# Educational and Skills-based Interventions for Preventing Relationship and Dating Violence in Adolescents and Young Adults

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# PROTOCOL

THE CAMPBELL COLLABORATION

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# 1 Background

## 1.1 DESCRIPTION OF THE CONDITION

Intimate partner violence is a form of interpersonal violence that occurs within relationships. It includes domestic violence and partner or relationship violence. Relationship violence comprises a range of violent behaviours, from verbal abuse to physical and sexual assault, and from threats to rape and homicide. Physical, sexual and verbal violence can be common responses to conflict within relationships and can have significant effects upon the mental, physical and social well-being of those involved.

Although much intimate partner violence is unreported, it is estimated that its prevalence within the adult population is quite high. A review of international studies surmised that prevalence rates vary between countries. Between 3% and 52% of women report having experienced some form of relationship violence at some point in the previous year (Heise 1999), and 10% to 50% of women report having experienced violence from partners or ex-partners at some point in their lives (Watts 2002).

Rates of relationship abuse vary according to age, sex and previous experience of violence (Foshee 1996; Foshee 1998; Archer 2000). The prevalence of relationship violence is higher in adolescents than in adults, with females aged 12 to 18 years having the highest victimisation rate (Home Office 1999; Wolfe 2003). This form of violence is called dating violence and perpetrators are most likely to be peers (Schewe 2006). It is estimated that approximately 20% of young women have experienced violence from a dating partner (O'Keeffe 1986; Bergman 1992). Additionally, studies on relationship violence have found that first episodes of violence frequently occur in adolescence (Henton 1983). In younger dating samples, relatively higher proportions of aggression by women against men has been described, although results vary according to the measurement methods used and must therefore be interpreted with caution (Archer 2000).
Early experiences of dating violence are linked to poor health outcomes such as sexually transmitted infections, teenage pregnancy, pre-term delivery of babies, substance misuse, cancer, coronary heart disease, attempted suicide and depression (Silverman 1995; BMA 1998; Spitz 2000; Campbell 2002;

Roberts 2003; WHO 2005). Recent reviews of the health consequences of partner violence have also found a 3.74 increased risk of suffering from post-traumatic stress disorder and a 1.4 increased risk in mothers of having low birth weight babies (Silva 1997; Murphy 2001). Moreover, adolescents who have experienced dating violence in the past are more likely to be perpetrators or victims of intimate partner violence as adults (Krug 2002; Loh 2006).

## 1.2   DESCRIPTION OF THE INTERVENTION

This review will focus on educational and skills-based interventions targeted at young people aged 12 to 25 years. It includes primary preventive interventions, where participants may have never experienced or perpetrated relationship violence, and secondary prevention, where participants have experienced or perpetrated relationship violence in the past. This review focuses only upon interventions that actively provide the participants with knowledge and skills aimed at preventing initial or further relationship violence. It therefore will not include 'screening programmes' that only offer referral to support agencies. The age group 12 to 25 years has been selected to include both adolescents and young adults.

Educational and skills-based interventions can be delivered in a number of environments, including the community, and in particular, within schools and higher education. Because schools play an important role in the development of social behaviour, they provide an appropriate environment to target children and adolescents in the prevention of dating violence and subsequently other forms of relationship violence. Previous systematic reviews have focused on the effectiveness of general violence prevention programmes, such as those against aggression and bullying (Mytton 2006; Adi 2007; Park-Higgerson 2008). However, there is further potential to utilise schools and other settings in preventing relationship violence. Recent studies from the USA suggest that interventions delivered to college-based populations may have an effect on reducing incidences of sexual assault and possibly intimate partner violence (Luthra 2006). Programmes can also be delivered within the community to raise awareness about abuse, promote positive relationships, enable help-seeking and peer support, challenge discriminative viewpoints and encourage the development of protective skills.

## 1.3   HOW THE INTERVENTION MIGHT WORK

Educational and skills-based programmes aiming to prevent or reduce dating and relationship violence may provide participants with the skills to communicate effectively; deal constructively with stress, disappointment, and

rejection; resolve conflicts, and promote healthier relationships. They may also provide young people with skills to protect themselves from the risk of relationship violence and to improve low self-esteem, which is linked to the likelihood of being a victim of relationship violence.

## 1.4 WHY IT IS IMPORTANT TO DO THIS REVIEW

The high prevalence of relationship violence and the severity and duration of its health consequences render this area an important public health issue. To date, little evidence on the effectiveness of interventions for the prevention of dating and relationship violence in adolescents and young adults has been available. A systematic review focusing on violence and health outcomes will strengthen the evidence base and provide a clearer idea of what works. It will also help to inform future policy, practice and research in this area.

# 2 Objectives of the review

To assess the efficacy of educational and skills-based interventions designed to prevent relationship and dating violence in adolescents and young adults.

# 3 Methods

## 3.1 CRITERIA FOR CONSIDERING STUDIES FOR THIS REVIEW

### 3.1.1 Types of studies

Randomised controlled trials; quasi-randomised controlled trials (in which participants were assigned to intervention or comparison/control groups according to date of birth, day of the week, simple alternation by order of enrolment or other similar methods), and cluster-randomised controlled trials in which classes, schools, groups or clusters are allocated to the intervention or control conditions.

### 3.1.2 Types if participants

Adolescents aged 12 to 18 years and young adults aged 19 to 25 years in any setting. We will include studies with a wider age range of participants if data can be extracted or obtained for these age groups or if more than 80% of the participants included in the study are within the age range 12 to 25 years.

### 3.1.3 Types of interventions

Any programme that is applied universally or to specifically targeted high-risk groups and actively provides adolescents or young adults with educational or skills-based interventions, or both, aimed at the prevention of dating or relationship violence. The intervention may be delivered in any setting and we will include interventions of any duration. We will compare all interventions with a control intervention, which may be no intervention, placebo intervention (for example, provision of first aid classes) or standard care.

*Excluded*

1. Any intervention where the prevention of dating or relationship violence is not stated in the aims or objectives, or that involves a multiple intervention programme in which it is not possible to isolate the relative effects of the violence prevention component.

2. Interventions that only screen for the occurrence of dating or relationship violence and then refer to a support agency, unless the intervention actively provides an educational or skills-based component, or both, following screening.

### 3.1.4    Types of outcome measures

Outcomes measured do not form part of the criteria for inclusion of studies in the review.

***Primary outcomes***

1. *Episodes of relationship and dating violence*
a.  Reduction in number of episodes of relationship or dating violence experienced:
  i.   as self-reported by victims or perpetrators,
  ii.   as reported by official (for example, police) records

2. *Physical health*
a. Reduction in injuries resulting from relationship and dating violence experienced:
  i.   as self-reported by victims or perpetrators,
  ii.   as reported by official (for example, police) records

3. *Psychosocial health*
a. Self-reported subjective improvement in mental well-being

4. *Adverse events*
a. Increase in the number of episodes of relationship or dating violence, or both reported

The 'Summary of findings' table, should the data permit this to be completed, will consist of these primary outcomes.

***Secondary outcomes***

These include outcomes that are closely associated with relationship or dating violence behaviour and that may help to explain how the interventions might work.

1. *Participant-reported improvements in the following:*
a.  Behaviour or knowledge about relationship and dating violence
b.  Access to, or knowledge of, help or support services
c.  Protective skills attained

2. *Intervention-related factors*
a. Cost of programme
b. Time commitment required
c. Acceptability of programme as measured by drop-out rate

### Measurement scales

A variety of measurement scales are available to assess outcomes of educational and skills-based interventions. We will only include data from studies in which a full description of the measurement scale and its scoring system is available. If further evaluations of the reliability or validity of a measurement scale exist in the literature, we will draw upon these to help determine the suitability and applicability of the scale in relation to the given outcome. We will provide reasons for our rejection of any measurement scales from our analysis.

### Timing of outcome assessment

We will categorise primary and secondary outcomes into three time periods: short-term outcomes (outcomes assessed immediately following the intervention to six months following the intervention); medium-term outcomes (outcomes assessed between six and 12 months following the intervention), and long-term outcomes (outcomes assessed more than 12 months following the intervention).

## 3.2   SEARCH METHODS FOR IDENTIFICATION OF STUDIES

We will consider both published and unpublished work eligible for inclusion in the review. The Cochrane Developmental, Psychosocial and Learning Difficulties Group (CDPLPG) Trials Search Co-ordinator will advise on and carry out the search. There will be no restriction on language or date of publication. We will assess articles published in languages other than those spoken by the review authors using the assistance of translators.

### 3.2.1  Electronic searches

We will search the following electronic databases.

1. Cochrane Central Register of Controlled Trials (CENTRAL)
2. MEDLINE
3. EMBASE
4. CINAHL
5. PsycINFO
6. ASSIA
7. Sociological Abstracts
8. Social Science Citation Index
9. ERIC
10. National Criminal Justice Reference Service Abstracts

11. metaREGISTER (mRCT)
12. ZETOC (conference proceedings)

We will use the following search strategy to search MEDLINE and adapt it for other databases:

1 Sexual Partners/
2 Spouses/
3 (boyfriend$ or boy-friend$ or girlfriend$ or girl-friend$ or partner$).tw.
4 or/1-3
5 homicide/ or rape/ or sex offenses/ or violence/ or domestic violence/ or aggression/ or stalking/
6 (violen$ or assault$ or abuse$ or manipulat$ or aggress$ or injur$ or coerc$ or rape$ or murder$ or homicid$ or femicid$).tw.
7 (stalking or stalker$).tw.
8 5 or 6 or 7
9 4 and 8
10 spouse abuse/
11 Battered Women/ or (batter$ adj3 wom#n).tw.
12 ((date or dating) adj3 (violen$ or rape$ or assault$ or abuse$ or manipulat$ or aggress$ or injur$ or coerc$ or murder$ or homicid$ or femicid$)).tw.
13 ((relationship$ or partner$ or acquaintance$) adj3 (violen$ or rape$ or assault$ or abuse$ or manipulat$ or aggress$ or injur$ or coerc$ or murder$ or homicid$ or femicid$)).tw.
14 or/9-13
15 Adolescent/
16 Young Adult/
17 (adolescen$ or teen$ or preteen$ or pre-teen$ or young people or young person$ or young adult$ or youth$ or girl$ or boy$ or juvenile$).tw.
18 or/15-17
19 intervention studies/
20 evaluation studies/
21 Treatment Outcome/
22 ((interven$ or evaluat$ or effectiv$ or compar$) adj3 (study or studies or research)).tw.
23 19 or 20 or 21 or 22
24 randomized controlled trial.pt.
25 controlled clinical trial.pt.
26 randomi#ed.ab.
27 placebo$.ab.
28 drug therapy.fs.
29 randomly.ab.
30 trial.ab.

31 groups.ab.
32 or/23-31
33 exp animals/ not humans.sh.
34 32 not 33
35 14 and 18 and 34

### 3.2.2 Searching other resources

In order to identify further relevant literature that is not obtained by searching the databases listed above, we will carry out additional searches. We will screen reference lists of key articles included in the review for citations of papers not previously identified. We will handsearch the most relevant journals, such as the *Journal of Interpersonal Violence* and *Child Abuse and Neglect*. We will also screen the proceedings of relevant conferences to identify relevant unpublished material. Finally, we will contact key experts in the field and ask them to share any published, unpublished and ongoing work relevant to the review.

## 3.3 DATA COLLECTION AND ANALYSIS

### 3.3.1 Selection of studies

Two review authors will independently screen the titles and abstracts of articles identified in the search against the inclusion criteria to decide which reports should be retrieved. We will reject articles at this stage if the title or abstract do not focus on prevention of relationship and dating violence in adolescents or young adults. If there is insufficient information in the title and abstract to make such decisions, we will retrieve the full text. We will review selection decisions and we will resolve any disagreements by consultation with a third review author. If disagreements cannot be resolved in conjunction with the third author, we will consult the CDPLPG editor. We will document the principal reason for exclusion of each study that seems to meet inclusion criteria but on closer inspection does not.

### 3.3.2 Data extraction and management

Two review authors will independently extract data and we will compare results using data extraction sheets and the double-entry feature in Review Manager 5.0 (RevMan) ([Review Manager 2008](#)). We will extract data concerning population, age, the control group, baseline characteristics, intervention characteristics, duration, compliance and outcome measures

and present this in a 'Characteristics of included studies' table. We will request the specific data relevant to age groups included in the review from authors of trials where there is a wide spread of ages amongst participants.

### 3.3.3   Assessment of risk of bias in included studies

Two review authors will independently assess the risk of bias in each study using the Cochrane Collaboration's 'Risk of bias' tool ([Higgins 2008](#)). For each of the six domains listed below, we will describe what was reported to have happened in the study and give a judgement of low, high or unclear risk of bias.

#### *Sequence Generation*

- Description: the method used to generate the allocation sequence should be described in sufficient detail to enable assessment of whether it should have produced comparable groups.
- Review authors' judgement: was the allocation sequence adequately generated?

#### *Allocation Concealment*

- Description: the method used to conceal the allocation sequence should be described in sufficient detail to determine whether intervention allocations could have been foreseen in advance of, or during, enrolment.
- Review authors' judgement: was the allocation adequately concealed?

As inadequate allocation concealment can introduce bias into the study results, we will perform sensitivity analyses and we will exclude studies from meta-analysis if no allocation concealment was used, or if there was uncertainty about allocation concealment. Quasi-randomised studies may introduce bias due to inadequate allocation concealment. This will be considered in the discussion and, where feasible, we will perform a sensitivity analysis to assess the impact of including these studies.

#### *Blinding*

- Description: any measures used to blind study participants and assessors from knowledge of which intervention a participant was allocated to should be described.
- Review authors' judgement: was knowledge of the allocated intervention adequately prevented during the study?

#### *Incomplete Outcome Data*

- Description: the completeness (including attrition and exclusions from analysis) of outcome data for each main outcome should be reported.

- Review authors' judgement: were incomplete outcome data adequately addressed?

*Selective Outcome Reporting*

- Description: the possibility of selective outcome reporting should be examined.
- Review authors' judgement: were the reports of the study free of suggestion of selective outcome reporting?

### 3.3. 4  Measures of treatment effect

We will calculate the risk ratio (RR) to summarise dichotomous data rather than odds ratio (OR) as it is easier to comprehend, unless the events are infrequent when OR will be used. We will report continuous data as mean differences (MD) where the same scale is used for measurement and standardised mean differences (SMD) where different scales are used to measure the same thing.

### 3.3.5  Unit of analysis issues

If cluster-randomised trials are included in this review, we will follow the guidance on statistical methods for such trials outlined in the *Cochrane Handbook for Systematic Reviews of Interventions* ([Higgins 2008](), Section 16.3). We will use a summary measure of effect from an analysis which adequately accounts for the cluster design. If this is not available, we will extract or calculate appropriate measures of effect as for a parallel group trial and adjust the standard errors (SE) to account for the effect of clustering. This will involve using an intra-class correlation co-efficient (ICC), which describes the relative variability in outcomes within and between clusters. We will extract information on the ICC from the articles if available. If the ICC is not provided, we will contact the authors or use external estimates obtained from similar studies. We will use existing databases of ICCs to identify an ICC that matches our outcome measures and cluster types most closely. In the case that we are unable to identify an appropriate ICC, we will perform sensitivity analyses using a high ICC of 0.1, a moderate ICC of 0.01 and a small ICC of 0.001. We will combine these estimates and their adjusted SE with those from parallel designs using the generic inverse variance method in RevMan ([Review Manager 2008]()).

### 3.3.6  Dealing with missing data and incomplete data

If significant quantities of participant data are missing and the review authors agree that a study's conclusions are compromised as a result, we will contact trial authors and ask them to supply missing data (for example, on subgroup means and SDs, numbers of participants). If no reply is received or

if missing data are not available, we will exclude studies from the final analysis. For each study included in our review, we will report the drop-out rate (calculated as the number of participants included in the final analysis as a proportion of those who began the intervention) in a 'Risk of bias' table. We will discuss the extent to which the results and conclusions of the review could be altered by the missing data. We will exclude participants for whom no outcome data are available rather than conduct intention-to-treat analysis of all randomised participants with imputed values for the missing data (Higgins 2008).

### 3.3.7 Assessment of heterogeneity

In order to investigate the extent of variation between studies, we will assess the distribution of relevant participant (for example, age, gender) and trial (for example, randomisation, assessor blinding, attrition rate, and type and duration of intervention) factors. We will assess statistical heterogeneity using the $I^2$ statistic, which describes the proportion of variation in point estimates that is due to heterogeneity rather than sampling error (Higgins 2008). An $I^2$ value of greater than 50% will be considered to represent substantial heterogeneity (Higgins 2008). We will also use the $Chi^2$ test of homogeneity to determine the strength of evidence for genuine heterogeneity. If no substantial heterogeneity between studies is demonstrated, we will pool data and conduct meta-analyses.

### 3.3.8 Assessment of reporting biases

If sufficient studies are found, we will draw funnel plots to assess the presence of possible publication bias. While funnel plot asymmetry may indicate publication bias, this is not inevitably the case, and we will consider any possible explanations for any asymmetry found and discuss these in the text of the review (Egger 1997).

### 3.4 DATA SYNTHESIS

### 3.4.1 Data synthesis

4    If interventions across different studies are similar with respect to (i) the setting in which they were delivered and (ii) the duration and intensity with which they were delivered, we will combine results in a meta-analysis. Where there is substantial heterogeneity, we will compute pooled estimates only for those trials that can be analysed together and for which statistical data are available. We will carry out data synthesis using RevMan (Review Manager 2008). We will draw funnel plots

(estimating differences in treatment effects against their SE) if we find sufficient studies. If there is no severe funnel plot asymmetry, a random-effects model will be used to assess the impact of statistical heterogeneity. If significant funnel plot asymmetry is seen, we will use both fixed-effect and random-effects models and we will report the degree of agreement between the results of the two models. We will calculate the overall effects using the inverse variance method ([Higgins 2008](#)). For dichotomous outcomes, we will calculate an overall RR. For continuous outcomes and similar comparisons and outcome measures, we will calculate MDs. If continuous outcomes are measured with similar, but not identical, instruments across studies, we will calculate SMDs. For ease of interpretation, where possible, we will express results as a RR (or OR), and we will include 95% confidence intervals (CI). If the combination of data in a meta-analysis is inappropriate due to substantial heterogeneity (as defined above), we will describe the studies and present results for each study individually.

### 3.4.2 Subgroup analysis, moderator analysis and investigation of heterogeneity

If there is any evidence of heterogeneity amongst studies we include, we will explore the reasons for this. Irrespective of the degree of heterogeneity found, we plan to carry out subgroup analyses focusing on the following.

- Participant age.
- Participant gender.
- Intervention delivery setting (for example, school, community).
- Duration of intervention (time period over for intervention ran and number of sessions included).

### 3.4.3 Sensitivity analysis

We will conduct sensitivity analyses to assess the extent to which results are sensitive to the analysis being restricted to only those studies judged to be at a low risk of bias. We will run sensitivity analyses in which the analysis is restricted to the following.

1. Studies with a low risk of selection bias (as determined by the quality of the random sequence generation).
2. Studies with a low risk of assessment bias (as determined by the quality of blinding of assessors).
3. Studies with a low risk of attrition bias (as determined by the completeness of the data).

We will also assess the impact of imputing missing data.

# 4  Acknowledgments

This review is coregistered with the Cochrane Developmental, Psychosocial and Learning Problems Group, and also appears on *The Cochrane Library*.

# 5  Contribution of authors

All review authors contributed to the development of the protocol. Trial selection, data extraction and assessment of the risk of bias will be performed by GF and CH with JN, SH and DS serving as arbitrators. All review authors will contribute to the final write-up of the review.

# 6  Declarations of interest

Gracia LT Fellmeth - None known.
Joanna Nurse - None known.
Catherine Heffernan - None known.
Shakiba Habibula - None known.
Dinesh Sethi - None known.

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# 8  Sources of support

## INTERNAL SOURCES

No sources of support provided

## EXTERNAL SOURCES

No sources of support supplied