Comparative Effectiveness Research Review Disposition of Comments Report

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

Draft review available for public comment from July 24, 2012 to August 21, 2012.

Research Review Citation: Forman-Hoffman V, Knauer S, McKeeman J, Zolotor A, Blanco R, Lloyd S, Tant E, Woodell C, Viswanathan M. Child and Adolescent Exposure to Trauma: Comparative Effectiveness of Interventions Addressing Trauma Other Than Maltreatment or Family Violence. Comparative Effectiveness Review No. 107. (Prepared by the RTI International-University of North Carolina at Chapel Hill Evidence-based Practice Center under Contract No. 290-2007-10056-I.) AHRQ Publication No. 13-EHC054-EF. Rockville, MD: Agency for Healthcare Research and Quality. February 2013. Available at: http://effectivehealthcare.ahrq.gov/reports/final.cfm.

Comments to Research Review

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The tables below include the responses by the authors of the review to each comment that was submitted for this draft review. The responses to comments in this disposition report are those of the authors, who are responsible for its contents, and do not necessarily represent the views of the Agency for Healthcare Research and Quality.

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Published Online: February 11, 2013

Commentator & Affiliation	Section	Comment	Response
TEP #1	General	This was a very detailed and thorough report.	N/A
TEP #1	General	As expected we do not have real science on this issue, it was the same when I reviewed the suicide literature.	N/A
TEP #1	General	Well done.	N/A
TEP #1	General	The answer to all three questions was Yes.	N/A
TEP #2	General	The overall finding that there is a very limited evidence base supporting psychotherapy and no evidence supporting pharmacotherapy (and some indication of a need for caution due to potential adverse reactions) in treating post-traumatic stress and related symptoms not due to family violence or maltreatment is as the authors state a call to action suggesting a pressing need for research.	N/A
TEP #2	General	The fact that only one study used an active control group (vs. wait list) further indicates the need for research showing that treatment ingredients are effective as opposed to attention/expectancies/etc.	N/A
TEP #2	General	The marginally more promising evidence of brief psychotherapeutic interventions to prevent PTS from becoming a significant impairment suggests that early intervention warrants effectiveness trials on a larger scale, and with much longer follow-up intervals and more varied sources of data and a wider set of outcomes.	N/A
Peer Reviewer #1	General	I think there are a number of issues with the report that need addressing. Partially, the dilemma is related to the parameters of the report and the few numbers of studies that address the issue of childhood trauma other than maltreatment. While, I understand the rationale for the 4 key questions and its organization, I think that it would be clearer if the the subheadings were organized by trauma type or contest of interventions rather than interventions themselves. For example war, community violence, disaster, terrorism etc. In the same way that maltreatment differs from other forms of violence, the differing contexts have tremendous impact on outcome. This is especially the case in situations of on-going danger and stress.	We appreciate this comment but did not organize by trauma type or context of interventions because that is not what we found in the literature. We typically found studies that compared one type of treatment with another, a wait-list control, or no treatment/placebo using a sample of children with one or multiple trauma exposures. Organizing in the way suggested would have required us to make several judgment calls about how to organize the contexts of interventions, decreasing the validity and reproducibility of the report.
TEP #3	General	I am disappointed that the report was not able to attempt a quantitative review although I respect their appraisal of the evidence. Overall the report demonstrated the large gaps in knowledge, the small sample sizes, the lack of replication for models, and modest or no effects. It is sobering to realize how little evidence we have, given the high rates of trauma to children - not including maltreatment and exposure to family violence.	N/A

Commentator & Affiliation	Section	Comment	Response
Peer Reviewer #2	General	Given the methodological limitations (exclusion criteria, requirement for minimally rigorous design etc) the report is clinically meaningful. Target populations are explicitly defined? Key questions are appropriate and clearly stated.	N/A
Peer Reviewer #2	General	The Background frames importance of the issue well, with pertinent references. The authors point out that the complexity and clinical heterogeneity of maltreatment has generated diverse clinical approaches.	N/A
TEP #4	General	Title of the review: Given the age range of participants included in the studies reviewed (0-19), it may be more precise to revise the title to "Child and adolescent Exposure to Trauma…."	Thank you. We have renamed the report to include "and adolescent" but made a note in the text that we will refer to children and adolescents as, simply, child/children thereafter.

Commentator & Affiliation	Section	Comment	Response
TEP #4	General	Like the authors, I was surprised at the paucity of rigorous trials of interventions for the prevention and treatment of PTSD and related symptoms in children and adolescents exposed to trauma other than maltreatment or family violence. I concur with the interpretation as a call "to action" for future research (especially well-controlled replications of RCTs); however, I am concerned that the scope and inclusion/exclusion criteria in the protocol used may have contributed to this limitation by "falsely" restricting the research available for review. In focusing this review on trauma "other than maltreatment or family violence" and developing an independent/complimentary systematic review on children exposed to maltreatment or family violence, research relevant to this review may have been overlooked here (and included in the other SR). The "a priori" scoping decisions are based on expert consensus regarding differences between the exposed populations and the contexts of the interventions, as well as EPC resource issues, rather than empirical justification/evidence for meaningful differences; and this may have precluded appraisal of much clinically relevant research in the present review, especially in light of the notable overlap in types of trauma exposures in children (see overview of "type of trauma" on page 2). This leads me to wonder whether intervention with a greater SOE from the complimentary review (assuming these exist) may in fact have more evidence to support their for use in the present population than those reviewed here (or perhaps those trials provide further empirical support for the interventions covered in the present review). The review would benefit from further consideration of the effects of this scoping issue on the results. This would include more careful consideration of why the intervention used for child maltreatment or family violence may (or may not) be appropriate for use in the present population, and may also include discussion of the relevance of the present results vis a vis the proportion of PTSD attributable to exposures covered in the present review in the larger context of all child/adolescent PTSD. Regardless, some reconciliation of two reviews seems warranted and may have important clinical and policy implications (e.g., for treatment recommendations).	Thank you. We have added some description about the main findings of the child maltreatment complementary report in the Discussion section as well as some comment on what those findings might mean for the children exposed to non-maltreatment and family violence exposures.

Commentator & Affiliation	Section	Comment	Response
TEP #4	General	The clinical relevance of the review is also limited by the nature of the comparisons, which may not represent or inform the most important clinical decisions facing patients and practitioners, especially with notable limitations in the evidence base (e.g., few or no RCTs). In such instance, the key decisions/questions would include: 1) "Is there evidence enough to support doing something (an active treatment) vs. doing nothing?" and 2) "What are the relative potential risks/harms of alternative treatments (and no treatment)?" Hence, in addition to data on benefits, the clinical value of the review would be greatly enhanced with much more attention to the harms and risks, particularly with those treatments that are currently recommended in existing guidelines or that are commonly used in practice. In terms of what to do to assist patients suffering from PTSD in the face of limited effectiveness data, the relevant decision may be based on which treatments are least likely to produce harm. Greater attention to assessment of risk of harms and harms/benefits is merited and may suggest the need to review data from observational studies not yet included (as discussed in detail both in the IOM report on systematic reviews and the AHRQ Methods Guide, Chou et al., 2011). This raises additional concerns about restrictions created by the inclusion/exclusion criteria; namely, including only studies with measures of benefits and low or moderate risk of bias with regard to benefits would appear to exclude a large amount of research that could independently speak to harms and risks (this may include omitted observational studies and other trials/evidence sources that were allocated to the maltreatment and family violence review).	Thank you. We have included KQ4 as our assessment of harms in the included studies. We did not find many studies to report harms. We do make an effort to weigh these harms with the potential benefits. For example, we conclude that "the sertraline study [Robb, 2010] suggested that the intervention arm fared worse than the control arm. " While we acknowledge that some observational studies and other evidence sources may have provided more detail about potential harms, we do not feel that those studies have the rigor to be able to conclude with at least a moderate degree of certainty that the harms were due to the intervention itself, since too many variables are not controlled in these studies.
TEP #4	General	Finally, rather than declaring "no evidence was found" for commonly recommended/used interventions for PTSD in children (e.g., page 39, line 18 in Executive Summary and parallel section in full review, e.g., p. 63 lines 29-37), the clinical value of the review would be greatly enhanced by describing/appraising what evidence does exists for treatments that are recommended and commonly used. Likewise, future research efforts could be informed by describing the limitations of the evidence base for these interventions. In other words, the review would be much more useful for clinical decision making and for designing future research if it included review and evaluation of the "best evidence" for common practices (and what are the relevant biases and other limitations that need to be remedied in future studies).	We have discussed some of these issues in our "Research Gaps" section. We do mention what might be worth researching further (that is thus far showing possible benefits) as well as limitations of this field.

Commentator & Affiliation	Section	Comment	Response
TEP #5	General	The study is very well done in terms of the methodology of collecting potential studies, the categorization of characteristics of the studies (e.g., bias, strength of evidence), and the summarization of the results of studies. Basically, there are few quality studies and most interventions reviewed had at most one relatively small study done by intervention adherents. Thus, the "evidence base" for any of the proposed interventions is very thin. this is a conclusion I would strongly agree with	N/A
TEP #5	General	However, I am not much impressed with this approach to reviewing interventions that I would characterize as "actuarial." The various studies are considered as isolated independent studies, the numbers obtained in characterizing participants, outcomes, etc. are interpreted as inherently "valid" indicators, I guess because they are numbers, and the numbers are computed as indicating results (e.g., estimating "true effects."). In the trauma field RCTs give at best average results in small convenience samples. It is far more important clinically to know (1) for "conceptually well-developed" interventions; who the intervention works for and who it doesn't work for rather than obtaining an overall estimate of some number indicating an average effect , and (2) the adequacy of the conception of the internal processes the intervention is trying to change and the effectiveness of the intervention procedures that are attempting to change the internal processes. Although the review seemed very strong in the area of RCT methodology and summarization, it seems very weak in the area of conceptualizing child trauma intervention processes.	Thank you. We do agree with the validity of these statements. We have developed KQ#3 to look at which studies have made important distinctions about subgroups that have differential intervention effects on outcomes (thus, defining who the intervention works for or works for better). Conceptualization of internal processes is very important, yes, but none of the included studies focused on this way of conceptualizing their trials, making this a difficult task to complete with the evidence in the literature.
Public Reviewer #1	General	No Comment	N/A
Peer Reviewer #3	General	Very well organized review of the state of the science. Population and audience very clearly defined. The four key questions very well defined.	N/A
TEP #1	Introduction	Good	N/A
TEP #2	Introduction	Thorough and clear.	N/A
Peer Reviewer #1	Introduction	The most important part of the document is the abstract and executive summary, since the majority of people that read the document will just read the summary. So page iv, lines 42-48 make important statements that should be elaborated there. Also, while not the typical manner in which a report is written, I would consider beginning the executive summary with a statement about the lack of studies in the area and the need for more support for this work.	We have revised as suggested, thank you.
TEP #3	Introduction	The introduction is comprehensive and well organized. It sets up the study nicely.	N/A
Peer Reviewer #2	Introduction	In the Introduction, the PICOTS framework is described, and there is a good explanation why children exposed to DV were excluded. The KQs are laid forth, and analytic framework described and well illustrated in Fig 1.	N/A

Commentator & Affiliation	Section	Comment	Response
TEP #4	Introduction	Current Child Traumatic Stress guidelines (p. 10, lines 12-30): A table summarizing existing guideline recommendations and the evidence (and criteria) on which these were made (including consensus, if applicable), would be very helpful for readers as a basis for comparison with the present results.	We have summarized some of these similarities and differences at the end of the Introduction section. We did not put in table form due to the disparate categories across guidelines.
TEP #4	Introduction	Limitations of the review process (page 160, lines 21-37): I am not clear on why studies that examined relevant outcomes, such as suicidality and depression, but not PTSD symptoms were excluded. As noted, these "might be additional evidence of benefit" (and may also speak to harms/risks), so please provide stronger rational of exclusion (and if it is basically arbitrary, would it be possible to add this data to the review).	Those studies, which examined outcomes other than traumatic stress symptoms, were outside of the scope of our review. We did, however, include these outcomes for studies that also looked at traumatic stress symptoms as an outcome.
TEP #5	Introduction	The report focuses too heavily on PTSD-type symptoms as characterizing traumatic stress reactions. There are many other types of traumatic stress reactions, including exacerbation of existing mental health symptoms/problems, anger/aggression/helplessness, interpersonal problems, compromised developmental processes. The report could have been labeled "...Interventions Addressing PTSD-Related Trauma Other Than Maltreatment or Family Violence" rather than more generally addressing the range of traumatic stress reactions.	Those studies, which examined outcomes other than traumatic stress symptoms, were outside of the scope of our review. We did, however, include these outcomes for studies that also looked at traumatic stress symptoms as an outcome.

Commentator & Affiliation	Section	Comment	Response
TEP #5	Introduction	The report takes what I describe above as an "actuarial" approach to trauma intervention studies, i.e., considers each study as an isolated individual study. The assessment of trauma interventions could have benefitted from a more conceptual approach to the development of trauma interventions. The list of interventions cited in the Introduction are not all targeting the same type of trauma issues or populations (e.g., "trauma"), but have been developed to address aspects of traumatic stress responses in different populations or settings in which trauma services could be delivered. For example, interventions could be arrayed along a temporal dimension of interventions for the phases of psychological distress to trauma exposure, such as interventions for psychological distress in the immediate aftermath of a trauma exposure (e.g., Psychological First Aid), early/brief intervention for individuals most psychologically impacted by an event (e.g., Child Family Traumatic Stress Intervention), clinical treatment of significant enduring post-traumatic stress symptoms (e.g., TF-CBT) and interventions for developmental deficits associated with a history of serious traumas (e.g., ARC). Moreover, the list of trauma interventions in the Introduction were developed to address specific trauma issues that arise in intervening with different traumatized groups, populations or interventions settings, e.g., SPARCS was developed as a trauma intervention focused on adolescents and is, thus, for example, more focused on cognitive processing than is TF-CBT and CPP was developed for young children and focuses on the developmentally significant attachment relation between young children and caretakers which can be compromised by early trauma. A much better conceptualization of the effectiveness of these interventions would focus on what specific issues they are attempting to address rather than attempting to categorized there results as if they have similar trauma intervention targets.	This is an important point. A explicit discussion of underlying targets of each intervention, however, was not what we found in the literature. It would be difficult to scope the review in this manner because identification of specific issues each intervention is attempting to address would be somewhat subjective. Most literature is presented in terms of testing a specific intervention (manualized, in psychotherapeutic interventions) presented so that it can be replicated.
Public Reviewer #1	Introduction	No Comment	N/A
Peer Reviewer #3	Introduction	The introduction sets up the review well. It is clear to the audience what the review is intended to accomplish and how it will be accomplished. The writing is very clear.	N/A
TEP #1	Methods	Yes to all the above. The review is going to be more tedious than sifting thru the review.	N/A
TEP #2	Methods	Selection and search process well documented and justified. The judgments concerning strength of findings appear well founded.	N/A

Commentator & Affiliation	Section	Comment	Response
Peer Reviewer #1	Methods	I think this is one of the strongest parts of the white paper the authors describe their decision making clearly. The list of excluded studies is very helpful. I think it would be important to include the issue of risk and protective factors that moderate PTSD and PTSS outcomes. Some the studies mentioned report these. For instance, we know that single incident PTSD is relaitvely rare and that most individuals that develop PTSD have prior potential traumatic exposures or chronic stressors. A paragraph discussing the risk and protective factors and their relationship to intervention outcomes would be useful. Also, information from any study that reports on these factors is important to include.	We included KQ3 to look at whether there were risk and protective factors that influenced the effectiveness of the intervention (interaction effects), however, we did not include all risk and protective factors of the outcomes themselves (main effects) because it is outside of the scope of this report.
Peer Reviewer #1	Methods	think it is important to explain how the authors calculated outcome variables that were not supplies by the authors. (for instance the effect size for the study that I authored required additional data for a meta-analysis that was done by (Kramer, D and Landolt, M 2011) that was not requested by the authors. While is it very important that there be the ability to compare outcomes, the accuracy of the calculations is crucial.	We did not use any data from unreported studies (e.g., Kramer and Landolt, 2011) to calculate our effect sizes. The effect sizes reported are reported in the study themselves or we calculated the effect sizes as described in the methods section and marked these as “calculated”.
Peer Reviewer #1	Methods	Also, In terms of definitions and outcomes in the studies, the distinction between ASD, Acute PTSD and Chronic PTSD are blurred, at times. These distinctions are important as only a percentage with ASD develop PTSD and acute PTSD tends to be more amenable to treatment than chronic PTSD. Also, I am a proponent of the Developmental Trauma Disorder model, but since there aren’t RCT’s in the area, it might be best to describe this as a future need.	Yes, these are important considerations. When specifically addressed by a particular study, we did address whether ASD, acute PTSD, or chronic PTSD was part of either the inclusion criteria or outcomes tested. We also addressed the need for future research on DTD in the Discussion section.
Peer Reviewer #1	Methods	I have one concern about the inclusion of a few studies under KQ1, which is relevant to definitions. Prevention of PTSD needs to occur before it is diagnosable at the 30 day post event mark. Otherwise, it is not prevention, but treatment.	Thank you. We have revised our labels of KQ1 and KQ2 to depict a true representation of their categorization. We have renamed KQ1 as “treatment based on exposure” and KQ2 as “treatment based on symptoms” to indicate whether the inclusion criteria of each particular study required participants have trauma exposure (KQ1)or some predefined level of traumatic stress symptoms (KQ2)
Peer Reviewer #1	Methods	The studies by Goenjian et. al. occurred 1.5 years after the identified traumatic event. I don’t see how this could be considered prevention, given the amount of time that passed. Page 27, lines 56-57, states that children were selected based on exposure to therapy (should be trauma). Not symptoms or diagnosis. Because the researchers did not assess symptoms at baseline does not make it prevention, it is treatment. Many children likely had chronic PTSD, PTSS or other trauma related disorders.	Please see prior response. We have renamed KQ1 and KQ2 accordingly.

Commentator & Affiliation	Section	Comment	Response
Peer Reviewer #1	Methods	Also, the ERASE studies' participants were subject to terrorist attacks over a number of years prior to the intervention or to the Tsunami over a year earlier. Again I don't think this is secondary prevention given the know time course and diagnosis of PTSD. The same issue appears to be the case with the OTT intervention.	Please see prior response. We have renamed KQ1 and KQ2 accordingly.
Kemp	Methods	In relation to our study (Kemp, Drummond & McDermott, 2010), I have attached additional data which may assist with effect size calculations for your metaanalysis. Best regards, Michael Kemp	Thank you. We appreciate this data, however, we are presenting the results of the intervention's effect on each tested outcome rather than combined outcomes (as in the attached meta analysis). We present the data in a manner to be as consistent as possible across studies.
TEP #3	Methods	In retrospect, not including maltreatment and family violence decreased the number of eligible studies and therefore the ability to conduct more quantitative analyses. The search strategies were explicit and logical and definitions, diagnostic criteria and outcome measures were appropriate. Given the key questions, the scope of the review and PICOTS framework was appropriate and probably the strongest methodology that could be applied.	N/A
Peer Reviewer #2	Methods	Methods: Justification is given for inclusion and exclusion criteria, although these limit the literature review. Search strategies are logical and explicitly stated. Definitions or diagnostic criteria for the outcome measures seem appropriate.	N/A
Peer Reviewer #2	Methods	Authors describe limitations of data synthesis due to heterogeneity, insufficient numbers of similar studies, or varied outcome reporting, to explain that qualitative analysis was done, that effect size calculation were generally impossible etc.	N/A
Peer Reviewer #2	Methods	Concise description of evidence grading is given	N/A
TEP #5	Methods	The gathering of reports and rating of reports was exhaustive and exemplary.	N/A

Commentator & Affiliation	Section	Comment	Response
TEP #5	Methods	Among the rating criteria the review does not give sufficient weight nor highlight sufficiently the number of participants in the studies. The minimum number per group (at least 10) to be included as a study to review is very much too low. The basic rationale for randomization in research studies is that any confounding alternative variable that could affect results should be equitably distributed between group being compared if there are sufficient numbers in each group so that the effect of such confounders on group averages will be balanced out. Studies with 10-20 carry a high probability that such confounders will not balance out such confounders and these small samples have a significant probability of being biased with respect to random "population" samples.. How many subjects is necessary to ensure equitable distribution of confounders? At the low end some would say about 20 if there is one prominent potential (unknown) confounder, but confounders can also include combinations of variable so I would say that 40-50 is a minimum number. Given studies should include 40-50 in each group, this would eliminate most of the 20 studies included (and most of clinical research in the trauma area) but I would not have much confidence in any study with small sample sizes. Getting larger sample sizes in this area would be difficult because larger samples are quite costly to obtain in RCTs. The only anecdote for small sample sizes is a collection of smaller studies that reach similar conclusions. Such a collection of repetitive studies (especially by independent investigators) does not exist in this field and in many other areas of psychological interventions. Sample size should have been given much greater weight in determination of strength of evidence.	We scoped our review to include any well-designed study that adequately controlled for confounding to be as inclusive as possible, particularly since there was not a lot of research in this area. There were several other studies with low n's that we did not include. We do speak to the low n's of these studies in the Discussion section as a limitation of our review.
TEP #5	Methods	As I have stated in a previous review, basing the notion of "strength of evidence" on the notion of whether a study estimates well a "true effect" is, I think, highly debatable and from my perspective wrong. Interventions have a range of effects depending on client characteristics, intervenor characteristics, setting, additional treatment and outside context, outcome assessed etc. And the range of effectiveness and what influences it is more important to understand clinically than the "average effect" estimated in an RCT.	Thank you. We agree with you and have included KQ3 to this effect. Unfortunately, many of the studies simply don't test whether particular subgroups have more or less effectiveness of a particular intervention on outcomes.
Public Reviewer #1	Methods	No Comment	N/A
Peer Reviewer #3	Methods	I thought it was a thorough review that was well described. I liked the requirement for two researchers to conduct the review. The non-sexual abuse/maltreatment focus is important as it tends to receive less attention. Methods appropriate and well described.	N/A
TEP #1	Results	Yes, to the above. I am familiar with the literature and they covered all the one's I know of and then some. It was interesting that Cohen's work was removed but I guess her stuff is more clinically focused.	N/A

Commentator & Affiliation	Section	Comment	Response
TEP #2	Results	Although there is a great deal of redundancy, the results are very thorough and the Tables are descriptive.	N/A
TEP #2	Results	At least one relevant study that addressed a number of the methodological and ecological validity criteria was apparently not reviewed: Ford, J. D., Steinberg, K. L., Hawke, J., Levine, J., & Zhang, W. (2012). Randomized trial comparison of emotion regulation and relational psychotherapies for PTSD with girls involved in delinquency. [Comparative Study Randomized Controlled Trial Research Support, U.S. Gov't, Non-P.H.S.]. Journal of Clinical Child and Adolescent Psychology, 41(1), 27-37. doi: 10.1080/15374416.2012.632343	We have reviewed this study and it did not fit our inclusion criteria.
Peer Reviewer #1	Results	The detail presented in the results section seems very well done. Please see earlier comment about explaining how calculations of various outcome variables were derived?	Thank you. As described in the methods section, we either reported outcomes and statistical testing directly from the study themselves or calculated between group change scores if possible and noted these as "calculated" estimates. Methods for calculating confidence intervals when sufficient information was given in the study are described.
Peer Reviewer #1	Results	Re studies. There are several prevention studies that were not included by Kenardy's group in Australia and Landolt's group in Switzerland. Also, there are many studies post Katrina that are worth looking at including those by Cohen, J, Scheeringa, M and others.	We have reviewed all of these studies and determined that they did not meet our inclusion criteria.
TEP #3	Results	The major problem with the results is that they are based on only 20 articles from 18 studies of which only 12 were RCTs. The coders generally graded the quality of evidence in these studies as low. Given the small samples and diversity of intervention models, there is little that can be generalized that is of value to the field. I am not aware of any relevant studies that were overlooked. It is possible that some of the 23 studies that were dropped because of a high risk for bias might have been included in a weighted quantitative analysis.	We did not include studies with high risk of bias ratings because major flaws inherent in their methodology were apparent enough to suspect that the results might not be valid.
Peer Reviewer #2	Results	The results section masterfully selects enough detail to explain the analysis. Characteristics of the studies are succinctly and clearly described and linked to the key questions as well as could be expected. The figures, tables and appendices are descriptive and an excellent contribution to the field. The selection of studies is adequately comprehensive, and the selection process is described that led to reduction of analysis to 42 articles describing 26 studies (from 432 articles reviewed in full text).	N/A

Commentator & Affiliation	Section	Comment	Response
Peer Reviewer #2	Results	Key findings analyzed for KQs 1-6 Tables B (KQ 1,2) and C (KQ 4) very helpful. In discussion of KQ 3, re different formats, the important point is made that sparse evidence reflects the common use of multiple modalities for treating child maltreatment. The authors explain that differentiation of theoretical orientation is limited because in many studies, the description of the theoretical basis was unclear; this renders evidence for K3 inconclusive.	N/A
Peer Reviewer #2	Results	Note: re KQ 5 - text in ES states this is KQ 3 (typo?).	N/A
TEP #4	Results	Page 20, lines 17-24 in the Executive Summary (and same section in full review): The distinctions drawn in this section between effectiveness, precision, and SOE are not clear. Also, more detail and specificity is needed in reference to "clinically meaningful" (line 24); how was this evaluated?	Revised text to add greater clarity. We had deleted the sentence about clinical significance because that particular instance, which would have entailed looking at minimally important differences, did not arise.
TEP #4	Results	What conclusions may be merited for interventions with evidence of some effect on a several outcomes, especially those with a consistent effect across outcomes? Could this be interpreted as greater support for "promising" interventions than those demonstrating more limited benefits (either due to lack of measurement of demonstrated effects). In terms of make recommendations with a limited evidence base, would it be reasonable to favor psychosocial interventions shown to have an effect on functioning and quality of life, as well as symptoms/diagnosis?	The types of conclusions suggested by the reviewer require indirect comparisons or some weighting of preferences about outcomes. The volume and type of comparators in this body of evidence do not support quantitative indirect comparisons, and qualitative indirect comparisons are subject to risk of selective outcome and analysis reporting bias. Decisions about weighting one intervention over another because of the types of outcomes that the intervention influences typically lie outside the purview of the systematic review because it requires a judgment about preferences for outcomes.
TEP #4	Results	Excluded for "wrong publication type" (e.g., Appendix C) – I may have missed it, but I didn't see description of what this means or how it is this different from other exclusions.	The publication types that met the inclusion criteria for our review, including types of acceptable publication types, are described in the Methods section.
TEP #4	Results	KQ4 (p. 145, lines 25-28): There appear to be some problem with the numeric citations (e.g., Line 25 states "Five studies reported harms associated with interventions," but there is only a single reference (#49).	We have fixed this error, thank you.
TEP #5	Results	The review provides a wealth of detail about each of the small number of studies included in tabular form.	N/A
Public Reviewer #1	Results	No Comment	N/A

Commentator & Affiliation	Section	Comment	Response
Peer Reviewer #3	Results	The result detail was comprehensive and helpful. Tables and figures well organized. There are several studies published on opiate medications in the acute setting that could have fit within the prevention section.	N/A
TEP #1	Discussion/ Conclusion	Yes to the above. The future research section is fine.	N/A
TEP #2	Discussion/ Conclusion	Implications and limitations well stated.	N/A
Peer Reviewer #1	Discussion/ Conclusion	Yes, but as previously mentioned I think the statement of how needed this research is should be made at the outset of the paper and re-emphazised at the end. As the authors state the key point is that this white paper is a call to action and one that AHRQ may want to take on.	Thank you. We have included some language describing the need for this research at the outset of our paper and the ES sections.
TEP #3	Discussion/ Conclusion	The major finding of this report is that we do not have good evidence to guide our treatment of children exposed to traumas other than maltreatment and family violence. Although true, it is a discouraging message for the field that has little funding and often is called upon in an emergency to respond to children and families after natural and manmade disasters, accidents and loss. Given the limitations of the data, the report is understandably unable to endorse specific interventions beyond indicating that both ERASE Stress and CFTSI as possible preventative interventions. The report essentially acknowledges that clinicians are on their own for the most part and that more research is needed.	N/A
TEP #3	Discussion/ Conclusion	Given that most studies were given a low evidence rating, the report could be more explicit about ways to address some of the critical shortcomings in the current research designs.	We have added some more detail to the Discussion suggesting ways to address critical shortcomings in the current research designs.
Peer Reviewer #2	Discussion/ Conclusion	Implications of major findings are clearly stated, and the limitations of the review/studies are adequately described. The Discussion reveals important evidence gaps, and notes that limited target population and exclusion criteria constrain applicability. In particular, weak measures (e.g. parent self-report), few measures of attachment, and short followup duration in study design, are key limitations of the studies reviewed. This information is brought forward to recommendations for future study design, but not in great detail.	N/A

Commentator & Affiliation	Section	Comment	Response
TEP #4	Discussion/ Conclusion	The review states "psychotherapeutic intervention is generally beneficial relative to no treatment" (e.g., Abstract vii, ES-28) - On what criteria is the conclusion of "generally beneficial" based? After careful attention to specific definitions and strict methods throughout the review, this conclusion seems rather unclear. Moreover, conclusions about the benefits of interventions/treatments would appear to be limited by the focus on benefits, rather than on the balance of benefits to harms/risk, which is more patient centered and clinically relevant (and may suggest a need for additional review and appraisal of evidence of risks, as suggested above). Relatedly, the important difference between "no evidence of harms" (e.g., page 6 line 56) and "evidence of lack of harms" should made clear for readers who may confuse the two.	We have revised this statement as "psychotherapeutic intervention may be beneficial relative to no treatment, and appears not to have associated harms."The distinction between no evidence of harms and evidence of lack of harms is made in the Methods section of both the ES and report itself.
TEP #5	Discussion/ Conclusion	The conclusions of the review are clearly stated and evident from the review discussion: The review does clearly indicate the paucity of research on child trauma interventions. There do not exist any comparative effectiveness study of any alternative interventions. No strong recommendations can be made about the effectiveness of any child trauma intervention because of the lack of multiple well-designed studies of any of the interventions.	N/A
TEP #5	Discussion/ Conclusion	However, as I have try to indicate above: although there exist few if any compelling, well-designed RCTs on child trauma interventions, there is a much better developed conceptual and research base on the basic conceptualization of the characteristics of traumatic stress reactions and the internal mechanisms that occur in traumatic stress reactions than studies of specific intervention programs. Thus, it is very clear that trauma reminders drive traumatic stress symptoms and thus many trauma interventions target trauma reminders. The evidence for this mechanism is much stronger than for any specific set of trauma intervention procedures and this conceptual understanding can play a significant role in evaluating and improving the effectiveness of various specific trauma interventions. From my perspective the review "counts up" the numerical results of the small set of RCTs rather than focus on the underlying concepts of treatment of traumatic stress reactions.	We appreciate this comment. We did not, however, find the literature to report the underlying constructs and mechanisms of their interventions. Thus, we would have to subjectively assign these for each study, making it difficult for us to quantify as part of a systematic review.
TEP #5	Discussion/ Conclusion	A major limitation of the review's assessment of the strength of studies is the uncritical acceptance of numerical measures in clinical studies. There is very little strong evidence that many of the measures used in clinical studies (e.g., the CBCL) are good indicators of trauma symptomatology or good indicators of clinical improvement in trauma effects. Perhaps, the best measures with some clinical support might be the CAPS with adults and the UCLA Reaction Index for PTSD symptoms in children, but most of the other measures in these RCTs have about as thin a base for their clinical validity as do the studies of the effectiveness of child trauma interventions.	N/A

Commentator & Affiliation	Section	Comment	Response
Public Reviewer #1	Discussion/ Conclusion	No Comment	N/A
Peer Reviewer #3	Discussion/ Conclusion	The report - clearly- identifies an important gap in the literature related to the state of knowledge about treatments for non-maltreatment related to traumatic stress. The literature itself is quite small and the results of the few studies suggest limited effectiveness and few have sought to measure any harmful effects. The fact that the literature does not contain studies to allow conclusions about individual or contextual factors is very important. I donst think this problem is exclusive to child traumatic stress interventions research. That few studies in the adult or child mental health interventions literature allow for understanding of individual or contextual factors related to effectiveness contribute to the fact that few treatments have been able to be used in the typical services system.	N/A
TEP #1	Clarity/ Usability	It is well organized and main points presented clearly. I don't know what policy we can inform with these conclusions other than we need more information.	N/A
TEP #2	Clarity/ Usability	The volume of information and repetitiveness make the key findings difficult to identify. The Executive summary would be more useful if the extensive detail was appended and the summary sections describing the findings were provided together in a single 2-3 pp summary.	We have reduced the ES to remove unnecessary information. For example, we removed text discussion of outcomes since they are noted in the figure.
Peer Reviewer #1	Clarity/ Usability	As previously mentioned, I think the sub-sections should be either around trauma type or context (post disaster, terrorism, community violence, mixed etc. The take home message is that more research is needed on these and other interventions for violence exposed children.	Thank you. We have addressed this comment previously. We attempted to organize our report based on what a clinician typically encounters in a clinic. We captured all of these details for each included study but did not choose to organize by types of exposure and context.
TEP #3	Clarity/ Usability	The report is well structured and organized. The section on Implications for Clinical and Policy Decision making, however, is sparse and the obligatory call for more research is vague on what types of research. This is an opportunity to set an agenda for the field and to specify standards to improve the low quality of evidence identified by this report.	Thank you. We have added some additional language to this section.
Peer Reviewer #2	Clarity/ Usability	Overall, although lengthy, this well-designed report can inform research and policy decisions, and should be useful to practitioners planning interventions for maltreated children.	N/A

Commentator & Affiliation	Section	Comment	Response
TEP #5	Clarity/ Usability	The review is very well-structured and organized. The information on which the review is based is well-presented in detailed charts and table (partly because of the small number of studies that met even the minimum requirements). Despite the dearth of a compelling number of well-designed research studies of the various conceptually-well developed child trauma interventions, it should be recognized that these interventions are now widely distributed and used in the practive community and usually clinicians who are trained in these interventions continue to use at least elements of these interventions because they find them at least somewhat effective in reducing trauma symptoms. So in this, the review because of its narrow focus, does not connect with this vast body of practical clinical experience which has a much higher opinion of the effectiveness of existing child trauma treatments than would be obtained from the conclusion of the review of research studies.	We have added some text to the Discussion section describing how clinicians might find particular elements from interventions helpful in reducing trauma symptoms.
TEP #5	Clarity/ Usability	It is clear to me that alternative approaches need to be developed for assessing the effectiveness of the available child trauma interventions rather than relying on summarizing RCTs in this field because conducting a large number of very expensive RCTs with sufficient number of participants for the large number of distinct interventions in the field is probably not economically feasible. Alternative approaches would include systems for monitoring and reporting on the large number of uses of these clinical trauma interventions in practice settings in terms of estimates of the effectiveness and non-effectiveness of implementing these interventions with different types of traumatic stress presentations, different populations of trauma victims, and different setting in which trauma interventions are implemented. Such monitoring systems would be more akin to the surveillance function that are reported for the adverse effects of drugs after they are approved rather than the initial RCTs used to apply for approval for marketing the drugs in the first place.	This is a good point and has been incorporated into our Discussion section.
Public Reviewer #1	Clarity/ Usability	No Comment	N/A
Peer Reviewer #3	Clarity/ Usability	The report is well structured and organized. The conclusions are extremely important and call attention to the need for significantly more research in this area.	N/A