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Interventions for treating persistent pain in survivors of torture (Review)

Baird E, Williams ACDC, Hearn L, Amris K

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[Intervention Review]

Interventions for treating persistent pain in survivors of torture

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ABSTRACT

Background

Persistent (chronic) pain is a frequent complaint in survivors of torture, particularly but not exclusively pain in the musculoskeletal system. Torture survivors may have no access to health care; where they do, they may not be recognised when they present, and the care available often falls short of their needs. There is a tendency in state and non-governmental organisations' services to focus on mental health, with poor understanding of persistent pain, while survivors may have many other legal, welfare, and social problems that take precedence over health care.

Objectives

To assess the efficacy of interventions for treating persistent pain and associated problems in survivors of torture.

Search methods

We searched for randomised controlled trials (RCTs) published in any language in CENTRAL, MEDLINE, Embase, Web of Science, CINAHL, LILACS, and PsycINFO, from database inception to 1 February 2017. We also searched trials registers and grey literature databases.

Selection criteria

RCTs of interventions of any type (medical, physical, psychological) compared with any alternative intervention or no intervention, and with a pain outcome. Studies needed to have at least 10 participants in each arm for inclusion.

Data collection and analysis

We identified 3578 titles in total after deduplication; we selected 24 full papers to assess for eligibility. We requested data from two completed trials without published results.

We used standard methodological procedures expected by Cochrane. We assessed risk of bias and extracted data. We calculated standardised mean difference (SMD) and effect sizes with 95% confidence intervals (CI). We assessed the evidence using GRADE and created a 'Summary of findings' table.

Main results

Three small published studies (88 participants) met the inclusion criteria, but one had been retracted from publication because of ethical problems concerned with confidentiality and financial irregularities. Since these did not affect the data, the study was retained in this review. Despite the search including any intervention, only two types were represented in the eligible studies: two trials used cognitive behavioural therapy (CBT) with biofeedback versus waiting list on unspecified persistent pain (58 participants completed treatment), and one examined the effect of complex manual therapy versus self-treatment on low back pain (30 participants completed treatment). Excluded studies were largely either not RCTs or did not report pain as an outcome.

There was no difference for the outcome of pain relief at the end of treatment between CBT and waiting list (two trials, 58 participants; SMD -0.05, 95% CI -1.23 to 1.12) (very low quality evidence); one of these reported a three-month follow-up with no difference between intervention and comparison (28 participants; SMD -0.03, 95% CI -0.28 to 0.23) (very low quality evidence). The manual therapy trial also reported no difference between complex manual therapy and self-treatment (30 participants; SMD -0.48, 95% CI -9.95 to 0.35) (very low quality evidence). Two studies reported dropouts, one with partial information on reasons; none of the studies reported adverse effects.

There was no information from any study on the outcomes of use of analgesics or quality of life.

Reduction in disability showed no difference at the end of treatment between CBT and waiting list (two trials, 57 participants; SMD -0.39, 95% CI -1.17 to 0.39) (very low quality evidence); one of these reported a three-month follow-up with no difference between intervention and comparison (28 participants; SMD 0, 95% CI -0.74 to 0.74) (very low quality evidence). The manual therapy trial reported superiority of complex manual therapy over self-treatment for reducing disability (30 participants; SMD -1.10, 95% CI -1.88 to -0.33) (very low quality evidence).

Reduction in distress showed no difference at the end of treatment between CBT and waiting list (two trials, 58 participants; SMD 0.07, 95% CI -0.46 to 0.60) (very low quality evidence); one of these reported a three-month follow-up with no difference between intervention and comparison (28 participants; SMD -0.24, 95% CI -0.50 to 0.99) (very low quality evidence). The manual therapy trial reported superiority of complex manual therapy over self-treatment for reducing distress (30 participants; SMD -1.26, 95% CI -2.06 to -0.47) (very low quality evidence).

The risk of bias was considered high given the small number of trials, small size of trials, and the likelihood that each was underpowered for the comparisons it reported. We primarily downgraded the quality of the evidence due to small numbers in trials, lack of intention-to-treat analyses, high unaccounted dropout, lack of detail on study methods, and CIs around effect sizes that included no effect, benefit, and harm.

Authors' conclusions

There is insufficient evidence to support or refute the use of any intervention for persistent pain in survivors of torture.

PLAIN LANGUAGE SUMMARY

Treating persistent pain in torture survivors

Bottom line

There is no good evidence about any method of treating long-lasting pain following torture.

Background

Psychological problems following torture, such as depression and post-traumatic stress disorder (PTSD), receive a lot of attention in refugee healthcare. Physical problems after torture tend to be overlooked by staff trained in mental health care. Survivors of torture often suffer long-lasting pain, usually affecting muscles and joints.

Study characteristics

We wanted to know whether any treatments were successful in improving pain, and reducing disability and distress in survivors of torture. We searched the academic literature to February 2017 and found three randomised controlled trials (clinical studies where people are randomly put into one of two or more treatment groups).

Key results

Two studies (58 participants) compared cognitive behavioural therapy (CBT; talking therapy that helps people change the way they think and behave) plus learning to control muscles and breathing with no treatment, and we were able to combine these for analysis. Neither study showed any meaningful improvement in pain, reduction in disability, or reduction in distress, over eight to 13 weeks of treatment. One study (30 participants) compared complex manual therapy with self-treatment for low back pain but could not be combined with the other two studies; it reported no difference in pain relief, but did report that the physical intervention reduced disability and distress at the end of treatment.

Quality of the evidence

We rated the quality of the evidence from studies using four levels: very low, low, moderate, or high. Very low quality evidence means that we are very uncertain about the results. High quality evidence means that we are very confident in the results. The quality of the evidence was very low for pain relief, reduction in distress, and reduction in disability. This was due to the small size of the studies, poor study design, and substantial dropout of participants from studies.

SUMMARY OF FINDINGS FOR THE MAIN COMPARISON *[Explanation]*

CBT with biofeedback ± physical exercise versus waiting list control for pain in torture survivors at the end of treatment (data insufficient for analysis at follow-up)

Patient or population: adult torture survivors with chronic pain
Settings: various
Intervention: CBT with biofeedback ± physical exercise
Comparison: waiting list

Outcomes	Illustrative comparative risks* (95% CI)		Relative effect (95% CI)	No of participants (studies)	Quality of the evidence (GRADE)	Comments
	Assumed risk	Corresponding risk				
	Control	Intervention				
Pain relief At least 30% pain relief or pain < 5/10 Scales: VRS 0-6; SFMPQ-PRI (0-45)	Not known	The mean change at the end of the intervention was 0.05 standard deviations lower (1.23 lower to 1.12 higher)	-	58 (2 RCTs)	⊕◯◯◯ **Very low**[1,2,3]	Change was too small for any clinical relevance.
Adverse effects, including dropout and attrition	Not known	No data	-	-	-	Where reasons were given for dropout, they did not constitute adverse effects
Reduced use of analgesics	Not known	No data	-	-	-	No study reported use of analgesics.
Reduction in disability Pain Coping Questionnaire behavioural subscale (12 items: 0-7); WHODAS-II (12 items: 1-5)	Not known	The mean change in the intervention groups was 0.39 standard deviations lower (1.17 lower to 0.39 higher)	-	57 (1 missing) (2 RCTs)	⊕◯◯◯ **Very low**[1,2,3]	This represents < 15% improvement on baseline score. In Liedl 2011, participants still scored only around half of the non-disabled total; in Wang 2017, par-

						ticipants appeared to have low disability at baseline.
Quality of life	Not known	No data	-	-	-	No study reported quality of life.
Reduction in distress PDS (17 items: 0-3) HTQ-Part IV (30 items: 1-4)	Not known	The mean change in the intervention groups was 0.07 standard deviations higher (0.46 lower to 0.60 higher)	-	58 (2 RCTs)	⊕◯◯◯ **Very low**[1,3]	Change was too small for any clinical relevance.
Global improvement, satisfaction	Not known	No data	-	-	-	No study reported global improvement/ satisfaction.

*The basis for the **assumed risk** (e.g. the median control group risk across studies) is provided in footnotes. The **corresponding risk** (and its 95% confidence interval) is based on the assumed risk in the comparison group and the **relative effect** of the intervention (and its 95% CI).
CBT: cognitive behavioural therapy; **CI:** confidence interval; **FESV:** Fragebogen zur Erfassung der Schmerzverarbeitung (German Pain Coping Questionnaire); **HTQ:** Harvard Trauma Questionnaire; **PDS:** Post-traumatic Diagnostic Scale; **RCT:** randomised controlled trial; **SFMPQ-PRI:** Short-Form McGill Pain Questionnaire - Pain Rating Index; **VRS:** Verbal Rating Scale; **WHODAS-II:** World Health Organization Disability Assessment Schedule.

GRADE Working Group grades of evidence
High quality: we are very confident that the true effect lies close to that of the estimate of the effect.
Moderate quality: we are moderately confident in the effect estimate; the true effect is likely to be close to the estimate of effect, but there is a possibility that it is substantially different.
Low quality: our confidence in the effect estimate is limited; the true effect may be substantially different from the estimate of the effect.
Very low quality: we have very little confidence in the effect estimate; the true effect is likely to be substantially different from the estimate of effect

[1] Serious limitations in design: loss to follow-up; completer analysis, not intention-to-treat. Downgraded one level for serious limitations and two levels for very serious limitations.
[2] Inconsistency: could not explain heterogeneity. Downgraded one level.
[3] Imprecision of results: very small sample sizes; wide confidence intervals that included no effect, substantial risk and substantial benefit. Downgraded one level for serious limitations and two levels for very serious limitations.

BACKGROUND

Reports of torture and other ill-treatment come from over 150 countries (AI 2010). The International Rehabilitation Council for Torture Victims (IRCT) (IRCT 2010) estimates that around 400,000 torture survivors live in the EU alone, with similar estimates in the USA (Jaranson 1995). Many diverse injuries are inflicted during torture and ill-treatment, usually in conditions of poor nutrition and hygiene, to a highly stressed person, and without health care. The violence, extent, and complexity of injuries often lie outside medical problems addressed in textbooks and in the scientific literature (Amris 2007), and persistent pain is a common finding in survivors of torture (Amris 2007; Rasmussen 1990). Pain is defined by the International Association for the Study of Pain as "an unpleasant sensory and emotional experience associated with actual or potential tissue damage, or described in terms of such damage" (IASP 1994). Persistent or chronic pain is commonly defined as pain that is present for more than three months, assuming the initial injury to have healed in that time. In the case of injury from torture, which commonly goes untreated, this may not be the case.

Unlike many other client groups, the health concerns of torture survivors are defined not primarily by diagnosis or recognised classification systems but by their experience of torture and other ill-treatment. Torture is a deliberate assault upon the body, psyche, identity and integrity of the person, aiming to dehumanise, degrade, destroy, or debilitate and render the person helpless. It is defined by the United Nations Convention against Torture and Other Cruel, Inhuman or Degrading Treatment or Punishment (CAT), Article 1 (UN 1984) as "any act by which severe pain or suffering, whether physical or mental, is intentionally inflicted on a person for such purposes as obtaining from him or a third person information or a confession, punishing him for an act he or a third person has committed or is suspected of having committed, or intimidating or coercing him or a third person, or for any reason based on discrimination of any kind, when such pain or suffering is inflicted by or at the instigation of or with the consent or acquiescence of a public official or other person acting in an official capacity. It does not include pain or suffering arising only from, inherent in or incidental to lawful sanctions" (UN 1984). By extension, torture undermines communities and groups whose members are targeted, spreading distrust and fear (Patel 2007). We will use the wider definition from the World Medical Association (WMA 2006): "the deliberate, systematic or wanton infliction of physical or mental suffering by one or more persons acting alone or on the orders of any authority, to force another person to yield information, to make a confession, or for any other reason."

Physical health problems related to torture have been widely documented (Jacobs 2001; Moreno 2002; Norredam 2005; for reviews see Jaranson 2011; Montgomery 2011; Quiroga 2005), as have psychological health problems (e.g. Basoglu 2006; Johnson 2008; Patel 2007). Torture-related physical health problems not only cause disability and restricted functioning but also produce psychological problems, compounding the impact on overall personal and social functioning. Additionally, torture survivors in countries of exile can experience many social, legal, and practical difficulties (e.g. seeking asylum, being subject to racist attacks, inadequate housing, inability to communicate in the language of the host country, and concerns for family and friends with whom they have lost contact) which may take priority over their health problems; they may also be uncertain about their rights to health care, which may be restricted, and fearful of any perceived authority (Burnett 2001).

Torture survivors may not be recognised as such within the health service (Crosby 2006; Eisenman 2003), and the health care offered or accessible to them falls short of their needs (Amris 2007; Amris 2015; Berliner 2004; Burnett 2001; Quiroga 2005). Psychological services offered by non-governmental organisations have very variable methods and skills (Patel 2014); both they and mainstream mental health services tend to have a poor understanding of persistent pain, and may attribute it to evident psychological disturbance, in particular post-traumatic stress.

Description of the condition

Physical torture is in most instances directed towards the musculoskeletal system, aiming at producing soft tissue lesions and pain and usually at leaving either no visible, or non-specific, findings after the acute stage. Random beatings, systematic beating of specific body parts (head, palms, soles, and lumbar region), strapping/binding, suspension by the extremities, forced positions for extended periods, and electrical torture are frequent (Rasmussen 1990; Williams 2010). Other physical methods include asphyxiation, near-drowning, stabbing, cutting, burning, and sexual assaults, including hetero- and homosexual rape (Olsen 2007; Rasmussen 2006).

Persistent pain in the musculoskeletal system is recognised as one of the most frequent physical complaints presented by torture survivors (Amris 2007; Burnett 2001; Edston 2005; Olsen 2006; Rasmussen 1990; Rasmussen 2006), but other pain has been described and is often hard to classify or describe in terms of mechanism (Amris 2007; Lund 2008; Rasmussen 1990; Williams 2010). Survivors of torture are likely to present with complex and multiple pains, and often with moderate to severe symptoms of depression, anxiety, and traumatic stress (Berliner 2004; el Serraj 1996). There is no basis for the widespread belief that pain from torture is in some way produced by psychological disturbance, other than pain triggered by re-experiencing traumatic events (Taylor 2013); the origin of pain in torture does however add to the complexity of assessment and treatment (Sjölund 2009).

Description of the intervention

Any treatment intended to relieve pain or improve function despite ongoing pain was a possible intervention. Thus, interventions eligible for this review included pharmacotherapy by various routes (oral, sublingual, topical), peripheral nerve blockade and other injections, physiotherapy, psychological rehabilitative treatment, peripheral stimulation such as transcutaneous electrical nerve stimulation, acupuncture, neuromodulation (including spinal cord stimulation), and complementary and alternative therapies.

How the intervention might work

There is no suggestion that interventions would work differently in survivors of torture than in anyone who is not a survivor of torture, only that pain resulting from torture can be difficult to understand in the light of current knowledge, and that survivors are, because of their experience, often hypersensitive to medical procedures required for diagnosis and treatment.

Why it is important to do this review

In the era of evidence-based health care, there is considerable emphasis on services providing treatments demonstrated to be effective. However, health care of torture survivors is almost entirely addressed within the psychological literature, with serious neglect of physical sequelae and their treatment. Populations are diverse in cultural, ethnic, religious, and political backgrounds and are often unable to express themselves adequately in the language of the host country. Compared to the many reviews of interventions for psychological problems (see Jaranson 2011; Patel 2014), there are few reviews of interventions for medical problems, and all are either brief and generalised (e.g. Quiroga 2005), or specific to particular injuries or treatments (e.g. Amris 2000a; Amris 2000b). Most of the literature on physical health difficulties experienced by torture survivors (before or without treatment) consists of clinical opinions and case studies (for review, see Mckenna 2012; Mollica 2011). There are also descriptive studies which enumerate the variety of health problems of survivors, often published with the main aim of raising awareness and concern about the issues (Jaranson 2011; Montgomery 2011; Quiroga 2005).

Of more concern here is that in high-income countries, which have contributed most to the literature on health care for refugee survivors of torture, the focus of clinical and research effort has been on the psychological sequelae, often described in terms of post-traumatic stress disorder (PTSD), rather than on the physical sequelae. This, combined with the slow spread of understanding of pain mechanisms among some medical and paramedical specialities, including psychology and psychotherapy, means that reported pain is often recorded as a psychosomatic presentation of psychological disorder, reducing usefulness for the pain clinician or researcher.

OBJECTIVES

To assess the efficacy of interventions for treating persistent pain and associated problems in survivors of torture.

METHODS

Criteria for considering studies for this review

Types of studies

We included randomised controlled trials (RCTs), cluster RCTs, and quasi-RCTs. We wished to be as inclusive as possible and, since we expected to find a very small number of RCTs, quasi-RCTs were included because some methods of quasi-randomisation used in low-income country settings are unlikely to introduce bias.

There were no restrictions on publication type, status, language, or date, to maximise search yield. We included conference abstracts and other reports if full details could be obtained from the study authors, as relevant material is often published by torture survivor centres themselves.

Types of participants

Participants must have been identified as survivors of torture or ill-treatment, consistent with the UN 1984 definition, or at least 50% of the study population identified as such.

Torture survivors may be found among refugees, asylum seekers, war survivors, and survivors of organised violence, and in diverse settings, such as prison, detention centres, refugee camps, accommodation centres, healthcare facilities, and in the community. We included participants of all ages.

Types of interventions

Interventions could be of any modality and provided by any practitioner, or self-administered, as long as they were primarily aimed at pain relief. Comparators could be any alternative condition: no intervention, waiting list, care as usual, standard care, alternative treatment, or placebo condition. Studies needed to have at least 10 participants in each arm for inclusion.

Types of outcome measures

Primary outcomes

- Pain relief or reduction in pain as reported by the participant, without which the study was not eligible for inclusion in this review. Pain or pain relief may have been measured by any type of scale: numerical (including percentage),

verbal, or pictorial. The desired outcome was 30% pain relief or pain less than 5/10 or equivalent on a numerical scale, or 'none' or 'mild' on a verbal scale.

- Adverse effects, including dropout or attrition.

Secondary outcomes

- Reduced use of analgesics, as rescue analgesia or ongoing analgesic intake.
- Reduction in disability, improved overall function, reduced interference of pain with normal life, or improved quality of life.
- Reduction in distress, including anxiety, depression, traumatic stress symptoms, overall mood.
- Global improvement, satisfaction, as rated by participant.

Search methods for identification of studies

We conducted searches on electronic databases and websites; we handsearched reviews and reference lists.

Electronic searches

We used Medical Subject Headings (MeSH) or equivalent and text word terms. There were no language restrictions. Searches were tailored to individual databases. The search strategies used can be found in Appendix 2.

We searched the following electronic databases on 1 February 2017:

- Cochrane Central Register of Controlled Trials (CENTRAL; 2017, Issue 1) via CRSO;
- MEDLINE and MEDLINE in Process (via Ovid) 1946 to 1 February 2017;
- Embase (via Ovid) 1974 to 1 February 2017;
- Web of Science (ISI) SCI, SSCI, A&HCI, CPCI-S, CPCI-SSH searched to 1 February 2017;
- CINAHL (via EBSCO) 1982 to February 2017;
- LILACS (via Bireme)1985 to February 2017;
- PsycINFO (via Ovid) 1806 to February week 1 2017.

Searching other resources

We searched the following:

- OpenGrey (online database of reports and other grey literature produced in Europe);
- trials registers for details of ongoing trials: (www.clinicaltrials.gov); the metaRegister of controlled trials (www.controlled-trials.com/mrct); the World Health Organization (WHO) International Clinical Trials Registry Platform (ICTRP) (apps.who.int/trialsearch/);
- reference lists of reviews and retrieved full papers;
- citation searches on key articles;
- Online Library of the Rehabilitation and Research Centre for Torture Victims (RCT, now Dignity);
- tables of contents from the top 10 most frequently cited sources emerging from the search (expected to be journal issues).

We contacted or attempted to contact study authors where necessary for additional information.

Data collection and analysis

Selection of studies

Two authors (AW, EB) independently undertook an initial screening of titles and abstracts, using the inclusion criteria, to identify studies which might be eligible and for which the full paper should be obtained. Where abstracts were not available electronically, or were unclear about the criteria applied, we sought the full paper. Two authors (EB, LH) independently read and selected the full papers using the inclusion criteria. The final list was achieved after comparison, with disagreements resolved by discussion; where there continued to be doubt or difference, a third review author (KA) was consulted to achieve consensus.

We included a Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) flowchart to show the status of identified studies (Moher 2009), as recommended in Part 2, Section 11.2.1 of the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011). We included studies in the review irrespective of whether the measured outcome data were reported in a 'usable' way.

Data extraction and management

Two authors (EB, LH) independently extracted the following data where available, using a form developed in previous reviews, and checked for agreement before entry into Review Manager 5 (RevMan 2014). Where there was disagreement, a third author (AW or KA, depending on the topic) was consulted to resolve the difference.

- Methods: study design.
- Methods: sources of bias: sequence generation, allocation sequence concealment, blinding, incomplete outcome data, study size; other concerns about bias were therapist qualification, therapist allegiance, language of assessment.
- Participants: sample size at baseline and all post-treatment assessment points used for analysis; adherence to or participation in treatment; setting of intervention; baseline characteristics of the sample (age, gender, nationality, ethnicity, type of torture experienced, legal status if refugees or asylum seekers, living situation, separation from close family members).
- Interventions: number of arms; types of interventions (drugs, doses, intervention technique, or school of therapy); types of placebo/control condition; protocol for intervention; training of practitioner/therapists.

- Outcomes: assessment points (collected; reported); self-report versus other-report versus objective; psychometric properties of assessment instruments; language(s) of assessment and translation or interpretation.
- Number of participants in each intervention group; sample size; missing participants; completion rates.
- Funding source; key conclusions of study authors; allegiance of the trial authors.

Assessment of risk of bias in included studies

Two authors (EB, LH) independently assessed risk of bias for each study, using the criteria outlined in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011) and adapted from those used by the Cochrane Pregnancy and Childbirth Group, with any disagreements resolved by discussion. We completed a 'Risk of bias' table for each included study, using the 'Risk of bias' tool in Review Manager 5 (RevMan 2014).

We assessed the following for each study, using three categories: low risk, unclear risk (information not provided or effect not clear), and high risk of bias.

- Random sequence generation (checking for possible selection bias): we assessed the method used to generate the allocation sequence as: low risk (any truly random process, e.g. random number table; computer random number generator); unclear risk (method used to generate sequence not clearly stated); high risk (any process that is not truly random, e.g. odd or even year of birth).
- Allocation concealment (checking for possible selection bias): the method used to conceal allocation to interventions prior to assignment determines whether intervention allocation could have been foreseen in advance of, or during, recruitment, or changed after assignment. We assessed the methods as: low risk (e.g. telephone or central randomisation; consecutively numbered sealed opaque envelopes); unclear risk (method not clearly stated); high risk (any method that cannot be adequately concealed, e.g. case record number).
- Blinding of participants and personnel (checking for possible performance bias): it is not possible in many psychological and physical treatment trials to blind study personnel. We assessed the methods used to blind participants as: low risk (when equivalence of treatment expectations was demonstrated before treatment started and maintenance of blinding was demonstrated by inaccuracy of post-treatment guesses at allocation); unclear risk (neither equivalence of treatment expectations or maintenance of blinding was reported, or reported ratings showed lack of equivalent expectations or failure of blinding (or both)); high risk (e.g. trial arms clearly identifiable as treatment or control where control would not generate equivalent expectations of benefit to treatment).
- Blinding of outcome assessment (checking for possible detection bias): we assessed the methods used to blind study participants and outcome assessors from knowledge of which intervention a participant received as: low risk (study stated that it was blinded and described the method used to achieve blinding, e.g. identical tablets); unclear risk (study stated that it was blinded but did not provide an adequate description of how it was achieved); high risk (treatment staff performed outcome assessments).
- Incomplete outcome data (checking for possible attrition bias due to the amount, nature, and handling of incomplete outcome data): we assessed the methods used to deal with incomplete data as: low risk (less than 10% of participants did not complete the study or used 'baseline observation carried forward' analysis, or both); unclear risk (used 'last observation carried forward' analysis); high risk (used 'completer' analysis).
- Selective outcome reporting: we assessed studies as being at low risk (all outcomes reported); unclear risk (information unclear); high risk (one or more outcomes not reported).
- Size of study (checking for possible biases confounded by small size): we assessed studies as being at low risk (200 or greater participants per treatment arm); unclear risk (50 to 199 participants per treatment arm); high risk (fewer than 50 participants per treatment arm).
- Other. Therapist qualification: we assessed therapist qualification as low risk (where therapists were qualified); unclear risk (qualification not clearly stated); high risk (therapists not adequately trained to deliver treatment). Therapist allegiance: we assessed therapist allegiance as low risk (intervention and comparison used separate therapists or no therapist was required for comparison arm); unclear risk (no information on therapist allegiance to intervention method); high risk (clear allegiance of treating therapist to treatment under investigation). Language of assessment: we assessed language of assessment as low risk (assessment in language of participants); unclear risk (language of assessment not clearly stated or standardised translated questionnaires used in multiple languages); high risk (assessment translated for each patient with no standardisation). The overall rating for the 'Other' category represented the most frequently endorsed risk assessment category.

Measures of treatment effect

We planned to analyse dichotomous outcomes (e.g. improved/not improved) using odds ratios with 95% confidence intervals (CIs), using a random-effects model, combining into two categories those outcomes with more than two categories. We did not plan to calculate numbers needed to treat for an additional beneficial/harmful outcome.

We analysed continuous data using standardised mean differences (SMDs) or effect sizes, using pooled standard deviations and weighting for sample size, and calculating the 95% CI, using a random-effects model. We interpreted SMDs individually with reference to the quality and reliability of the measure where available. Where data were severely skewed, we planned to normalise them where possible by transformation or, if this did not produce

a satisfactory distribution, to dichotomise them (Higgins 2011 section 9.4.6).

Unit of analysis issues

Where treatments were sufficiently similar, we combined two or more treatment groups into a single treatment group for analysis. We planned to adjust for the effects of clustering using an intraclass correlation coefficient (ICC) in the case of cluster randomisation.

Dealing with missing data

We contacted or attempted to contact study authors to request missing data required for meta-analysis.

Where standard deviations were missing and unobtainable from study authors, we planned to calculate them where possible from F, t, or P values, or from standard errors. If this was not possible, we planned to treat the trial as having no useable data. We identified intention-to-treat (ITT) analysis as an important marker of effort to reduce bias (see Assessment of risk of bias in included studies).

Assessment of heterogeneity

We interpreted heterogeneity, as indicated by the I^2 statistic, using the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011), with reference to variation between studies.

Assessment of reporting biases

The search strategy was broad, particularly in the grey literature, in an attempt to address publication bias.

Data synthesis

We used Review Manager 5 software to conduct meta-analysis wherever feasible (RevMan 2014). We used a random-effects model, given the various sources of diversity. Where meta-analysis was not possible, we provided a narrative summary of evidence relating to the primary and secondary outcomes.

Grading of evidence

Two authors (LH, AW) independently rated the quality of the outcomes. We used the GRADE system to rank the quality of the evidence using GRADEpro Guideline Development Tool software (GRADEpro GDT 2015), and the guidelines provided in Chapter 12.2 of the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011).

The GRADE approach uses five considerations (study limitations, consistency of effect, imprecision, indirectness, and publication bias) to assess the quality of the body of evidence for each outcome. The GRADE system uses the following criteria for assigning grade of evidence:

- high: we are very confident that the true effect lies close to that of the estimate of the effect;
- moderate: we are moderately confident in the effect estimate; the true effect is likely to be close to the estimate of effect, but there is a possibility that it is substantially different;
- low: our confidence in the effect estimate is limited; the true effect may be substantially different from the estimate of the effect;
- very low: we have very little confidence in the effect estimate; the true effect is likely to be substantially different from the estimate of effect.

We decreased grade if we identified:

- serious (-1) or very serious (-2) limitation to study quality;
- important inconsistency (-1);
- some (-1) or major (-2) uncertainty about directness;
- imprecise or sparse data (-1);
- high probability of reporting bias (-1).

'Summary of findings' table

We included a 'Summary of findings' table to present the main findings in a transparent and simple tabular format. In particular, we included key information concerning the quality of evidence, the magnitude of effect of the interventions examined, and the sum of available data on the outcomes of pain relief, reduction in disability, and reduction in distress at the end of the interventions. We found no information for the outcomes of adverse effects, reduced use of analgesics, change in quality of life, and global improvement/satisfaction. Data were insufficient to analyse outcomes at follow-up.

Subgroup analysis and investigation of heterogeneity

We planned the following subgroup analyses:

- child and adult studies separately, since methods and outcomes usually differ, as does the type of torture experienced;
- by types of pain or by treatment modality or specific treatment, or both.

Sensitivity analysis

We planned to test sensitivity by successively removing:

- quasi-RCTs to leave only RCTs;
- cluster-RCTs to leave individually randomised trials;
- trials using non-ITT methods to leave only those analysed using ITT (to be considered ITT analysis, the analysis must have included all participants who entered treatment, whether or not they provided data at the end of treatment: Nuesch 2009 found that trials with ITT analyses produce smaller treatment effects in meta-analyses, and this difference is greater in meta-analyses in the presence of heterogeneity);
- unpublished trials to leave only studies in peer-reviewed journals. Some treatment studies in this literature are published

in non-peer-reviewed sources, such as chapters and internal reports of non-government organisations. This analysis would address concerns about differences in quality between the two types of source.

RESULTS

Description of studies

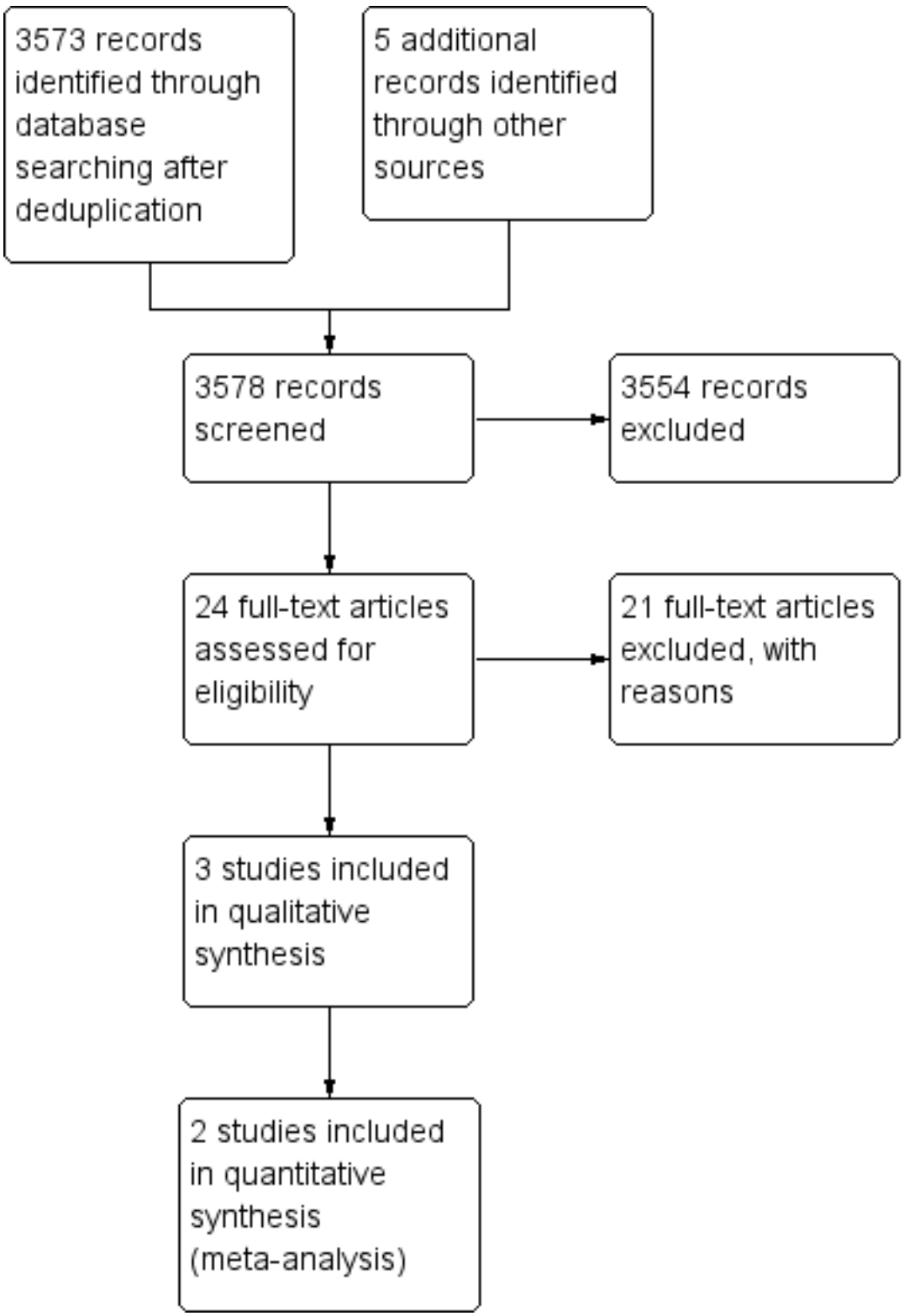
Results of the search

The search of the databases (see Methods, Electronic searches) retrieved 3573 records after deduplication. Our search of the trials registers identified five further studies. Our searches of other resources (grey literature, reviews, *Dignity* online library, contents of 10 most cited journals from electronic searches) identified no additional studies that appeared to meet the inclusion criteria. Our screening of the reference lists of the included publications did not reveal any additional RCTs. Therefore, we had a total of 3578 records.

We excluded 3554 records based on titles and abstracts. We obtained the full text of the remaining 24 records. We excluded 21 studies, with reasons (see Characteristics of excluded studies table). We found no trials to be entered under Characteristics of studies awaiting classification and identified one ongoing study (see Characteristics of ongoing studies table; Phaneth 2016).

We included three studies reported in eight published papers. For a further description of our screening process, see the study flow diagram (Figure 1).

Figure 1. Study flow diagram.



Included studies

Three studies met criteria for inclusion (see Characteristics of included studies table) (Kim 2015; Liedl 2011; Wang 2017). Two provided published data and Wang and coauthors kindly shared data not yet published.

Two studies were conducted in European refugee rehabilitation centres under the umbrella of the IRCT: in Kosovo (Wang 2017), and in Germany/Switzerland (Liedl 2011); the third study was conducted in a treatment centre in Korea (Kim 2015). A retraction was issued (for data mishandling and financial irregularities) in 2013 for Liedl 2011 that included the statement, "Data quality, data analyses, and clinical conclusions drawn from the results were not affected," so we retained the study. The studies had 100 participants at the start of treatment and 88 at the end; the losses were in two studies (Liedl 2011; Wang 2017). The participants in the Korean study were male torture survivors, with a mean age of 61 years (Kim 2015): in the other two studies, participants were predominantly male (57% Liedl 2011; 55% Wang 2017), with a majority of torture survivors (70% Liedl 2011; 80% Wang 2017), in their forties (mean ages 42 years Liedl 2011 and 48 years Wang 2017). In one study, the pain condition was exclusively chronic low back pain (Kim 2015), but in the two other studies the pain condition was described as chronic and was not further elaborated on, though Liedl 2011 excluded neuropathic pain. No study explicitly defined chronic pain.

One study had three arms, the two intervention arms being sufficiently similar to be combined for our analysis (Liedl 2011), while the others each had two. The intervention in two studies consisted of biofeedback-based cognitive behavioural therapy (CBT) (Liedl 2011; Wang 2017), delivered over 10 sessions of 15 hours in total; signals of muscle activity, breathing, or heart rate were used ('biofeedback') to monitor learning of relaxation and stress management techniques. Liedl 2011 used graduate clinical psychology students trained for the trial and working to a manual. The Liedl 2011 study added physiotherapist-designed instructions to exercise at home to one of the intervention groups, while the Wang 2017 study added group physiotherapy to the biofeedback-CBT intervention arm. The control group in both studies was a waiting list with no contact with therapists. The third study described a very different form of intervention, manual therapy designed to stretch and relax muscles and delivered by physiotherapists at least twice a week over eight weeks, with a control group who were provided with exercise instructions to be performed for the same total time as the manual therapy but without a therapist present (Kim 2015).

Therapists and participants spoke the same language in one study (Kim 2015), while interpreters and translation of assessment materials were used for Liedl 2011. Interpreted materials were used by Wang 2017: assessment was by someone who spoke the same language as the participants and it was implied that all therapists spoke the same language. Some details of method and of scoring results were missing from Kim 2015. Attempts to contact the study authors by email and post for clarification were unsuccessful.

Excluded studies

We excluded 16 studies: 10 were not RCTs (Blyhammar 2009; Callaghan 1993; Farrag 2005; Highfield 2012; Hinton 2006; Jansen 2011; Kaysen 2013; Müller 2009 RETRACTED; Phaneth 2014; Schwarz-Langer 2006), and six did not report the outcome pain relief (Adenauer 2011; Bolton 2014a; Bolton 2014b; Johnson 2001; Taing 2011; Weiss 2015).

Three of the non-RCTs provided psychoeducation about pain, two for Arabic-speaking refugees in Swedish treatment centres (Blyhammar 2009; Jansen 2011), and one in Cambodia (Phaneth 2014). Three provided some combination of CBT, biofeedback, and relaxation (Hinton 2006; Müller 2009 RETRACTED; Schwarz-Langer 2006). One study treated Asian refugees in the US with Chinese traditional medicine, predominantly acupuncture (Highfield 2012).

Ongoing studies

We identified one ongoing study, which was a two-arm study comparing pain school (education) with a waiting list control for torture survivors in Cambodia (Phaneth 2016). The outcomes are pain relief and reduction in disability.

Risk of bias in included studies

We used standard Cochrane methods for assessing risk of bias (see Figure 2; Figure 3).

Figure 2. Risk of bias graph: review authors' judgements about each risk of bias item presented as percentages across all included studies.

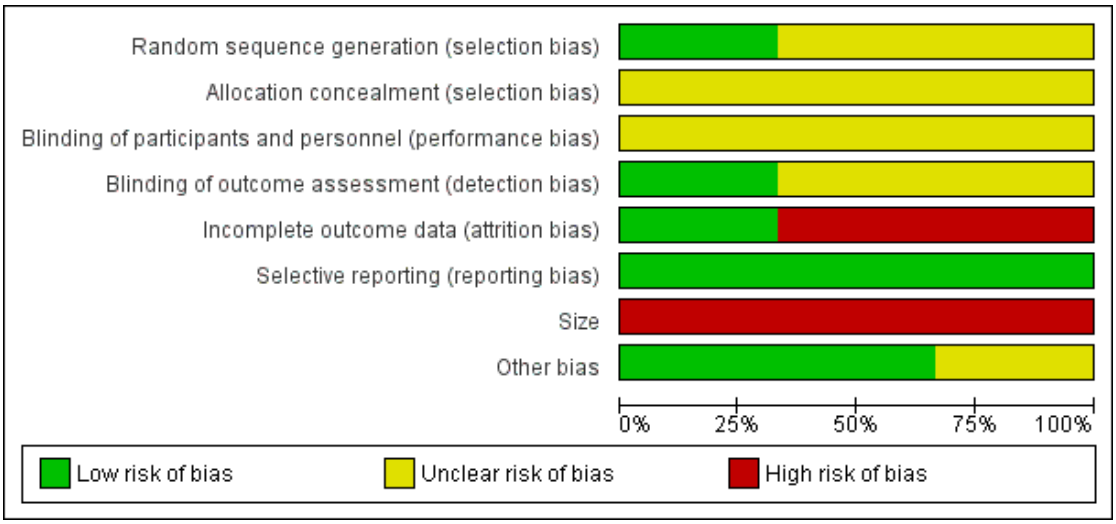
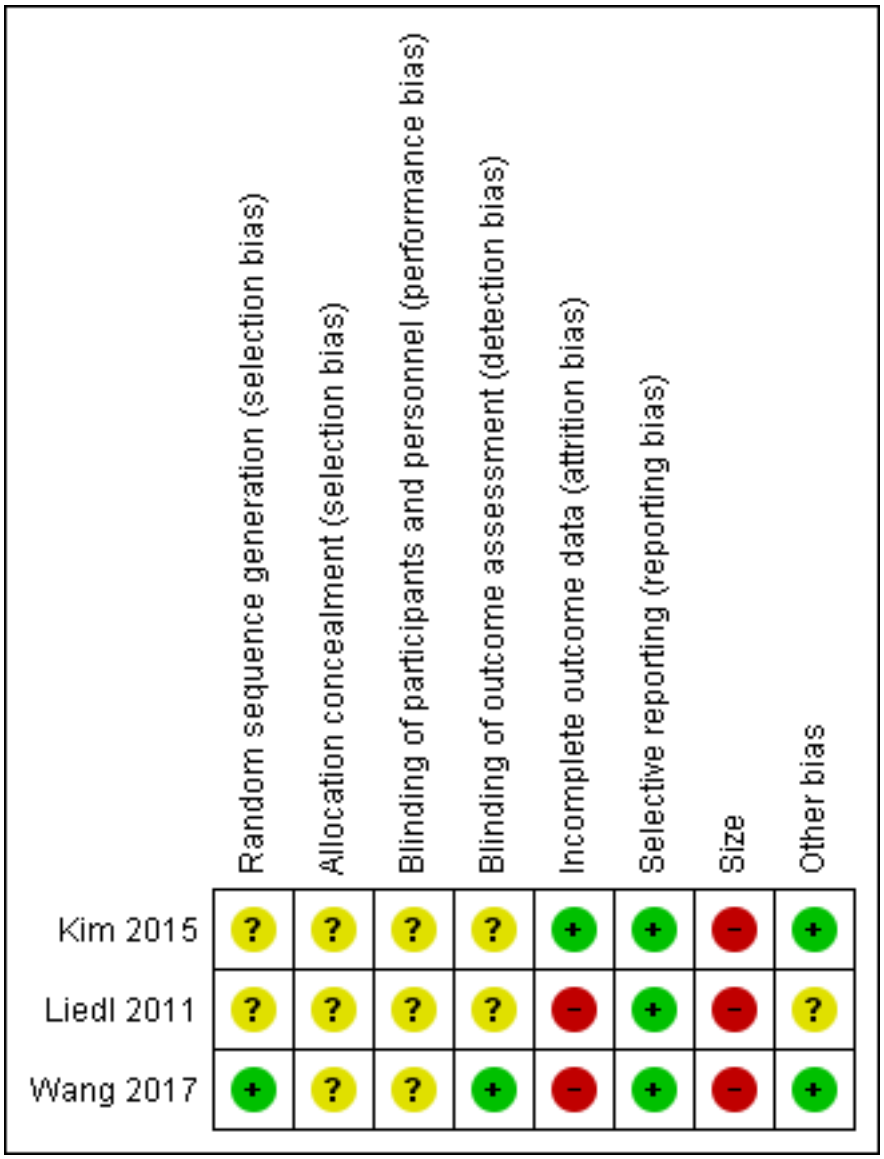


Figure 3. Risk of bias summary: review authors' judgements about each risk of bias item for each included study.



Allocation

One study used a block randomisation method that was rated low risk (Wang 2017). We rated the two other studies as unclear risk: Kim 2015 described a coin toss method used for randomisation but did not explain how this produced equal group sizes, and Liedl 2011 provided no details on randomisation method or information on allocation concealment.

Blinding

Performance bias

There was no information in any study about attempts to blind participants to allocation or to assess their expectation of benefit or post-treatment guess at allocation group. In the Kim 2015 study, there was a self-exercise regimen for the control group which might have seemed like an intervention. We would expect that the waiting list control groups in Liedl 2011 and Wang 2017 would have realised that they were not receiving an intervention. Therefore, we rated all three studies at unclear risk of bias.

Detection bias

Since Wang 2017 used an independent psychiatrist speaking the language of participants and blind to which arm participants were in to assess outcomes, it was rated at low risk of bias. Liedl 2011 and Kim 2015 reported assessment of outcomes of interest by self-report and were rated at unclear risk of bias.

Incomplete outcome data

Kim 2015 reported no withdrawals and was at low risk of bias. Liedl 2011 reported 20% withdrawals and analysed only completers and was at high risk of bias. Wang 2017 analysed data from completers only (82% of participants randomised) but stated that this was an "intent-to-treat" analysis. Dropouts from this study were reported but reasons were not stated, despite a specific request to the main author. We rated this study at high risk of bias.

Selective reporting

Kim 2015 and Liedl 2011 reported all the outcome measures listed in their Methods sections, and we found all of the relevant outcomes listed in the Wang 2017 trial protocol, so we rated all three studies at low risk of bias.

Size of study

Numbers of participants ranged from 28 to 30 so all studies were at high risk of bias for size. In no study was the issue of statistical power raised.

Other potential sources of bias

Therapist qualification

We rated two studies at unclear risk since information provided, if any, was insufficient to ascertain therapist qualifications (Kim 2015; Liedl 2011). The third study provided details of therapist professional qualifications and experience so we rated it at low risk of bias (Wang 2017).

Therapist allegiance

We rated all studies at low risk since none of the comparison arms required any substantial therapist involvement.

Language of assessment

We rated two studies at low risk, since they assessed participants in their own language (Kim 2015; Wang 2017). The other study was at unclear risk of bias since at least some questionnaires were translated into several languages at the point of assessment and so were not standardised (Liedl 2011).

Effects of interventions

See: **Summary of findings for the main comparison** CBT with biofeedback ± physical exercise versus waiting list control for pain in torture survivors

For the reported outcomes, we reported SMDs (effect sizes) and 95% CIs, with P values unless they were not significant (ns).

Pain relief

We judged the quality of evidence for pain relief to be very low. At the end of treatment, two studies contributed data to pain relief as an outcome of CBT with biofeedback with or without exercise, compared to waiting list control (Liedl 2011; Wang 2017). There were 58 people included in this analysis, for which both intervention arms of Liedl 2011 were combined. The SMD was -0.05 (95% CI -1.23 to 1.12; z = 0.09, ns, I^2 = 79%). The manual therapy study with 30 participants provided an SMD of -0.48 (95% CI -9.95 to 0.35; z = 1.83, P = 0.07) (Kim 2015). Follow-up values were only available for Wang 2017: at three months, the SMD was -0.03 (95% CI -0.28 to 0.23).

We downgraded the quality of evidence by the maximum of three levels: for very small sample size, for imprecision since CIs included no effect, substantial risk and substantial benefit; most risk of bias was unclear due to lack of information; analyses were by completers, not ITT; and the dropout rate was unexplained.

Adverse effects, including dropout or attrition

None of the studies reported adverse effects. One study retained participants to the end of treatment (Kim 2015), while Liedl 2011 lost 6/36 (17%) participants and Wang 2017 lost 6/34 (18%) participants during the study. Wang 2017 reported that one participant found a job, and others left because of illness or surgery; Liedl 2011 gave no reasons.

Reduced use of analgesics

None of the studies reported the levels of analgesic use, though Wang 2017 reported the effect of non-analgesic medications on other outcomes.

Reduction of disability

We judged the quality of evidence for reduction of disability to be very low. At the end of treatment, two studies measured disability using different scales (behavioural coping subscale for Liedl 2011; WHO disability scale for Wang 2017) at the end of treatment, with 57 participants in the analysis (Liedl 2011; Wang 2017). The SMD was -0.39 (95% CI -1.17 to 0.39; z = 0.97 ns, I^2 = 52%). The manual therapy study with 30 participants provided an SMD of -1.10 for a translated standard scale of disability (95% CI -1.88 to -0.33; z = 2.79; P = 0.005) (Kim 2015). None of the studies used either pain-specific scales or broader quality of life scales. Follow-up values were only available for Wang 2017: at three months, the SMD was 0 (95% CI -0.74 to 0.74; z = 0.00, ns).
We downgraded the quality of evidence by the maximum of three levels: for very small sample size, for imprecision since CIs included no effect, substantial risk and substantial benefit; most risk of bias was unclear due to lack of information; analyses were by completers, not ITT; and the dropout rate was unexplained.

Reduction of distress

We judged the quality of evidence for reduction of distress to be very low. All three studies assessed distress in terms of post-traumatic stress scales, with additional assessment of depression (Wang 2017), and anxiety (Liedl 2011; Wang 2017). End of treatment values for PTSD were available from two studies using CBT with biofeedback (Liedl 2011; Wang 2017). The SMD was 0.07 (95% CI -0.46 to 0.60; z = 0.25, ns, I^2 = 0%). The manual therapy study also yielded PTSD values (Kim 2015). The SMD was -1.26 (95% CI -2.06 to -0.47; z = 3.12; P = 0.002). Follow-up values were only available for Wang 2017: at three months, the SMD was -0.24 (95% CI -0.50 to 0.99; z = 0.64, ns).
We downgraded the quality of evidence by three levels because of serious methodological limitations: for very small sample size, for imprecision since CIs included no effect, substantial risk and substantial benefit; most risk of bias was unclear due to lack of information; analyses were by completers, not ITT; and the dropout rate was unexplained.

Global improvement, satisfaction, as rated by participant

None of the three studies used any assessment of global improvement.

DISCUSSION

Summary of main results

We found only three eligible studies and obtained data for all three, but were only able to combine data for the two similar interventions (biofeedback-based CBT with or without physiotherapist-led exercise for mixed chronic pain, compared to waiting list (Liedl 2011; Wang 2017); the third study compared manual therapy for low back pain with exercise at home (Kim 2015). None of the studies demonstrated that their interventions reduced pain, and only the manual therapy study claimed at the end of treatment to have reduced disability and distress.
There were no data on adverse effects, so possible harms were unknown; where reasons were given for attrition from treatment, they did not indicate harm. Outcomes of analgesic use, quality of life, and global satisfaction were also lacking.

Overall completeness and applicability of evidence

Pain is a defining feature of torture and studies of torture survivors set in specialised centres are consistent in reporting a high prevalence of persistent pain, with overall estimates as high as 83% (Olsen 2006; Williams 2010). Yet the research literature on rehabilitation for survivors of torture is predominantly targeted at mental health problems without specific reference to pain. Therefore, we were not surprised to find very few RCTs on the management of post-torture pain. As expected, most of the available studies were uncontrolled. The evidence obtained from the included RCTs was relevant to the research question, although lacking several outcomes and follow-up results, and overall at unclear to high risk of bias, providing evidence of very low quality for outcomes of pain relief, reduction in distress, and reduction in disability.

No studies used current evidence on multicomponent pain rehabilitation programmes for chronic pain (Williams 2012), neither did we find any analgesic trials. Interventions appeared to be informed by peripheral models of pain aetiology for which evidence was poor. The enthusiasm for biofeedback is intriguing, since the muscle tension model of chronic pain used in these trials has long been disputed, with recognition that any benefits from relaxation and biofeedback treatment arise from cognitive change, such as gaining a sense of control (Jensen 2014); further, dependence on biofeedback equipment to achieve relaxation can produce poor maintenance over the longer term after the equipment is withdrawn (Newton-John 1995).

None of the studies provided evidence that either CBT with biofeedback or manual therapy produced pain relief or reduction of disability or distress for survivors of torture with chronic pain. Pain treatment is ideally integrated into multidisciplinary psychosocial rehabilitation in services for torture survivors (Jaranson 2011). There was a wider range of interventions in non-RCTs, and many excluded studies tested similar psychological interventions to those included, but with psychological rather than pain outcomes. In the wider literature on rehabilitation of survivors of torture, post-torture pain is often classified as psychosomatic, reflecting a poor understanding of chronic pain and resulting in failure to address potentially treatable pain.

The few trials found selected a narrow range of outcomes, with none reporting use of analgesics or ratings of global improvement, and neglect of adverse effects. Reporting on attrition, including reasons for dropout, was inadequate, leaving unanswered questions about acceptability of and adherence to treatment, and possible harm. Fundamental to the application of a given intervention is the requirement that it be well tolerated and compatible with the person's understanding of the illness, cultural background, and rehabilitation needs: it is not clear that these have been addressed in these studies. Pain after torture can have multiple meanings, and increased knowledge about torture survivors' preferences, perception of and satisfaction with their health outcomes is therefore important.

We had planned to analyse child and adult studies separately, since methods and outcomes usually differ, as does the type of torture experienced. However, our search identified no studies on child survivors of torture, leaving a substantial gap in the evidence.

Characteristics of participants given in the studies included gender, mean age, legal status (refugee or not), and, in two studies (Kim 2015; Wang 2017), nationality/ethnicity. The studies did not mention type of torture experienced, living situation, and separation from close family members.

In all studies, data were continuous, not dichotomous. Only Wang 2017 gave the funding source for their study.

Quality of the evidence

We identified three studies that met the criteria for inclusion, with 88 participants completing treatment. We judged the overall quality of evidence from these studies to be very low, meaning we have very little confidence in the effect estimate; the true effect is likely to be substantially different from the estimate of effect, for the following reasons.

We downgraded the quality of evidence by the maximum of three levels for the outcomes of pain relief and reduction in disability: for limitations in design, for inconsistency, and for imprecision; and by two levels for the outcome of reduction in distress, for limitations in design and for imprecision.

- Limitations in design: there was substantial unexplained loss of participants, and completer analysis, rather than ITT. These limitations are likely to lower confidence in the estimate of effect.
- Inconsistency: there was unexplained heterogeneity in the outcomes of pain relief and disability, despite very similar interventions.
- Imprecision of the estimate, with no or small and clinically irrelevant change and CIs that included no effect, substantial risk, and substantial benefit.

Potential biases in the review process

The extensive nature of our search, including grey literature, gives us confidence that eligible studies were not missed.

Agreements and disagreements with other studies or reviews

We found no systematic reviews covering the effectiveness of interventions for treating persistent pain in survivors of torture. In most of the literature on health care for torture survivors, the focus of clinical and research efforts has been on the psychological sequelae, often described in terms of PTSD or other trauma-related psychopathology, and not on pain as a significant cause of distress and disability. In accordance with our findings, one Cochrane Review revealed that, based on very low quality evidence, the effects of psychological, social, and welfare interventions for torture survivors are disappointing, producing changes, if any, which fall far short of recovery (Patel 2014). Several methodological issues and constraints were highlighted, including lack of theoretical framework for provided interventions, use of unstandardised assessment methods, and very small sample sizes.

There are several reasons why evidence in this field is limited. Rehabilitation of torture survivors was initiated and carried out mainly by health professionals working in human rights organisations and to date these services remain largely separate from mainstream healthcare provision. Many of these organisations face a constant struggle for resources, with staff under immense pressure to focus on what many perceive as the core task, providing treatment and care. Combining the skills of those who work with torture sur-

vivors with those of pain clinicians and researchers provides the best opportunity for building understanding and increasing the effectiveness of treatment and management interventions.

AUTHORS' CONCLUSIONS

Implications for practice

For torture survivors with chronic pain

We found no direct evidence for or against either cognitive behavioural therapy (CBT) with biofeedback or manual therapy, for achieving pain relief, reduced disability, or reduced distress. However, there are very few studies, and the quality of evidence they provide is very low.

For clinicians

We found no direct evidence for or against CBT with biofeedback or manual therapy, for achieving pain relief, reduced disability, or reduced distress in survivors of torture, as the interventions were delivered in the three included trials. As in other chronic pain conditions, any pain treatment should be based on thorough assessment and identification of pain mechanisms involved, including of neuropathic pain and sensitisation phenomena, and should aim not only to reduce pain but to improve function and quality of life. Assessment and treatment should, as is recognised best practice, involve a multidisciplinary team. The patient should be asked about possible countertherapeutic associations of particular treatments with torture methods, such as forcible medication or electrical interventions.

A human rights context, with reference to cultural difference in expressing pain and distress and seeking help, and with reference to the personal meaning of torture, is highly desirable as a basis for treatment initiatives.

For policy makers

The small number of randomised controlled trials and the resulting paucity of information means that no conclusions concerning the management of post-torture pain can be drawn. As recommended for chronic pain in general, an interdisciplinary, multimodal approach to pain management in survivors of torture is probably optimal, with a focus on agreed goals of improved understanding, functioning, and social participation. This should be applied sensitively to patients who may be seriously traumatised.

For funders of the intervention

Rehabilitation after torture is a human right, yet provision is scant even in well-resourced countries where refugees settle. It is important that best practice from pain treatment in general is extended to torture survivors, and that pain is not mistakenly assumed to be a symptom of post-traumatic stress and pain treatment neglected in favour of intervention for post-traumatic stress disorder. Funders can take a role in requiring robust assessment of the outcomes of interventions, and partnerships with academic teams where intervention teams do not have the necessary expertise.

Implications for research

General implications

The search yielded only three studies, and one more ongoing. All four used psychological and physical therapy methods: none used pharmacotherapy or other medical interventions for pain. This means that we know almost nothing about whether treatments for pain that are otherwise of known effectiveness can also reduce pain and pain-associated problems in survivors of torture. The impact of torture, flight, and exile are factors that may complicate behavioural and cognitive aspects of pain and disability, and undermine treatment feasibility, adherence, and outcome. Given the large number of refugees in high-income countries, healthcare services will be treating pain in survivors of torture, although not necessarily identified as such, but without any research literature to guide clinical decisions.

Our understanding of persistent pain from torture is seriously lacking, so we have little to inform development of research questions or interpretation of outcome study data. Despite contributions from forensic and accident medicine, little is known about the long-term effects of many forms of torture.

Careful studies of torture survivors are beginning to establish connections between some forms of torture and persistent pain, better described by mechanism than by site (Amris 2015), and this could be used to advance theory development and guide future studies addressing outcome of pain rehabilitation.

Design

Most studies of torture survivors are set in specialised non-governmental treatment centres in high-income countries, or in less well-resourced countries, and academic expertise may be lacking when it is needed to design worthwhile studies. Despite the difficulties of recruitment from what is often an unstable population (in terms of civil status, income, and accommodation), studies must be adequately powered. Ideally, treatment methods are drawn from those showing best outcomes in the general chronic pain population, and delivered by therapists qualified in those methods. The studies included in this review appeared weak in their understanding of

pain and of its effective treatment, so interventions fell short of what could be provided and tested.

In the short-term, careful observational studies of torture survivors in treatment for pain would help to formulate research hypotheses and questions to be addressed by controlled studies.

Measurement

Outcomes in the studies in this review were very narrow and did not consider harm, a significant risk in this population, particularly in participants with moderate to severe post-traumatic stress symptoms. However, conceptualising psychological distress, whether at baseline or after treatment, only in terms of PTSD diagnosis or caseness is inadequate for the range and extent of psychological problems in this population (Patel 2014; Quiroga 2005). Similarly, many torture survivors' lives are very constrained by lack of resources (money, language, social networks, and others) that both disability and quality of life scales may be difficult to answer in terms only of pain, and care should be taken to select those (perhaps from international bodies such as the WHO) with most appropriate content.

Interview assessment is inevitably unstandardised, and conducting it via an interpreter adds further unreliability. While questionnaires can be translated and checked by back-translation, this falls short of adequate development of a questionnaire incorporating cultural as well as linguistic equivalence: these methods are well described in Sousa 2011.

Given the impossibility of blinding participants or therapists to most psychologically based treatments, collecting ratings from participants of their expectation of benefit from their assigned treatment (or comparison condition) before it starts, and satisfaction when it ends, is a helpful substitute, particularly when the treatments may be drawn from psychological models and methods that are culturally unfamiliar to participants.

Best practice

The right to rehabilitation after torture is enshrined in international law, as is the right to asylum, but is not yet realised in any country. Apart from the human rights grounds, there are obvious humanitarian grounds for trying to provide best health care to torture survivors with the aim of restoring as far as possible their capacity to participate in their host country and chosen communities. There is no substitute for good quality treatment studies, using best clinical and scientific practice.

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* *Indicates the major publication for the study*

CHARACTERISTICS OF STUDIES

Characteristics of included studies *[ordered by study ID]*

Kim 2015

Methods	RCT: 2 arms: treatment, active control; single centre.
Participants	**Inclusion criteria**: male torture survivors with low back pain attending treatment centre **Exclusion criteria**: referred to but not specified. **Pain condition**: chronic low back pain. **Number of participants**: 30 at start of treatment, 15 per arm (no dropouts reported) **Mean (SD) age in years**: intervention: 59.2 (6.6); control: 62.6 (6.6). **Sex**: 30 men.
Interventions	**Experimental group**: complex manual therapy 24 × 90-minute (possibly 2 hour) sessions over 8 weeks. Requests for clarification were unanswered **Control group**: self-exercise for the back, using manual therapy (after education), 90-minute sessions, 3 × weekly for 8 weeks (36 hours) **Therapists**: not stated, but presumably physical therapists.
Outcomes	**Primary outcomes** • Pain intensity: Visual Analogue Scale (0-100). **Secondary outcomes** • PTSD: Korean PDS (17 items rated 0-3; maximum score 51: higher was worse). • Disability: KODI. Version used in study had 9 items rated 0-5; maximum score should therefore be 45 but in Table 1 values between 50 and 80 were given. Elsewhere, KODI was described as having 10 items. Version used here seems to be missing the question about sexual activity but no justification was given for this. Requests for clarification on the number of items and the scoring were unanswered. • Dynamic balance: Balance System Static and Dynamic (time on balance test: seconds) (details of test not given, despite request for clarification). **Time points for assessment**: baseline, end of treatment. **Language of assessment**: Korean.
Notes	Means and SDs available for all outcomes, pre- and post-treatment We approached study authors twice via published email address and twice by post to published addresses to clarify discrepancies in intervention descriptions but had no response **Study period**: unknown. **Country**: Republic of Korea. **Language of assessment:** Korean (language of participants and therapists). **Funding source**: not stated. **Declarations of interest among the primary researchers**: not stated.

Risk of bias

Bias	Authors' judgement	Support for judgement

Kim 2015 (*Continued*)

Random sequence generation (selection bias)	Unclear risk	Coin toss method used. This was unlikely to result in a 15:15 split. Achievement of parity not explained
Allocation concealment (selection bias)	Unclear risk	Not described.
Blinding of participants and personnel (performance bias) All outcomes	Unclear risk	Not described but difficult to achieve in circumstances.
Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	Not described.
Incomplete outcome data (attrition bias) All outcomes	Low risk	No withdrawals reported.
Selective reporting (reporting bias)	Low risk	All outcomes reported.
Size	High risk	< 50 participants per treatment arm.
Other bias	Low risk	No information on therapist qualifications. No information on therapist allegiance. Language of assessment Korean (language of participants and therapists)

Liedl 2011

Methods	RCT: 3 arms: enhanced treatment, treatment, waiting list; recruited from 2 centres or referred by workers in the field
Participants	**Inclusion criteria**: chronic (excluding neuropathic) pain; refugee status; trauma in home country (70% torture) **Exclusion criteria**: psychotic symptoms; substance-related symptoms; suicidal ideation; severe dissociative symptoms **Pain condition**: chronic (excluding neuropathic) pain. **Number of participants**: 36 at start of treatment (12 per arm); 30 at end of treatment (10 per arm) **Mean (SD) age in years**: 41.7 (10.0). **Sex**: 17 men; 13 women (completers).
Interventions	**Experimental groups**: CBT-BF-PE and CBT-BF. **CBT-BF**: manual-based; 10 × 90-minute sessions over 3 months. **CBT-BF-PE:** as above plus PE: physiotherapist-instructed, handbook-illustrated, 20 minutes daily, at home **Control group**: waiting list (treatment received after 4 months). **Therapists**: 4 graduate clinical psychology students specially trained in CBT-BF for such a client group and who had observed a professional CBT-BF therapist

Liedl 2011 (*Continued*)

Outcomes	**Primary outcomes** • Pain intensity: Verbal Rating Scale (0-6). • Adverse effects: not reported, but dropouts reported without explanation. **Secondary outcomes** • PTSD: PDS (17 items rated 0-3; maximum score 51: higher was worse). • Anxiety: HSCL-25, Anxiety subscale (10 items rated 1-4; mean > 1.75 = symptomatic). • Disability: FESV. Behavioural coping subscale (12 items: 0-7). Higher scores indicated less disability. **Other outcomes** • Physiological factors (heart rate; electromyography). **Time points for assessment**: baseline, end of treatment, 3-month follow-up. **Languages of assessment:** questionnaires translated into multiple languages of participants and completed with computer or spoken
Notes	Data available for all outcomes, pre- and post-treatment and follow-up **Study period**: recruitment 2007-2009. **Country**: Germany and Switzerland. **Language of assessment**: participants provided with interpreters for interview; questionnaires translated **Funding source**: not stated. **Declarations of interest among the primary researchers**: no conflicts of interest.

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Unclear risk	No details given of process.
Allocation concealment (selection bias)	Unclear risk	Not described.
Blinding of participants and personnel (performance bias) All outcomes	Unclear risk	Not described but difficult to achieve in the circumstances.
Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	Not described.
Incomplete outcome data (attrition bias) All outcomes	High risk	Completers only analysed (30/36 participants).
Selective reporting (reporting bias)	Low risk	All outcomes reported.
Size	High risk	< 50 participants per treatment arm.

Liedl 2011 (*Continued*)

Other bias	Unclear risk	Therapists were graduate students, not authors of study. Therapist allegiance not stated. Language of assessment: interpreted or translated so unstandardised

Wang 2017

Methods	RCT: 2 arms: treatment, waiting list.
Participants	**Inclusion criteria**: adults aged 18-65 years; experience of ≥ 1 of torture (80%), sexual violence, extralegal detention, witnessing killings of relatives **Exclusion criteria**: conditions impeding assessment; schizophrenia; substance abuse; cancer treatment; previous CBT **Pain condition**: any (multiple sites) (all randomised had pain). **Number of participants**: 34 at start of treatment (17 per arm); 28 at end of treatment (treatment: 13 analysed (12 only for disability assessed by WHO Disability Assessment Schedule-II; waiting list: 15 analysed) **Mean (SD) age in years**: 47.7 (SD not reported). **Sex**: 55% men; 45% women (not clear at which stage calculation made)
Interventions	**Experimental group**: CBT-BF-PE plus daily dose of multivitamins. 10 × 90-minute sessions of individual therapy (total 15 hours); 10 weekly 90-minute sessions of group PEs and transcutaneous electrical nerve stimulation (total 15 hours) **Control group**: waiting list (treatment received after 3 months) plus daily dose of multivitamins **Therapists**: doctor, physiotherapists, psychologists.
Outcomes	**Primary outcomes** • Pain intensity: Short-Form McGill Pain Questionnaire - Pain Rating Index (15 descriptors rated 0-3). • Adverse effects: not reported, but dropouts reported "mainly due to illness" and one found a job. **Secondary outcomes** • Depression: HSCL-25. • Anxiety: HSCL-25 (changes only reported). • PTSD: Harvard Trauma Questionnaire - Part IV (30 trauma symptoms, responses "Not at all," "A little," "Quite a bit," "Extremely," ranked 1-4). Higher score indicated greater traumatisation. • Disability: WHO Disability Assessment Schedule-II: 12 items, rated 1-5. Higher score indicates greater disability. **Other outcomes** • Margolis Pain Diagram (number of sites on body map). • Standing balance, right/left foot. • Grip strength, right/left hand. • Body mass index. **Time points for assessment**: baseline; 3 months (end of treatment); 6 months (follow-up)

	Language of assessment: Albanian; questionnaires part-translated into Albanian (and Serbian)
Notes	Data on means and SDs were supplied after email approach to main author. Data not available for all outcomes pre- and post-treatment, and follow-up, but outcomes of interest were supplied by study authors on request **Study period**: recruitment 2012. **Country**: Kosovo. **Language of assessment**: participants interviewed in their own language by experienced interviewers and senior clinical psychologist, all Kosovors **Funding source**: Novo Nordisk Research Foundation. **Declarations of interest among the primary researchers**: no conflicts of interest.

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Low risk	"Block randomisation procedure using a computerised random number generator by two blocks of size 17 created by a DIGNITY staff not involved in the trial."
Allocation concealment (selection bias)	Unclear risk	Not described.
Blinding of participants and personnel (performance bias) All outcomes	Unclear risk	Not described but difficult to achieve in the circumstances.
Blinding of outcome assessment (detection bias) All outcomes	Low risk	Assessment by Kosovo psychiatrist blind to allocation of interviewees
Incomplete outcome data (attrition bias) All outcomes	High risk	Completers only analysed (28/34) although described as intention to treat
Selective reporting (reporting bias)	Low risk	All relevant outcomes reported.
Size	High risk	< 50 participants per treatment arm.
Other bias	Low risk	Data and clarification of published data supplied on request by first author Therapist qualifications: qualified healthcare professionals Therapist allegiance: not stated. Language of assessment: participants interviewed in their own language by experienced interviewers and senior clinical psychologist, all Kosovors

BF: biofeedback; CBT: cognitive behavioural therapy; FESV: Fragebogen zur Erfassung der Schmerzverarbeitung (German Pain Coping Questionnaire); HSCL-25: Hopkins Symptoms Checklist-25 items; KODI: Korean Oswestry Disability Index; PDS: Post-traumatic Diagnostic Scale; PE: physical exercise; PTSD: post-traumatic stress disorder; RCT: randomised controlled trial; SD: standard deviation; WHO: World Health Organization.

Characteristics of excluded studies *[ordered by study ID]*

Study	Reason for exclusion
Adenauer 2011	No pain outcome.
Blyhammar 2009	Not RCT.
Bolton 2014a	No pain outcome.
Bolton 2014b	No pain outcome.
Callaghan 1993	Not RCT.
Defrin 2017	Not RCT.
Esala 2017	Not pain.
Farrag 2005	Not RCT.
Highfield 2012	Not RCT.
Hinton 2006	Not RCT.
Jansen 2011	Not RCT.
Johnson 2001	No pain outcome.
Jorgensen 2015	Not RCT.
Kaysen 2013	Not RCT; no pain outcome.
Morville 2015	Not RCT.
Müller 2009 RETRACTED	Not RCT.
Phaneth 2014	Not RCT.
Puvimanasinghe 2016	Not RCT.
Schwarz-Langer 2006	Not RCT.
Taing 2011	No pain outcome.

(*Continued*)

Weiss 2015	No pain outcome.

RCT: randomised controlled trial.

Characteristics of ongoing studies *[ordered by study ID]*

Phaneth 2016

Trial name or title	Randomized Controlled Trial on the Effect of DIGNITY Pain School Initiative
Methods	RCT: 2 arms: treatment, waiting list; single centre; pre-, post-treatment, 6 months' follow-up
Participants	120 Cambodian torture survivors (Khmer Rouge regimen) with chronic pain
Interventions	1 week group-based "pain school:" education programme: 10 sessions × 2 hours
Outcomes	Reduction in chronic pain (Brief Pain Index); improvement in pain-associated functioning (Disability Rating Index)
Starting date	Not stated.
Contact information	DIGNITY Danish Institute Against Torture, Bryggervangen 55, DK - 2100 Copenhagen, Denmark
Notes	Not published: information from Polatin. Pain school devised by DIGNITY. Pilot study Phaneth 2014.

DATA AND ANALYSES

Comparison 1. Cognitive behavioural therapy (CBT) versus waiting list control at end of treatment

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Pain relief	2	58	Std. Mean Difference (IV, Random, 95% CI)	-0.05 [-1.23, 1.12]
2 Reduction in disability	2	57	Std. Mean Difference (IV, Random, 95% CI)	-0.39 [-1.17, 0.39]
3 Reduction in distress	2	58	Std. Mean Difference (IV, Random, 95% CI)	0.07 [-0.46, 0.60]

Analysis 1.1. Comparison 1 Cognitive behavioural therapy (CBT) versus waiting list control at end of treatment, Outcome 1 Pain relief.

Review: Interventions for treating persistent pain in survivors of torture

Comparison: 1 Cognitive behavioural therapy (CBT) versus waiting list control at end of treatment

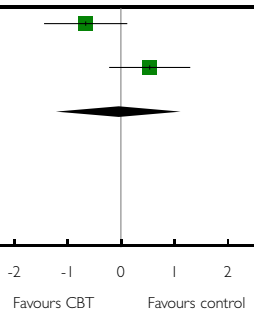
Outcome: 1 Pain relief

Study or subgroup	CBT N	Mean(SD)	Control N	Mean(SD)	Std. Mean Difference IV,Random,95% CI	Weight	Std. Mean Difference IV,Random,95% CI
Liedl 2011	20	3.9 (1.3)	10	4.7 (0.9)		49.7 %	-0.66 [-1.44, 0.12]
Wang 2017	13	1.1 (0.3)	15	0.9 (0.4)		50.3 %	0.54 [-0.22, 1.30]
Total (95% CI)	**33**		**25**			**100.0 %**	**-0.05 [-1.23, 1.12]**

Heterogeneity: $Tau^2 = 0.57$; $Chi^2 = 4.67$, df = 1 (P = 0.03); $I^2 = 79\%$

Test for overall effect: Z = 0.09 (P = 0.93)

Test for subgroup differences: Not applicable



Analysis 1.2. Comparison 1 Cognitive behavioural therapy (CBT) versus waiting list control at end of treatment, Outcome 2 Reduction in disability.

Review: Interventions for treating persistent pain in survivors of torture

Comparison: 1 Cognitive behavioural therapy (CBT) versus waiting list control at end of treatment

Outcome: 2 Reduction in disability

Study or subgroup	CBT N	Mean(SD)	Control N	Mean(SD)	Std. Mean Difference IV,Random,95% CI	Weight	Std. Mean Difference IV,Random,95% CI
Liedl 2011	20	32.3 (15.5)	10	43.8 (10.4)		49.0 %	-0.80 [-1.58, -0.01]
Wang 2017	12	1.6 (0.7)	15	1.6 (0.7)		51.0 %	0.0 [-0.76, 0.76]
Total (95% CI)	**32**		**25**			**100.0 %**	**-0.39 [-1.17, 0.39]**

Heterogeneity: $Tau^2 = 0.16$; $Chi^2 = 2.03$, df = 1 (P = 0.15); $I^2 = 51\%$

Test for overall effect: Z = 0.98 (P = 0.33)

Test for subgroup differences: Not applicable

Analysis 1.3. Comparison 1 Cognitive behavioural therapy (CBT) versus waiting list control at end of treatment, Outcome 3 Reduction in distress.

Review: Interventions for treating persistent pain in survivors of torture

Comparison: 1 Cognitive behavioural therapy (CBT) versus waiting list control at end of treatment

Outcome: 3 Reduction in distress

Study or subgroup	CBT N	Mean(SD)	Control N	Mean(SD)	Std. Mean Difference IV,Random,95% CI	Weight	Std. Mean Difference IV,Random,95% CI
Liedl 2011	20	25.3 (13.1)	10	26.8 (13.1)		49.1 %	-0.11 [-0.87, 0.65]
Wang 2017	13	2.4 (0.4)	15	2.3 (0.4)		50.9 %	0.24 [-0.50, 0.99]
Total (95% CI)	**33**		**25**			**100.0 %**	**0.07 [-0.46, 0.60]**

Heterogeneity: $Tau^2 = 0.0$; $Chi^2 = 0.43$, df = 1 (P = 0.51); $I^2 = 0.0\%$

Test for overall effect: Z = 0.25 (P = 0.80)

Test for subgroup differences: Not applicable

APPENDICES

Appendix 1. GRADE assessment

The GRADE approach uses five considerations (study limitations, consistency of effect, imprecision, indirectness, and publication bias) to assess the quality of the body of evidence for each outcome. The GRADE system uses the following criteria for assigning grade of evidence:

- high: we are very confident that the true effect lies close to that of the estimate of the effect;
- moderate: we are moderately confident in the effect estimate; the true effect is likely to be close to the estimate of effect, but there is a possibility that it is substantially different;
- low: our confidence in the effect estimate is limited; the true effect may be substantially different from the estimate of the effect;
- very low: we have very little confidence in the effect estimate; the true effect is likely to be substantially different from the estimate of effect.

We decreased grade if we identified:

- serious (-1) or very serious (-2) limitation to study quality;
- important inconsistency (-1);
- some (-1) or major (-2) uncertainty about directness;
- imprecise or sparse data (- 1);
- high probability of reporting bias (-1).

Appendix 2. Search strategies

CENTRAL (CRSO)

#1 MESH DESCRIPTOR Torture
#2 torture*:TI,AB,KY
#3 #1 OR #2
#4 victim*:TI,AB,KY
#5 MESH DESCRIPTOR Survivors
#6 survivor*:TI,AB,KY
#7 survive*:TI,AB,KY
#8 #4 OR #5 OR #6 OR #7
#9 MESH DESCRIPTOR pain EXPLODE ALL TREES
#10 MESH DESCRIPTOR Stress Disorders, Post-Traumatic EXPLODE ALL TREES
#11 pain*:TI,AB,KY
#12 MESH DESCRIPTOR chronic pain EXPLODE ALL TREES
#13 MESH DESCRIPTOR Pain, Intractable EXPLODE ALL TREES
#14 (((chronic or persist*) adj2 pain)):TI,AB,KY
#15 #9 OR #10 OR #11 OR #12 OR #13 OR #14
#16 #3 OR #8
#17 #15 AND #16

MEDLINE and MEDLINE in Process (Ovid)

1 Torture/
2 torture*.tw.
3 1 or 2
4 victim*.tw.
5 Survivors/
6 survivor*.tw.
7 survive*.tw.
8 or/4-7
9 exp Pain/ or Stress Disorders, Post-Traumatic/
10 pain*.tw.
11 exp chronic pain/ or exp intractable pain/
12 ((chronic or persist*) adj2 pain).tw.
13 or/9-12
14 3 and (8 or 13)

Embase (Ovid)

1 Torture/
2 torture*.tw.
3 1 or 2
4 victim*.tw.
5 Survivors/
6 survivor*.tw.
7 survive*.tw.
8 or/4-7
9 exp Pain/ or Stress Disorders, Post-Traumatic/
10 pain*.tw.
11 exp chronic pain/ or exp intractable pain/
12 ((chronic or persist*) adj2 pain).tw.
13 or/9-12
14 3 and (8 or 13)
15 limit 14 to embase

PsycINFO (Ovid)

1 Torture/
2 torture*.tw.
3 1 or 2
4 victim*.tw.
5 Survivors/
6 survivor*.tw.
7 survive*.tw.
8 or/4-7
9 exp Pain/ or Stress Disorders, Post-Traumatic/
10 pain*.tw.
11 exp chronic pain/ or exp intractable pain/
12 ((chronic or persist*) adj2 pain).tw.
13 or/9-12
14 3 and (8 or 13)

CINAHL (EBSCO)

S15 S3 AND S14
S14 S7 OR S13
S13 S8 OR S9 OR S10 OR S11 OR S12
S12 ((chronic or persist*) N2 pain)
S11 (MH "Chronic Pain")
S10 pain*
S9 (MH "Stress Disorders, Post-Traumatic+")
S8 (MH "Pain+")
S7 S4 OR S5 OR S6
S6 survivor* or survive*
S5 (MH "Torture Survivors")
S4 victim*
S3 S1 OR S2
S2 torture*
S1 (MH "Torture")

ISI Web of Science

# 7 #6 AND #1
Indexes=SCI-EXPANDED, SSCI, A&HCI, CPCI-S, CPCI-SSH, CCR-EXPANDED, IC Timespan=All years
# 6 #5 OR #4
Indexes=SCI-EXPANDED, SSCI, A&HCI, CPCI-S, CPCI-SSH, CCR-EXPANDED, IC Timespan=All years
# 5 TOPIC: (pain*)
Indexes=SCI-EXPANDED, SSCI, A&HCI, CPCI-S, CPCI-SSH, CCR-EXPANDED, IC Timespan=All years
# 4 #3 OR #2
Indexes=SCI-EXPANDED, SSCI, A&HCI, CPCI-S, CPCI-SSH, CCR-EXPANDED, IC Timespan=All years
# 3 TOPIC: (victim*)
Indexes=SCI-EXPANDED, SSCI, A&HCI, CPCI-S, CPCI-SSH, CCR-EXPANDED, IC Timespan=All years
# 2 TOPIC: (survivor* or survive*)
Indexes=SCI-EXPANDED, SSCI, A&HCI, CPCI-S, CPCI-SSH, CCR-EXPANDED, IC Timespan=All years
# 1 TOPIC: (torture*)
Indexes=SCI-EXPANDED, SSCI, A&HCI, CPCI-S, CPCI-SSH, CCR-EXPANDED, IC Timespan=All years

LILACS (Birme)

torture$ [Words] and (victim$ or survivor$ or survive$) or (pain$) [Words]

CONTRIBUTIONS OF AUTHORS

EB co-ordinated writing the protocol.

AW and EB screened titles and abstracts.

EB and LH read full papers and extracted data.

AW, KA, and LH contributed to writing the review.

DECLARATIONS OF INTEREST

EB: none known; EB is a specialist physician who has in the past provided medical treatment to survivors of torture.

AW: none known; AW is a clinical psychologist specialised in pain who has treated survivors of torture with chronic pain, and conducted research on pain from torture.

LH: none known.

KA: none known; KA is a certified specialist in rheumatology who has treated survivors of torture with chronic pain, and conducted research on pain from torture.

DIFFERENCES BETWEEN PROTOCOL AND REVIEW

We added several additional risk of bias items, collectively referred to as 'Other': therapist qualification, therapist allegiance, and language of assessment. We also added assessment of risk of bias from methods of blinding of participants and personnel, and selective outcome reporting.

We did not attempt transformations of skewed data as planned.