Cover sheet

Title

Cognitive behavioural therapy for men who physically abuse their female partner

Reviewers

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Dates

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This protocol is co-registered within the Cochrane Developmental, Psychosocial and Learning Problems Group.

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Contribution of reviewers

Therese Dalsbø and Geir Smedslund wrote the protocol. The search strategy was developed by Torill Johme.

Literature screening, review of potential trials, data extraction and data entry will be done independently by all reviewers. Analyses will be performed by Dalsbø and Smedslund. The text of the completed systematic review will be written by Dalsbø and Smedslund. Johme and Asbjørn Steiro will contribute by giving comments on the text of the review, assess studies and act as mediators if necessary. Responsibility for updating the review will be shared between Dalsbø and Johme.

Internal sources of support

Norwegian Health Services Research Centre, NORWAY

External sources of support

None

What's new

Since first publication, a change has been made in the Methods section to conform to comments of the Campbell Collaboration Methods Group.

Text of review

Background

Violent behaviour constitutes a serious problem in societies worldwide. Intimate partner abuse is especially problematic because it takes place in the private family sphere, making it a difficult arena for intervention and help. The physical abuse of women by their male partner is a serious concern because "it affects a distressingly high percentage of the population and it results in physical, psychological, social, and economic consequences" (CDCP 2003). The World Health Organisation (WHO) reported that "the overwhelming health burden of partner violence is borne by women at the hands of men" (WHO 2002). The WHO also provided evidence about the extent of the problem: in national surveys between 10 % and 34 % of the women reported being physically assaulted by an intimate male partner (WHO 2002).

Domestic violence occurs in the family and takes many different forms, including sexual, psychological, emotional and physical abuse. In this review the focus is solely on physical abuse. Domestic violence can occur between spouses/partners and between adults and children in the family. This review focuses only on partnership abuse, and specifically on men who physically abuse their female partner or ex-partner. The term domestic violence is therefore too broad to give meaning for this review, and more useful terms are physical abuse, battering, and intimate partner abuse. Another important limitation is that this review does not focus on the causality for violent behaviour. Several biological, psychological and sociological studies have attempted to find the one answer to what causes men to commit violent actions. In general it is now more focus on the correlation of different behavioural variables leading to violence. Therefore this review sets out to include more outcome variables than strictly physical violence, for example self-esteem, substance use and emotional problems.

One of the most frequently used treatment programs for physically abusive men is a psychological intervention called cognitive behavioural therapy (CBT). Participants either enrol voluntarily or are obliged to participate in CBT by means of a court order. CBT not only seeks to change behaviour using established behavioural strategies, but also targets the thinking patterns and beliefs that are thought to contribute to violence. CBT is "designed to help the patient test certain maladaptive cognitions and assumptions" (Beck 1979). CBT techniques aim to identify thoughts and beliefs that precede violent behaviour, challenging the patterns that violent men use to justify

their violence after the event. The goal is to bring about changes in the way that physically abusive men think about violence and the circumstances that lead to violence, thereby interrupting the chain of events that lead to physical abuse. The CBT can be given in individual, couple or group format.

An American review of state and provincial programs for intervening in spouse abuse cases reported simply "the jury remains out on the effectiveness of these programs" (Arias 2002). When spouse abusers are sent on programs, it is important to know the positive or negative effect it has. If a programme does not work or has adverse outcomes, we risk putting women in danger of future abuse.

The scope of this review is the effectiveness of cognitive behavioural therapy delivered to men engaged in physical abuse, against their female partner. A previous review of cognitive therapy (Butler 2000) for violent offenders did not include physically violent spouse abusers, but concluded that the therapy had a beneficial effect for those with problems such as marital distress and anger. To date there has been no systematic review of the effects of CBT for men who are physically violent toward their partners. The results from this review will be of importance for perpetrators and victims of this form of violence, and those who seek this form of treatment for the problem, and also for therapists, researchers, the judiciary, and the general public.

Objectives

To measure the efficacy of cognitive behavioural therapy (CBT) in ending men's physical abuse of their female partners.

Criteria for considering studies for this review

Types of studies

Randomised controlled trials (including cluster randomised controlled trials) and quasi-randomised controlled trials will be included in this review. The control group can consist of persons who receive no intervention, other interventions, or are on a waiting list.

Types of participants

Men who physically abuse their female partner/spouse/wife (whether current or former). Primary studies where the focus is on women who abuse their partner/spouse are excluded from this review. In the event of trials having a mixed population of men who have been violent to women and those who have been violent to men, we will request data separately from the trial investigators. Trials in which the participants attended the treatment program voluntarily or were sentenced/ mandated to participate are included.

Types of interventions

Interventions stated by the authors to be cognitive behavioural or recognisably so from contact with the authors and/or acquisition of a manual or other materials. A cognitive behavioural intervention typically aims to educate patients about the inter-relations among how they think, how they feel, and how they act. Interventions typically seek to changing behaviour either by changing specific cognitions, e.g. changing negative automatic thoughts) or via altering the antecedents and consequents of the behaviours. Programs may be individual, couple or group based and delivered in any setting.

Types of outcome measures

The primary outcome measure is physically violent behaviour, including verbal aggression, and other aggressive behaviour, perpetrated by the male participants in the primary studies. Other, secondary outcome measures that will be included are: improved self-esteem, reduced substance abuse and managing anger. Regarding self-esteem, substance abuse, and emotional distress these will be recorded for both perpetrators and victims where possible. Follow-up times will be recorded at post-treatment, short follow-up time (0-6 months), intermediate 7-18 months, and long-term (19 months and beyond). Any formats for measuring the outcome are included but will be separately reported (e.g. self reports, victim reports, judicial and police reports). Both standardized and non-standardized measures will be included.

Search strategy for identification of studies

We will search the Cochrane Controlled Trials Register (CENTRAL) on the Cochrane Library, MEDLINE, C2-SPECTR, Dissertation Abstracts, EMBASE, CINAHL, PsycINFO, ERIC, Care Data, Sociological Abstracts, Criminal Justice Abstracts, Bibliography of Nordic Criminology, and SIGLE.

This is the search strategy that will be used to search MEDLINE. The search strategy will be modified as required across databases. For a Cochrane review the search strategy shown should be the one that is used to search either CENTRAL or MEDLINE.

Battered Women/ OR
Domestic violence/ or spouse abuse/OR
(abuse$ adj3 (wom?n or partner$ or spouse$ or female$ or wife or wives)).tw OR
(batter$ adj3 ((wom?n or partner$ or spouse$ or female$ or wife or wives)).tw OR
(violen$ adj3 (partner$ or spous$ or family or families or domestic)).tw OR
AND
Behavior therapy/ or cognitive therapy OR
(cognitive$ adj3 (therap$ or train$)).tw OR
(behavio?r$ adj3 (therap$ or train$)).tw OR
(behavio?r$ adj3 modif$).tw OR
(family adj3 therap$).tw
Santé mentale au Québec, an online scientific journal, will be hand searched online from 1976 to the last number available.
The reviewers will contact field experts and the authors of retrieved studies in order to possibly find more studies.

Conference proceedings will be searched, in order to minimise the threat of publication bias.
Reference lists in included studies will be searched for relevant literature.
Studies will be included regardless of language and country of origin.

Methods of the review

Selection of studies
Selection of primary studies will be based on the inclusion criteria described above. The selection of studies for inclusion will follow a 'three-level' process. The original Reference Manager database of search results will be transferred to SRS (software for electronic screening and data abstraction) (SRS 2005). All reviewers will contribute to the process of screening. At the first screening level, any citation chosen by any reviewer will be added to a new database (the second level). Thereafter, two reviewers working independently have to approve a citation for it to be forwarded to Level 3 (and ordered in full text). . If two authors disagree, a third author will mediate, and the decision on whether to include or not will be reached through consensus or referred to editors. Data from all relevant trials at Level 3 will be extracted.

Data extraction and management
Two reviewers will independently extract data from the included studies using an online data extraction form. Any disagreement between two reviewers will generate a conflict in SRS, which will be solved through a discussion. If disagreement persists, a third reviewer will be consulted. Relevant information will be entered into the Table of Included Studies. Results of all analyses will be presented in RevMan. . Any study that initially appears to meet the inclusion criteria, but later on is excluded after extraction of the data will be described in the Table of Excluded studies. Missing data will be sought from primary investigators. The following data from the included studies will be extracted:

- Study characteristics: Country where the study was conducted, year of publication, publication type;
- Participants: age, socio-economic status, ethnicity, previous history of violent behaviour and treatment for it, current substance abuse, additional problems/disorders, marital status, and whether currently living with partner/not living with partner;
- Intervention: content, duration/time, profession of person delivering the programme (or intervention), gender and number of therapist(s)/group leader(s), support for women, the degree of mandatory delivery, attrition, adherence, type of comparison group;
- Type of outcome measure: physical violence, aggression, self-esteem, substance abuse and managing anger;
- Source of outcome data: official statistics; self-reports, partner report, or other forms for gathering outcome data;
- Length of follow-up time;
- Outcome measures: standardised or non-standardised measures and/or raw data.

Quality assessments of included studies
Because variation in validity can impact on variation in the study results (Higgins

2005) we will assess internal validity of included studies in the following way. Two reviewers will independently assess each selected study against quality categories described below (as relevant dependent on whether investigators describe their own studies as randomised, or not; as this review will contain both types of study not all quality criteria will apply to all studies).

Uncertainty or disagreement will be solved by discussion with a third reviewer. The reviewers will not be blinded to the authors or other information about the publication when assessing study validity. If information about study quality, or other information about the study, is missing, Dalsbø will contact the author(s) of the study, to minimise the danger of measuring the quality of the reporting, rather than of the study. Our aim is to get an overall assessment of internal validity based on a summary of the following seven methodological criteria on all included studies:

Prevention of selection Bias (generation of allocation sequence)
MET = Resulting sequences are unpredictable (explicitly stated use of either computer-generated random numbers, table of random numbers, drawing lots or envelopes, coin tossing, shuffling cards, or throwing dice
UNCLEAR = Statement that the study was randomised but not describing the generation of the allocation sequence.
NOT MET = Explicit description of inadequate generation of sequence, e. g. (e.g., using case record numbers, alternation, date of admission, date of birth). Care will be taken to record details as to whether in certain cases, such sequences may be 'functionally random'.

Concealment of allocation sequence
MET = Neither participants nor investigators can foresee assignment (e.g. central randomisation performed at a site remote from trial location; or, use of sequentially numbered, sealed, opaque envelopes).
UNCLEAR = Statement that the study was randomised but not describing the concealment of allocation.
NOT MET = Explicit statement that allocation was not concealed OR statement indicating that participants and investigators can foresee upcoming assignment (e. g., open allocation schedule, unsealed or non-opaque envelopes).

Prevention of performance bias
MET = Interventions other than cognitive behavioural programmes avoided or controlled for across comparison groups.
UNCLEAR = Use of interventions other than cognitive behavioural programmes not reported and cannot be verified by contacting the investigators.
NOT MET = Dissimilar or similar use of interventions other than cognitive behavioural programmes across comparison groups, i.e. differences in the care provided to the participants in the comparison groups other than the intervention under investigation.

Prevention of detection bias
MET = Assessor unaware of the assigned treatment when collecting outcome measures
UNCLEAR = "Blinding" of assessor not reported and cannot be verified by contacting investigators.

NOT MET = Assessor aware of the assigned treatment when collecting outcome measures.

Prevention of attrition bias
MET = Losses to follow up less than 20% and equally distributed (as judged by two reviewers) between comparison groups (e.g. 18% and 18%).
UNCLEAR = Losses to follow up not reported.
NOT MET = Losses to follow up 20% or greater, or not equally distributed (as judged by two reviewers) between comparison groups.

Intention-to-treat
MET = Intention to treat analysis performed or possible with data provided.
UNCLEAR = Intention to treat not reported, and cannot be verified by contacting the investigators.
NOT MET = Intention to treat analyses not done and not possible for reviewers to calculate independently.
The impact of quality will be explored in sensitivity analyses where appropriate.

Data analysis and presentation
We will express binary outcome measures (e.g., violent/ not violent) as risk ratios (relative risks) and number needed to treat (NNT).
Continuous measures will be calculated as weighted mean differences or (when different scales are used) standardised mean differences. We will report the 95% confidence intervals for all of the above.

Concerning cluster-randomised trials, in which the unit of allocation comprises for example prisons or geographical areas), rather than individual perpetrators of violence against women themselves, are randomised to different arms. In such studies, care should be taken to avoid unit of analysis errors. If there for instance are a total of 100 perpetrators in a study comprised of 25 individuals from one of four jurisdictions, and two jurisdictions are randomised to receive the intervention and the other two are randomised to receive the control condition, the correct N to use in the analysis is four, not 100. However, this greatly reduces the power of the analysis. The total variance in the outcome can be partitioned into variance between groups (VBG) and variance within groups (VWG). The intracluster correlation (ICC) is calculated as VBG/(VBG+VWG). But the ICC is seldom reported in the primary studies. The number of perpetrators can be used in the analyses if the ICC is used as a correcting factor.

In some primary studies, several different outcomes may be measured on the same participants. Sometimes the same outcome will be measured at multiple points in time. Because these data are from the same sample of participants, and, therefore, are not independent estimates of treatment effect, we will analyse the data in such a way that each outcome at each follow-up interval will be analysed separately (e.g., post-treatment, medium-term and long-term follow-up. For cluster randomised controlled trials the intra-class correlation will be taken into account if possible.

Heterogeneity and sensitivity analysis
The consistency of results will be assessed using the I^2 statistic (Higgins 2002) in Review Manager 4.2. If there is a substantial heterogeneity (which we will define as

(a) a statistically significant homogeneity test coupled with (b) an I^2 value of 25% or greater among primary outcome studies) the following factors will be considered as possible explanations: design quality, publication bias, voluntary or mandatory participation, intensity or length/period of the intervention, and differences in participant characteristics such as multiple problems/disorders. If there are many primary studies we will subgroup them according to these variables, and perform a moderator analysis (meta-analysis analogue toANOVA, or meta-regression) in order to identify whether these possible sources of heterogeneity appear to be important. If the primary studies are judged to be substantially heterogeneous even within these subgroupings, only a descriptive analysis will be performed, particularly if there is variation in direction of effect.

Assessment of bias
A funnel plot will be used where possible to determine the likelihood of bias. Asymmetry of the funnel plot may indicate possible publication bias in this review, but also may indicate other sources of bias such as those attributable to methodological or sample size issues within the trials. If asymmetry of the funnel plot is found, the clinical diversity of the studies will be examined (Egger 1997).

Sensitivity analysis
Impact of differing methodological quality will be assessed by sensitivity analyses.

Acknowledgements

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Potential conflict of interest

None known.

Other references

Additional references

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Notes

Unpublished CRG notes

Exported from Review Manager 4.3 Beta

Published notes

This review is co-registered within the Cochrane Collaboration.

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