AC + WL	CBT	Teacher	10	PAS	?	+	+	?	+
Aune & Stiles, 2009	Norway	Universal	11–15 years	1439	NUPP-SA	NI	CBT	MHP	7	SPAI-C	?	?	?	?	+
Balle & Tortella-Feliu, 2010	Spain	Targeted (selective)	11–17 years	145	FRIENDS	WL	CBT	Grad + MHP	6	SCAS	?	?	–	?	–
Barrett & Turner, 2001	Australia	Universal	10–12 years	489	FRIENDS	AC + NI	CBT	Teacher vs MHP	10 + 2 booster	SCAS (Dep: CDI)	?	?	–	?	+
Barrett, Lock, & Farrell, 2005*	Australia	Universal	9–16 years	692	FRIENDS	NI	CBT	Grad + MHP	10 + 2 booster	SCAS (Dep: CDI)	?	?	–	?	+
Berger, Pat-Horenczyk, & Gelkopf, 2007	Israel	Universal	7–11 years	142	OTT	WL	CBT	Teacher	8	SCARED-GAD	–	?	+	?	–
Bouchard, Gervais, Gagnier, & Loranger, 2013	Canada	Universal	9–12 years	59	DHS	WL	CBT	Grad + MHP	10	MASC	?	?	+	?	–
Calear et al., 2016^^	Australia	Universal	12–18 years	1767	e-Couch	WL	CBT	Digital (teacher vs. MHP supported)	6	SCAS	+	+	+	+	+
Cooley-Strickland, Griffin, Darney, Otte, & Ko, 2011	America	Targeted (selective/ indicated)	8–12 years	93	FRIENDS	WL	CBT	Grad	13	RCMAS	?	?	?	?	–
Dadds, Spence, Holland, Barrett, & Laurens, 1997; Dadds et al., 1999	Australia	Targeted (indicated)	7–14 years	128	CK	NI	CBT	MHP	10 + 2 booster	RCMAS	?	?	?	?	+
Essau, Conradt, Sasagawa, & 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, & Jaknavorian, 1989 (Study 2)	Canada	Universal	13–14 years	113	–	AC	CBT	Teacher	11	STAI	?	?	–	?	–
Hunt, Andrews, Crino, Erskine, & Sakashita, 2009	Australia	Targeted (indicated)	11–13 years	259	FRIENDS	NI	CBT	School MHP + Teacher	10 + 2 booster	SCAS (Dep: CDI)	?	?	+	?	+
Keogh, Bond, & Flaxman, 2006	UK	Universal	15–16 years	80	SMI	NI	CBT	MHP	10	TA	?	?	–	?	–
Kiselica, Baker, Thomas, & Reedy, 1994	America	Targeted (indicated)	NR	48	SIT	NI	CBT	MHP	8	STAI	–	?	?	?	–
Kozina, 2020*^^	Slovenia	Universal	9–10 years	85	FRIENDS	NI	CBT	MHP	10	AN-UD	+	?	?	?	–
Lock & Barrett, 2003; Barrett, Farrell, Ollendick, & Dadds, 2006	Australia	Universal	9–16 years	737	FRIENDS	WL	CBT	Grad + MHP	10+ 2 booster	SCAS (Dep: CDI)	?	?	–	?	+
McLoone & Rapee, 2012	Australia	Targeted (indicated)	7–10 years	152	CK	WL	CBT	School MHP	10	SCAS	+	+	+	?	–
Miller, Short, Garland, & Clark, 2010	Canada	Universal	7–12 years	116	TWD	WL	CBT	Teacher	8	MASC	?	+	–	?	+
Miller et al., 2011	Canada	Universal	7–13 years	533	FRIENDS-CE	WL	CBT	Teacher + School MHP	9	MASC	?	+	–	?	+

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Table 1 (*continued*)

Study	Country	Sample type	Age range	N	Program	Control	Program content	Mode of delivery	Number sessions	Outcome measure	Quality Ratings				
											a	b	c	d	e
Miller et al., 2011 (Study 1)	Canada	Targeted (indicated)	9–12 years	191	FRIENDS	WL	CBT	Teacher + School MHP	9	MASC	?	+	+	?	+
Miller, Laye-Gindhu, Liu, et al., 2011 (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	CK	WL	CBT	MHP + School MHP	8	SCAS	?	?	+	?	+
Pedro-Carroll & Cowen, 1985	America	Targeted (selective)	9–12 years	72	CODIP	WL	CBT	School MHP, MHP, Grad	10	STAI	?	?	?	?	–
*Pina et al., 2020	America	Indicated	NR	109	SPEI	AC	CBT	School MHP	6	MASC	?	?	+	?	–
*Rodgers and Dunsmuir, 2015	Ireland	Universal	12–13 years	62	FRIENDS	WL	CBT	MHP	10	SCAS	?	?	?	?	–
Ruttledge et al., 2016^^	Ireland	Universal	9–13 years	709	FRIENDS	WL	CBT	Teacher	10	SCAS	+	?	?	?	+
Sportel, du Hullu, de Jong, & Nauta, 2013; *de Hulle et al., 2017	The Netherlands	Indicated	12–16 years	240	–	NI	CBM and CBT	Digital + MHP	10–20	RCADS	+	?	+	+	+
Stallard et al., 2014	England	Universal	9–10 years	1448	FRIENDS	AC + NI	CBT	Teacher vs MHP	9	RCADS	+	+	+	–	+
van Starrenburg et al., 2017	The Netherlands	Indicated	7–13 years	141	Coping Cat	WL	CBT	MHP	12	SCAS	+	+	+	–	+
Waters et al., 2019^^	Australia	Universal	7–11 years	303	–	NI	CBT	Digital (researcher-supported) vs. MHP	8	SCAS (Dep: SMFQ-C)	+	+	+	?	–
Depression & Anxiety Studies															
Ahlen, Hursti, Tanner, Tokay, & Ghaderi, 2018; Ahlen, Lenhard, & Ghaderi, 2019^^	Sweden	Universal	8–11 years	349	FRIENDS	WL	CBT	Teacher	10	Dep: CDI-S Anx: SCAS	+	+	+	+	+
Brown et al., 2019^^	England	Universal	16–19 years	155	DISCOVER	WL	CBT	MHP	1 + optional goal review	Dep: MFQ Anx: RCADS	+	+	+	+	+
Burckhardt et al., 2015*^^	Australia	Universal	12–18 years	338	BITEBACK	AC	PP	Digital (teacher-supported)	4–6	Dep: DASS-21 Anx: DASS-21	+	+	+	?	–
Burckhardt et al., 2018^^	Australia	Universal	14–16 years	96	–	WL	DBT	MHP	6	Dep: CES-D 8 Anx: PROMIS	+	?	?	?	–
Calear et al. 2009	Australia	Universal	12–17 years	1477	MG	WL	CBT	Digital (teacher supported)	5	Dep: CES-D Anx: RCMAS	+	+	+	?	+
*Garcia-Escalera et al., 2020	Spain	Universal	14–16 years	151	UP-A	WL	TD	Grad + Researcher	9	Dep: CDN Anx: EAN	+	–	+	?	–
Gillham, Hamilton, Freres, Patton, & 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	?	?	?	?	–
*Haines et al. 1994	America	Universal	NR	21	SIT	WL	CBT	Grad + MHP	13	Dep: RADS Anx: STAI	?	?	–	–	–
*Hains and Ellmann, 1994	America	Universal	16–17 years	21	SIT	WL	CBT & AM	MHP	9	Dep: BDI Anx: STAI	?	?	?	?	–
Johnson, Burke, Brinkman, & Wade, 2016^^	Australia	Universal	12–14 years	308	.b	NI	MI	MHP	8	Dep: DASS-21 Anx: DASS-21	+	?	+	?	–
*Johnson et al., 2017	Australia	Universal	12–14 years	555	.b	NI	MI	MHP	9	Dep: DASS-21 Anx: DASS-21	+	?	+	?	–
*Johnstone, Rooney, Hassan, & Kane, 2014; Rooney, Hassan, Kane, Roberts, & Nesa, 2013; Rooney et al., 2013	Australia	Universal	9–10 years	910	AOP-PTS	NI	CBT	Teacher	10	Dep: CDI Anx: SCAS	?	?	–	?	–
Johnstone, Middleton, Kemps, & Chen, 2020^^	Australia	Universal	8–13 years	295	–	NI	ER + BA	Grad + MHP	8	Dep: RCAD Anx: RCADS	+	+	?	?	+
Jordans et al., 2010	Nepal	Targeted (indicated)	11–14 years	325	–	WL	CBT + CEET	MHP	15	Dep: DSRS Anx: SCARED	+	+	+	?	+
Kraag, Van Breukelen, Kok, & Hosman, 2009	Netherlands	Universal	NR	1467	LYLF	NI	CBT	Teachers	8 + 5 booster		?	?	+	?	+

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Table 1 (*continued*)

Study	Country	Sample type	Age range	N	Program	Control	Program content	Mode of delivery	Number sessions	Outcome measure	Quality Ratings				
											a	b	c	d	e
										Dep: SDIC Anx: STAI					
Livheim et al., 2015 (Study 2)	Sweden	Indicated	14–15 years	32	–	NI	ACT	Grad	8	Dep: DASS-21 Anx: DASS-21	+	?	+	?	–
Lowry-Webster, Barrett, & Dadds, 2001; Lowry-Webster, Barrett, & Lock, 2003	Australia	Universal	10–13 years	594	FRIENDS	NI	CBT	Teacher	10 + 2 booster	Dep: CDI Anx: SCAS	?	?	–	?	+
Makover et al., 2019*^^	America	Indicated	13–14 years	497	HSTP	NI	Coping skills	Grad	12 + 4 booster	Dep: SMFQ Anx: HSQ	+	+	+	?	–
Manassis et al., 2010	Canada	Targeted (indicated)	8–12 years	148	FC	AC	CBT	Grad + MHP	12	Dep: CDI Anx: MASD	+	+	–	?	–
Martinsen et al., 2019^^	Norway	Indicated	8–12 years	795	EMOTION	NI	TD	School MHP + MHP	20 + 2 booster	Dep: SMFQ-C Anx: MASC-C	+	+	+	?	+
Nobel, Manassis, & Wilansky-Traynor, 2012	Canada	Targeted (indicated)	8–11 years	78	FC	AC	CBT	MHP	12	Dep: CDI Anx: MASC	?	?	?	?	–
Pophillat et al., 2016^^	Australia	Universal	6–9 years	206	AOP: FF	NI	CBT	Teacher	10	Dep: CDI Anx: SCAS	?	?	+	?	–
Rasing et al., 2018^^	The Netherlands	Indicated + selective	11–14 years	142	Een Sprong Vooruit	WL	CBT	Grad + MHP	6	Dep: CDI-2 Anx: SCAS	?	?	–	?	+
Roberts et al., 2010	Australia	Universal	11–13 years	496	AOP	NI	CBT	Teacher	20	Dep: CDI Anx: RCMAS	?	?	–	?	+
Ruini, Belaise, Brombin, Caffo, & 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	?	?	–	?	–
Tol et al., 2008*	Indonesia	Targeted (selective)	NR	403	–	NI	CBT	MHP	15	Dep: DSRS Anx: SCARED	+	?	+	?	+
Wong, Kady, Mewton, Sunderland, & Andrews, 2014	Australia	Universal	14–16 years	976	TWU-S	NI	CBT	Digital (teacher supported)	6	Dep: PHQ-9 Anx: GAD-7	?	?	–	?	+

Note. * = Not included in meta-analysis; ^^ = new study included in revised review. **Programs –** ITFA = I Think Feel Act; PRP-A = Penn Resiliency Program; PM-CBI = Personality-Matched Cognitive Behavioural Intervention; PE = Psychoeducation; CBI = Behavioural Intervention; CWSC = Coping with Stress Course; OVK = Op Volle Kracht; YPSA = I Yo (I) Pienso (Think) Siento (Feel) Actuo (Act); Pozik-Bizi = Live Happily; PRP-A = Penn Resiliency Program for Adolescents; WEI = Wisconsin Early Intervention; PTA = Positive Thoughts and Actions; RAP-Kiwi = Resourceful Adolescent Program (Kiwi Adaptation); TRT = Teaching Recovery Techniques; PPP = Penn Prevention Program; L-T = LISA-T; L&L = LARS&LISA; 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; SPEI = Streamlined Prevention and Early Intervention; MG = MoodGYM; UP-A = Unified Protocol for Transdiagnostic Treatment of Emotional Disorders in Adolescents; .b = Dot be mindfulness program; AOP: PTS= Aussie Optimism Program: Positive Thinking Skills Program; LYLF = Learn Young Learn Fair; HSTP = High School Transition Program; FC = Feelings Club; AOP: FF = Aussie Optimism Program: Feelings and Friends; Een Sprong Vooruit = A Leap Forward; 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, ITPI = Incremental Personality Theory Interventions; EDU = Educational; IPT = Interpersonal Psychotherapy; SS = Social Skills; ACT = Acceptance and Commitment Therapy; MI = Mindfulness Interventions; Biblio = Bibliotherapy; SR = Stimulus Response Intervention; CBM = Cognitive Bias Modification; PP = Positive Psychology: DBT = Dialectical Behaviour Therapy; TD = Transdiagnostic; AM = Anxiety Management; ER = Emotion Regulation; BA = Behavioural Activation; CEET = Creative Expressive Experiential Therapy. **Depression outcome measures –** BDI-II = Beck Depression Inventory; CAS = Child Assessment Scale (Diagnostic); SCID-IV = Structured Clinical Interview for DSM-IV Disorders (Diagnostic); CDI = Children's Depression Inventor; BSI-DEP = Brief Symptom Inventory – Depressive Subscale; CES-D = Centre for Epidemiological Studies – Depression Scale; EATQ-R = Early Adolescent Temperament Questionnaire – Revised; CDRS = Child Depression Rating Scale; RADS = Reynolds Adolescent Depression Scale, MFQ = Mood and Feelings Questionnaire; DSRS = Depression Self-Rating Scale; 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 = Kellner's Symptom Questionnaire; PHQ-9 = Patient Health Questionnaire-9. **Anxiety outcome measures –** RCADS - Revised Children's Anxiety & Depression Scale; PAS = Preschool Anxiety Scale; SPAI-C = Social Phobia and Anxiety Inventory for Children; SCAS = Spence Children's Anxiety Scale; SCARED = Screen for Child Anxiety Related Disorders; MASC - Multidimensional Anxiety Scale for Children, RCMAS – Revised Child Manifest Anxiety Scale; STAI = State Trait Anxiety Inventory; TA = Test Anxiety Scale; AN-UD = AN-UD Anxiety Scale; HSQ = High School Questionnaire; EAN = Anxiety Scale for Children (Escala de ansiedad para Ninos); 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.

staff to deliver prevention programs (one was digital).

3.8. Program format

Most studies (n = 105, 89%) were delivered to groups of students, most frequently in classrooms. Four studies involved both group and individual components (3%). This updated review saw a significant increase in the number of digital programs being evaluated, with a total of 8 programs included (7%), but it is worth noting that 16% of the programs (n = 6) in the updated version of this review were digital, compared to the two programs (2%) included in the original review.

3.9. Program sessions

The length of the programs included in the review ranged from single session interventions (n = 3), to 40 sessions (n = 1), with most programs (n = 77, 65%) being delivered in 8–12 sessions. Some interventions were briefer, ranging from 2 to 7 sessions (n = 21, 18%), while others were longer, spanning 13–24 sessions (n = 16, 14%). In terms of study duration, most sessions ran for 45–60 min (n = 65 studies, 55%, median = 60 min), although some were briefer running from 25 to 45 min (n = 7 studies, 6%), while a quarter of the included sessions went for longer, between 75 and 120 min (n = 28 studies, 24%). One intervention was delivered in a full day workshop, while the remaining 17 studies did not report on session duration. Very few programs offered booster or follow up sessions (n = 16, 14%). Of the 16 programs that did involve booster sessions, 4 studies (25%) offered a single session, 9 studies (56%) offered two sessions, two studies (13%) delivered four sessions and the final program involved up to five sessions.

3.10. Control groups

Approximately half (n = 56) of the included studies (47.5%) involved no-intervention control groups, against which to compare the prevention intervention. No-intervention control groups were often referred to as class-as-usual but do not involve any intervention over and above what is usually received at school. Thirty-three studies (28%) compared the program to a waitlist control condition (essentially another form of a no-intervention control group), while 17 studies (14.5%) employed an active control group. Although this number is low, 25% of studies conducted in the last five years included an active control condition, compared to the 10% previously, suggesting there has been a trend towards active control groups. The remaining 12 studies (10%) included more than one comparison group: 10 including a no-intervention and active control condition, and two involving a wait-list and active control condition.

3.11. Length of follow-up

For inclusion, studies were required to include a post-intervention assessment. Sixty-two studies included a short-term follow-up assessment (0–6 months), while 47 studies involved a medium-term follow-up assessment between 6 and 12 months after the intervention, and 19 studies followed participants beyond 12 months for a long-term follow-up. Of those in the long-term category, the average follow-up period across the study set was 27.25 months, with a median and mode of 24 months. Some studies involved more than one follow-up period.

3.12. Outcome measures

The most frequently used measure to assess depressive symptoms was the Children's Depression Inventory (CDI; Kovacs, 1992), used in 32% of the studies measuring depression as an outcome. The Centre for Epidemiological Studies – Depression Scale (CES-D; Radloff, 1977) was used in 12% of studies, followed by the Reynolds Adolescent Depression Scale (RADS; 9%; Reynolds, 2010) and the Depression Anxiety and Stress Scale – Depression Subscale (DASS; 6%; Lovibond & Lovibond, 1995). For studies that measured anxiety, the most frequently used scale was the Spence Children's Anxiety Scale (SCAS; 30%; Spence, 1998), followed by the Multidimensional Anxiety Scale for Children (MASC; 15%; March, Parker, Sullivan, Stallings, & Conners, 1997), the DASS – Anxiety Subscale (9%) and finally the Revised Children's Manifest Anxiety Scale (RCMAS; 12%; Reynolds & Richmond, 1978).

3.13. Risk of bias

The quality of the studies reported varied significantly across the studies (see Table 1 for individual study quality ratings). There was evidence of selection bias. Four studies (3%) did not use a randomisation approach that ensured comparability between groups and more than half of the studies (n = 64, 54%) did not provide enough information to evaluate how groups were randomised. A substantial number of studies (n = 50, 42%) reported that the allocation sequence had been adequately generated without risk of bias. The other source of selection bias, allocation concealment, was reported as high in three studies (3%). Approximately 64% of studies (n = 75) did not report enough information to ascertain whether intervention allocations could have been foreseen prior to or during enrolment, with about one third of studies (n = 40) reporting methods ensuring that allocation was concealed to an acceptable standard.

Risk of bias of attrition was generally reported with more detail to allow for assessment, with 55 studies (47%) indicating low risk of bias for addressing incomplete data meaning that the proportion of missing data was comparable across study groups. Twenty-six studies (22%) reported systematic differences in the level of missing data between study conditions, and 37 studies (31%) did not provide enough information to assess level of risk. Bias from selective reporting was challenging to assess, with most studies (n = 96, 81%) not reporting enough information to make an assessment. Ten studies (8%) clearly indicated a high risk of bias, where the statistics reported in the paper did not match that of the study protocol or a-priori registration details. There were 12 studies (10%) that clearly indicated low risk of bias in terms of selective reporting, with symmetry across intended and completed analyses. Finally, risk of contamination was able to be assessed as either low or high risk across all studies. Forty-two studies (36%) reported low risk of contamination by randomising students to condition at the school level, while the remaining 76 studies (64%) used individual or class level randomisation, meaning that the risk of cross-condition contamination was higher. Although the contamination risk was mitigated in these 43 studies, just under half of these (19; 45%) reported making adjustments using the intraclass correlation coefficient, which means that effects of clustering cannot be ruled out in the remaining 23 studies.

As outlined in the Method, studies were divided into high- or low-quality studies overall, with the high-quality criteria being set at having three or more risk of bias categories which were judged as having low risk of bias. There were 33 studies (28%) judged as high quality on this basis, with four studies being judged as having low risk of bias on all five dimensions (Ahlen et al., 2018; Brown et al., 2019; Calear et al., 2016; Wijnhoven, Creemers, Vermulst, Scholte, & Engels, 2014). The remainder of the studies (n = 85, 72%) were classified as low-quality studies.

4. Synthesis of results

4.1. Primary analysis

Meta-analyses using random effects models were conducted to compare the intervention and control groups on the primary outcomes (depression and anxiety) at post-intervention and follow-up (see Figs. 2

and 3 for forest plots of depression and anxiety symptoms at post-intervention, respectively). The effect size at post-intervention for depression was small (n[3] = 101, g = 0.21, 95%CI: 0.17–0.24), with moderate heterogeneity (I^2 = 47, 95%CI: 0.33–0.58). The effect size for depression was also small at short-term (n = 60, g = 0.17, 95%CI: 0.13–0.22); medium-term (n = 48, g = 0.10, 95%CI: 0.06–0.13); and long-term (n = 20, g = 0.10, 95%CI: 0.05–0.15) follow-up. For anxiety, the overall effect size at post-intervention was small (n = 72, g = 0.18, 95%CI: 0.12–0.26), with moderate heterogeneity (I^2 = 46, 95%CI: 0.29–0.59). The effect size for anxiety was slightly higher at the first two follow-up periods, with conventionally small effects at short- (n = 29, g = 0.19, 95%CI: 0.12–0.26) and medium-term (n = 31, g = 0.23, 95% CI: 0.10–0.35) follow-up. At long-term follow-up, the effect size for anxiety was small (n = 8, g = 0.11, 95%CI: 0.03–0.18).

4.2. Subgroup analyses

4.2.1. Prevention type

A subgroup analysis (see Table 2) was conducted to assess whether effect sizes at post-intervention differed according to prevention type (universal vs targeted). For depression, there was a statistically significant difference (Q = 7.59, df = 1, p < .01) in the effect size obtained for universal (n = 58, g = 0.17, 95%CI: 0.13–0.21) compared to targeted (n = 43 g = 0.29, 95%CI: 0.22–0.37) prevention programs. For anxiety, there was no statistically significant difference between the effect sizes as a function of prevention type (Q = 0.50, df = 1, p = .48; universal: n = 47, g = 0.16, 95%CI: 0.12–0.21; targeted: n = 25, g = 0.20, 95%CI: 0.11–0.29). There was evidence of a trend-level statistical difference at short-term follow-up for anxiety programs, (Q = 3.84, df = 1, p = .05), with larger effect sizes detected for targeted programs (n = 10, g = 0.34, 95%CI: 0.16–0.52) relative to universal programs (n = 19, g = 0.18, 95% CI: 0.11–0.24). Other than this, there were no other significant differences between universal and targeted programs for short, medium, or long-term follow-up time point; short-term follow-up (depression: Q = 3.17, df = 1, p = .08; universal: n = 31, g = 0.15, 95%CI:0.09–0.20; targeted: n = 29, g = 0.23, 95%CI:0.15–0.30); medium-term (depression: Q = 2.31, df = 1, p = .13; universal: n = 27, g = 0.07, 95% CI:0.04–0.11; targeted: n = 21, g = 0.14, 95%CI:0.06–0.22; anxiety: Q = 2.22, df = 1, p = .14; universal: n = 21, g = 0.26, 95%CI:0.10–0.43; targeted: n = 10, g = 0.12, 95%CI:0.03–0.21); or long-term (depression: Q = 1.94, df = 1, p = .16; universal: n = 9, g = 0.07, 95%CI:.-0.01–14; targeted: n = 11, g = 0.15, 95%CI:0.06–0.24; anxiety: Q = 0.08, df = 1, p = .38; universal: n = 4, g = 0.09, 95%CI:.-0.01–02; targeted: n = 4, g = 0.11, 95%CI:-0.03–0.19).

4.2.2. Program personnel

A second sub-group analysis (see Table 2 for results) was conducted to explore if the personnel involved in delivering the program (external providers, typically involving researchers or external mental health professionals vs classroom teachers/school health staff) influenced the size of the effects obtained. For the nine studies that involved a combination of external providers and school staff, they were classified as 'external providers' for this analysis, given that at least some degree of support from outside the school was provided.

For depression, there was no significant difference, Q = 2.69, df = 1, p = .10 between programs being delivered with the involvement of external personnel (n = 72, g = 0.24, 95%CI: 0.19–0.29), relative to programs delivered or supported by school staff (n = 29, g = 0.17, 95% CI: 0.12–0.23). At short-term follow-up, there was a statistical difference between provider type, Q = 4.75, df = 1, p = .03, with externally-delivered programs showing larger effects (n = 43, g = 0.21, 95%CI: 0.15–0.27), compared to those delivered by school staff (n = 17, g = 0.17, 95%CI: 0.12–0.21). At medium- and long-term follow-up this difference was no longer significant (medium term: Q = 0.04, df = 1, p = .85, external: n = 35, g = 0.10, 95%CI: 0.06–0.14; school staff: n = 16, g = 0.11, 95%CI: .0.01–0.13; long-term: Q = 0.26, df = 1, p = .61, external: n = 10, g = 0.12, 95%CI: 0.04–0.18; school staff: n = 10, g = 0.09, 95%CI: 0.03–0.15).

For anxiety symptoms, there was no difference between effect sizes for externally delivered vs school delivered programs at post-intervention, Q = 0.33, df = 1, p = .57, with similar effect sizes found for externally-delivered programs (n = 50, g = 0.19, 95%CI:0.13–0.26), and school staff delivered/supported programs (n = 22, g = 0.16, 95% CI: 0.08–0.23). Similar to the depression findings, at the short-term follow up period, a significant difference was detected between the personnel subgroups, Q = 4.32, df = 1, p = .04, with externally delivered programs demonstrating a larger effect size (n = 24, g = 0.24, 95% CI:0.15–0.32) than anxiety prevention programs delivered exclusively by school staff (n = 5, g = 0.10, 95%CI:0.00–0.19). Consistent with the depression outcomes, this difference was no longer evident at medium or long term follow-up (medium term: Q = 0.07, df = 1, p = .42, external: n = 20, g = 0.26, 95%CI: 0.08–0.45; school staff: n = 11, g = 0.17, 95%CI: 0.04–0.30; long-term: Q = 0.06, df = 1, p = .80, external: n = 4, g = 0.09, 95%CI: .-0.11–0.28; school staff: n = 4, g = 0.11, 95%CI: 0.03–0.20).

4.2.3. Control condition

Subgroup analyses were conducted to compare if the magnitude of effect sizes differed as a function of the control condition the prevention program was compared to. For depression programs, there was no statistically significant difference as a function of control group type (Q = 1.78, df = 2, p = .41). The effects for all three control group types were in the small range (No intervention: n = 57, g = 0.20, 95%CI: 0.15–0.25; Wait-list: n = 17, g = 0.28, 95%CI: 0.17–0.40; Active control: n = 27, g = 0.20, 95%CI: 0.12–0.27). No significant differences were apparent at any of the three follow-up time points (all ps > 0.05). Similarly, there was no difference in effect size as a function of control group type for anxiety programs, Q = 3.69, df = 2, p = .16 at post intervention (No intervention: n = 36, g = 0.19, 95%CI: 0.14–0.25; Wait-list: n = 25, g = 0.19, 95%CI: 0.11–0.28; Active control: n = 11, g = 0.10, 95%CI: 0.01–0.18). Again, there were no significant differences at any of the three follow-up time points (all ps > 0.05).

Given that in school-based programs, the content of the no-intervention control groups and the wait-list groups more often than not involve school or class as usual across both control group types, we collapsed these two categories into a broad 'inactive control group' and compared this to the active control groups in a subsequent subgroup analysis. There was no difference in effect size as a function of control group type at post-intervention or any of the follow-ups (all ps > 0.05).

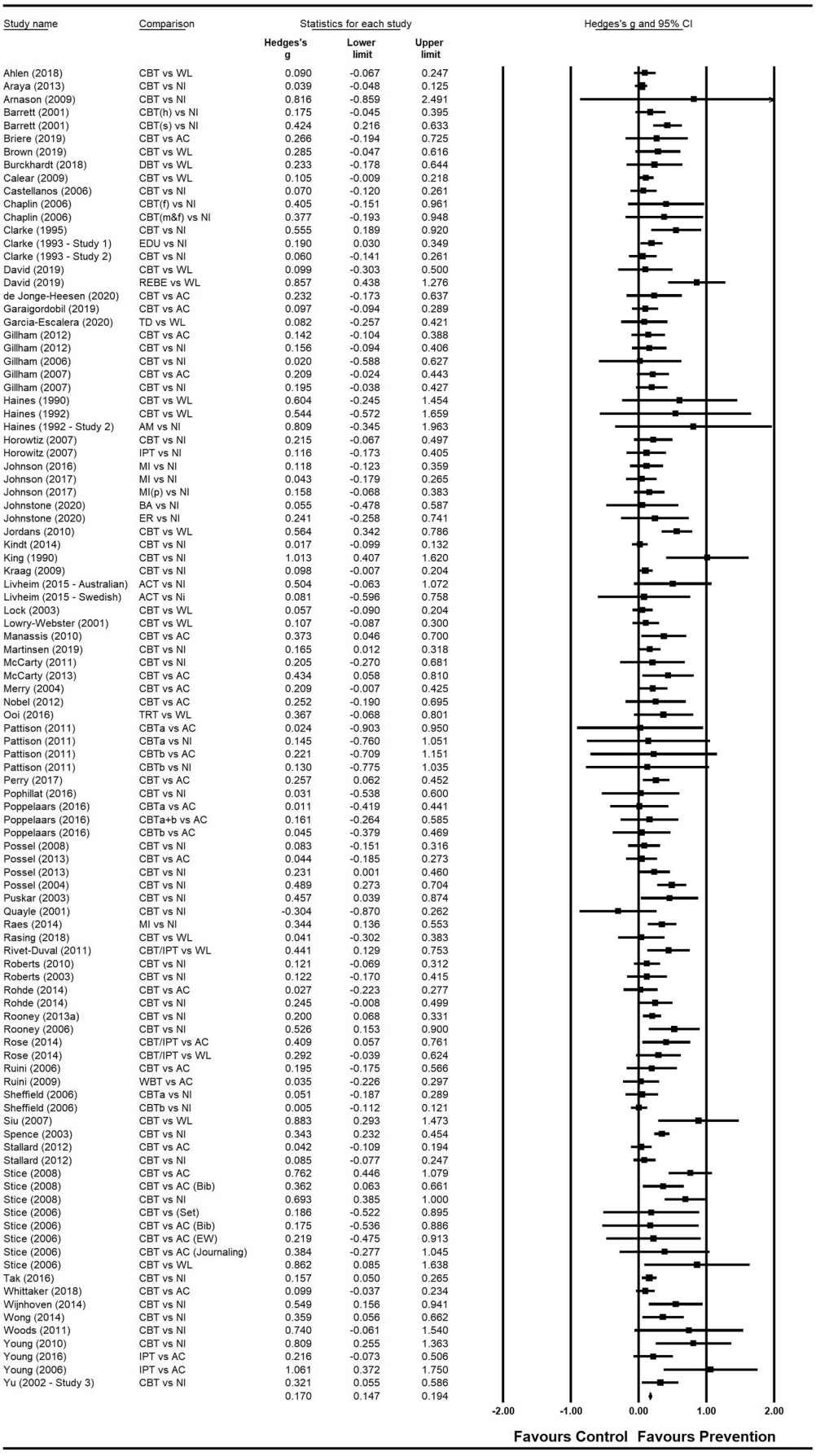
4.2.4. Program delivery age

Subgroup analyses were conducted to investigate whether the size of the intervention effects differed according to the age at which programs were delivered to participants (see Table 2). For depression, no significant between-group differences were found, Q = 0.65, df = 2, p = .72, (children: n = 8, g = 0.28, 95%CI: 0.11–0.44, early adolescents: n = 48, g = 0.20, 95%CI: 0.15–0.26, older adolescents: n = 45, g = 0.21, 95%CI: 0.15–0.26). Similarly, for anxiety outcomes, there was no difference in effect size for the separate age groups, Q = 3.85, df = 2, p = .15, (children: n = 21, g = 0.20, 95%CI: 0.09–0.31, early adolescents: n = 32, g = 0.19, 95%CI: 0.14–0.25, older adolescents: n = 19, g = 0.11, 95%CI: 0.05–0.18). No differences emerged over the follow-up periods for depression or anxiety outcomes (all ps > 0.05).

4.2.5. Program content

The comparison of effect size magnitude as a function of program content indicated that there was no statistically significant difference for CBT-based programs vs other therapeutic approaches (e.g., mindfulness, IPT, relaxation) for depression, Q = 0.41, df = 1, p = .56, (CBT: n = 86, g

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



Study name	Comparison	Hedges's g	Lower limit	Upper limit
Ahlen (2018)	CBT vs WL	0.090	-0.067	0.247
Araya (2013)	CBT vs NI	0.039	-0.048	0.125
Arnason (2009)	CBT vs NI	0.816	-0.859	2.491
Barrett (2001)	CBT(h) vs NI	0.175	-0.045	0.395
Barrett (2001)	CBT(s) vs NI	0.424	0.216	0.633
Briere (2019)	CBT vs AC	0.266	-0.194	0.725
Brown (2019)	CBT vs WL	0.285	-0.047	0.616
Burckhardt (2018)	DBT vs WL	0.233	-0.178	0.644
Calear (2009)	CBT vs WL	0.105	-0.009	0.218
Castellanos (2006)	CBT vs NI	0.070	-0.120	0.261
Chaplin (2006)	CBT(f) vs NI	0.405	-0.151	0.961
Chaplin (2006)	CBT(m&f) vs NI	0.377	-0.193	0.948
Clarke (1995)	CBT vs NI	0.555	0.189	0.920
Clarke (1993 - Study 1)	EDU vs NI	0.190	0.030	0.349
Clarke (1993 - Study 2)	CBT vs NI	0.060	-0.141	0.261
David (2019)	CBT vs WL	0.099	-0.303	0.500
David (2019)	REBE vs WL	0.857	0.438	1.276
de Jonge-Heesen (2020)	CBT vs AC	0.232	-0.173	0.637
Garaigordobil (2019)	CBT vs AC	0.097	-0.094	0.289
Garcia-Escalera (2020)	TD vs WL	0.082	-0.257	0.421
Gillham (2012)	CBT vs AC	0.142	-0.104	0.388
Gillham (2012)	CBT vs NI	0.156	-0.094	0.406
Gillham (2006)	CBT vs NI	0.020	-0.588	0.627
Gillham (2007)	CBT vs AC	0.209	-0.024	0.443
Gillham (2007)	CBT vs NI	0.195	-0.038	0.427
Haines (1990)	CBT vs WL	0.604	-0.245	1.454
Haines (1992)	CBT vs WL	0.544	-0.572	1.659
Haines (1992 - Study 2)	AM vs NI	0.809	-0.345	1.963
Horowtiz (2007)	CBT vs NI	0.215	-0.067	0.497
Horowitz (2007)	IPT vs NI	0.116	-0.173	0.405
Johnson (2016)	MI vs NI	0.118	-0.123	0.359
Johnson (2017)	MI vs NI	0.043	-0.179	0.265
Johnson (2017)	MI(p) vs NI	0.158	-0.068	0.383
Johnstone (2020)	BA vs NI	0.055	-0.478	0.587
Johnstone (2020)	ER vs NI	0.241	-0.258	0.741
Jordans (2010)	CBT vs WL	0.564	0.342	0.786
Kindt (2014)	CBT vs NI	0.017	-0.099	0.132
King (1990)	CBT vs NI	1.013	0.407	1.620
Kraag (2009)	CBT vs NI	0.098	-0.007	0.204
Livheim (2015 - Australian)	ACT vs NI	0.504	-0.063	1.072
Livheim (2015 - Swedish)	ACT vs Ni	0.081	-0.596	0.758
Lock (2003)	CBT vs WL	0.057	-0.090	0.204
Lowry-Webster (2001)	CBT vs WL	0.107	-0.087	0.300
Manassis (2010)	CBT vs AC	0.373	0.046	0.700
Martinsen (2019)	CBT vs NI	0.165	0.012	0.318
McCarty (2011)	CBT vs NI	0.205	-0.270	0.681
McCarty (2013)	CBT vs AC	0.434	0.058	0.810
Merry (2004)	CBT vs AC	0.209	-0.007	0.425
Nobel (2012)	CBT vs AC	0.252	-0.190	0.695
Ooi (2016)	TRT vs WL	0.367	-0.068	0.801
Pattison (2011)	CBTa vs AC	0.024	-0.903	0.950
Pattison (2011)	CBTa vs NI	0.145	-0.760	1.051
Pattison (2011)	CBTb vs AC	0.221	-0.709	1.151
Pattison (2011)	CBTb vs NI	0.130	-0.775	1.035
Perry (2017)	CBT vs AC	0.257	0.062	0.452
Pophillat (2016)	CBT vs NI	0.031	-0.538	0.600
Poppelaars (2016)	CBTa vs AC	0.011	-0.419	0.441
Poppelaars (2016)	CBTa+b vs AC	0.161	-0.264	0.585
Poppelaars (2016)	CBTb vs AC	0.045	-0.379	0.469
Possel (2008)	CBT vs NI	0.083	-0.151	0.316
Possel (2013)	CBT vs AC	0.044	-0.185	0.273
Possel (2013)	CBT vs NI	0.231	0.001	0.460
Possel (2004)	CBT vs NI	0.489	0.273	0.704
Puskar (2003)	CBT vs NI	0.457	0.039	0.874
Quayle (2001)	CBT vs NI	-0.304	-0.870	0.262
Raes (2014)	MI vs NI	0.344	0.136	0.553
Rasing (2018)	CBT vs WL	0.041	-0.302	0.383
Rivet-Duval (2011)	CBT/IPT vs WL	0.441	0.129	0.753
Roberts (2010)	CBT vs NI	0.121	-0.069	0.312
Roberts (2003)	CBT vs NI	0.122	-0.170	0.415
Rohde (2014)	CBT vs AC	0.027	-0.223	0.277
Rohde (2014)	CBT vs NI	0.245	-0.008	0.499
Rooney (2013a)	CBT vs NI	0.200	0.068	0.331
Rooney (2006)	CBT vs NI	0.526	0.153	0.900
Rose (2014)	CBT/IPT vs AC	0.409	0.057	0.761
Rose (2014)	CBT/IPT vs WL	0.292	-0.039	0.624
Ruini (2006)	CBT vs AC	0.195	-0.175	0.566
Ruini (2009)	WBT vs AC	0.035	-0.226	0.297
Sheffield (2006)	CBTa vs NI	0.051	-0.187	0.289
Sheffield (2006)	CBTb vs NI	0.005	-0.112	0.121
Siu (2007)	CBT vs WL	0.883	0.293	1.473
Spence (2003)	CBT vs NI	0.343	0.232	0.454
Stallard (2012)	CBT vs AC	0.042	-0.109	0.194
Stallard (2012)	CBT vs NI	0.085	-0.077	0.247
Stice (2008)	CBT vs AC	0.762	0.446	1.079
Stice (2008)	CBT vs AC (Bib)	0.362	0.063	0.661
Stice (2008)	CBT vs NI	0.693	0.385	1.000
Stice (2006)	CBT vs (Set)	0.186	-0.522	0.895
Stice (2006)	CBT vs AC (Bib)	0.175	-0.536	0.886
Stice (2006)	CBT vs AC (EW)	0.219	-0.475	0.913
Stice (2006)	CBT vs AC (Journaling)	0.384	-0.277	1.045
Stice (2006)	CBT vs WL	0.862	0.085	1.638
Tak (2016)	CBT vs NI	0.157	0.050	0.265
Whittaker (2018)	CBT vs AC	0.099	-0.037	0.234
Wijnhoven (2014)	CBT vs NI	0.549	0.156	0.941
Wong (2014)	CBT vs NI	0.359	0.056	0.662
Woods (2011)	CBT vs NI	0.740	-0.061	1.540
Young (2010)	CBT vs NI	0.809	0.255	1.363
Young (2016)	IPT vs AC	0.216	-0.073	0.506
Young (2006)	IPT vs AC	1.061	0.372	1.750
Yu (2002 - Study 3)	CBT vs NI	0.321	0.055	0.586
		0.170	0.147	0.194

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

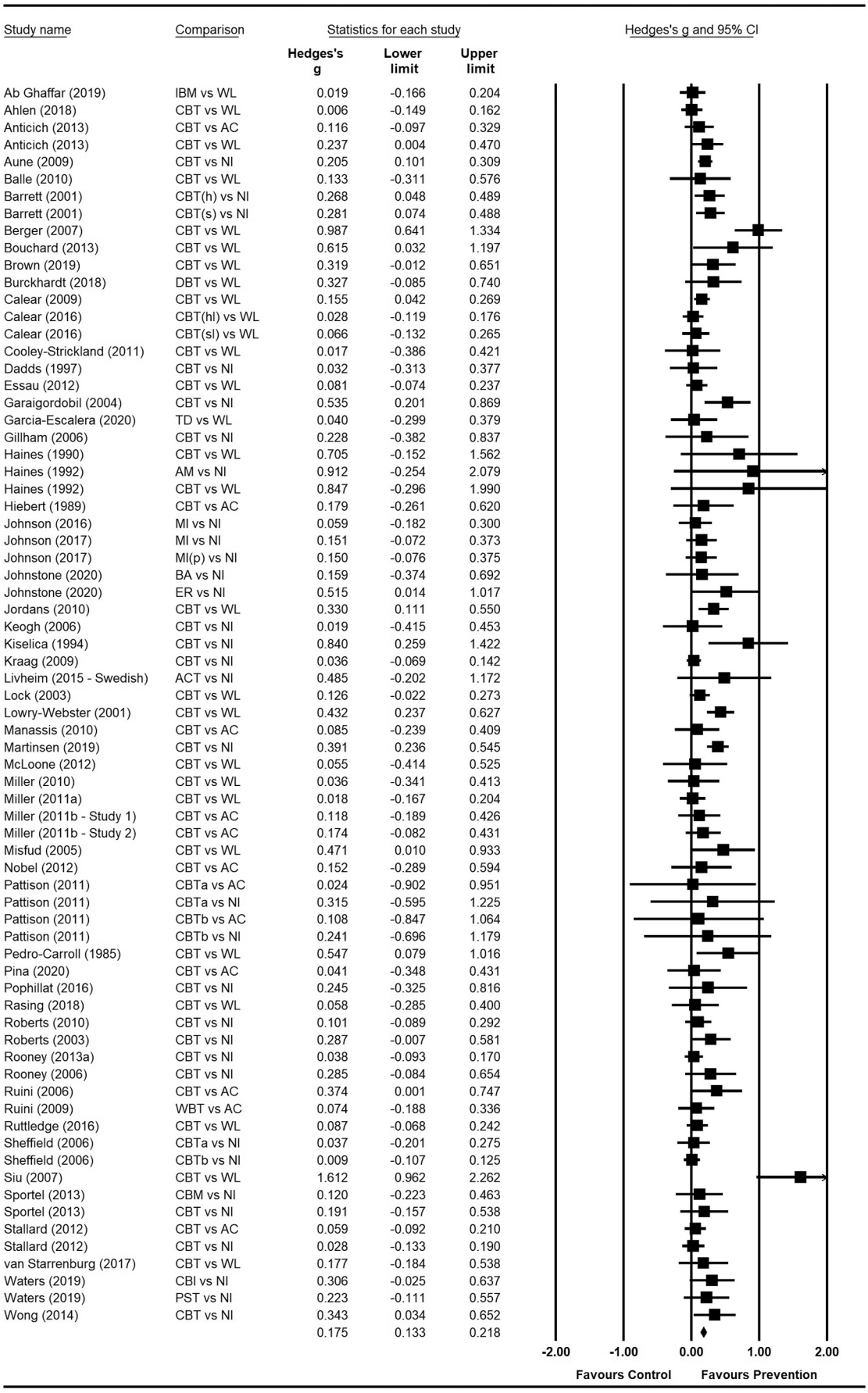


Fig. 3. Forest plot of effect sizes for comparisons between prevention programs and control conditions on anxiety symptoms at post-intervention.

Table 2
Subgroup analyses at post-intervention.

Subgroup Analyses		Measure	N	*g*	95% CI	I^2
Prevention Type	Universal	Depression	58	.17	.13–.21	34
	Targeted		43	.29	.22–.37	57
	Universal	Anxiety	47	.16	.12–.21	43
	Targeted		25	.20	.11–.29	50
Personnel Delivering Program	External	Depression	72	.24	.19–.29	72
	School-staff		29	.17	.12–.23	29
	External	Anxiety	50	.19	.13–.26	32
	School-staff		22	.16	.08–.23	63
Control Group Comparison	No intervention	Depression	57	.20	.15–.25	49
	Waitlist		17	.28	.17–.40	62
	Active		27	.20	.12–.27	30
	No intervention	Anxiety	36	.19	14–.25	42
	Waitlist		25	.19	.11–.28	73
	Active		11	.10	.01–.18	0
Age	Child	Depression	8	.28	.11–.44	62
	Early adolescent		48	.20	.15–.26	35
	Older adolescent		45	.21	.15–.26	55
	Child	Anxiety	21	.20	.09–.31	64
	Early adolescent		32	.19	.14–.25	33
	Older adolescent		19	.11	.05–.18	25
Program Content	CBT	Depression	86	.21	.17–.25	47
	Other		15	.25	.13–.36	46
	CBT	Anxiety	61	.18	.13–.23	50
	Other		11	.16	.06–.25	12
Program Format	Face-to-face	Depression	92	.21	.17–.25	47
	Digital		9	.19	.07–.32	49
	Face-to-face	Anxiety	66	.18	.14–.23	49
	Digital		6	.12	.05–.19	0
Study Quality	High	Depression	32	.19	.13–.25	62
	Low		69	.22	.18–.27	31
	High	Anxiety	24	.14	.08–.19	31
	Low		48	.20	.14–.27	52

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.

= 0.21, 95%CI: 0.17–0.25, other: $n = 15$, $g = 0.25$, 95%CI: 0.13–0.36), or anxiety, $Q = 0.17$, $df = 1$, $p = .67$, (CBT: $n = 61$, $g = 0.18$, 95%CI: 0.13–0.23, other: $n = 11$, $g = 0.16$, 95%CI: 0.06–0.25). There were no differences at any of the follow-up intervals (all *p*s > 0.05).

4.2.6. Delivery format

Subgroup analyses were conducted to examine whether the format by which the prevention program was delivered (face-to-face or digital) impacted the magnitude of effect sizes. Analyses indicated that there were no significant differences in effects for depression ($Q = 0.08$, $df = 1$, $p = .78$, face-to-face: $n = 92$, $g = 0.21$, 95%CI: 0.17–0.25, digital: $n = 9$, $g = 0.19$, 95%CI: 0.07–0.32), or anxiety programs ($Q = 1.83$, $df = 1$, $p = .18$, (face-to-face: $n = 66$, $g = 0.18$, 95%CI: 0.14–0.23, digital: $n = 6$, $g = 0.12$, 95%CI: 0.05–0.19). There were no differences at any of the follow-up intervals for either depression or anxiety (all *p*s > 0.05).

4.2.7. Study quality

Subgroup analyses were conducted to compare the effect sizes between low quality and high-quality studies. Results showed that there were no statistically significant differences in the effect size estimates for the depression outcomes at post intervention, $Q = 0.61$, $df = 1$, $p = .43$, high quality: $n = 32$, $g = 0.19$, 95%CI: 0.13–0.25, low quality: $n = 69$, $g = 0.22$, 95%CI: 0.18–0.27), nor for the anxiety outcomes, $Q = 0.2.4$, $df = 1$, $p = .12$, high quality: $n = 24$, $g = 0.14$, 95%CI: 0.08–0.19, low quality: $n = 48$, $g = 0.20$, 95%CI: 0.14–0.27). There were no differences at any of the follow-up intervals for either depression or anxiety (all *p*s > 0.05).

4.3. 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 were 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 $r = 0.26$, 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 type, age of delivery, program content, program format, study quality), 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 prevention programs having larger effect sizes than universal programs. There were no significant predictors of effect size for anxiety prevention studies.

4.4. Publication bias

There was some evidence of publication bias for the depression studies, as demonstrated by inspection of the funnel plot (Appendix C). 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 $g = 0.21$ to $g = 0.14$ (28 studies removed). There was also some evidence of publication bias for anxiety studies (Appendix D), and after using Duval and Tweedie's trim and fill procedure, the mean effect size estimate reduced from $g = 0.18$ to $g = 0.10$ (21 studies removed).

5. Discussion

5.1. Main findings

The aim of this study was to provide a review and update on the status of randomised controlled trials evaluating programs designed to prevent depression and/or anxiety in children and adolescents delivered in the school environment. This review is an update to a previous review, where the study search was carried out in 2015 (Werner-Seidler et al., 2017). Since the previous review, an additional 37 randomised controlled trials have been published which met inclusion criteria, with 33 included in the meta-analysis (see Appendix B for details). Overall, there were 118 unique trials included in the current review, involving a total of 45,924 participants, of which 108 were included in the meta-analysis. Results showed that relative to a control group, there was a small preventive effect of school-based prevention programs on symptoms of depression and anxiety.

For depression prevention programs, the effect sizes at post and short-term follow-up were small ($g = 0.21$ and $g = 0.17$ respectively), with medium and long-term follow-up intervals indicating very small effect sizes (both $g = 0.10$). These effect sizes are similar to those detected in our earlier review (*g* values = 0.23 at post-intervention, 0.20 as short-term follow-up, 0.12 for medium-term follow-up, and 0.11 for long-term follow up). Effect sizes for anxiety prevention studies showed a different pattern, with a small effect at post ($g = 0.18$), short ($g = 0.19$) and medium-term follow-up intervals ($g = 0.23$), before reducing to a very small effect at long-term follow-up ($g = 0.11$). Again, these values mirror the pattern found in our previous review (*g* values = 0.20 at post intervention, 0.23 at short-term follow-up, 0.23 at medium-term follow-up, and 0.13 at long-term follow up). The current review included 33 *new* trials (30% of the overall trials included in this review were not included in the original review), and more than an additional 14,000 participants. The consistency in effect sizes detected across our previous and current review suggests that these effects have remained stable. Specifically, there are genuine but modest preventive effects of psychological programs delivered at school for depression and anxiety, and the size of these effects dissipate over time.

With respect to the pattern of findings in the current review, the increase in effect between post/short term to medium term follow-up is in line with suggestions that the effects of psychological interventions, and particularly CBT, the most commonly delivered preventive intervention for anxiety, can actually increase over time, which is proposed to be because individuals have the opportunity to practice and use their newly acquired skills (Kodal et al., 2018). However, this pattern was not detected for depression prevention programs. Overall though, these results suggest that the effect sizes for school-based prevention programs remain modest, and seem to dissipate in the case of depression programs over time, but may be maintained for anxiety, at least until 12 months following the intervention. It is possible that refresher training or brief booster session/s at 12 months follow-up may help to maintain benefits. It is notable that only 19 studies (16%) involved follow-up periods beyond 12 months which is concerning given that preventive effects require significant intervals of time in order to show genuine preventive effects. It is therefore recommended that future prevention studies consider including longer-term follow-up intervals and identify factors associated with maintenance of effects beyond 12 months.

Fewer than one-third of studies were judged as high quality, meaning that most studies involved concerning degrees of risk of bias with significant room for improvement. Encouragingly, there were no significant differences in effect size estimates between the high- and low-quality studies. It is expected that the rigour and quality of RCTs will continue to improve, given the increasing requirement to prospectively register and publish study protocols, together with the need to submit CONSORT checklists with papers.

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		−0.14	0.05	>0.01**
Targeted				
Universal	Anxiety	Ref		
Targeted		−0.03	0.06	0.58
Personnel Delivering Program				
External	Depression	Ref		
School-staff		−0.07	0.05	0.13
External	Anxiety	Ref		
School staff		−0.03	0.05	0.48
Control Group				
No intervention	Depression	Ref		
Waitlist		0.04	0.05	0.39
Active		0.10	0.06	0.11
No intervention	Anxiety	Ref		
Waitlist		0.10	0.08	0.16
Active		0.08	0.08	0.28
Age				
Child	Depression	Ref		
Early adolescent		−0.08	0.08	0.33
Older adolescent		−0.08	0.08	0.26
Child	Anxiety	Ref		
Early adolescent		−0.02	0.06	0.74
Older adolescent		−0.05	0.07	0.48
Program Content				
CBT	Depression	Ref		
Other		0.03	0.06	0.59
CBT	Anxiety	Ref		
Other		0.02	0.07	0.77
Program Format				
Face-to-Face	Depression	Ref		
Digital		−0.09	0.08	0.24
Face-to-Face	Anxiety	Ref		
Digital		0.01	0.09	0.97
Study Quality				
High	Depression	Ref		
Low		0.08	0.05	0.07*
High	Anxiety	Ref		
Low		0.04	0.06	0.53

Note: Ref = reference group. ** = significant at α<.05, *= trend level at α<0.10.

5.2. Additional findings

Subgroup analyses indicated that the effect size for universally delivered prevention programs for depression prevention was smaller than that for targeted programs. This effect is consistent with previous findings (Merry et al., 2011; Werner-Seidler et al., 2017) and not surprising, given that statistically, it is more likely to see effects among groups of young people who are already showing elevated symptom levels. To see effects for universal prevention programs, very large sample sizes are required (Muñoz, Cuijpers, Smit, Barrera, & Leykin, 2010). Data from the current review shows mean and median sample sizes of 386 and 209 participants (respectively), which are small samples for interventions that are likely to have their largest benefits at a population-level, particularly if delivered universally. Moreover, this data indicates that sample sizes for school-based prevention studies has not changed in a decade (Merry et al., 2011; Werner-Seidler et al., 2017). Therefore, studies evaluating prevention programs, particularly those delivered universally, would do well to consider using large samples, and employ pragmatic trial designs to make this feasible. An additional benefit of trials involving large samples is the opportunity to examine individual level effects such as predictors of prevention response, which may assist to optimise and individually tailor prevention programs for groups of young people. Encouragingly, several large-scale prevention trials included in this updated review have indicated that they will follow this approach of involving substantial sample sizes, with trials planned or in progress which involve thousands of young people (Kuyken et al., 2017; Werner-Seidler et al., 2020). Given the huge amount of resourcing required to conduct large scale prevention studies, another approach to appropriately power the analysis of universal prevention programs would be to use a data harmonization approach which uses integrative data analysis to synthesise participant level data across RCTs (Brincks et al., 2018). The appropriate evaluation of universal prevention approaches should be a priority because schools have repeatedly reported a preference to deliver universal programs, both for logistical reasons but also to adhere to whole-of-school wellbeing policies and programs (Beames et al., 2021; Horowitz & Garber, 2006). How other school-based interventions (e.g., education and lifestyle programs) interact or contribute to preventive effects reported here is not yet known and could be assessed in future studies.

Results indicated that there could be some value in having prevention programs delivered by external personnel rather than school staff for depression and anxiety programs. At the short-term follow-up, effect sizes were larger for externally delivered programs compared to school personnel for both depression and anxiety. It is unclear why this effect was not apparent at post-intervention, but nonetheless suggests that having external providers, who usually have a background in mental health, deliver these interventions is beneficial, and again replicates the findings from our previous review suggesting this finding is robust. One potential reason for benefits associated with external providers might be that school personnel, typically teachers or wellbeing staff (nurses, counsellors), may not always view it as their role to deliver mental health prevention programs. Evidence suggests that school counsellors and psychologists often spend their time working with highly distressed students, while teachers focus on more traditional academic learning (Beames et al., 2021). However, there are practical limitations associated with external providers delivering mental health programs in schools because this can be expensive and difficult to sustain beyond the conclusion of a trial. It is suggested that future studies investigate whether school staff may be in a position to *support* rather than *deliver* programs, which could be done using digital formats. It is noted that this is an increasingly appealing model, with half of the digital programs involving school-personnel to support their delivery rather than external personnel. One of the benefits associated with digital delivery is that fidelity to the intervention is preserved. On this note, we did not assess intervention fidelity in this review because our attempt to do so previously indicated that approximately half of studies did not report on

fidelity at all, and that among those that did, measures substantially varied, limiting the interpretation of this information (Werner-Seidler et al., 2017). It is recommended that future studies assess program fidelity using rigorous approaches.

The current review identified an increased number of digitally delivered prevention programs relative to the previous review, with a total of eight studies evaluating digital programs overall, six of which have been conducted in the last five years. This change is not surprising given the increased focus on the potential value of digital interventions as an appealing, low-intensity and scalable solution to address youth mental health (Bergin et al., 2020). While there were no differences in effect sizes between programs delivered digitally vs those delivered face-to-face, this should be interpreted cautiously given the relatively few digital programs evaluated. Randomised controlled trials comparing face-to-face and digital programs, and digital programs supported by external providers vs school staff are now needed to establish whether digital delivery is as effective as face-to-face, and whether school-staff supported digital programs are an effective and feasible longer term solution. There are clear benefits in digital approaches which are robust against situations where face-to-face options are not available (e.g., during a pandemic) or are too expensive to deliver, which is particularly problematic in low resource schools. However, challenges remain, including how to engage young people in digital programs, equity in access to digital infrastructure in low-to-middle income countries, how to sustain and ensure digital programs remain relevant and up-to-date in a rapidly evolving digital environment, as well as how to upskill school staff in the confidence and competence to support such programs (Bevan Jones et al., 2020; Fu, Burger, Arjadi, & Bockting, 2020; Liverpool et al., 2020).

While there were no differences in effect sizes reported as a function of control group type, it is notable that 75% of studies continue to compare outcomes to non-active control groups, despite calls for more rigorous comparison groups (Furukawa et al., 2014; Guidi et al., 2018). The selection and reporting of control groups in school-based research is convoluted, with several inconsistencies. For example, non-active control groups often involve the delivery of the usual school curriculum material (often focused on health) and sometimes this is identified as a non-intervention control group (e.g., Pophillat et al., 2016), while at other times studies which offer the intervention to participants after the trial but class-as-usual during the trial are labelled waitlist control groups (e.g., Ruttledge et al., 2016). This led us to collapse non-active control groups into one category for this review, and active control groups into another. While inactive control groups still dominate, it is encouraging that 25% of studies involved an active control group, which is a considerable increase in the proportion of active control groups included in the previous review (10%), suggesting that the field is moving towards the inclusion of more rigorous control groups.

Relatedly, in our review we did not assess exposure to other broad school-based programs which were not focused on depression and/or anxiety prevention. For example, many schools have social and emotional learning programs and/or resilience courses that may have been delivered to participants previously or simultaneously. This is a limitation of the current review and it is recommended that consideration of effects of multiple programs and/or a wide variety of program types be investigated in future studies. This is likely to be challenging to do and non-traditional trial designs, such as stepped wedge trials that measure the effects of multiple school initiatives and programs may help to clarify this issue.

There was no difference in effect sizes according to the age at which young people were delivered preventive interventions, or therapeutic content of the programs. The onset of mental disorders, particularly anxiety, often begins during childhood and increases exponentially during adolescence (Merikangas et al., 2010), meaning that the delivery of prevention programs to young people anytime during this developmental window is warranted. In light of reports of increasing rates of mental illness in young people (Keyes, Gary, O'Malley, Hamilton, & Schulenberg, 2019; Twenge et al., 2019), the delivery of prevention programs early in the course of illness is increasingly important. While the content of the programs that are delivered remain dominated by CBT approaches, there is greater diversity in the range of programs that are being evaluated since our previous review. This review identified a range of different programs, including mindfulness-based trials, personality-focused interventions, cognitive bias modification, dialectical behaviour therapy and positive psychology programs. While CBT has the strongest evidence-base and has been by far the most studied psychological intervention, other approaches should be considered, given that non-response to CBT is common (e.g., Loerinc et al., 2015) and may extend to the prevention field.

Related to program content, was the inclusion of several studies involving transdiagnostic programs for depression and anxiety, rather than focusing on one or the other (Johnson et al., 2016; Martinsen et al., 2019; Sanchez-Hernandez et al., 2019). These approaches were either CBT or mindfulness based, and there were not enough of these to warrant a formal analysis. However, the adult literature has documented large effects for transdiagnostic treatments (Newby, McKinnon, Kuyken, Gilbody, & Dalgleish, 2015) and whether this is also the case for young people in a prevention context warrants consideration. A recent review identifying the shared risk factors for psychopathology among young people (Lynch, Sunderland, Newton, & Chapman, 2021), together with the fact that comorbidity in disorders is common (e.g., Lawrence et al., 2015), suggest that it may be appropriate for prevention programs to target multiple disorders. In line with the move toward dimensional conceptualisations of mental ill-heath (Kotov et al., 2017), transdiagnostic approaches to prevention may offer greater 'bang for buck' given the overlapping risk factors and features of emerging mental illness such as anxiety and depression.

5.3. Comparison to previous research

The effect sizes reported in the current review are comparable to those reported previously (Calear & Christensen, 2010; Neil & Christensen, 2009; Stockings et al., 2016; Werner-Seidler et al., 2017), but differ to those reported in the study by Caldwell et al., 2019, which adopted a network meta-analysis approach, incorporating all available evidence on intervention effects into a single analysis. It is likely that the methodological differences in the approach taken here compared to that by Caldwell are responsible for the different outcomes. In our review, we found evidence for preventive effects on depression and anxiety symptoms, while in the Caldwell et al. (2019) study the authors reported insufficient evidence for preventive effects. However, the studies included in these two reviews varied. For example, in contrast to Caldwell et al. (2019), our review included studies of transdiagnostic anxiety and depression prevention programs (e.g., Rasing et al., 2018; Wong et al., 2014), only studies that were fully randomised, and studies in which we did not exclude on the basis of participants having had a diagnosed mental illness. These inclusion criteria differences resulted in different sets of studies being examined in the meta-analyses, and not surprisingly, the results. What is consistent across these reviews is that effects are likely to be modest and additional research is needed to identify the conditions under which school-based prevention programs are most likely to be effective.

Extending the knowledge of previous reviews, the current findings provide more robust evidence that prevention programs delivered external to the school have larger effects than those delivered by school personnel, at least in the short term. Our review also highlighted a three-fold increase in the number of trials of digital preventative interventions, showing a promising and effective prevention and early intervention approach. The findings also show relative to our previous review, more trials are using active control comparison groups, yet study quality still remains variable and needs to be improved.

5.4. Limitations

The results of the current review should be considered in light of several limitations. First, the primary outcomes, depression and anxiety, were self-reported symptoms rather than clinician rated diagnoses. Most prevention studies have taken a practical approach by using symptom measures because clinical interviews are expensive and time consuming to administer, challenging to do in the school environment (e.g., issues of stigma, cost and time), and because follow-up intervals in prevention studies are generally not long enough to detect changes in diagnostic status due to low base rates of disorder. Although it is often not feasible to administer diagnostic interviews in prevention trials, this is nonetheless the gold standard approach because it allows for a true test of the prevention of incident cases of depression and anxiety over time, and for assessment of fluctuations in symptoms during the follow-up period. We recommend that future studies employ this approach when it is feasible, particularly at the pilot or proof-of-concept stage so that the long-term preventive effects can be identified, prior to taking an intervention to scale. When this is not possible, relying on symptom change as an index of preventive effects is valid given that symptoms are a modifiable yet powerful risk factor for disorder onset (Cuijpers & Smit, 2004; Fergusson, Horwood, Ridder, & Beautrais, 2005), and therefore shifting a cohort of young people down the symptom severity continuum lowers the risk of onset. Reviews of the depression and anxiety prevention literature from the broader field (e.g., outside the school environments and from the adult literature) which employ diagnostic assessments and exclude those with a history of depression or anxiety (ie., include a test of 'true prevention') are consistent with this review (Stockings et al., 2016; van Zoonen et al., 2014), underscoring the validity of this approach.

Second, while most included studies had a follow-up assessment, studies required at a minimum a post-intervention assessment only. This is a limitation in a review of prevention interventions because in some cases, a reduction in symptoms from pre-to-post intervention may represent early intervention and not true prevention (Horowitz & Garber, 2006), something that is more likely in indicated prevention programs where individuals are already symptomatic. That said, our analysis of short, medium and long-term follow-up assessment points ensures that our review accurately captures both prevention and early intervention effects. Third, there was evidence of publication bias, meaning that the effect size estimates may have been overestimated. The increased requirement of journals to publish protocols and/or prospectively register trials is likely to reduce the risk of publication of trials reporting significant group differences, over null trials in the future. Fourth, we focused on the prevention of depression and anxiety only, as common mental disorders in this age group and limited our review to psychological interventions targeting these disorders. This meant we excluded educational and awareness raising programs, and made this decision based on the large number of student wellbeing initiatives which are homogeneous and do not necessarily focus on symptom outcomes (e.g., Mansfield, Patalay, & Humphrey, 2020; Mellor, 2014; O'Reilly, Svirydzenka, Adams, & Dogra, 2018). Finally, although we conducted subgroup analyses, there are potential confounding factors which could have accounted for the differences we detected. Trials that directly comparing factors such as external provider vs school staff delivered programs are needed to establish causality.

5.5. Practical implications and future research directions

The current review suggests that school-based depression and anxiety prevention programs have modest but positive effects. However, it also highlights areas of opportunity and improvement, including the identification of ways to refine and improve the efficacy of existing programs. For example, the effects of these programs as a function of sex was not assessed in most of the studies we evaluated, and the effectiveness of programs differentiated by sex could be considered in future work. Based on the available evidence, there does not seem to be a moderating effect of sex (e.g., Calear et al., 2016; Pophillat et al., 2016), but given that girls are around two times more likely to experience depression and anxiety than their male counterparts (Lawrence et al., 2015), a closer examination of this issue is warranted. Across the studies reviewed, very few include descriptions or plans for the maintenance of prevention programs beyond the trial period (although it is noted that this may be beyond the scope of the reporting of the trial). Given that schools now have an increased focused on student mental health and wellbeing, finding ways to integrate effective programs into standard practice needs to be a priority for the future. The increasing number of trials testing digital interventions is promising, and as effectiveness data for these programs for prevention accumulates (Rigabert et al., 2020), this may offer a more feasible way to sustain the delivery of effective programs in schools. If these could be effectively supported internally by school-staff, this is likely to represent a more sustainable, less costly model.

Related to the longevity of these programs, the focus on how to best implement school-based prevention programs is needed. Delivery of mental health programs in the school environment is already logistically challenging and contextual factors, such as support from the school leaders and the existing school climate, are likely to be critically important for successful implementation and delivery (Hudson, Lawton, & Hugh-Jones, 2020; Kidger, Araya, Donovan, & Gunnell, 2012). It is therefore recommended that future trials attend to implementation factors, consider using hybrid trial designs which examine effectiveness and implementation factors simultaneously (Curran, Bauer, Mittman, Pyne, & Stetler, 2012), or embedding process evaluations into existing trials where possible (Oakley, Strange, Bonell, Allen, & Stephenson, 2006).

Finally, the interventions reviewed here focus on prevention programs involving brief psychological interventions to which the individual is exposed. It is encouraging that small effect sizes are evident from these programs and this approach should be incorporated into a more comprehensive approach to the prevention of mental disorders. Young people do not exist in a vacuum and a broader systems level approach is recommended to enhance the effects we have reported based on individual-level exposure. For example, there is evidence that whole of school approaches may be effective for student mental health (Goldberg et al., 2019), and it is recommended that future interventions could target parents, teachers and the whole school community, in addition to students. Furthermore, prevention approaches could consider broader social determinants that lead to the emergence of illness in the first place, such as adverse childhood experiences, and target modifiable risk factors to reduce the risk of experiences (Jorm & Mulder, 2018). It is encouraging to see governments recognise the need for this broader, systems-based approach (Productivity Commission, 2020), and how this impacts on individual-level mental illness will likely become clearer in the future.

6. Conclusion

Overall, the findings from this meta-analysis suggest that there are small but beneficial effects in delivering school-based depression and anxiety prevention programs. The parameters under which these programs are optimally delivered requires further research. However, given the low-intensity nature of these interventions (i.e., delivered in groups or digitally), they should be delivered in schools as part of a comprehensive school-based mental health prevention strategy. Although the effect sizes detected in this review were modest, they nonetheless are likely to have a significant public health impact when delivered at scale. For example, estimates suggest that existing programs can prevent 21% of new cases of depression, with the number needed to treat to prevent a case of depression being around 22 (Ormel, Cuijpers, Jorm, & Schoevers, 2019; van Zoonen et al., 2014). Continued optimisation and refinement of these programs, as well as structurally embedding them within

schools and communities will serve to enhance their impact.

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Author contribution statement

AW-S and HC designed the study and all other authors collaborated on the design. AWS, SS, ALC, YP, MT, BOD and JN extracted, coded and assessed the data. AWS and JN performed the analysis and AWS drafted the manuscript. All authors provided critical revisions and approved the final manuscript.

Declaration of Competing Interest

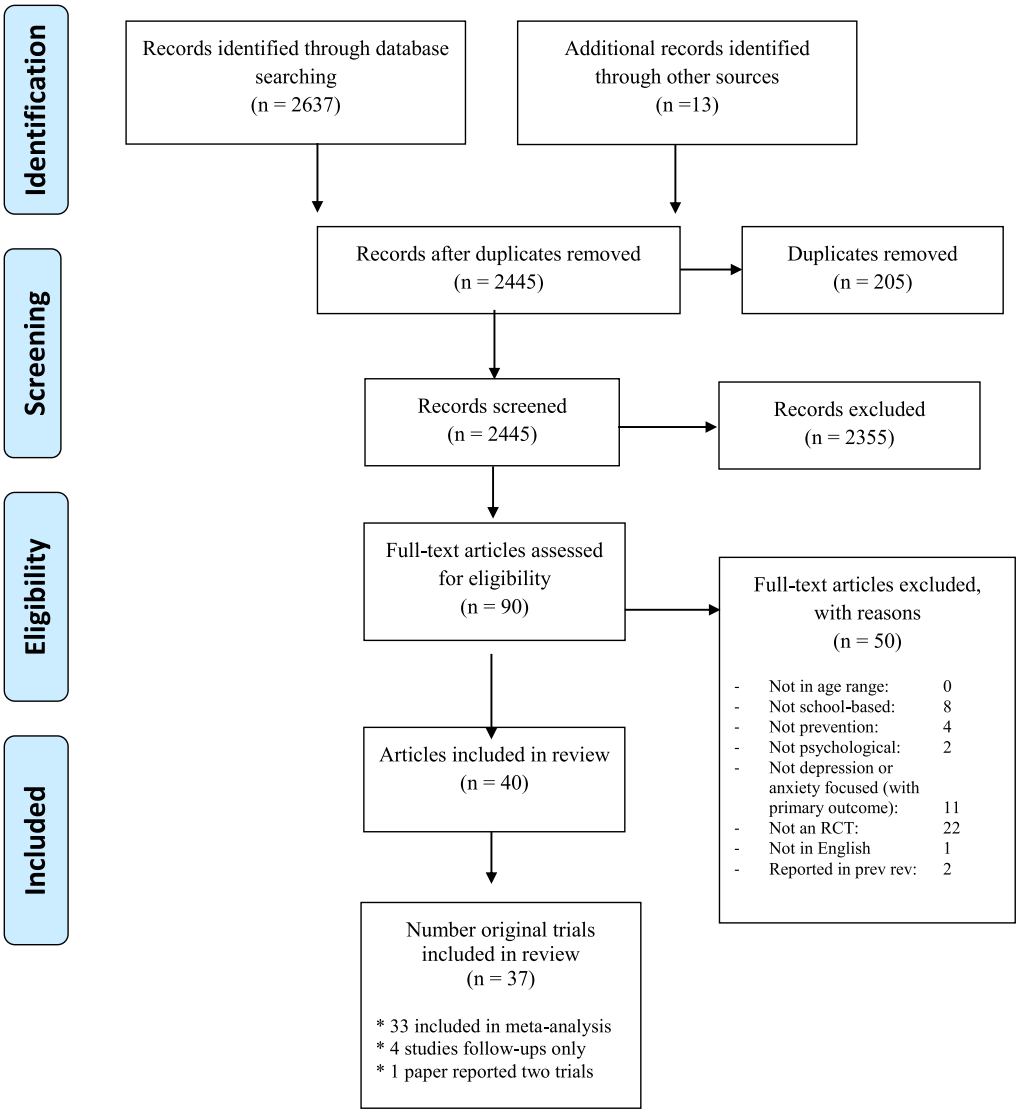
All authors declare that they have no conflicts of interest.

Appendix A. Search string example

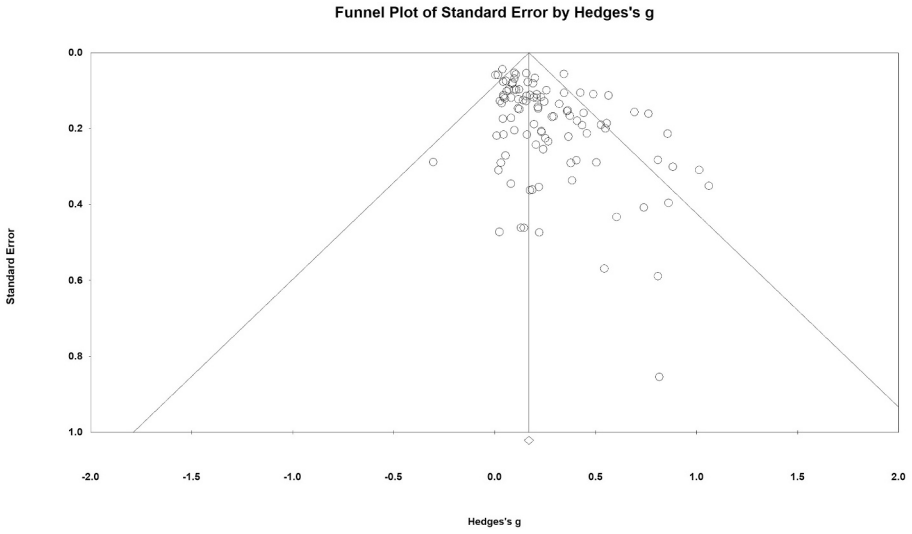
Search string from PsycINFO:

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

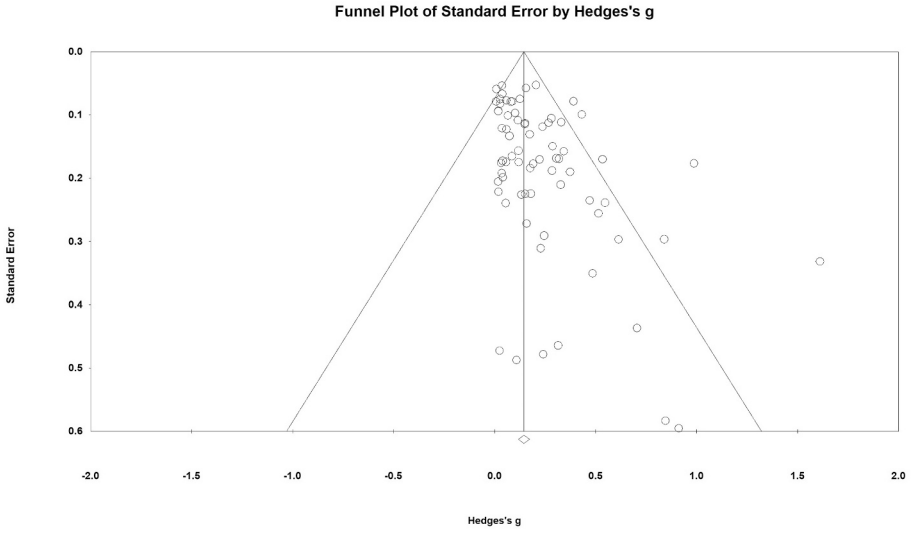
Appendix B. PRISMA flow chart of randomised controlled trials identified in the updated review



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



Appendix D. Funnel plot of depression effect size data at post-interventio



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