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Psychosocial Interventions for Adults Who Were Sexually Abused as Children (Protocol for a Cochrane Review)

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[Intervention Protocol]

Psychosocial interventions for adults who were sexually abused as children

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ABSTRACT

This is the protocol for a review and there is no abstract. The objectives are as follows:

1. To synthesize the literature on psychosocial interventions for CSA survivors compared to control conditions through pair-wise meta-analysis.
2. To assess and compare the effectiveness of different therapeutic interventions in treating adult survivors of child sexual abuse.
3. To determine which interventions are more effective than others in the eligible populations and for specific subgroups of survivors.

BACKGROUND

Description of the condition

Childhood sexual abuse (CSA) is a pervasive and egregious crime defined here as “a sexual act between an adult and a child, in which the child is utilized for the sexual satisfaction of the perpetrator” (Lev-Weisel 2008). CSA includes a wide range of acts, perpetrated over various lengths of time, by people with various relationships to the victim. Thirty-two percent of women and 14% of men in the United States report having been sexually abused as children (Briere 2003) with 10% of victims being abused between birth and the age of three years, 54% between the age of 4 to 11, and 36% over the age of 12 (APA 2012). Childhood disclosure of CSA is relatively uncommon; hence, many survivors do not receive clinical treatment until they reach adulthood (Alaggia 2005).

Adult survivors of CSA are at increased risk for a number of mental health issues including depression, anxiety, and post-traumatic stress disorder (PTSD) (Dube 2005; Sachs-Ericsson 2009). Evidence suggests that CSA victims are 2.4 times more likely to experience these and other mental health disorders than non-victims, even after controlling for social, family, and individual factors (Fergusson 2008). CSA survivors are also more likely to engage in risk-taking behavior, including substance abuse, self-mutilation, and unprotected sex (Vigil 2008). Furthermore, female survivors have a higher incidence of physical health issues, including breast cancer, sexual dysfunction, and headaches than their non-abused peers (Lemieux 2008; Sachs-Ericsson 2009; McGregor 2010). Survivors are also at increased risk of committing suicide and being diagnosed with an eating disorder (Sachs-Ericsson 2009). Finally, the experience of sexual abuse in childhood can impact a survivor’s parenting capacities when they reach adulthood. Kim 2009 found that parents who experienced CSA were more likely to physically abuse and neglect their children, compared to parents who did not experience any childhood victimization. This risk was compounded if the parent also experienced physical abuse or neglect as a child.

As children, victims often engage in denial, dissociation, and other avoidant approaches to cope with the trauma of CSA (Cole 1992; Walsh 2010). While these strategies may help the victim survive the actual traumatic event, they are associated with reduced mental health functioning in adulthood (Wright 2007). As adults, CSA survivors sometimes recognize that old coping mechanisms are no longer working. Additionally, since 60% to 80% of survivors do not report victimization until adulthood (Alaggia 2005; Hebert 2009), most are entering clinical treatment for the first time after the age of 18. The relational tasks of adulthood, such as, forming intimate relationships, committing to a life partner, and having children, may reactivate distorted thinking patterns and lead survivors to seek psychological treatment (Cole 1992). Given the particularly vulnerable nature of this population, it is crucial to identify and implement the most effective treatments to assist in their healing.

Description of the intervention

There are two competing theories for predicting therapeutic effectiveness. Common factors theory states that all psychotherapeutic treatment modalities share a common set of “active ingredients” and all treatment modalities are equally effective. Other theorists assert that certain therapeutic techniques are responsible for therapeutic outcomes, and some modalities are superior to others. Common factors theory, first proposed by Saul Rosenzweig in 1936, posits that “certain *unrecognized factors* in any therapeutic situation... may be even more important than [the specific therapeutic techniques] being purposely employed” (Rosenzweig 1936). Since that time, there have been approximately 30 different conceptualizations of common factors (Sexton 2004a), but the most commonly accepted list of common factors includes: providing attention, demonstrating unconditional positive regard towards the client, a strong therapeutic alliance, and the existence of hope (Jensen 2005). Critics of the Common Factors model claim that it is overly-simplified, under-studied, and lacking in concrete theories of change for each of the factors (Sexton 2004a; Sexton 2004b). It has been argued that Common factors theory “overlooks the multilevel nature of practice, the diversity of clients and settings, and the complexity of therapeutic change” (Sexton 2004a). Furthermore, theorists cannot agree on a universal set of common factors, and there are discrepancies in how different factors are defined. It seems most likely that common and specific factors interact and create complex pathways to change. To the extent possible, we will extract data on common factors and specific treatment characteristics.

How the intervention might work

We will briefly describe some of the most common therapeutic treatment interventions for adult CSA participants and the theories of change behind each modality.

Cognitive-behavioral therapy

As its name suggests, cognitive behavior therapy (CBT) grew out of both the cognitive and behavioral traditions and is grounded in the belief that problematic or undesired behavior is linked to distorted or overly negative thoughts. The goal of therapy then is to change these thoughts in order to change behavior. CBT, intended to be brief in duration, focuses on immediate concerns, targeting symptoms by assessing automatic thoughts and core beliefs (Greenberger 1995; McGinn 2001). In the case of trauma survivors, cognitive-behavioral theorists believe that “fear appraisal involves the activation of a pre-existing (trauma-induced) cognitive schema that leads the person to attend to evidence that is consistent with the schema and to ignore evidence that is inconsistent” (Resick 1992). Therefore, benign or ambiguous circumstances can trigger a fear appraisal in trauma survivors (Beck 1985). Through

homework, the therapist's use of Socratic questioning, and cognitive restructuring, participants begin to evaluate their thoughts and see the world as a less threatening place.

Prolonged exposure therapy

Prolonged exposure (PE) therapy is a behaviorally-oriented model that usually consists of 9 to 12 manualized sessions, each of 90 minutes, that are aimed at desensitizing the client to their traumatic experience (SAMHSA 2003). The therapist begins by educating the client about the effects and impact of trauma. Once the client and therapist have established safety guidelines, the therapist leads the client through the emotional reliving of the events; this re-experiencing process is done repeatedly over a number of sessions, with *in vivo* exposure gradually added (Foa 1999; SAMHSA 2003). The theory of change undergirding PE is based on Lang's theories about fear (Lang 1977), particularly that a fear memory has to be activated to be addressed (Foa 1986). Then, through the recounting of the narrative, the participant encounters new information that is "incompatible with some of those that exist in the fear structure, so that, [therefore], a new memory can be formed. This new information, which is at once cognitive and affective, has to be integrated into the evoked information structure for an emotional change to occur" (Foa 1986). In essence, systematic exposure to a traumatic event in a safe environment habituates a survivor to the trauma and helps her to re-examine potentially threatening situations in the future.

Cognitive processing therapy

Cognitive processing therapy (CPT) draws heavily on both PE and CBT, but was designed initially to treat adults who experienced one incident of sexual assault or rape and suffered from subsequent PTSD (Resick 1992). CPT was adapted for CSA survivors and consists of 17 group sessions of 90 minute that focus on memory activation and emotional reprocessing. Therapists ask clients to tune into their "stuck points", parts of the traumatic narrative that cause them greatest conflict (Resick 1992; Resick 2002). In addition to the group sessions, clients meet with one of the two group therapists individually for the first eight weeks of treatment and again during the final week (Chard 1997). Unlike PE, but similar to CBT, CPT seeks to directly correct participants' misconceptions or misinformation about their trauma (for example, "I'm not safe anywhere" or "I can't trust anyone"). CPT also encourages participants to feel their emotions; written and verbal narratives of the event are supposed to incorporate detailed documentation of the emotions experienced during the assault. Unlike other treatments designed for all anxiety disorders, CPT accounts for the unique fears and societal implications of sexual violence and PTSD, and the group format provides participants with social support (Resick 1992; Chard 1997). The adapted version also focuses on developmental theory and self-esteem building, which may be especially important for CSA victims (Chard 1997).

Eye movement desensitization and reprocessing

Eye movement desensitization and reprocessing (EMDR) is a controversial PTSD treatment that combines the use of repetitive, systematic eye movement with the continued recounting of the trauma narrative. In EMDR, the therapist places two fingers 12 to 14 inches from the participant's face and asks the participant to follow their bilateral movement while envisioning both the traumatic event and the desired positive beliefs (Edmond 1999). The number of sessions, duration, and speed of the eye movement are tailored to individual clients, but there are some claims that EMDR can reduce symptoms after as few as one to four sessions (Shapiro 1989; Edmond 1999). Although eye movement was initially considered a crucial component of EMDR, it was later suggested that any dual-attention stimulation, like finger tapping or alternating tones, can produce the same effect (Shapiro 2002). There is no clear theory of change for EMDR; however, some theorize that the dual-attention process disturbs the traumatic memory and the negative emotions associated with it allowing more adaptive beliefs to emerge (SAMHSA 2010). Others hypothesize that EMDR mimics the REM cycle and allows for the subconscious re-processing of the troubling events (Edmond 1999).

Psychodynamic psychotherapy

Psychodynamic theory assumes that conscious thoughts and actions are shaped by unconscious processes and that troubling thoughts and memories are intentionally excluded from conscious awareness (Matthews 1997). Childhood traumas can be particularly detrimental to adult functioning, and childhood defences (including self-blame and repression), which were once effective, will likely result in dysfunction in adulthood (Matthews 1997). According to Anna Freud (Freud 1967), five factors mediate a person's experience of trauma: the nature and intensity of the event, sensitization due to prior trauma, hereditary factors that affect the level of defensive functioning, developmental stage at the time of the trauma, and the environment at the time of the trauma. The therapist assesses a participant's strength and weaknesses based, in part, on those criteria and then proceeds with treatment aimed at identifying the meaning of the participant's symptoms (Lord 2008). By modelling a supportive relationship and helping the client find insight on the traumatic experience, the therapist aids healing (Matthews 1997).

Supportive therapy

Supportive therapy is an umbrella term used to describe an eclectic mix of therapeutic techniques; this modality is usually used in randomized trials as a comparison control group for the treatments described above. Supportive therapy is almost always non-directive: the participant is empowered to guide the session content and the therapist avoids offering direct advice (Deblinger 2001; Cohen 2005). Unlike psychodynamic therapy in which the therapist remains a neutral presence, the focus is on developing a supportive,

emotionally-involved relationship between the therapist and participant (Cohen 2005). Supportive therapy can be conducted in either an individual or group format.

Why it is important to do this review

There are several published reviews and meta-analyses on the effectiveness of treatments for CSA survivors. Narrative reviews (Price 2001; Kessler 2003; Martsoff 2005) have suggested that abuse-focused psychotherapy is beneficial to adult survivors of CSA; however, these reviews used the unreliable method of “vote-counting” (Bushman 2009). There are three more sophisticated meta-analyses (Callahan 2004; Pelekis 2005; Taylor 2010), but these reviews are limited in quality and scope. One review is limited in terms of treatment modality (Callahan 2004), one is limited to women survivors only (Pelekis 2005), and the fourth is limited to English-language studies (Taylor 2010). All of the above reviews included studies without a control or comparison group (in addition to randomized controlled trials and quasi-experiments), using the treatment group’s pre-and post-test data to assess outcomes; this is highly problematic since the lack of a control or comparison group greatly compromises internal validity. Only the Taylor 2010 review conducted duplicate data extraction, explicitly included unpublished studies, included specific reasons for inclusion, and described characteristics of included studies. None of the reviews conducted a risk of bias assessment. Only two examined publication bias (Pelekis 2005; Taylor 2010), but used the outdated Failsafe N (Becker 2005).

A comprehensive systematic review that includes a network meta-analysis is an important step in synthesizing the available research on treatment interventions to determine which treatments are most effective for survivors of CSA. This information can help survivors and their clinical providers make the best treatment decisions possible. This review may help inform funding decisions and it will also highlight areas for future research as we will identify gaps in the current knowledge base.

OBJECTIVES

1. To synthesize the literature on psychosocial interventions for CSA survivors compared to control conditions through pairwise meta-analysis.
2. To assess and compare the effectiveness of different therapeutic interventions in treating adult survivors of child sexual abuse.
3. To determine which interventions are more effective than others in the eligible populations and for specific subgroups of survivors.

METHODS

Criteria for considering studies for this review

Types of studies

Studies must be randomized controlled trials (RCTs).

Types of participants

Women and men over the age of 18 who were sexually abused as children. It should be noted that the term “child”, in the context of CSA, is defined differently in different cultures. We will include studies that set varying age cut-offs as long as their definition excludes consensual sex-play between children and sexual interactions between adolescents whose age differential does not meet the legal definition of statutory rape in that state or country. We will exclude participants if they are developmentally disabled, experiencing active psychosis, or are victims of human sex trafficking. We will not exclude participants who have received prior psychosocial treatment.

Types of interventions

A psychosocial intervention must meet the definition of therapy as “any intervention designed to alleviate psychological distress, reduce maladaptive behavior, or enhance adaptive behavior through counselling, structured or unstructured interaction, a training program, or a predetermined treatment plan” (Weisz 1987). This definition for therapy has been used in a previous meta-analysis on this topic (Taylor 2010). The central focus of the intervention must be the specific treatment of adult CSA survivors. Interventions can be conducted in individual, couples, family, or group therapy settings and must be performed by any psychological, social work, or psychiatric professional or professional in training. Pharmacological and physical or physiological treatments (such as yoga, Reiki, etc.) will be excluded.

Types of comparisons

The intervention group may be compared to another treatment, no treatment, or another amount (dose) of the focal treatment. If a study has concomitant treatments that are the same in both arms (for example, A+C vs. B+C), we will assume that the study measures the relative effectiveness of the interventions that differ across arms (that is, A vs. B).

Types of outcome measures

Outcomes can be measured on established scales that have some validity or reliability testing (this would include any instrument with at least one test of inter-rater reliability, Cronbach’s alpha,

content validity, criterion validity, etc.). If both dichotomous and continuous measures of the same outcome in different studies (for example, depression) are available, we will give preference to continuous measures.

Primary outcomes

1. PTSD symptoms, which are often measured on the following established, continuous scales: Impact of Events Scale (IES), Clinical Administered PTSD Scale (CAPS), Modified PTSD Symptom Self-Report Scale (MPSS)
2. Depression symptoms, which are often measured on the following established, continuous scales: Beck Depression Inventory (BDI), Hamilton Rating Scale for Depression (HRSD)
3. Anxiety symptoms, which are often measured on the following established, continuous scales: State-Trait Anxiety Inventory (STAI), Hamilton Anxiety Scale (HAS), Beck Anxiety Inventory (BAI)

We will accept dichotomous measures of any of the primary outcomes if the study authors used established scales with clear thresholds (cut-points) to determine whether participants meet clinical or diagnostic criteria for PTSD, depression, or anxiety disorders. We will not accept a combined measure (for example, meets criteria for mood/anxiety disorder) nor will we accept simple clinical diagnoses (for example, DSM-IV TR) as outcome measures.

Secondary outcomes

1. Global mental health functioning/distress, which is frequently measured by either the Global Severity Index and the Positive Symptom Distress Index of the SCL-90-R or the BASIS-32 (Behavior And Symptom Identification Scale); both of which are established, continuous measures.
2. Perpetration of child abuse or neglect, which is primarily measured through administrative data on results of official investigations of cases reported to authorities. We will exclude self-report measures, unless they are provided on established scales, such as the Parent-Child version of the Conflict Tactics Scale (PC-CTS).
3. Substance use, which is measured by a number of established scales including the Michigan Alcoholism Screening Test (MAST), Drug Abuse Screening Test (DAST), Addiction Severity Index (ASI) and Alcohol Use Inventory (AUI), all of which are both continuous measures. Substance use can also be measured through biologic tests (for example, urine and hair analysis), which are often used for program administrative purposes; results of biologic tests are often expressed as dichotomous outcomes (positive or negative).
4. Self-harming behaviors, often measured by the Deliberate Self-Harm Inventory (DSHI) and the Self-Injury Questionnaire (SIQ).
5. Disordered eating, which is commonly measured by either the Eating Attitudes Test (EAT-26) or the Eating Disorder Diagnostic Scale (EDDS).

6. Dissociation, which is measured on any of the following scales: Dissociative Experiences Scale (DES), Questionnaire on Experiences of Dissociation (QED), Clinician-Administered Dissociative States Scale (CADSS), or Dissociative Processes Scale (DPS).

Search methods for identification of studies

Electronic searches

We will search the following databases.

- The Cochrane Central Register of Controlled Trials (CENTRAL), part of *The Cochrane Library*
- MEDLINE
- EMBASE
- PsycINFO
- CINAHL
- Sociological Abstracts
- Social Science Citation Index (SSCI)
- Science Citation Index expanded (SCI exanded)
- SCOPUS
- Cochrane Database of Systematic Reviews (CDSR)
- Database of Abstracts of Reviews of Effects (DARE)
- NIH's RePORTER
- PILOTS
- Dissertation Abstracts International
- Conference Paper Citation Index - Science (CPCI-S)
- Conference Paper Citation Index - Social sciences & humanities (CPCI-SS)
- Dissertation Abstracts International
- National Research Register(NRR) Archive (<http://www.nihr.ac.uk/Pages/NRRArchive.aspx>)
- ClinicalTrials.gov
- ICTRP

We will also search the following electronic sources:

- SAMHSA's National Registry of Evidence-Based Programs and Practices (<http://www.nrepp.samhsa.gov/>);
- World Health Organization (<http://www.who.int/publications/en/>);
- Sagepub.com;

We will base our searches on the following Ovid MEDLINE search strategy which uses the Cochrane highly sensitive search strategy for identifying randomized trials (Lefebvre 2008). The search terms and syntax will be adapted appropriately for other databases.

1. Sex Offenses/
2. Rape/
3. Incest/
4. (sex\$ adj5 abuse\$.tw.
5. (sex\$ adj5 offenc\$.tw.
6. (sex\$ adj5 offens\$.tw.
7. incest\$.tw.

8. rape\$.tw.
9. molest\$.tw.
10. (sex\$ adj5 victim\$).tw.
11. (sex\$ adj5 coerc\$).tw.
12. (sex\$ adj5 exploit\$).tw.
13. (sex\$ adj5 assault\$).tw.
14. (sex\$ adj5 violen\$).tw.
15. (sex\$ adj5 inappropriate).tw.
16. or/1-15
17. exp Infant/
18. exp Child/
19. Adolescent/
20. (baby or babies or infant\$ or preschool\$ or pre-school\$ or child\$ or teen\$ or adolescen\$ or youth\$ or young people\$ or young person\$).tw.
21. or/17-20
22. 16 and 21
23. Child Abuse, Sexual/
24. (child\$ adj5 sex\$).tw.
25. or/22-24
26. exp adult/
27. survivors/
28. (adult\$ or wom#n\$ or men\$ or surviv\$).tw.
29. or/26-28
30. 25 and 29
31. "Adult Survivors of Child Abuse"/
32. 30 or 31
33. randomized controlled trial.pt.
34. controlled clinical trial.pt.
35. randomi#ed.ab.
36. placebo\$.ab.
37. drug therapy.fs.
38. randomly.ab.
39. trial.ab.
40. groups.ab.
41. or/33-40
42. exp animals/ not humans.sh.
43. 41 not 42
44. 32 and 43

Searching other resources

We will search conference proceedings for: Society for Social Work Research, American Psychological Association, American Sociological Association, European Sociological Association, Society for the Scientific Study of Sexuality, International Society for Research on Aggression, and Interdisciplinary Research Center on Family Violence and Violence Against Women. We will also reach out to the Child Maltreatment Researchers Listserv, to issue a "call for studies" in attempt to find grey literature.

We will contact key experts for information on any unpublished or in press studies as well as suggestions for other researchers to contact. Our preliminary list of contacts include: JE Taylor, K

Callahan, D Pelekis, D Finkelhor, P Resick, D Meichenbaum, E Foa, and F Shapiro.

We will also scan reference lists of identified studies.

Data collection and analysis

Selection of studies

Two review authors will independently screen the titles and abstracts of all studies found. If both review authors deem the study to be irrelevant, it will be discarded. If either screener thinks a study may be eligible based on its title and abstract, we will find the full text and examine it further. Once we have the full texts for seemingly relevant studies, the two review authors will independently screen and code all studies using the screening and data extraction form as shown in [Appendix 1](#) to determine if studies meet the inclusion criteria. We will calculate kappas on all items needed for eligibility decisions (Level 2 in the data extraction form). Disagreements will be resolved through discussion and/or consultation with a third review author as needed. We will record specific reasons for exclusion for all excluded studies that make it past the initial screening stage (Level 1).

Data extraction and management

The two review authors will independently extract data from the selected studies using the data extraction form. The data extraction form includes sections on: research methods, study information, intervention characteristics, control characteristics, participant data, and outcome data. If there are multiple reports of a single study, they will be coded onto a single data extraction form. The first report that we find will be extracted first and then additional reports will be used to fill in any gaps. If there are any discrepancies in reports, we will contact the study authors for clarification. We will note any disagreements between reviewers and these will be negotiated, and, if necessary, arbitrated by a third review author.

Assessment of risk of bias in included studies

Two independent review authors will assess each study for risk of bias and report the findings as risk of bias tables using the Cochrane Collaboration's statistical software, Review Manager ([Review Manager 2011](#)). The review authors will assess each of the five categories of bias identified in the *Cochrane Handbook for Systematic Reviews of Interventions* ([Higgins 2011](#)): sequence generation, allocation concealment, blinding, incomplete outcome data, and selective reporting. We will also assess two additional categories: performance bias and conflicts of interest. Each domain will be assessed as 'low risk', 'high risk', or 'unclear'.

Sequence generation

We will determine whether studies used computer-generated random numbers, table of random numbers, drawing lots or envelopes, coin tossing, shuffling cards, or throwing dice.

- Low risk: the study authors explicitly stated that they used one of the above methods.
- High risk: the authors did not use any of the above methods.
- Unclear: there is no information on randomization method or it is not clearly presented.

Allocation concealment

We will evaluate whether investigators and participants could foresee assignments before screening was complete and consent was given.

- Low risk: researchers and participants were unaware of future allocation to treatment conditions.
- High risk: allocation was either not used or was not concealed from researchers before eligibility was determined or participants before consent was given.
- Unclear: information regarding allocation concealment is not known or not clearly presented.

Blinding

We do not expect that participants or treatment providers (therapists) could be kept blind to the intervention condition. Since many of the outcome measures used will be self-report, the possibility of blinding will be low. Therefore, in this item we will assess whether those who assessed and coded the measures were blind to the treatment conditions.

- Low risk: assessors were blind to the treatment conditions.
- High risk: assessors were not blind to the treatment conditions
- Unclear: information on the blinding of assessors is unclear or unavailable from study authors.

Performance bias

This item will assess whether there were treatment differences between groups other than the main intervention contrasts (for example, additional services)

- Low risk: there were no treatment differences between groups other than the main intervention.
- High risk: there were treatment differences between groups other than the main intervention.
- Unclear: it is unclear whether there were differences between groups or this information was not available from study authors.

Incomplete outcome data

- Low risk: there are no drop-outs/exclusions; there is some missing data but the reasons for missing data are unlikely to be related to the true outcome; or missing data are balanced in proportion across intervention groups, with similar reasons for missing data across groups.
- High risk: there is differential attrition across groups, reasons for drop-out are different across groups, there was inappropriate application of simple imputation (for example, assuming certain outcomes, last observation carried forward (LOCF), etc.).
- Unclear: the attrition rate is unclear or authors state that intention-to-treat analysis was used but provide no details.

Selective reporting bias

To assess outcome reporting bias, we will attempt to collect all study reports (and protocols, if possible) and will track the collection and reporting of outcome measures across all available reports for each included study.

- Low risk: all outcome measures and follow-ups are reported.
- High risk: data from some outcome measures are not reported.
- Unclear: it is not clear whether all data collected by study authors was reported.

Conflicts of interest

- Low risk: there is no evidence that researchers or data collectors would benefit if results favored the intervention or control group.
- High risk: there is evidence that researchers or data collectors would benefit if results favored the intervention or control group (study authors also created therapeutic intervention, study authors received funding from a particular therapeutic intervention, etc.)
- Unclear: it is unclear whether researchers or data collectors would benefit if results favored the intervention or control group.

Measures of treatment effect

We will record all outcomes measured, but only primary and secondary outcomes will be described in detail. We will contact authors for valid *n*'s, means, and standard deviations if necessary. We will use Hedge's *g* to correct for small sample size. We will analyze multiple follow-ups separately.

Dichotomous data

While the primary and secondary outcomes are usually assessed with continuous measures, we expect that some investigators will present dichotomous data on these outcomes. For example, dichotomous indicators have been used to show whether cases are

above or below a clinical threshold (for example, for depression, using the Beck Depression Inventory), whether participants experienced substantial symptom relief, perpetrated child abuse/neglect, and tested positive on biologic measures of substance use. For dichotomous measures, we will calculate odds ratios (ORs) with 95% confidence intervals (CIs).

Continuous data

When studies have used the same continuous outcome measure, we will calculate the mean differences (MDs) with 95% CIs. When studies have used different outcome measures to assess the same construct, we will calculate standardized mean differences (SMDs) and 95% CIs. Conceptually distinct outcomes will be presented in separate forest plots.

Unit of analysis issues

We do not anticipate unit of analysis problems in this review, but if we identify any cluster-randomized trials we will adjust the standard errors or sample sizes using the method described in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011). The adjustment method requires the intraclass correlation coefficient (ICC). If this is not available, we will use the ICCs from analogous cluster-randomized trials. If analogous studies are not available, we will use a series of plausible values in a sensitivity analysis.

Dealing with missing data

We will make every effort to contact the original authors of the studies to gather information missing in the written reports. We will ask questions in an open-ended manner to prevent the skewing of responses in a positive direction (Higgins 2011).

For missing dichotomous data, we will simulate intent-to-treat analysis using imputation under a variety of assumptions. We will impute missing values for cases lost to follow-up using the best and worst case scenarios (all positive outcomes in one group and negative outcomes in the other). We will then conduct a sensitivity analysis, comparing results with imputed data to those obtained when we assume that the data are missing at random.

For continuous data, we will extract all available data from reports including available cases, completers only, and last observation carried forward. We will then see which is the most commonly used method. If available cases is the most common, as we assume, we will conduct a sensitivity analysis by assuming data are missing at random and that reported means and standard deviations apply to missing cases. If possible, we will explore other plausible values of missing data in sensitivity analysis.

Assessment of heterogeneity

It is anticipated that some degree of heterogeneity will be present due to between-study variations in sample characteristics, treatment implementation, and research methods; thus, we will rely on results of random-effects models with 95% CIs. We will evaluate heterogeneity with Chi^2 and I^2 to determine the proportion of heterogeneity that is not due to chance. If there is significant heterogeneity, we will try to identify possible explanations.

Assessment of reporting biases

If there are at least ten studies in the meta-analysis, we will create contour-enhanced funnel plots to investigate relationships between effect size and standard error, and we will explore possible statistical analyses (Peters 2008).

For the total network, we will employ a variation of the funnel plot by adjusting for the fact that study effect sizes refer to different comparisons (Chaimani 2012).

Data synthesis

We will present descriptive statistics on both population and treatment characteristics across trials. We will then perform pairwise meta-analyses of all studies that compare similar interventions on conceptually similar outcomes (for example, all studies of effects of CBT vs. no treatment on PTSD symptoms). We will use inverse variance weights to pool results across studies and present results in forest plots.

In order to increase the number of studies (and statistical power) in these analyses, we will include both dichotomous and continuous measures of the same outcome in the forest plot; to do this, we will first transform ORs to Hedges' g s, using the Cox formula (log odds ratio divided by 1.65) described by Sanchez-Meca 2003. For ease of interpretation, we will also consider transforming SMDs into risks if we can identify meaningful cut points and if the data meet other assumptions (Anzures-Cabrera 2011).

If the data suggest we have a connective network, we will then consider a network meta-analysis (NMA). A NMA will allow us to compare all treatments to each other, as well as to different control conditions, using both direct and indirect comparisons (Higgins 1996, Lu 2006, Salanti 2008). In other words, if A and B are active treatments (for example, EMDR and CBT) and C is a comparison condition (for example, waitlist), we can compare A to B if we have direct comparisons (studies of A vs. B) and/or indirect comparisons created from joint analysis of studies of A vs. C and studies of B vs. C. If the direct and indirect comparisons are in agreement, we will combine them to create mixed estimates of the relative effects of different psychosocial interventions for adults survivors of CSA on the three primary outcomes: PTSD symptoms, depression symptoms, and anxiety symptoms. In the analysis, each conceptually distinct treatment modality and control condition (EMDR, CBT, PE, CPT, Psychodynamic psychotherapy, supportive therapy, no-contact waitlist control, and minimal contact waitlist control) will form a separate node. If the

assumption of transitivity is deemed appropriate for the data, we will synthesize the studies in the network so that each node will be compared to all other nodes, using all available direct and indirect evidence.

The assumption of transitivity underlies NMA and can manifest itself as consistency (agreement between different sources of evidence) in closed loops in the network. For a common comparator to be transitive, it must link sets of studies that are comparable in all effect modifiers and the common comparator itself must be similar in both sets of studies (Salanti, in press). For example, a “waitlist control” which dictated absolutely no contact between the control group and therapeutic staff is different from a “waitlist control” in which a therapist checks in weekly by phone to assess for client suicidality. These control conditions may be too different to form a single node and may not provide valid indirect evidence for the treatments to which they are compared. Transitivity also implies that patients are equally likely to be randomized to all conditions and that there are no interactions between type of treatment an effect modifiers such as the severity of symptoms, study location, or date. We do not anticipate any systematic differences between treatments in terms of sample characteristics, methodological variables, location or timing; however, we will look for evidence to the contrary by examining associations between treatment type and other study characteristics.

The lack of transitivity is often reflected in the data as inconsistency; that is, disagreement between direct and indirect treatment effects. We will examine each of the closed loops that are formed through the Bucher method (Bucher 1997); we will calculate differences between direct and indirect estimates and determine if there are material discrepancies (Salanti 2009). After testing each closed loop, we will evaluate the network as a whole by using the design-by-treatment interaction (White 2011). In the event of inconsistency, we will examine its possible sources (for example, errors in the data extraction process, uneven distribution of effect modifiers across groups of trials that compare different treatments) as well as possible sources of heterogeneity. That is, we will examine the distribution of clinical and methodological variables that may contribute to inconsistency or heterogeneity in each compar-

ison-specific group of trials. We may decide to split the network in order to improve consistency.

We will employ a Bayesian statistical framework to conduct the NMA. This will allow us to estimate the effectiveness of all treatments in the analysis. We will obtain probabilities that result from Bayesian analysis and we will present data in graphs and tables (Salanti 2011).

Subgroup analysis and investigation of heterogeneity

If there are ten or more studies that provide similar comparisons and there is evidence of heterogeneity in the pair-wise meta-analysis, we will test a number of different moderators using the ANOVA (analysis of variance) analog for categorical moderators and meta-regression for continuous moderators.

We will examine the following potential moderators: baseline severity of symptoms on the primary outcomes, treatment format (group, individual, family), duration of treatment, whether the treatment modality teaches coping skills, whether the treatment was manualized, and whether the treatment included the use of homework.

Subgroup analyses are not planned.

Sensitivity analysis

In addition to the analyses described above, we will use sensitivity analysis to determine the effects of inclusion and exclusion of the following types of studies:

1. studies that were deemed to have a high risk of incomplete outcome data;
2. studies with imputed data;
3. studies with attrition rates greater than 20%;
4. studies with a high risk of conflict of interest.

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* Indicates the major publication for the study

APPENDICES**Appendix I. Data extraction form**

Level 1: Initial Screening

1) Is this paper about psychosocial interventions for adults who were sexually abused as children?

= Yes

No [STOP HERE]

- Uncertain

2) What is this paper?

= An evaluation of a psychosocial intervention for adults who were sexually abused as children [CONTINUE]

= A review of psychosocial interventions for adults who were sexually abused as children [SCAN REFERENCES]

= Descriptive, epidemiological, correlational or case study [STOP HERE]

Theoretical or position paper, editorial or book review [STOP HERE]

- Practice guidelines or treatment manual [STOP HERE]

= Other _____

= Can't tell [GET FULL REPORT]

Level 2: Eligibility Decisions

Study ID _____

Coder's initials _____

Date _____

Reports associated with this study:

Report ID	First 3 Authors	Date
1		
2		
3		

- 1) Does this study include participants over the age of 18 who were sexually abused as children?
 - Yes
 - = No [STOP HERE]
 - = Can't Tell
- 2) Does this study include participants who are developmentally disabled, experiencing active psychosis, or victims of human sex trafficking?
 - = Yes [STOP HERE]
 - No
 - = Can't tell
- 3) Does this study assess an intervention "designed to alleviate psychological distress, reduce maladaptive behavior, or enhance adaptive behavior through counseling, structured or unstructured interaction, a training program, or a predetermined treatment plan" (Weiss et al., 1987)?
 - = Yes
 - No [STOP HERE]
 - = Can't Tell
- 4) Is the intervention conducted by psychological, social work, or psychiatric professionals or professionals in training (Masters-level or above)?
 - Yes
 - = No [STOP HERE]
 - Can't Tell
- 5) Is this study a randomized control trial?
 - = Yes
 - No [STOP HERE]
 - Can't Tell

Level 3: Study Level

Research methods

- 1) Specify random assignment design:
 - = Simple/systematic
 - Stratified/blocked (identify stratifying variables)
 - Yoked pairs (created by timing of enrolment into study)
 - = Matched pairs (identify matching variables)
 - Cluster (group) randomized
 - = Other
 - = Can't tell
- 2) Who performed group randomization?
 - Research staff
 - = Program/school staff
 - Other
 - = Can't tell
- 3) How many intervention groups were there (the primary prevention program counts as one)?
 - One
 - Two

- = Three or more
- 4) How many intervention groups are relevant for this review?
- = One
- = Two
- 5) How many different control/comparison groups were there? (Groups that received different treatments not counting multiple sites)
- = One
- = Two or more
- 6) How many control/comparison groups are relevant for this review?
- = One
- = Two or more

Study Information

- 7) Start and end dates of enrolment in the study:
- 8) Funding source for the study:

Intervention Characteristics

- 9) Theoretical orientation of psychosocial intervention (select one)
- Cognitive Behavioral Therapy (CBT)
- = Stress Inoculation Therapy (SIT)
- = Prolonged Exposure therapy (PE)
- Cognitive Processing Therapy (CPT)
- = Eye Movement Desensitization and Reprocessing (EMDR)
- = Psychodynamic Psychotherapy
- Supportive/Talk therapy (if yes, describe in further detail)
-
- = Other
- 10) Treatment modality of psychosocial intervention (select all that apply)
- = Individual
- = Group
- Family
- = Couples
- 11) Does the treatment intervention utilize homework assignments?
- Yes
- = No
- = Can't tell
- 12) Does the psychosocial intervention explicitly teach new or enhanced coping skills?
- = Yes (how do we know?)
- = No
- Can't tell
- 13) Was psychosocial intervention described as manualized?
- = Yes
- No
- = Can't Tell
- 14) Did therapeutic staff receive any specialized training in the intervention technique?
- Yes
- = No
- = Can't Tell
- 15) Is there any information on program adherence/fidelity?
- = Yes (what?)
- = No
- Can't tell
- 16) Therapist characteristics

	Primary Pgm.	Control	Total	Pg # & Notes
Gender (% female)				
Level of training				
Mean years of experience				

17) Sample Size

	Primary Pgm.	Control	Total	Pg # & Notes
Referred to study				
Consented				
Randomly assigned				
Started Treatment				
Completed Treatment				
Completed Post-Tx Data				
Completed Follow-Up				

18) Sample Characteristics

	Primary pgm.	Control	Total	Pg # & notes
Gender (% female)				
Mean age at start of study				
Mean age at 1 st offence				
Mean age at primary offence				
Mean # of offenders				

(Continued)

Mean # of CSA incidents					
Mean duration of primary offence					
% who experienced incest					
% who experienced penetration					
% who experienced oral sexual contact					
% who experienced kissing/fondling					

Other characteristics:

19) Were there any differences between program and control groups at baseline? (Note those that are significantly different, as well as those with a 10% or greater difference)

- Yes (what?)
- = No (how do we know?)
- = Can't tell

20) Was there any analysis of differences between intervention completers and drop-outs within the intervention?

- Yes
- = No
- = Can't tell

21) Psychosocial intervention characteristics

	Intended	Minimum	Maximum	Mean	SD	Pg # & Notes
Number of -Ind. Sessions -Group Sessions						
Duration in -Weeks -Months						

22) Is there information on the cost of implementing the psychosocial interventions?

- = Cost per case
- = Total cost
- = No information

23) A.) Is there a measure of therapeutic alliance?

- = Yes (what?)
- = No

B.) If yes, what were the results of this measure?

.....
24) A.) Is there a measure of hope/expectation?

= Yes (what?)

= No

B.) If yes, what are the results of this measure?

25) Did the therapist demonstrate unconditional positive regard towards the client?

- Yes (how do we know?)

= No

Services provided to control cases

26) Theoretical orientation of control (select one)

= Cognitive Behavioral Therapy (CBT)

- Stress Inoculation Therapy (SIT)

= Prolonged Exposure therapy (PE)

= Cognitive Processing Therapy (CPT)

- Eye Movement Desensitization and Reprocessing (EMDR)

= Psychodynamic Psychotherapy

= Supportive/Talk therapy (if yes, describe in further detail)

= Waitlist/Minimal Attention

= Other

27) Treatment modality of control (select all that apply)

= Individual

= Group

- Family

= Couples

= N/A

28) Does the control intervention utilize homework assignments?

= Yes

= No

- Can't tell

29) Does the psychosocial intervention explicitly teach new or enhanced coping skills?

= Yes (how do we know?)

- No

= Can't tell

30) Was the control described as manualized?

- Yes

= No

= Can't Tell

31) Did therapeutic staff receive any specialized training in the control technique?

= Yes

= No

- Can't Tell

32) Is there any information on program adherence/fidelity?

= Yes (what?)

- No

= Can't tell

= Not applicable

33) Was there any analysis of differences between completers and drop-outs in the control group?

= Yes

= No

- Can't tell

34) Control intervention characteristics

	Intended	Minimum	Maximum	Mean	SD	Pg # & Notes
Number of -Ind. Sessions -Group Sessions						
Duration in -Weeks -Months						

35) Is there information on the cost of implementing the control?

- = Cost per case
- = Total cost
- No information

36) A.) Is there a measure of therapeutic alliance?

- Yes (what?)
- No
- B.) If yes, what were the results of this measure?

37) A.) Is there a measure of hope/expectation?

- = Yes (what?)
- No
- B.) If yes, what are the results of this measure?

38) Did the therapist demonstrate unconditional positive regard toward the client?

- Yes (how do we know?)
- No

39) Was attention given to the client in the control condition

- = Yes
- = No

Level 4: Outcome Measures

1) When were data collected? (check all that apply)

- Baseline
- Post-tx
- = 1st follow-up (when?)
- = 2nd follow-up (when?)
- Other

2) How was data collected? (check all that apply)

- Self-report
- Interview
- = Focus group
- = Other

3) Were data collected in the same manner for tx and control groups?

- = Yes
- No
- Can't tell

Level 4: Outcome measures (outcome level)

Instructions: Enter outcomes measures in Excel in the order in which they are described in the text. Enter each conceptually-distinct outcome and instrument, regardless of whether data were collected (at the time of the report) or reported. Note that a single outcome measure can be completed by multiple sources and at multiple points in time (data from specific sources and time-points will be entered later).

	<i>Timing of data collection</i>	<i>Reliability & Validity</i>	<i>Format</i>	<i>Sources (identify all)</i>	<i>Pg# & notes</i>
<i>Conceptual domain code:</i>	<input type="radio"/> Baseline <input type="radio"/> Post-tx <input type="radio"/> 1 st f-u <input type="radio"/> 2 nd f-u <input type="radio"/> Other	Info from: <input type="radio"/> Other samples <input type="radio"/> This sample <input type="radio"/> Unclear Info provided:	<input type="radio"/> Dichotomous (e.g., event) <input type="radio"/> Continuous (e.g., scale)	<input type="radio"/> Individual <input type="radio"/> Therapist <input type="radio"/> Family Member <input type="radio"/> Other <input type="radio"/> Unclear	
<i>Instrument or Definition:</i>	<i>Blind?</i> <input type="radio"/> Yes <input type="radio"/> No <input type="radio"/> Unclear		<i>Direction</i> High score or event is <input type="radio"/> Positive <input type="radio"/> Negative <input type="radio"/> Unclear	<i>Mode of Admin</i> <input type="radio"/> Self (paper) <input type="radio"/> Interview <input type="radio"/> Focus Group <input type="radio"/> Other	

Conceptual domains codes:

- 1= PTSD Symptoms
- 2= Depression Symptoms
- 3= Anxiety Symptoms
- 4= Global Mental Health Functioning/Distress
- 5= Abuse or Neglect of Child
- 6= Substance Use
- 7= Self-harming Behaviors
- 8= Disordered Eating
- 9= Social Functioning
- 10= Dissociation

Note: Repeat as often as necessary to code all outcome measures.

Level 4: Outcome data

Please enter outcome data in the Excel sheet. Enter dichotomous outcomes first, then continuous outcomes. Outcome # refers to the measures described above.

Dichotomous outcome data

Enter data only if it is provided (do not perform calculations). OR = odds ratio. Enter exact P value if available. If covariates (control variables) are used in the analysis, please identify these variables under Statistics (cov).

Outc #	Timing	Source	Valid Ns	n w/ event	% w event	Statistics	Pg #
	<input type="radio"/> Baseline <input type="radio"/> Post tx <input type="radio"/> 1 st f-u <input type="radio"/> 2 nd f-u <input type="radio"/> Other	<input type="radio"/> Individual <input type="radio"/> Therapist <input type="radio"/> Family Member <input type="radio"/> Other	Pgm	Pgm	Pgm	OR 95%CI (LB UB) Chi2 Df p-val Other	

- = High risk: Authors did not use reliable and valid instruments.
 - Unclear: It is unclear whether authors used reliable and valid instruments.
 - 7) Conflicts of interest.
 - = Low risk: There is no evidence that researchers or data collectors would benefit if results favored the intervention or control group.
 - High risk: There is evidence that researchers or data collectors would benefit if results favored the intervention or control group (study authors also created therapeutic intervention, study authors received funding from a particular therapeutic intervention, etc.)
 - = Unclear: It is unclear whether researchers or data collectors would benefit if results favored the intervention or control group.
- Further comments:

WHAT'S NEW

Date	Event	Description
28 August 2012	Amended	Note on Campbell coregistration added.

HISTORY

Protocol first published: Issue 9, 2012

CONTRIBUTIONS OF AUTHORS

JSW drafted the protocol with input from JHL and GS.

JSW will code all studies, train and supervise other coders, conduct all statistical tests, and be the primary author of the finished study.

JHL will arbitrate disagreements between coders and serve as advisor to JSW.

GS will serve as consultant for the statistical analysis.

DECLARATIONS OF INTEREST

Jessica Schaffner Wilen - none known.

Julia H Littell - none known.

Georgia Salanti - none known.

SOURCES OF SUPPORT

Internal sources

- None, Not specified.

External sources

- None, Not specified.

NOTES

This review is coregistered with the Campbell Collaboration Social Welfare Group and will also appear on the Campbell Library.