Comparative Effectiveness Review
Number 107

Effective Health Care Program

Child and Adolescent Exposure to Trauma: Comparative Effectiveness of Interventions Addressing Trauma Other Than Maltreatment or Family Violence

Executive Summary

Background

Approximately two-thirds of children and adolescents will experience at least one traumatic event, creating a critical need to identify effective child trauma interventions. While most children exposed to trauma do not experience long-term negative sequelae in terms of psychological and social functioning, some go on to develop traumatic stress syndromes, including post-traumatic stress disorder (PTSD).[1-3] Studies have indicated that childhood traumatic stress syndromes are associated with a high degree of impairment that can carry into adolescence and adulthood. For example, childhood PTSD increases the risk for developing comorbid mental disorders, such as depression, substance abuse, and conduct disorder.[4] Suicidality is a particular concern for children with PTSD.[4,5] Decreased social, home, school (lower academic achievement[6]), and relational functioning have also been observed in children and adolescents with PTSD. Although several guidelines on the treatment of PTSD during childhood and adolescence exist, the recommendations have not been largely based on evidence resulting from Comparative Effectiveness Reviews. Furthermore, the guidelines offer inconsistent recommendations for interventions.

Effective Health Care Program

The Effective Health Care Program was initiated in 2005 to provide valid evidence about the comparative effectiveness of different medical interventions. The object is to help consumers, health care providers, and others in making informed choices among treatment alternatives. Through its Comparative Effectiveness Reviews, the program supports systematic appraisals of existing scientific evidence regarding treatments for high-priority health conditions. It also promotes and generates new scientific evidence by identifying gaps in existing scientific evidence and supporting new research. The program puts special emphasis on translating findings into a variety of useful formats for different stakeholders, including consumers.

The full report and this summary are available at **www.effectivehealthcare.ahrq.gov/reports/final.cfm**.

Scope

The current review is the second in a two-part series focusing on interventions that address child trauma. The first in the series focuses on the comparative effectiveness

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of interventions that address child exposure to trauma in the form of maltreatment (physical, sexual, and emotional/psychological abuse, and neglect).[7] This review, the second in the series, addresses the treatment of children exposed to traumatic events other than child maltreatment or family violence, some of whom are already experiencing symptoms. Interventions for children exposed to family violence (i.e., intimate partner violence and other forms of violence exposure in the home) are not covered by either review given the heterogeneity in this population and the interventions used to treat family violence exposure. That is, children who witness but do not directly experience interpersonal violence represent different clinical populations in terms of the nature of the relationship disturbance and implications for treatment. For the sake of brevity, we refer to children and adolescents as "children" for the remainder of this report.The review also seeks to understand whether evidence exists for differences in the efficacy of interventions by specific child or treatment characteristics or by setting of the delivered intervention. Finally, the review attempts to identify adverse events associated with the interventions reviewed.

An overarching goal of this review is to identify gaps in the current scientific literature, and to highlight important areas for future research, to help build the evidence base for interventions targeting traumatic stress symptoms or syndromes with children exposed to trauma other than maltreatment or family violence.

Our population, intervention, comparator, outcome, timing, and setting (PICOTS) framework presented in the Methods section defines the populations, interventions, comparators, outcomes, and settings of interest for the review. The results presented in this review, therefore, only apply to this specific set of PICOTS. We note several other differences across studies, such as type or severity of trauma experienced by children included in each tested intervention, as limitations to the applicability of findings.

Key Questions

Key Question 1: What is the comparative effectiveness of different types of pharmacotherapy, psychotherapy, complementary and alternative therapy, or other therapy, such as combined, for children ages 0 to 17 years exposed to trauma other than maltreatment? Traumatic stress symptoms and syndromes, as well as other specific outcomes examined, are detailed in Figure A.

Key Question 2: What is the comparative effectiveness of different types of pharmacotherapy, psychotherapy, complementary and alternative therapy, or other therapy, such as combined, for children ages 0 to 17 years with traumatic stress symptoms from trauma other than maltreatment who are already experiencing symptoms? Traumatic stress symptoms and syndromes, as well as other specific outcomes examined, are detailed in Figure A.

Key Question 3: Do interventions targeting children who were exposed to trauma and are already experiencing symptoms vary in their effectiveness by characteristics of the child, treatment, or setting?

Key Question 4: What are the harms (e.g., low adherence/dropouts, side effects, retraumatization) associated with specific types of therapies targeting children exposed to trauma or targeting children who were exposed to trauma and are already experiencing symptoms?

Figure A depicts the analytic framework that presents the Key Questions (KQs) within the context of PICOTS. KQ 1 addresses the efficacy of interventions for children exposed to trauma other than maltreatment and family violence. KQ 2 examines the efficacy of interventions for children exposed to trauma other than maltreatment and family violence who are already experiencing symptoms. KQ 3 evaluates the efficacy of interventions in different subpopulations, varying by child, treatment characteristics, or setting. KQ 4 illustrates the harms associated with specific interventions, including retraumatization, side effects, low adherence, and dropout.

Methods

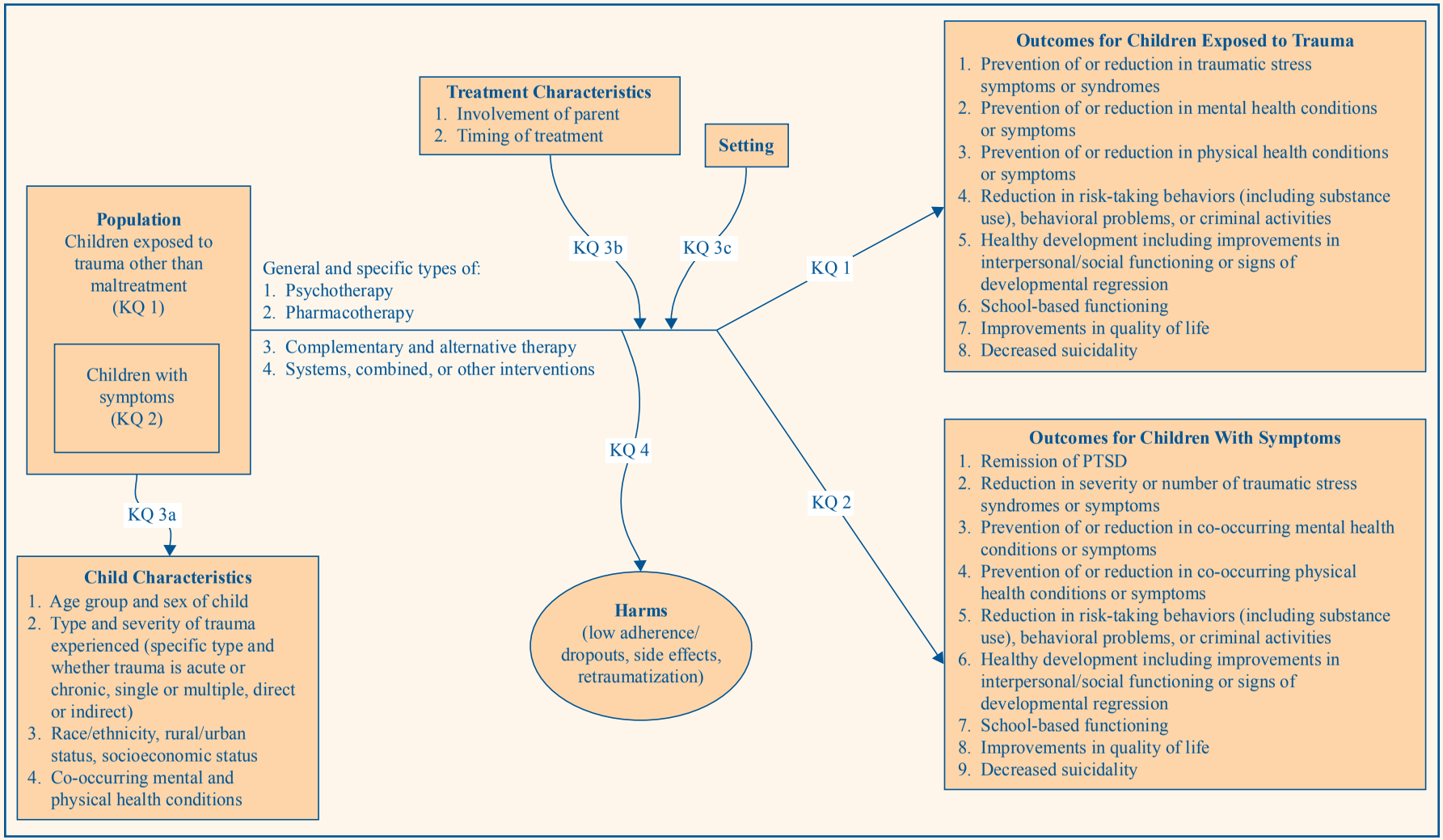
Topic Refinement

The topic nomination resulted from a public process. With Key Informant input, the RTI International-University of North Carolina at Chapel Hill (RTI-UNC) Evidence-based Practice Center (EPC) worked on clarifying the scope of the project. After we generated an analytic framework, preliminary KQs, and preliminary inclusion and exclusion criteria in the form of PICOTS, AHRQ posted KQs for public comment from November 15, 2011, to December 13, 2011. We incorporated public commenton the KQs and clinical and methodological input from a Technical Expert Panel into the final research protocol, which was also posted on the AHRQ Web site on March 26, 2012.

Literature Search and Review Strategy

We systematically searched, reviewed, and analyzed the scientific evidence for each KQ. We began with a focused PubMed search on traumatic stress disorders and psychological and pharmacological therapies using a variety of terms, medical subject headings (MeSH®), and

Figure A. Analytic framework



KQ = Key Question, PTSD = post-traumatic stress disorder

major headings. We limited results to children and human-only studies published from 1990 onward. We selected this time range to ensure therapeutic modalities were currently applicable. Because of limited resources, we also limited the search to studies published in English; however, this may bias the report because more studies from English-speaking countries were included.

We searched the Cochrane Library, Embase®, PsycINFO®, CINAHL, International Pharmaceutical Abstracts (IPA), and Web of Science using analogous search terms. We conducted quality checks to ensure that known studies were identified by the search. If they were not, we revised and reran our searches. Further, AHRQ requested Scientific Information Packets (SIPs) from the developers and distributors of the interventions identified in the literature review. SIPs allow an opportunity for the intervention developers and distributors to provide us with both published and unpublished data that they believe should be considered for the review. We included studies from the SIPs that meet our review criteria.

Two trained members of the research team independently reviewed each of the titles and abstracts against the inclusion and exclusion criteria listed in Table A. We applied the same criteria to systematic reviews and primary studies. For each article that either or both reviewers chose to include, both members of the research team reviewed the full text for eligibility against the inclusion and exclusion criteria. During full-text review, if both reviewers agreed that a study did not meet the eligibility criteria (including designation of high risk of bias), we excluded the study. Reviewers resolved conflicts by discussion and consensus or by consulting a third member of the review team.

For studies that met our inclusion criteria, a trained reviewer abstracted information into structured evidence tables; a second senior member of the team reviewed all data abstractions for completeness and accuracy. Reviewers resolved conflicts by discussion and consensus or by consulting a third member of the review team.

Risk-of-Bias Assessment

Two independent reviewers assessed risk of bias (internal validity) for each study using predefined criteria described in the AHRQ "Methods Guide for Effectiveness and Comparative Effectiveness Reviews,"[8] using questions specified in the RTI Item Bank[9] and the Cochrane Risk of Bias tool.[10] We resolved disagreements between the two reviewers by consulting an experienced member of the team. We selected items based on relevance to the topic and anticipated sources of bias. We assessed the potential for selection bias, performance bias, attrition bias, detection bias, and reporting bias. We then rated each

Table A. Population, intervention, comparator, outcome, timing, and setting

Domain	Description
Population	• Children ages 0–17 years who have been exposed to a trauma other than maltreatment, neglect, or family violence. Specific types of trauma include terrorism, community violence, war, school violence, natural disasters, medical trauma, and death of loved ones[a] • Children ages 0–17 years who have been exposed to a trauma other than maltreatment, neglect, or family violence who already are experiencing symptoms[a]
Intervention	**Interventions for children exposed to trauma** • Psychotherapy (e.g., cognitive behavioral therapy, hypnotherapy, psychodynamic therapy, community- or classroom-based interventions) • Pharmacotherapy (e.g., SSRIs, TCAs, benzodiazepines, beta blockers, alpha blockers, mood stabilizers, antipsychotics, combined therapy, other therapy) **Interventions for children exposed to trauma who already have symptoms** • Psychotherapy, including trauma-focused vs. nontrauma-focused groupings (e.g., cognitive behavioral therapy, parent-child interaction therapy, child-parent psychotherapy, eye movement desensitization and reprocessing, dialectical behavior therapy, complementary and alternative therapies [e.g., equine-assisted therapy], and community- or classroom-based interventions) • Pharmacotherapy (e.g., SSRIs, TCAs, benzodiazepines, beta blockers, alpha blockers, mood stabilizers, antipsychotics, combined therapy, other therapy)
Comparator	The comparison condition as defined in the respective studies, including active controls (such as usual care) and inactive controls (such as wait-list groups)

Table A. Population, intervention, comparator, outcome, timing, and setting (continued)	
Domain	**Description**
Outcome	**Outcomes for studies targeting children exposed to trauma[b]** • Prevention of or reduction in traumatic stress symptoms or syndromes (e.g., PTSD, acute stress disorder, developmental trauma disorder) • Prevention of or reduction in mental health conditions or symptoms (e.g., depression, anxiety) • Prevention of or reduction in physical health conditions or symptoms (e.g., sleep disorders, eating disorders, pain, overweight or obesity, asthma, cardiovascular problems, gastrointestinal problems, headaches) • Reduction in risk-taking behaviors (including substance use), behavioral problems (including conduct disorder and ADHD), or criminal activities • Healthy development (including improvements in interpersonal and social functioning), or reductions in the signs of developmental regression • School-based functioning • Improvements in quality of life • Decreased suicidality • Low adherence/dropouts • Side effects • Retraumatization **Outcomes for studies targeting children exposed to trauma who already have symptoms[b]** • Remission of PTSD • Reduction in severity or number of traumatic stress syndromes or symptoms • Prevention of or reduction in co-occurring mental health conditions or symptoms (e.g., depression, anxiety) • Prevention of or reduction in co-occurring physical health conditions or symptoms (e.g., sleep disorders, eating disorders, pain, overweight or obesity, asthma, cardiovascular problems, gastrointestinal problems, headaches) • Reduction in risk-taking behaviors (including substance use), behavioral problems (including conduct disorder and ADHD), or criminal activities • Healthy development (including improvements in interpersonal/social functioning), or signs of developmental regression • School-based functioning • Improvements in quality of life • Decreased suicidality • Low adherence/dropouts • Side effects • Retraumatization
Timing	• All outcomes included, regardless of timing of measurement
Setting	• Studies conducted in the United States or internationally • Specialty (e.g., outpatient and inpatient primary care or mental health care settings) • Nonspecialty (e.g., schools, community-based providers, shelters) • Home-based settings and out-of-home care (e.g., residential treatment)

Table A. Population, intervention, comparator, outcome, timing, and setting (continued)	
Domain	**Description**
Publication type	• Not editorials, letters to the editor
Study design	• Included designs: systematic reviews, randomized controlled trials, nonrandomized controlled trials, prospective cohort studies, and nested case-control studies • Excluded designs: case reports, case series, cross-sectional studies, nonsystematic reviews, retrospective cohort studies, non-nested case-control studies
Sample size	• N ≥10
Time of publication	• 1990 to present
Language of publication	• English
Risk of bias	• Low or medium. We excluded studies with a high risk of bias, as determined by one or more significant flaws that invalidated the findings (e.g., attrition bias of overall attrition ≥20% or differential attrition ≥15% without appropriate handing of missing data, such as the use of intention-to-treat analyses), detection bias, selection bias, performance bias, and/or reporting bias.

ADHD = attention deficit hyperactivity disorder; N = number; PTSD = post-traumatic stress disorder; SSRI = selective serotonin reuptake inhibitors; TCA = tricyclic antidepressants

[a]At least 95% of the sample was required to be between 0 and 17 years of age.

[b]At least one outcome had to relate to the assessment of trauma for the study to be included. For each study, we also included findings that showed nonbeneficial outcomes associated with the intervention (e.g., no significant changes in outcomes between groups or significantly worse outcomes in the intervention group).

study as having a low, medium, or high risk of bias for individual outcomes.

A study with a low risk of bias had a strong design, measured outcomes appropriately, used appropriate statistical and analytical methods, reported low attrition, and reported methods and outcomes clearly and precisely. Studies with a medium risk of bias did not meet all criteria required for low risk of bias. These studies had flaws in design or execution (e.g., imbalanced recruitment, high attrition) but they provided information (e.g., through sensitivity analysis) to allow the reader the ability to evaluate and determine that those flaws did not likely cause major bias. Missing information often led to a medium risk of bias rating (as opposed to low).

Studies with a high risk of bias had at least one or more major flaws that likely caused significant bias, and, thus, invalidated the results. Major flaws precluded the ability to draw causal inferences between the intervention and the outcome. Examples of flaws likely to result in a high risk of bias rating include poorly randomized studies that failed to account for imbalances at baseline; observational studies that failed to account for potential confounders; and studies of any design with overall attrition of 20 or more or differential attrition of 15 percent or more without appropriate handling of missing data, such as the use of intention-to-treat analyses.

Data Synthesis

We report results from direct comparisons of different interventions. Quantitative analysis was not appropriate because of heterogeneity, insufficient numbers of similar studies, or insufficiency or variation in outcome reporting; thus, we synthesized the data qualitatively. We report magnitude of effect data provided by authors in the studies reviewed. We did not perform additional effect size calculations with the exception of one study that provided the effect size without the significance level. We did not attempt indirect comparisons given the heterogeneity of usual care comparators. KQ 1, KQ 2, and KQ 4 present outcomes categorized by intervention type. KQ 3 presents outcomes of interventions categorized by child characteristics. Because the intent of KQ 3 was to evaluate whether characteristics of the child moderated the effect of the interventions, we included only those studies that tested whether the effect of an intervention on outcome differed by subgroup characteristics via an interaction term. We did not synthesize the evidence for KQ 3 from studies that met our overall inclusion criteria for KQ 1 and KQ 2 but did not compare effects between subgroups. We elected not to summarize findings that presented results stratified by subgroups because of the risk of over-interpreting results from underpowered subsamples.

Strength of Evidence Grading

We graded the strength of evidence (SOE) for all available outcomes in our prespecified list based on the guidance established for the EPC program.[11] This approach incorporates four key domains: risk of bias (including study design and aggregate quality), consistency, directness, and precision of the evidence. We used the SOE grades defined by Owens and colleagues.[11] The SOE grades are:

- **High—High confidence that the evidence reflects the true effect.** Further research is very unlikely to change our confidence in the estimate of effect.
- **Moderate—Moderate confidence that the evidence reflects the true effect.** Further research may change our confidence in the estimate of the effect and may change the estimate.
- **Low—Low confidence that the evidence reflects the true effect.** Further research is likely to change our confidence in the estimate of the effect and is likely to change the estimate.
- **Insufficient—Evidence either is unavailable or does not permit estimation of an effect.**

At a minimum, two reviewers assessed each domain for each key outcome and resolved any differences by consensus. We used a qualitative process, considering each of the domains, to determine the overall SOE grade for each relevant outcome. Our team discussed differences in overall SOE grades to reach consensus.

For outcomes having only a single study to provide evidence, we evaluated consistency as not applicable. When a study had estimates of effects with confidence intervals that permitted clinically distinct conclusions, we rated that domain as imprecise. When studies provided sufficient information (i.e., standard deviation or standard error) to calculate confidence intervals around between-group changes without making assumptions about the correlation between available measures of variance, we calculated confidence intervals for the difference in the change in outcomes for the study groups. For studies that did not provide estimates of variance for between-group differences in outcomes, we relied on either measures of statistical significance from between-group adjusted analyses (where available) or unadjusted analyses if no other data were available. We did not rely solely on measures of statistical significance to evaluate precision for differences in post-test assessment that failed to account for pretest differences. We also considered whether studies were adequately powered.

For outcomes with a single study with imprecise results and for which power was not ensured, we considered this to be insufficient evidence that the estimate from the single study was robust enough to have any confidence in the finding. For a single study with precise results, we graded it as low. Therefore, although effectiveness is synonymous with neither precision nor SOE, individual studies that showed an effect generally merited a rating of low SOE.

Applicability

We assessed the applicability of the evidence following guidance from Atkins and colleagues.[12] We used the PICOTS framework to explore factors that affect or limit applicability.

Results

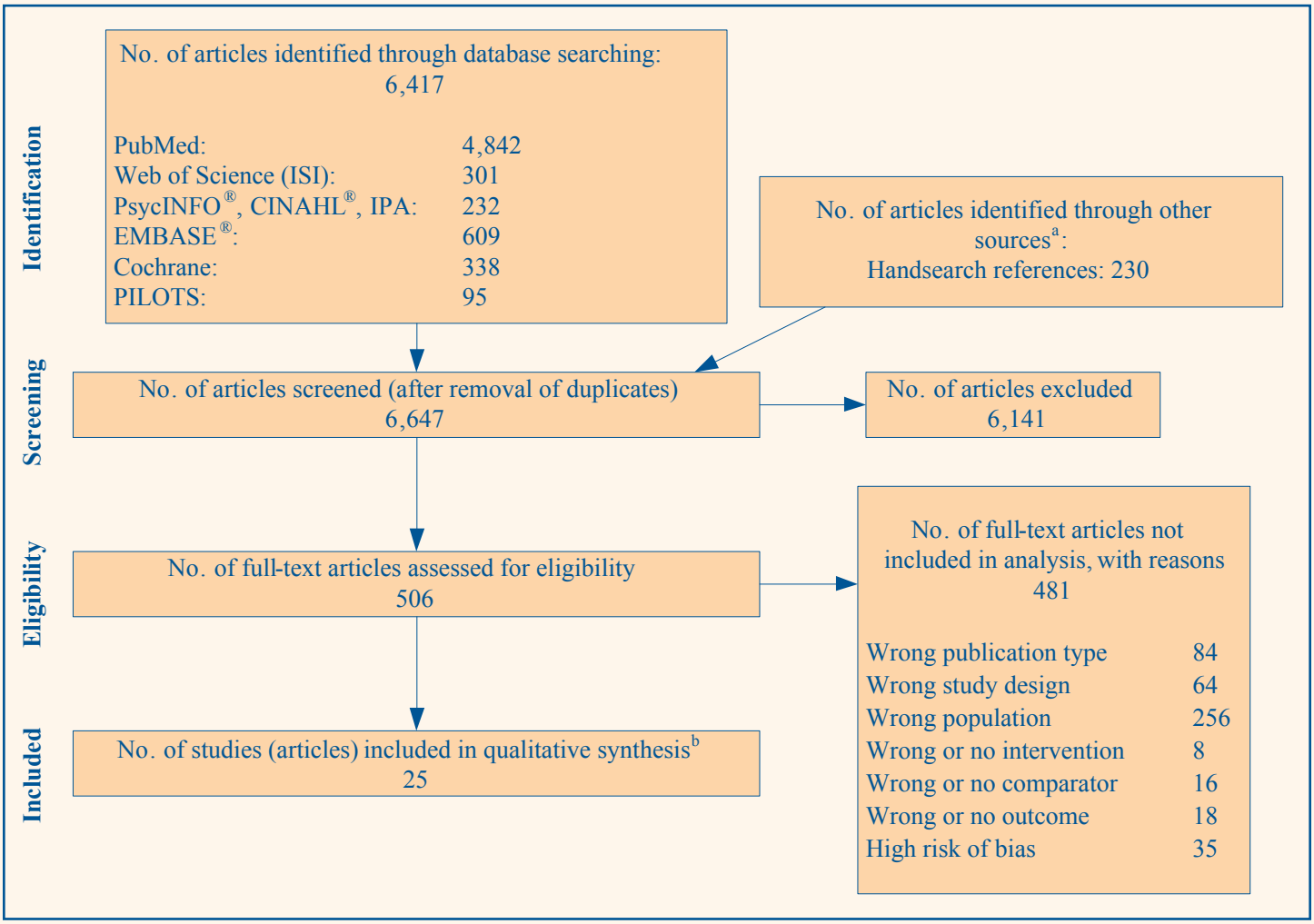
We provide a summary of results by KQ. Detailed descriptions of included studies, key points, detailed synthesis, summary tables, and expanded SOE tables that include the magnitude of effect can be found in the full report. Our summary of results presents the SOE grades.

Results of Literature Searches

Figure B presents our literature search results. Literature searches through August 3, 2012, for the current report identified 6,647 unduplicated citations. We excluded 6,141 at the title and abstract review stage. For the 506 articles reviewed at the full-text stage, we eliminated 446 for a variety of reasons before risk-of-bias review. We recorded the reason for excluding full-text publications and provide a table of all excluded studies in Appendix C of the full report, organized by reason for exclusion. The most common reasons for exclusion at the full-text stage were wrong population or wrong publication type. After assessing risk of bias for all included studies (before data abstraction), we eliminated 35 studies that we rated high risk of bias (described in detail below).

The 25 articles included in this review represent 23 studies testing 20 interventions. Of the 25 included articles, 16 were RCTs, 6 were cluster RCTs, 2 were prospective cohort studies, and 1 was a systematic review. We assessed 19 included articles as medium risk of bias and 5 as low risk of bias. We did not assess the risk of bias for the single systematic review that met our criteria because tools such as AMSTAR cannot easily be applied to systematic reviews with no included studies. No other systematic reviews could be used in our review in their entirety because their inclusion/exclusion criteria did not match ours, although we evaluated the citation lists for several systematic reviews for additional studies.

Figure B. Literature search results



NO = number

[a]Additional articles were identified through grey literature searches, scientific information packet searches, peer and public review comments, and by means of manual entry or Medline, ProQuest, and Worldcat Online Computer Library Center search engines.

[b]We identified one systematic review[15] for inclusion in this report. The review found no eligible studies.

We reviewed 58 unduplicated articles, obtained through SIPs, 43 of which we excluded during the abstract review stage and 13 of which we excluded during the full-text review stage. From the remaining two articles, we eliminated one study[13] because of high risk of bias and included the other study[14] in this report. Of the 58 articles we examined, 5 were unpublished; 4 of these studies were excluded during the abstract review stage, and 1 was excluded during the full-text review stage.

Our search of the grey literature yielded six articles, two of which we excluded during the abstract review stage and one of which we excluded during the full-text review stage. After assessing risk of bias for the remaining three studies, we eliminated one study[16] for high risk of bias and included the other two studies[17,18] in this report. Of the six studies we examined, only one was unpublished; however, it was eliminated at the risk-of-bias review stage.

Overall, the evidence from 21 trials and 1 observational study (25 articles) evaluated 6 types of interventions targeting children with trauma exposure (7 studies, 8 articles)[18-25] and 13 types of interventions targeting children with trauma exposure already experiencing traumatic stress symptoms (15 studies, 16 articles).[15,17,26-39] These interventions were marked by substantial heterogeneity in components, dose, frequency, involvement of family members, and mode and method of delivery. The wide variety of approaches presented challenged our attempts to combine or categorize interventions as we had anticipated. We kept our main framework of organization by psychotherapy and

pharmacotherapy approaches. For the psychotherapy approaches, we described cognitive-based therapies first, followed by other types of psychotherapies. For the cluster of school-based therapies, we first reported on specific individualized approaches and school-based approaches identified in our protocol (e.g., Cognitive Behavioral Intervention for Trauma in Schools [CBITS]) that have both individual and group components. Following these interventions, we described school-based psychotherapies with mixed components.

Although we identified numerous potential interventions in our protocol, few studies met our inclusion criteria, likely because the interventions had not been implemented among children with trauma from sources other than maltreatment or family violence. For example, we did not find any evidence on child-parent psychotherapy, an intervention primarily used for maltreated children.

We also dropped 35 studies for high risk of bias. We most commonly eliminated studies with high risk of bias because of selection bias (n=30), including poor randomization, lack of allocation concealment for trials, and failure to control for confounding factors for observational studies (see Appendix E in the full report for more details). Other common reasons for the removal of studies with high risk of bias included attrition bias or differential attrition bias (n=12; e.g., loss to followup of ≥20% or differential loss to followup of ≥15% without appropriate handling of missing data), detection bias (n=11; e.g., bias in outcome assessment), and performance bias (n=9; e.g., not controlling for concurrently occurring or unintended interventions). Of these, we dropped 34 of 35 for multiple reasons; we dropped only 1 study with a single reason for the high risk-of-bias rating that invalidated all findings: a 77% drop-out rate (see Appendix E in the full report for more details).

Having a study design less rigorous than a controlled trial did not drive our decision to drop a study for high risk of bias; we excluded only 4 of the 35 studies that had observational (prospective cohort) study designs. Most of the dropped studies tested interventions similar to those included in our review (e.g., psychotherapeutic interventions, such as cognitive behavioral therapy [CBT] and eye movement desensitization and reprocessing [EMDR]; exposure therapies; school-based interventions, such as CBITS; and pharmacotherapeutic interventions, such as sertraline and other SSRIs). Although high risk-of-bias studies may have added to some of the sparse evidence in this literature, their inclusion would not have materially altered SOE because they would not have increased our confidence in the estimate of effect.

Key Question 1: Treatment Based on Exposure

We sought evidence on the effectiveness of interventions targeting children exposed to trauma according to traumatic stress, mental health, physical health, and other outcomes. These outcomes included the following:

- Prevention of traumatic stress symptoms or syndromes (e.g., PTSD, acute stress disorder, developmental trauma disorder [DTD])
- Prevention of or reduction in mental health conditions or symptoms (e.g., depression, anxiety)
- Prevention of or reduction in physical health conditions or symptoms (e.g., sleep disorders, eating disorders, pain, overweight or obesity, asthma, cardiovascular problems, gastrointestinal problems, headaches)
- Reduction in risk-taking behaviors, including substance use; reduction in behavioral problems, including conduct disorder and attention deficit hyperactivity disorder (ADHD); or reduction in criminal activities
- Healthy development, including improvements in interpersonal and social functioning or reductions in developmental regression
- School-based functioning
- Improvements in quality of life
- Decreased suicidality

At least one outcome from each included study had to relate to the assessment of trauma symptoms or syndromes. We also included findings that showed nonbeneficial outcomes associated with the intervention (e.g., no significant changes in outcomes between groups or significantly worse outcomes in the intervention group).

Summary of Findings by Intervention

Seven studies (in eight articles) on six different interventions provided information on a subset of these outcomes.[19-25] Five interventions evaluated a variety of psychotherapeutic approaches compared with wait-list controls,[22-24] no treatment,[19,20] usual care,[18] or supportive therapy;[21] the sixth intervention evaluated the efficacy of propranolol compared with placebo.[25] The propranolol study[25] and the early psychological intervention study[18] found no improvement in any outcomes. All other interventions reported some improvement in one or more outcomes.[19-24]

Three of four interventions showing evidence of benefit (trauma-focused cognitive behavioral therapy [TF-CBT] and both mixed school group interventions--ERASE Stress and Overshadowing the Threat of Terrorism) compared

outcomes from interventions with outcomes from wait-list controls or no intervention.[19,20,22-24] The Child and Family Traumatic Stress Intervention (CFTSI) trial was the only study showing evidence of benefit with an active group comparator.[21]

Summary of Findings Across Interventions

Table B presents a summary of the SOE across all evaluated outcomes for interventions targeting children exposed to trauma. All studies evaluated traumatic stress symptoms, although the specific measure varied by study.

Five studies (four treatment types) evaluated PTSD diagnosis[21-25]; of these, three studies (two treatment types, CFTSI and mixed school group ERASE Stress) found evidence of improvement favoring intervention arms.[21-23] Four studies (three treatment types) evaluated severity of PTSD symptoms;[22-25] three studies representing two treatments found evidence of improvement favoring intervention arms (both school-based interventions).[22-24] Three studies (one study presented in two publications) evaluating PTSD symptoms found evidence of improvement[19-21,24]; the early intervention study found no benefit (early psychological intervention).[18]

Six studies evaluated mental health outcomes, specifically anxiety, depression, and dissociative symptoms.[19-23,24] Both studies evaluating anxiety[21,24] reported improvement in anxiety; three studies (four publications) evaluating

Table B. Summary of strength of evidence grades for interventions targeting children exposed to trauma (Key Question 1)

Intervention	Comparator	Number of Studies	PTSD Diagnosis	PTSD Severity	PTSD Symptoms	Anxiety	Depression	Dissociative Symptoms	Somatic Complaints	Physiological Reactivity	Functional Impairment	Behavioral Problems
Trauma-focused cognitive behavioral therapy (school group and individual)	No treatment	1[19,20]	NE	NE	L (+)	NE	L (+)	NE	NE	NE	NE	NE
Child and Family Traumatic Stress Intervention	Supportive therapy	1[21]	L (+)	NE	L (+)	L (+)	NE	I	NE	NE	NE	NE
Mixed (psychoeducational material, cognitive behavioral skills, meditative practices, bioenergetic exercises, art therapy, narrative techniques, and home assignments), ERASE Stress (school groups)	Wait-list control that received religious classes	2[22,23]	L (+)	L (+)	NE	NE	L (+)	NE	L (+)	NE	L (+)	NE
Mixed (psychoeducational material and skills training with meditative practices, bioenergetic exercises, art therapy, and narrative techniques for reprocessing traumatic experiences), Overshadowing the Threat of Terrorism (school groups)	Wait-list control	1[24]	I	L (+)	L (+)	L (+)	NE	NE	L (+)	NE	L (+)	NE
Early psychological intervention	Usual care	1[18]	NE	NE	I	NE	I	NE	NE	NE	NE	I
Propranolol	Placebo	1[25]	I	NE	I	NE	NE	NE	NE	I	NE	NE

I = insufficient strength of evidence because of lack of evidence of effect; L (+) = low strength of evidence of benefit; NE = not evaluated by study authors; PTSD = post-traumatic stress disorder

depression[19,20,22,23] reported improvement in depression; the early psychological intervention found no improvement in depressives symptoms;[18] and one study found no improvement in dissociative symptoms.[21]

Four studies evaluated physical health outcomes.[22-25] All three studies that evaluated somatic complaints found evidence of benefit favoring the intervention arm.[22-24] A single study evaluating physiological reactivity found no evidence of benefit.[25]

Regarding other outcomes, all three studies that evaluated functional impairment found evidence of benefit.[22-24] The single study that evaluated behavior problems found no evidence of benefit.[18]

Summary of Findings by Outcome

Table C presents detailed findings by outcome for interventions with some evidence of benefit. We rated the evidence as low for all of these outcomes, based on the limited number of studies (generally no more than one study per intervention) and small sample sizes.

Key Question 2: Treatment of Traumatic Stress Symptoms

As in KQ 1, we sought evidence of the effectiveness of interventions designed to treat traumatic stress symptoms in children on a variety of traumatic stress, mental health, physical health, and other outcomes. Specifically, these included:

- Remission of PTSD
- Reduction in severity or number of traumatic stress syndromes or symptoms
- Prevention of or reduction in co-occurring mental health conditions or symptoms (e.g., depression, anxiety)
- Prevention of or reduction in co-occurring physical health conditions or symptoms (e.g., sleep disorders, eating disorders, pain, overweight or obesity, asthma, cardiovascular problems, gastrointestinal problems, headaches)
- Reduction in risk-taking behaviors, including substance use; reduction in behavioral problems, including conduct disorder and ADHD; or reduction in criminal activities
- Healthy development, including improvements in interpersonal/social functioning, or reductions in signs of developmental regression
- School-based functioning
- Improvements in quality of life
- Decreased suicidality

As with KQ 1, at least one outcome from each included study had to relate to the assessment of trauma symptoms or syndromes. We also included findings that showed nonbeneficial outcomes associated with the intervention (e.g., no significant changes in outcomes between groups or significantly worse outcomes in the intervention group).

Summary of Findings by Intervention

Fifteen studies reported on a subset of outcomes for 13 different interventions.[14,17,26-33,35-39] Ten of 13 interventions (presented in 12 studies[14,17,26-33,38,39]) evaluated a variety of psychotherapeutic approaches; of these interventions, 5 (reported in 7 studies) compared outcomes with wait-list controls,[14,26,27,30,31,33,39] and 2 with usual care.[17,32]

Three interventions used active comparators: one compared outcomes for narrative exposure therapy with meditation-relaxation therapy outcomes;[28] one grief- and trauma-focused intervention (GTFI) compared group therapy with individual therapy;[29] and a third compared outcomes for GTFI with coping skills and narrative processing with GTFI with coping skills only.[38] Three of 13 interventions focused on medications: one compared imipramine to chloral hydrate;[35] a second compared imipramine to fluoxetine and placebo;[36] and a third compared sertraline to placebo.[37]

As in the cluster of studies reporting on interventions targeting children exposed to trauma, no pharmacological interventions found evidence of benefit for any outcome, and the sertraline study suggested that the intervention arm fared worse than the control arm.[35-37] Three studies with active arms (Narrative Exposure Therapy and both GTFI treatments) did not report evidence of benefit for any outcome.[28,29,38] All of the other interventions that compared outcomes to wait-list controls found some evidence of benefit for one or more outcomes.[26,27,30,31,33]

Summary of Findings Across Interventions

Table D presents a summary of the SOE across all evaluated outcomes for interventions targeting children exposed to trauma. All studies evaluated traumatic stress symptoms, although the specific measure varied by study.[14,17,26-33,35-39] Four studies evaluated PTSD diagnosis;[26,28-30,38] of these, two found evidence of improvement favoring intervention arms (TF-CBT,

Table C. Summary of results for interventions targeting children exposed to trauma (Key Question 1)

Outcome	Intervention	Comparator	Number of Trials, Number of Participants	Strength of Evidence, Magnitude of Effect	Type of Exposure
PTSD diagnosis	CFTSI	Supportive therapy	1,[21] 106	Low; difference of 4.54 points on the UCLA PTSD-RI Index favoring CFTSI	Mixed (MVA, sexual abuse, witnessing violence, physical assaults, injuries, threats of violence)
	Mixed ERASE Stress (school groups)	Wait-list control that received religious classes	2,[22,23] 273	Low; significantly greater decrease in PTSD diagnosis on the UCLA PTSD-I in one study (24.7% greater decrease in proportion); second study significance not reported (11.3% greater decrease in proportion)	Natural disaster (tsunami), war/terror attacks
PTSD symptoms/severity	TF-CBT	No treatment	1,[19,20] 65	Low; difference of 19.2 points on the child PTSD reaction index at 18 months favoring TF-CBT	Natural disaster (earthquake)
	CFTSI	Supportive therapy	1,[21] 106	Low; difference of 4.71 points on the TSCC PTSD Index favoring CFTSI	Mixed (MVA, sexual abuse, witnessing violence, physical assaults, injuries, threats of violence)
	Mixed ERASE Stress (school groups)	Wait-list control that received religious classes	2,[22,23] 273	Low; significantly greater decrease in PTSD symptom severity on the UCLA PTSD-I in both studies (mean differences of 7.21, 9.0)	Natural disaster (tsunami), war/terror attacks
	Mixed Overshadowing the Threat of Terrorism (school groups)	Wait-list control	1,[24] 142	Low; significantly greater decrease in PTSD symptoms on the UCLA PTSD-I (mean difference of 4.6) and significantly greater decrease in PTSD severity (mean difference of 12.1)	War/terror attacks
Depression symptoms	TF-CBT	No treatment	1,[19,20] 65	Low; difference of 5.7 points on Depression Rating Scale at 18 months favoring TF-CBT	Natural disaster (earthquake)
	Mixed ERASE Stress (school groups)	Wait-list control that received religious classes	2,[22,23] 273	Low; significantly greater decrease in depression symptoms in both studies on the Brief Beck Depression Inventory (mean differences of 1.55, 1.8)	Natural disaster (tsunami), war/terror attacks

Table C. Summary of results for interventions targeting children exposed to trauma (Key Question 1) (continued)

Outcome	Intervention	Comparator	Number of Trials, Number of Participants	Strength of Evidence, Magnitude of Effect	Type of Exposure
Anxiety symptoms	CFTSI	Supportive therapy	1,[21] 106	Low; difference of 5.52 points on the TSCC Anxiety Index favoring CFTSI	Mixed (MVA, sexual abuse, witnessing violence, physical assaults, injuries, threats of violence)
	Mixed Overshadowing the Threat of Terrorism (school groups)	Wait-list control	1,[24] 142	Low; significantly greater decrease in generalized anxiety symptoms (mean difference of 2.8) and significantly greater decrease in separation anxiety symptoms on the SCARED (mean difference of 2.4)	War/terror attacks
Somatic complaints	Mixed ERASE Stress (school groups)	Wait-list control that received religious classes	2,[22,23] 273	Low; significantly greater decrease in somatic complaints in both studies on the DPS (mean differences of 1.01, unknown magnitude in second study)	Natural disaster (tsunami), war/terror attacks
	Mixed Overshadowing the Threat of Terrorism (school groups)	Wait-list control	1,[24] 142	Low; significantly greater decrease in somatic complaints on the DPS (mean difference of 1.1)	War/terror attacks
Functional impairment	Mixed ERASE Stress (school groups)	Wait-list control that received religious classes	2,[22,23] 273	Low; significantly greater decrease in functional impairment in both studies on the DPS (mean differences of 2.45, 2.0)	Natural disaster (tsunami); war/terror attacks
	Mixed Overshadowing the Threat of Terrorism (school groups)	Wait-list control	1,[24] 142	Low; significantly greater decrease in functional impairment on four items from the Childhood Diagnostic Interview Schedule (mean difference of 1.8)	War/terror attacks

CFTSI = Child and Family Traumatic Stress Intervention; DPS = DISC predictive scales; ERASE Stress = Enhancing Resiliency Among Students Experiencing Stress; MVA = motor vehicle accident; PTSD = post-traumatic stress disorder; SCARED = Screen for Child Anxiety Related Emotional Disorders; TF-CBT = trauma-focused cognitive behavioral therapy; TSCC = Trauma Symptom Checklist for Children; UCLA PTSD-I = University of California, Los Angeles Posttraumatic Stress Disorder–Index for DSM-IV; UCLA PTSD-RI = University of California, Los Angeles Posttraumatic Stress Disorder Reaction Index, Revised

Table D. Summary of strength of evidence grades for interventions to treat traumatic stress symptoms (Key Question 2)

Intervention	Comparator	Number of Studies	PTSD Diagnosis/Criteria	PTSD Severity	PTSD Symptoms	Anxiety	Depression	Internalizing Behavior	Physical Symptoms	General Functioning	Psychosocial Dysfunction	Acting Out\ Aggression	Shyness/Anxiety	Learning	Quality of Life	Externalizing/ Conduct Problem Behavior	Global Distress	Anger	Supernatural Complaints
Trauma-focused cognitive behavioral therapy	Wait-list control	1[26]	L (+)	NE	L (+)	L (+)	L (+)	NE	NE	NE	NE	NE	NE	NE	NE	NE	NE	NE	NE
Cognitive processing therapy	Wait-list control	1[27]	NE	NE	L (+)	NE	L (+)	NE	NE	NE	NE	NE	NE	NE	NE	NE	NE	NE	NE
Narrative exposure therapy	Meditation-relaxation therapy	1[28]	I	NE	I	NE	NE	NE	I	I	NE	NE	NE	NE	NE	NE	NE	NE	NE
Group grief- and trauma-focused intervention	Individual grief- and trauma-focused Intervention	1[29]	NE	NE	I	NE	I	NE	NE	NE	NE	NE	NE	NE	NE	NE	NE	NE	NE
Grief-and trauma-focused intervention with coping skills and narrative processing	Grief-and trauma-focused intervention with coping skills only	1[38]	I	NE	I	I	I	I	NE	NE	NE	NE	NE	NE	NE	I	I	NE	NE
Emotion regulation therapy	Relational supportive therapy	1[17]	NE	NE	I	I	I	NE	NE	NE	NE	NE	NE	NE	NE	NE	NE	I	NE
Eye movement desensitization and reprocessing	Wait-list control	1[30]	L (+)	NE	L (+)	I	I	I	I	I	NE	NE	NE	NE	NE	I	NE	NE	NE

Table D. Summary of strength of evidence grades for interventions to treat traumatic stress symptoms (Key Question 2) (continued)

Intervention	Comparator	Number of Studies	PTSD Diagnosis/Criteria	PTSD Severity	PTSD Symptoms	Anxiety	Depression	Internalizing Behavior	Physical Symptoms	General Functioning	Psychosocial Dysfunction	Acting Out\ Aggression	Shyness/Anxiety	Learning	Quality of Life	Externalizing/ Conduct Problem Behavior	Global Distress	Anger	Supernatural Complaints
Cognitive Behavioral Intervention for Trauma in Schools	Wait-list control	2[14,31]	NE	NE	I	NE	L (+)	NE	NE	NE	L (+)	I	I	I	I	NE	NE	NE	NE
Trauma and grief component therapy, (school groups)	Usual care	1[32]	NE	NE	L (+)	NE	L (+)	NE	NE	NE	NE	NE	NE	NE	NE	NE	NE	NE	NE
Mixed (cognitive behavioral therapy techniques and creative expressive elements), school groups	Wait-list control	2[33,39]	NE	NE	I	I	I	NE	NE	I	I	I	NE	NE	NE	L (+)	NE	NE	I
Imipramine	Chloral hydrate or placebo	2[35,36]	NE	NE	I	NE	NE	NE	NE	NE	NE	NE	NE	NE	NE	NE	NE	NE	NE
Fluoxetine	Placebo	1[36]	NE	NE	I	NE	NE	NE	NE	NE	NE	NE	NE	NE	NE	NE	NE	NE	NE
Sertraline	Placebo	1[37]	NE	L (-)	L (-)	NE	I	NE	NE	NE	NE	NE	NE	NE	L (-)	NE	NE	NE	NE

I = insufficient strength of evidence because of lack of evidence of effect; L (+) = low strength of evidence of benefit; L (-) = low strength of evidence of no benefit; NE = not evaluated by study authors; PTSD = post-traumatic stress disorder

EMDR).[26,30] Fifteen studies evaluated PTSD symptoms, but only four interventions were graded as having low SOE of improvement.[26,27,30,32] One study suggested evidence of worse outcomes for the sertraline intervention arm, compared with the placebo arm, for parent-rated PTSD symptoms and clinician-rated PTSD severity.[37]

Twelve studies representing 10 interventions evaluated mental health outcomes, specifically anxiety, depression, and internalizing symptoms.[14,17,26,27,29-33,37-39] Six studies reported no improvement in one or all outcomes evaluated.[17,29,30,33,37,38] One[26] of 5 interventions reported in 6 studies[17,26,30,33,38,39] evaluating anxiety symptoms reported improvements; 4 interventions reported in 5 studies[14,26,27,31,33] out of 10 interventions reported in 12 studies[14,17,26,27,29-33,37-39] reported improvement in depression; and 2 studies found no improvement in internalizing behaviors.[30,38]

Two studies evaluated physical symptoms or general health outcomes; neither found evidence of benefit.[28,30]

Seven studies evaluated[28,30,31,33,37-39] a range of other outcomes, including functional symptoms, psychosocial dysfunction, acting out or aggression, shyness/anxiety, learning problems, quality of life, externalizing/conduct problem behaviors, global distress, anger, and supernatural complaints. One study suggested evidence of no benefit for quality of life for the intervention arm, sertraline, compared with the placebo arm.[37] Two[28,30] of three studies evaluating general functioning did not find evidence of benefit. A third study found mixed results.[33] One study found evidence of benefit for the intervention arm on psychosocial dysfunction.[31] One[39] of three studies[33,38,39] found evidence of benefit for the intervention arm on externalizing/conduct problem behavior. No studies found any evidence of benefit for acting out or aggression, shyness, learning problems, quality of life, externalizing/conduct problem behaviors, global distress, anger, and supernatural complaints.

Summary of Findings by Outcome

Table E presents detailed findings by outcome for interventions with some evidence of benefit. We rated the evidence as low for all of the outcomes, based on the limited number of studies (generally no more than one study per intervention and no intervention having more than two studies combined) and small sample sizes.

Key Question 3: Treatment Subgroup Comparisons for Interventions Targeting Children Exposed to Trauma, Some of Whom Already Have Symptoms

Our review found only two studies that examined subgroup characteristics that moderated the effect of the intervention tested by an interaction term. We elected not to summarize findings that merely presented results stratified by subgroups because of the risk of over interpreting results from underpowered subsamples.

Both studies that examined subgroup characteristics that moderated the effect of an intervention on an outcome were school based. The first intervention examined the effect of trauma-focused cognitive behavioral therapy (TF-CBT) targeting children exposed to trauma.[20] The second intervention examined the effect of CBT targeting children exposed to trauma who already have symptoms.[34] Both studies examined sex subgroups; in addition, one study evaluated age group and exposure to violence.[34]

The TF-CBT study did not find any differences in relationship between intervention and PTSD symptoms or depression.[20] The CBT study found no significant differences by age group or exposure to violence with respect to PTSD symptoms or functional impairment. The study did, however, find significant differences by sex, suggesting that the intervention effect on PTSD symptoms and functional impairment were greater for girls than boys.[34] Table F presents the findings of the single trial with evidence of subgroup differences with respect to intervention efficacy.

Key Question 4: Harms Associated With Targeting Children Exposed to Trauma, Some of Whom Already Have Symptoms

Five studies reported harms associated with interventions.[26,32,35,36] One study examined harms of TF-CBT versus wait-list control and found no adverse events in either group.[26] No mention was made of how harms were assessed or evaluated.

A second study examined harms of trauma and grief component therapy (TGCT) for adolescents with classroom-based psychoeducation and skills training versus classroom-based psychoeducation and skills

Table E. Summary of results for child post-traumatic stress disorder treatment interventions (Key Question 2)					
Outcome	**Intervention**	**Comparator**	**Number of Trials, Number of Participants**	**Strength of Evidence, Magnitude of Effect**	**Type of Exposure**
PTSD diagnosis	TF-CBT	Wait-list control	1,[26] 24	Low; Cohen effect size 2.20 on the C-RIES scale favoring TF-CBT and Cohen effect size 1.59 on the CAPS-CA scale favoring TF-CBT	Mixed (MVA, assault, witnessed violence)
	EMDR	Wait-list control	1,[30] 27	Low; 75% decrease in the EMDR group versus 0% change in the wait-list control group in number of children with 2 or more DSM-IV criteria	MVA
PTSD symptoms/severity	TF-CBT	Wait-list control	1,[26] 24	Low; Cohen effect size 2.48 on CPSS scale favoring TF-CBT	Mixed (MVA, assault, witnessed violence)
	CBITS	Wait-list control	1,[31] 126	Low; difference of 7 points on CPSS favoring CBITS	Community violence
	CPT	Wait-list control	1,[27] 38	Low; difference of 10.09 points on PSS-SR scale favoring CPT and difference of 14.19 on Impact of Events Scale favoring CPT	Mixed
	EMDR	Wait-list control	1,[30] 27	Low; magnitude of effect not reported by intervention type	MVA
	TGCT (school groups)	Wait-list control	1,[32] 159	Low; reduction in PTSD symptoms of 6.18 favoring TGCT group	War-exposed in Bosnia
	Sertraline	Placebo	1,[37] 129	Low for no benefit; placebo with greater decrease in parent-rated PTSD symptoms over sertraline (LS mean difference 95% CI of -9.1, -0.6 with CSDC); placebo with greater decrease in clinician-rated PTSD severity via CGI-S (LS mean difference 95% CI of -0.8, 0)	Mixed

Table E. Summary of results for child post-traumatic stress disorder treatment interventions (Key Question 2) (continued)

Outcome	Intervention	Comparator	Number of Trials, Number of Participants	Strength of Evidence, Magnitude of Effect	Type of Exposure
Depression symptoms	TF-CBT	Wait-list control	1,[26] 24	Low; difference of 12.6 points on the RCMAS favoring TF-CBT	Mixed (MVA, assault, witnessed violence)
	CBITS	Wait-list control	1,[31] 126	Low; difference of 3.4 points on CDI favoring CBITS	Community violence
	CPT	Wait-list control	1,[27] 38	Low; difference of 7.8 points on BDI scale favoring CPT	Mixed
	TGCT (school groups)	Wait-list control	1,[32] 159	Low; calculated mean between group difference of 2.78 points favoring TGCT	War-exposed in Bosnia
Anxiety symptoms	TF-CBT	Wait-list control	1,[26] 24	Low; difference of 9.7 points on the DSRS favoring TF-CBT	Mixed (MVA, assault, witnessed violence)
Functional impairment	Mixed school group	Wait-list control	1,[33] 403	Low; significantly greater decrease in functional impairment on a 10-item child-reported checklist in treatment group at 1 week (effect size 0.42) and 6 months postintervention (effect size 0.26)	Poverty and political violence/instability
Psychosocial dysfunction	CBITS	Wait-list control	1,[31] 126	Low; difference of 6.4 points on PSC favoring CBITS	Community violence
Conduct problems	Mixed school group	Wait-list control	1,[39] 397	Low; significantly greater reduction in conduct problems in treatment group than in wait-list group (LGCM estimate, SE: -0.132, 0.045; $p<0.01$)	War and political violence/instability
Quality of life	Sertraline	Placebo	1,[37] 129	Low for no benefit; placebo with greater improvement in quality of life than sertraline (LS mean difference 95% CI: 0.2, 6.8)	Mixed

BDI = Beck Depression Inventory; CAPS-CA = Clinician-Administered Post-Traumatic Stress Disorder scale for children and adolescents; CBITS = Cognitive Behavioral Intervention for Trauma in Schools; CDI = Child Depression Inventory; CGI-S = Clinical Global Impressions-Severity Scale; CI = confidence interval; CPSS = Child Post-Traumatic Stress Disorder Symptom Scale; CPT = cognitive processing therapy; C-RIES = Children's Revised Impact of Event Scale; CSDC = Child Stress Disorder Checklist; DSM-IV = "Diagnostic and Statistical Manual of Mental Disorders-IV"; DSRS = Depression Self-Rating Scale; EMDR = Eye movement desensitization and reprocessing; LGCM = Latent Growth Curve Modeling: LS = least-squares; MVA = motor vehicle accident; PSC = Pediatric Symptom Checklist; PSS-SR = Posttraumatic Stress Disorder Symptom Scale Self Report; PTSD = post-traumatic stress disorder; RCMAS = Revised Children's Manifest Anxiety Scale; SE = standard error; TF-CBT = trauma-focused cognitive behavioral therapy; TGCT = Trauma and Grief Component Therapy

Table F. Summary of results for child post-traumatic stress disorder treatment subgroup comparisons (Key Question 3)						
Subgroup	**Intervention**	**Comparator**	**Number of Trials, Number of Participants**	**Outcome**	**Strength of Evidence, Magnitude of Effect**	**Type of Exposure**
Sex	Mixed school group	Wait-list control	1,[33] 403	PTSD symptoms	Low; intervention effect on reducing PTSD symptoms significantly greater for female than male students (Group 1: -0.090 [-0.161 to -0.019] vs. Group 2: 0.060 [-0.011 to 0.131])	Poverty and political violence/instability
				Functional impairment	Low; intervention effect on reducing functional impairment significantly greater for female than male students (Group 1: -0.120 [-0.179 to -0.061] vs. Group 2: 0.012 [-0.047 to 0.071])	Poverty and political violence/instability

PTSD = post-traumatic stress disorder

training alone.[32] The study used a Reliable Change Index (RCI) for post-traumatic stress, depression, traumatic grief, and existential grief in order to quantify the number of reliably deteriorated cases. The authors found no significant differences in reliable deterioration for post-traumatic stress, depression, traumatic grief, and existential grief by study arm at post-treatment or at the 4-month followup.

Three studies evaluated the harms of medications.[35-37] Two studies found no adverse events for imipramine compared with chloral hydrate[35] or placebo,[36] or imipramine compared with fluoxetine.[36] These studies did not, however, report how adverse events or harms were assessed.

One study found no increase in several types of adverse events associated with sertraline compared with placebo, including disturbed sleep, agitation, headache, abdominal pain, nausea, pharyngitis, vomiting, accidental injury, respiratory tract infections, diarrhea, dizziness, hyperkinesis, and rhinitis. However, the study reported some incidents of other types of serious adverse events (undefined), dry mouth, and dysmenorrhea among patients taking sertraline compared with none for patients in the placebo arm. The study reported higher incidents of dropouts because of adverse events, increased suicidality ratings, and active suicidality in the sertraline arm compared with the placebo arm but did not report the results of statistical significance tests.[37]

Discussion

Key Findings

We found a total of 21 trials and 1 cohort study (reported in 25 articles) of either medium or low risk of bias from our review of 6,647 unduplicated abstracts. We did not find studies that attempted to replicate findings of effective interventions; rather, studies tested unique interventions. No pharmacotherapy intervention demonstrated effectiveness. Studies demonstrating improvement in outcomes generally compared results of interventions with waitlist controls. With a single exception, studies comparing interventions with active controls did not show benefit. Some psychotherapy interventions targeting children exposed to trauma appeared promising based on the magnitude and precision of effects found. These interventions were school-based treatments with elements of CBT. There was less compelling evidence regarding potentially promising interventions targeting already existing symptoms; each also had elements of CBT.

The study authors typically evaluated short-term outcomes. The body of available evidence provided no insight into how interventions targeting children exposed to trauma, some of whom already have symptoms, might influence healthy long-term development. We found little evidence on how effectiveness might vary by child characteristics; and we found no evidence on how effectiveness might vary by treatment characteristics or setting. We also found little evidence addressing possible harms associated with psychological treatments. Only pharmacological interventions attempted to assess harms in this vulnerable population.

Applicability

Population

The evidence base of interventions for children exposed to trauma other than sexual trauma and family violence is limited. Although age groups represented by individual studies ranged from 7 to 17 years old and, in some cases, older (up to 19 years old), only two studies included children younger than age 7.[35,36] No studies that addressed KQ1 and recruited children exposed to a traumatic event included children younger than age 7.

In addition, the type of exposure varied widely across studies. The studies targeting children exposed to trauma that addressed KQ 1 included two studies of children exposed to a natural disaster, two studies of children exposed to war/terrorism, three studies of children exposed to accidents, and one study with mixed trauma types.

The treatment studies that addressed KQ 2 included children who exhibited some level of symptoms, but trauma type also differed across studies. Three of the four pharmacotherapy studies[25,35,36] included children treated in an emergency department who had already experienced accidents (motor vehicle, thermal injuries, or mixed), two of which included children experiencing acute stress symptoms.[35,36] The applicability of these findings is unknown in children exposed to mixed traumas, natural disasters, war or political violence, or other types of traumas. Thus, the applicability of the evidence is somewhat limited to characteristics of children included in each specific study.

Intervention

The evidence base reflects the diverse range of intervention approaches in the field. Several interventions noted in the evidence base were not found in this review. Only four trials (two ERASE Stress school-based mixed intervention

trials and two CBITS trials) addressing KQ 2 were able to be combined in the evidence table.

Most interventions varied in intensity, with delivery ranging from 4 to 20 sessions for the psychotherapeutic interventions, and from 1 to 10 weeks for medication administration in the pharmacotherapeutic interventions. Most were low intensity (up to 12 weekly sessions or approximately 3 months in duration); and only one intervention[32] was of medium intensity (13 to 24 weekly sessions or approximately 6 months in duration).

The majority of studies delivered the intervention under more ideal than real-world conditions, such as by staff with specialized training and/or under close supervision of a highly specialized clinician (often the intervention developer). As noted, the interventions analyzed in the results all indicated the use of a manual. However, the interventions varied considerably by degree of dissemination readiness; and the studies offered minimal discussion of fidelity. Thus, the studies did not provide clarity on whether children received interventions as manualized or adapted interventions fit to the target population; the potential for translation of these interventions into real-world settings is, therefore, unclear.

Comparators

The evidence was primarily composed of studies that used inactive controls, usual care, or wait-list[40-42] controls. For treatment studies addressing KQ 2, only two psychotherapies were head-to-head comparisons;[29,38] and only one pharmacotherapy was a head-to-head comparison of two different types of antidepressants[36] versus a third (control) group. The other interventions targeting children exposed to trauma addressing KQ 1 consisted of two inactive control comparisons,[19,20] two usual care comparators,[18,21] and three wait-list controls,[22-24] and, for the single pharmacotherapy trial, one placebo comparator. Most of the remaining KQ 2 psychotherapy trials[14,26-28,30,31,33,39] used wait-list control comparators; two trials had usual care comparators.[17,32] The KQ 2 pharmacotherapy trials used more rigorous sets of comparators including a usual care comparator (chloral hydrate)[35] and a placebo comparator.[37]

Outcomes

Of the many outcomes searched for in the literature, few were found in the studies included in this review. For example, no studies examined decreased suicidality, risk-taking behaviors such as substance use, conduct disorders, criminal activities, or individual physical health conditions such as obesity, cardiovascular disease, or sleep problems as a study outcome. Thus, the applicability of these types of outcomes that concern clinicians is unknown.

In addition, no studies relied on clinician diagnosis of PTSD either during the baseline period or during followup. Studies that did examine PTSD diagnosis as an outcome[21-24,26,28,30] used a self-reported diagnostic instrument such as the University of California, Los Angeles (UCLA) PTSD Index and Child PTSD Symptom Scale (CPSS). None of the mental health outcomes examined were assessed via clinician diagnosis. The evidence base for the efficacy or effectiveness of interventions in improving trauma symptoms or syndromes, mental health outcomes, physical health outcomes, and other outcomes, such as functional impairment and quality of life, were mostly based on child self-report, with few relying on parent[14,30,31,33,38] or teacher reports[14,31] of impairment or behaviors.

Most of the outcomes were measured at baseline and at the end of the interventions. Few followups were completed at multiple end points, and the long-term effects of the interventions are largely unknown. These limitations on outcome measures reduce the applicability for clinicians needing to choose a treatment based on these findings.

Setting

Nearly half of the studies were conducted outside the United States (Armenia,[19,20] Sri Lanka,[22,28,39] Israel,[23,24] the United Kingdom,[26] Bosnia,[32] Switzerland,[18] and Indonesia [33]). Several studies conducted in the Middle East and Asia that were delivered in school settings[22-24,39] may not be applicable to school settings in the United States.

A majority of the pharmacotherapies recruited subjects via the emergency department,[25,35,36] with followup either in the hospital during an inpatient stay or in an outpatient setting.

Limitations of the Review Process

The applicability of our systematic review was limited by the population, outcomes, and setting limits we placed on our included studies. Our exclusions, described in the Methods section, served to focus the review (particularly in relation to its companion on interventions to address child maltreatment) and to control for sources of heterogeneity. Nonetheless, these exclusions necessarily limited the scope of this review. We describe important limitations below.

First, several of our population criteria limited the review. We focused our review on children only ages 0 to 17 because of the differences in intervention types, outcomes of interest, and developmental aspects of how

adults and children process traumatic events. Effectiveness of adult treatments for trauma exposures are covered in a separate AHRQ review.[43] We also excluded studies that examined children exposed to maltreatment or family violence, also described in a separate AHRQ review,[7] because of the critical differences in these types of trauma exposures and the associated impact on type and delivery of the intervention.

Our outcome criteria also limited our review. We required that studies report change in traumatic stress symptoms or syndromes as an outcome to align with our primary objective of examining intervention effectiveness on these outcomes. The criterion requiring traumatic stress symptoms or syndromes as a study outcome resulted in the exclusion of 16 articles that were identified through our search strings.

The nature of trauma interventions targeting other mental health conditions and functioning, such as suicide or conduct problems, may differ in objectives, design, and delivery from trauma interventions targeting traumatic stress symptoms or syndromes. We included these other types of outcomes as secondary outcomes of interest for studies that examined traumatic stress symptoms or syndromes as an outcome because of the importance of identifying other potential benefits that result from a single intervention.

Additional criteria served to focus our review further. We required a publication date of 1990 or later to focus on supportive evidence from currently relevant treatments because of the evolving nature of the field. We also required a sample size of 10 or more to ensure that we focused on hypothesis-testing studies rather than descriptive accounts from case series or case reports. We excluded cross-sectional, nonsystematic reviews, retrospective cohort studies, and non-nested case control studies because these types of study designs make isolating the effect of an intervention difficult to validly assess. Finally, we excluded studies that were not written in English, thus decreasing the applicability to countries where researchers publish in other languages.

Finally, as noted, we limited the synthesis to trials and observational studies with low and medium risk of bias. Given the limitations of the included studies and their applicability to other contexts, however, including high risk-of-bias studies would likely have increased the pool of evidence without resulting in more actionable evidence.

Limitations of the Evidence

This Comparative Effectiveness Review finds that the field of interventions targeting children exposed to trauma other than maltreatment or family violence is still in its infancy. We did not find evidence of publication bias from our review of SIPs and grey literature; we found few trials that addressed each of the KQs of intervention efficacy, and, especially, whether efficacy differed by subgroups or whether the interventions were associated with harms. Most were unique interventions; thus, combining the findings across studies or replicating significant findings was not permitted from the evidence base. Furthermore, several of the known types of interventions used to treat child traumatic stress (noted in the introduction section) were not found in any study included in this review. Therefore, the efficacy of these types of interventions (e.g., child-parent psychotherapy, Skills Training in Affective and Interpersonal Regulation/Narrative Story-Telling, dialectical behavior therapy, structured psychotherapy for adolescents responding to chronic stress, parent-child interaction therapy, trauma systems therapy, particular antidepressants, stimulants, antipsychotics, benzodiazepines, equine-assisted psychotherapy) to treat children exposed to trauma other than maltreatment or family violence was not evaluated in this review.

Data on pharmacological interventions are sparse and marked by methodological limitations. Only one trial targeted children exposed to trauma, and three trials focused on treatment trials for children already experiencing symptoms. These pharmacologic interventions were small trials and none had findings of benefit. Two trials administered medications for only 7 days; this duration is inadequate because antidepressants typically take 1-4 weeks to become effective.[44] Reaching steady-state for serum concentrations for a medication such as fluoxetine typically takes longer than 7 days.[45] None of the included studies determined the actual efficacy of fluoxetine administered for longer durations in accordance with usual practices. Finally, many other types of medications routinely used to treat traumatic stress in adults and children exposed to maltreatment and family violence have not been adequately tested in this population.

In addition, the heterogeneity in samples, particularly with respect to child characteristics and type of trauma, makes synthesis of the findings difficult.

Most studies did not note or study the important clinical distinctions of whether each child had experienced a single trauma or multiple traumas, or whether each child had comorbid mental health conditions that can affect the efficacy of interventions on outcomes.

Few studies included young children (ages 5 or younger), and only one[34] compared efficacy of an intervention across child age. These child characteristics important to clinical decisions have not been accounted for in the evidence base of interventions targeting children exposed to trauma other than maltreatment or family violence, some of whom already have symptoms.

Another limitation of the evidence base results from outcome assessment methods. The outcomes studied were mostly based on child self-reports. Few studies used a clinical interview to assess PTSD diagnosis or other mental health outcomes. Although controversy exists regarding whether PTSD is an appropriate diagnosis for children, determining whether an intervention can affect clinically meaningful syndromes of traumatic stress symptoms requires future research. As noted, few included studies assessed long-term outcomes.

Finally, the applicability of the findings is limited by setting and type of trauma exposure. Nearly half of the included studies (11 of 23) were conducted outside the United States. In addition, the findings of individual studies are only applicable to children with similar characteristics and exposure to the same types of trauma. The types of trauma experienced by children in the included studies varied widely. For example, of the seven PTSD studies targeting exposure to trauma that addressed KQ 1, two studies included children exposed to a natural disaster, two studies included children exposed to war/terrorism, two studies included children exposed to accidents, and one study included children with mixed trauma types. The treatment studies that addressed KQ 2 included children with similar heterogeneity. Findings may not translate across setting, culture, economic conditions, and trauma type.

Research Gaps

Future studies on interventions targeting children exposed to trauma other than maltreatment and family violence, some of whom already have symptoms, are warranted for several reasons. First, the evidence base for well-designed interventions that lack sufficient bias addressing child trauma other than maltreatment and family violence is small. The heterogeneity in types of interventions prevented combining the results of more than two studies per intervention, thus precluding examination of the consistency of associations. No evidence was found for several interventions commonly used to treat children with trauma exposures. Although most psychotherapy interventions were manualized for delivery, several did not assess treatment fidelity. In addition, only four pharmacotherapy trials were included in this review, and those trials did not study many types of commonly prescribed medications for children exposed to trauma.

Second, the sample sizes of the studies included in this review were small to medium. Identifying children with trauma exposure and obtaining informed consent limits the feasibility of recruiting large sample sizes for randomized controlled trials. Insufficient funding also may contribute to small sample sizes. The small sample sizes created several problems with the reliability of the analyses, and rendered subgroup analysis all but impossible. Thus, several analyses were likely underpowered to detect significant associations. The lack of power becomes even more problematic when attempting to adjust analyses for important covariates that may confound the relationship between the intervention and outcomes. Loss of subjects to followup makes the issues related to sample size even more pronounced. Subgroup analyses become difficult as well with small sample sizes, evidenced by the review finding only two studies that examined the intervention-outcome link across varying subgroup characteristics. This is especially problematic given that the efficacy of particular interventions is thought anecdotally to differ across factors such as developmental age of the child, and type, severity, or experience of single versus multiple traumas. Whether this hypothesis holds true in research trials remains unknown. The difficulty of conducting studies in this population suggests that future research may require focus on observational studies, including heightened attention to research involving registry data.

Third, the outcomes reported were largely based on self-report symptomatology instead of clinical interview diagnosis. Although there is controversy surrounding the appropriateness of the PTSD diagnosis in children, the use of a standardized interview to qualify clinical syndromes rather than changes in symptoms is needed. Demonstrating that a statistically significant change in symptoms is clinically relevant is difficult. The current shift to a more inclusive diagnostic system in DSM-V focused on DTD might inform future research efforts that target and treat children based on already occurring DTD and targeting prevention of DTD among exposed children. Only one study[32] used the RCI to quantify whether symptom changes over time were differentially significant, although RCI was used to study harms (i.e., deterioration in symptoms over time) rather than improvements in outcomes. Few studies reported actual effect sizes, but there were many outcomes for which intervention may provide benefits to children exposed to trauma (e.g., suicidality, conduct problems), but they were not tested in any included trial.

Finally, few studies assessed harms associated with participating in a particular intervention. Although study dropouts could be quantified based on reported numbers of participants at baseline and at each follow-up assessment, adherence to the protocol was not assessed in any study. Future studies of child trauma interventions require formal testing for harms, especially for risk of retraumatization.

Conclusions

Our findings may be interpreted as a call to action: psychotherapeutic intervention may be beneficial relative to no treatment, but far more research is required to produce definitive guidance on the comparative effectiveness of psychotherapeutic or pharmacological interventions targeting children exposed to trauma, some of whom already have symptoms.

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Full Report

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