Protocol:

Universal Community-based Social Development Interventions for Preventing Community Violence by Young People 12 to 18 Years of Age

Gavine A, MacGillivray S, Williams DJ

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Universal community-based social development interventions for preventing community violence by young people 12 to 18 years of age (Protocol)

Gavine A, MacGillivray S, Williams DJ

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Universal community-based social development interventions for preventing community violence by young people 12 to 18 years of age

Anna Gavine¹, Stephen MacGillivray², Damien J Williams¹

¹School of Medicine, University of St Andrews, St Andrews, UK. ²Social Dimensions of Health Institute, University of Dundee, Dundee, UK

Contact address: Anna Gavine, School of Medicine, University of St Andrews, Medical and Biological Sciences Building, Norh Haugh, St Andrews, Fife, KY16 9TF, UK. ajg16@st-andrews.ac.uk.


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ABSTRACT

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

To examine the effectiveness of community-based social development interventions in the prevention of community violence among young people aged 12 to 18 years.

BACKGROUND

Description of the condition

Youth violence is a form of interpersonal community violence, defined by the World Health Organization (WHO) as "violence between individuals who are unrelated, and who may or may not know each other, generally taking place outside the home" (Dahlberg 2002, p.6). Such violence has far-reaching consequences with the most unacceptable being that violence is a leading cause of death in young people. There are an estimated 199,000 global youth homicides per year, which equates to 9.2 per 100,000 of the population (Dahlberg 2002). Mercy 2002 also reports that the rate is significantly higher in young men aged 15 to 29 years (19.4 per 100,000) compared with young women (4.4 per 100,000). The onset of serious violence typically begins from 12 years of age and peaks between 16 and 18 years of age (Office of the Surgeon General (US) 2001), with physical aggression peaking at 15 years of age (Brame 2001).

Beyond the extreme outcome of death, violence is associated with non-fatal injuries, illness, disability, and reduced quality of life (Dahlberg 2002). For instance, the WHO reports that for every youth homicide there are 20 to 40 victims of non-fatal assault (Dahlberg 2002). Indeed, it has been estimated that violence-related morbidity and mortality is responsible for 3% of the global burden of disease (Brundtland 2002). Violence not only impacts its victims, but also the health and well-being of their families, friends, and the wider community (Dahlberg 2002). For example, in deprived inner-city areas, the majority of children have been a victim of, or exposed to, some form of community violence, and this can result in anxiety and depression, and can negatively impact...
their developmental trajectories (Margolin 2000). Furthermore, parents in an inner-city deprived community reported experiencing high levels of anxiety regarding their own personal safety and that of their children (Weir 2006). This can result in decreased physical activity within this population, which is a risk factor for other adverse health complications (e.g. obesity).

Exposure to violence also puts young people at risk of developing chronic conditions and engaging in health-risk behaviours (Felitti 1998). For instance, exposure to violence as a child is associated with adverse health behaviours (including multiple sexual partners, sexually transmitted infections, alcoholism, and smoking cigarettes) and mood-related disorders (Dube 2003). Furthermore, young people exposed to adverse childhood experiences are at increased risk of developing several of the leading causes of death in adults, independent of their involvement in associated health-risk behaviours (e.g. smoking), including ischaemic heart disease, chronic lung disease, liver disease, and cancer (Felitti 1998).

Engaging in violent behaviour is associated with a range of other health-risk behaviours (Dahlberg 2002). In particular, alcohol use is strongly associated with youth violence and is estimated to be responsible for 26% of male homicides globally (WHO 2006). Moreover, alcohol is frequently involved in non-fatal violence. For example, 42% of people presenting to Accident and Emergency departments in Canadian hospitals with a violent injury were found to have a high blood alcohol level (Macdonald 1999), and the British Crime Survey reported that in 40% of violent incidents, the victim considered the perpetrator to be under the influence of alcohol (Budd 2003). Illicit drug use is also strongly linked with violence, with individuals involved in drugs being at higher risk of becoming either a perpetrator or victim of violence (Atkinson 2009). For instance, a study of methamphetamine users in Los Angeles found that 35% had committed violent behaviour while under the influence (Baskin-Sommers 2006), and one study of young European tourists reported that cocaine use increased the risk of fighting by three-fold (Hughes 2008). Due to the strong links between alcohol and drug abuse and violence, some violence prevention programmes target these risk factors as an indirect means to address violence (Sethi 2010).

Violence also represents inequalities in health, with homicide rates varying greatly between regions and countries, and being higher in low- and middle-income countries than in high-income countries. For example, in Western Europe, Germany has a youth homicide rate of 0.8 per 100,000, whereas in Latin America the rates are significantly higher with Colombia having a rate of 84.4 per 100,000 and El Salvador a rate of 50.2 per 100,000 (Dahlberg 2002; see also Fajnzlber 2002; UNODC 2011). Moreover, rates vary significantly within countries, with young people from the areas of most socioeconomic disadvantage being disproportionately affected (Sethi 2010). For instance, Scottish men under 65 years of age living in the most deprived quintiles have a death rate due to assault that is 31.9 times that of men living in the most affluent quintiles (Leyland 2010). This social patterning of violence is evident across the globe with studies in the US (Cubbin 2000), Brazil (Caicedo 2010), and Russia (Chenet 1998) all reporting an increased prevalence of violence and fatal violent injuries in lower socioeconomic populations.

In addition to socioeconomic factors, longitudinal studies have identified other risk factors for involvement in violent behaviour at the neighbourhood, family, and individual level (Farrington 1995; Hawkins 1995). Neighbourhood risk factors include availability of weapons, laws, and social norms favourable to violence; media portrayal of violence; poor social cohesion; and high levels of residential mobility. Family risk factors include family conflict, poor family management, child abuse, and pro-violent parental attitudes and behaviour. Finally, individual factors include friends involved in problem behaviour, early onset of antisocial behaviour, impulsivity, attention problems, low intelligence, and academic failure. These multiple risk factors demonstrate the complex nature of violence. Indeed, the WHO acknowledged this complexity and has applied an ecological model to understanding and addressing violence that accounts for the complex linkages between the individual, their relationships, and the community and society in which they live (Dahlberg 2002).

As discussed, there are a number of neighbourhood risk factors for violent behaviour and as such the WHO argue that a community in which a young person lives can strongly influence their involvement in violence, with those living in high crime neighbourhoods or neighbourhoods with gangs being more likely to be involved in violence (Dahlberg 2002). For instance, one large study conducted in Chicago reported that spatial proximity to homicide is strongly related to increased homicide rates (Morenoff 2001). Therefore, community-based programmes tend to have been implemented in communities where young people are at risk of community violence due to socioeconomic disadvantage (Buka 2001; Bellis 2008; Sethi 2010), high levels of residential mobility (Dahlberg 2002), and poor social cohesion (Sampson 1997; Martikainen 2003). More specifically, youth violence prevention programmes are frequently implemented in a school setting (Myton 2002); however, in low- and middle-income countries the majority of youths will, at most, only attend primary school (UN Statistics Division 2011), and in high-income countries youths engaged in violence are more likely to have dropped out of school (Elllickson 1997). Thus, school-based violence prevention programmes will not necessarily be successful in engaging with the youths most at risk whereas community-based programmes (e.g. The Communities that Care System; Hawkins 2008) have demonstrated the ability to reduce violence, and are able to engage with high-risk adolescents who cannot access school-based programmes.

**Description of the intervention**

Violence is a multifaceted problem with biological, psychological, social, and environmental risk factors. The most effective prevention programmes tend to act on combinations of the different fac-
Dahlberg 2002). Moreover, it is increasingly recognised that a primary prevention approach, which aims to prevent violence before it occurs by addressing these factors, is necessary to reduce violence (Prothrow-Stith 2010). The WHO broadly divide strategies for the primary prevention of violence into direct and indirect approaches (Sethi 2010). First, direct approaches aim to prevent violence by altering the environment in which violence occurs, including: enhancing legislation on buying and carrying knives, and the use of safe drinking vessels (Sethi 2010). Second, indirect approaches target the individual risk and protective factors that affect whether a young person will become involved in violence and include: social development programmes, parenting programmes, and pre-school enrichment programmes (Sethi 2010). The majority of indirect interventions are considered downstream interventions, which are defined as interventions “directed towards individuals to address health behaviours, attitudes, and knowledge” (Smedley 2000, p.28). According to the Health Development Agency, the current available evidence supports the use of downstream interventions (Kelly 2005). More specifically to violence, while there is more evidence to support the use of early indirect interventions, such as parenting programmes and pre-school enrichment (Sethi 2010), these by their nature will not be applicable to adolescents without children of their own. Therefore, the majority of indirect youth violence prevention programmes that target adolescents are social development programmes and focus on increasing the level of protective factors that reduce the risk of involvement in violence (Mercy 2002). Moreover, as it is often not easy or indeed possible to change upstream risk factors for violence (i.e. living in poverty), enhancing a young person’s capacity to manage such risk factors and develop resilience is an important area for violence prevention (Dahlberg 2001).

As the evidence suggests that indirect approaches may be more appropriate for reducing violence (Kelly 2005), and because this review is examining the effectiveness of programmes that are targeted at young people aged 12 to 18 years, the review will focus on social development programmes that aim to reduce homicide, non-fatal assault, and weapon possession; or change attitudes, beliefs, and perceptions about violence; or both. Direct approaches that are targeted at adolescents are by their nature more policy focused and as such merit a separate review of their effectiveness elsewhere.

The WHO states that social development programmes aim to develop “social skills and competencies including: anger management, problem-solving, conflict resolution, assertiveness, active listening, knowledge about healthy relationships and empathy” (Sethi 2010, p. 53). Social development programmes take the form of classes and will be included in this review if they aim to develop one of the aforementioned skills in young people aged to 12 to 18 years and take place in a community setting (e.g. community centres, church halls, and youth centres). We will take community-level interventions to be those that are implemented in community settings rather than those targeting aspects of community life or dynamics, or both. These classes may or may not be combined with diversionary activities or mentoring programmes, which also aim to enhance a young person’s social development. Programmes that aim to reduce violence among young people already known to be engaging in violent behaviour are considered a form of secondary (i.e. addressing the presence of risk factors for violence) or tertiary (i.e. preventing the re-occurrence of violence) prevention and are significantly different to warrant a separate review.

**How the intervention might work**

Social development interventions (as detailed in Description of the intervention) are thought to work by reducing the influence of risk factors (as detailed in Description of the condition) and developing protective factors (Sethi 2010).

The concept of protective factors comes from a large body of empirical research in developmental psychopathology on resilience, which suggests that some children and adolescents have the capacity for successful adaptation (i.e. maintaining internal states of well-being and effective functioning) and for not engaging in problem behaviour (e.g. alcohol and drug abuse, antisocial behaviour) despite challenging or threatening circumstances that are considered risk factors for such behaviour (e.g. living in poverty; see Garmezy 1984; Masten 1990; Werner 1993; Luthar 2000). Further research evaluating the involvement of adolescents in problem behaviour has identified protective factors as being one explanation for this ability to avoid problem behaviours and maintain psychosocial well-being in the presence of risk factors (Hawkins 1992; Newcomb 1992;Jessor 1995).

The majority of research on protective factors has been undertaken in relation to antisocial behaviour in general and not specifically to violence; however, it is believed that developing protective factors (particularly individual attributes) can decrease a young person’s risk of involvement in violent behaviour by minimising or buffering the effects of risk factors (Office of the Surgeon General (US) 2001; Farrington 2007). The Surgeon General's Report on Youth Violence proposed a number of potential protective factors, including intolerant attitudes toward deviance and violent behaviour (reported as potentially having the strongest effect); high IQ, positive social orientation, perceived sanctions for transgressions, supportive relationships with parents or other adults, parental monitoring, commitment to school, recognition for involvement in conventional activities, and having friends who engage in conventional activities (Office of the Surgeon General (US) 2001).

A substantial body of research in this area led to the development of the social developmental model, which is a general theory of behaviour, grounded in control theory, social learning theory, and differential association theory. It hypothesizes that antisocial or pro-social behaviour can be predicted by the presence of risk and protective factors (Catalano 1996a; Catalano 1996b). The theory predicts that interventions that enhance protective factors (e.g. bonding with families) can reduce the effects of risk factors and
set children and adolescents on a different developmental trajectory with positive outcomes (Hawkins 1999). This theory consequently underpins indirect violence prevention programmes for 12 to 18 year olds, which targets knowledge, skills, and attitudes to reduce involvement in violence (Farrell 2001), and can be delivered as social development programmes.

Social development programmes are believed to enhance protective factors for violence by developing pro-social skills (see Figure 1) which can be defined as competence in peer interactions and friendships, and interpersonal conflict resolution skills (Grossman 1997). Enhancing skills and competencies (as detailed in Description of the intervention) can enable young people to develop and maintain healthy relationships, and provide them with alternative skills to deal with conflict and solve problems without violence (WHO 2009). A social skills development approach has demonstrated success in school-based violence prevention interventions, with a systematic review reporting that such programmes were associated with 19.1% relative change (i.e. reduction in violent outcome) in the intervention group compared to the control group (Hahn 2007). However, no systematic review has been conducted to evaluate the effectiveness of such programmes in a community setting. Social development programmes may have a mentoring element, which pairs young people with a volunteer who will provide support, understanding, experience, and advice (Roberts 2004). It is believed such relationships may help develop interpersonal skills, and help young people cope with and avoid a high-risk lifestyle (Mihalic 2004). Social development programmes may also incorporate diversionary activities that aim to provide young people with an alternative to antisocial behaviour by reducing boredom and unsupervised leisure time, while also improving social skills and enhancing community involvement (Morris 2003).

**Figure 1. Logic model of universal community based social development programmes for preventing community violence by young people 12 to 18 years of age**

**Why it is important to do this review**

Violence is a leading cause of death and disability in young people aged 15 to 29 years worldwide (Mercy 2002), and is associated with health-risk behaviours (e.g. alcohol and drug abuse) and the development of chronic diseases (Felitti 1998). Despite, the belief that “violence is preventable, not inevitable” as stated in the World Report on Violence and Health (Dahlberg 2002, p.3), the actual evidence base for violence prevention interventions is in need of further development (Rutherford 2007).

To date, systematic reviews examining the effectiveness of universal youth violence prevention interventions have focused on school-based programmes (Cooper 2000; Mytton 2002; Wilson 2003;
Hahn 2007), dating violence prevention (Fellmeth 2011), and youth gang involvement prevention (Fisher 2008a; Fisher 2008b).

These issues are important to understanding what works in the prevention of youth violence; however, they are either specific to a type of violence (dating and gang) or location (school) or have been limited to interventions conducted in the US (Limbos 2007). Indeed, the majority of social development programmes that aim to reduce violence in children and adolescents have been developed and evaluated in high-income countries, in particular the US (WHO 2009), and may not, therefore, be applicable to other nations.

While there is some evidence for the effectiveness of community-based social development programmes in the prevention of youth violence (see Description of the intervention), no systematic review of such interventions has been undertaken. Due to the potential importance of community-based social development interventions, particularly to engage the most at-risk youths, a systematic review of global interventions in this area is necessary to inform violence prevention policy and guide the development of future interventions.

OBJECTIVES

To examine the effectiveness of community-based social development interventions in the prevention of community violence among young people aged 12 to 18 years.

METHODS

Criteria for considering studies for this review

Types of studies

Interventions can be evaluated using a range of study designs. Randomised controlled trials (RCT) can produce the most reliable evidence as they are less likely to be affected by bias, in particular selection bias (Reeves 2011), compared with non-randomised trials (NRT). However, RCTs are not always possible due to feasibility and ethical issues, and thus NRTs may offer the best available evidence (Armstrong 2011). It is, therefore, possible that in this review, a number of potentially important studies may be omitted by restricting the review to RCTs, so we intend to include NRTs.

We have followed guidance from both The Campbell Collaboration (Shadish 2004), and The Cochrane Collaboration (Reeves 2011), in our plans for the inclusion of NRTs. The review will specifically include the following study types in addition to RCTs.

- Randomised design, including cluster-randomised trials and quasi-randomised trials whereby participants are allocated to a treatment or control group via methods that are not truly random (e.g. by date of birth).
- Quasi-experimental: interrupted time series (ITS) design with at least 50 time points, at least 20 of which are recorded before or after the start of the intervention. Alternatively, three or more data points must have been recorded before and at three times after the intervention and a clearly defined point in time when the intervention occurred. The study authors must have conducted either a repeated measures analysis (i.e. Analysis of Variance (ANOVA) or multiple t-tests with appropriate correction of alpha) with at least 30 data points per time point as per the recommendations by the Cochrane Effective Practice and Organisation of Care (EPOC) Group (EPOC 1998).
- Quasi-experimental: non-equivalent comparison group design (i.e. controlled before-and-after (CBA) studies) where participants are allocated to either the treatment or control group using non-random methods with at least two intervention sites and two control sites (EPOC 2013).

Types of participants

All young people aged 12 to 18 years who are either:

1. living in an area where the intervention has been implemented, or
2. involved in or acting as controls for the intervention for the prevention of community violence.

We will exclude interventions that specifically target children with an existing mental illness as it is considered that these interventions will be significantly different from universal prevention interventions to warrant a separate review.

Types of interventions

Any community-based social development violence prevention programme that aims to develop anger management, problem-solving, conflict resolution, assertiveness, active listening, knowledge about healthy relationships, and empathy (as defined in the Background) in order to reduce homicide, non-fatal assault, and weapon possession; change attitudes, beliefs, and perceptions about violence; or a combination of these. We will include studies evaluating school-community partnerships or health service-community partnerships if there is a specific community-based element that targets violence prevention by development of the aforementioned skills. The primary comparison will be a control intervention (i.e. no treatment, standard practice, or waiting list control).

We will exclude:

- programmes targeting other forms of interpersonal violence involving youths (e.g. family violence and structural violence) as they are considered to be sufficiently different forms of violence with different aetiologies to warrant a separate review;
- any primary or universal approaches which do not specifically aim to develop the aforementioned social skills and...
instead use other forms of universal or primary prevention (e.g. academic enrichment); 
- any secondary or selective prevention approaches targeting individuals already engaged in antisocial or aggressive behaviour (e.g. multisystemic therapy) as they do not fall under the category of universal prevention programmes; 
- any tertiary or targeted prevention approaches targeting individuals already significantly involved in violence (e.g. cognitive-behavioural therapy) as they do not fall under the category of universal prevention programmes; 
- any completely health- or school-based intervention, which does not have a community component.

Types of outcome measures

Primary outcomes
We will consider each of the primary outcomes specified below at the level of the individual and the level of the community.
1. Incidences of violence (e.g. homicides; stabbings; shootings; beatings from hospital records, police intelligence, and self report).*
2. Incarceration due to violence (from police intelligence and self report).*
3. Weapon possession (from self report data and police intelligence).*
4. Adverse effects.

Secondary outcomes
1. Attitudes towards violence: self reported pro-violent attitudes, measured using the Attitudes Towards Violence Scale (Funk 2003), or a similar, validated instrument.*
2. Pro-social skills: self reported levels of empathy, relationship skills, or conflict resolution skills measured by a validated instrument.
3. Levels of aggression: self reported levels of aggressive behaviour or feelings, measured using the Aggression Questionnaire (Buss 1992), or a similar, validated instrument.*

*Outcomes to be included in a 'Summary of findings' table (Schünemann 2011).

In the case that several sources of data are presented for one outcome, we will report each data source separately and will consider administrative data (i.e. health records and criminal justice system records) the most robust indicator of effectiveness followed by self report data and then scales or instruments.
In order to assess whether the effects of the intervention are sustained over time, if data permits, we intend to group intervention time points as follows: immediately post-intervention, after a short-term follow-up period of up to six months, after a medium-term follow-up period of up to 18 months, and after a long-term follow-up period of up to five years.

Search methods for identification of studies

Electronic searches
We worked with the Trials Search Co-ordinator of the Cochrane Developmental, Psychosocial and Learning Problems Group to design the search strategy. We will search databases for published and unpublished studies. We will apply no language restrictions on any results from any search attempts. We will search the following databases.
- Cochrane Central Register of Controlled Trials (CENTRAL).
- Ovid MEDLINE.
- Embase.
- LILACS.
- ASSIA.
- PsycINFO.
- ERIC (Educational Resources Information Centre).
- Science Citation Index.
- Proquest Dissertation and Abstracts.
- IBSS (International Bibliography of Social Sciences).
- NYAM Grey Literature (www.nyam.org/library/).
- OpenGrey (opengrey.eu/)
- Trials Register of Promoting Health Interventions (TRoPHI) (eppi.ioe.ac.uk)
- National Research Register Archive.
- ClinicalTrials.com.
- ICTRP (www.who.int/ictrp/en/).
- Google Scholar.

We will base the searches on the following Ovid MEDLINE search strategy, which we will adapt for the other databases.
1 violence/
2 homicide/
3 exp Aggression/
4 Hostility/
5 dangerous behavior/
6 ((criminal or deliberate or intentional) adj5 (attack$ or assault$ or injur$)).tw.
7 (violent or violence).tw.
8 weapons/ or firearms/
9 (gang or gangs or gangland$ or gang-land).tw.
10 or/1-9
11 child/
12 adolescent/
13 young adult/
14 (child$ or adolescen$ or teen$ or boy$ or girl$ or juvenil$ or pupil$ or student$ or young person$ or young people or youth$ or under 18$).tw.
15 or/11-14
16 10 and 15
verse roles in youth violence prevention (locally, nationally, and globally) to obtain information on completed and ongoing studies. In order to identify such key stakeholders, we will screen the list of participants identified by the WHO Violence Prevention Alliance (www.who.int/violenceprevention/en/) and then contact those stakeholders using the names and addresses provided. We will also make every effort to contact authors of all included studies to identify whether they have other unpublished or ongoing studies that would meet our inclusion criteria. We will also conduct an Internet search using Google and Google Scholar and will search the following websites.

- Centers for Disease Control and Prevention: Violence Prevention (www.cdc.gov/violenceprevention/).
- Asian Development Bank (www.adb.org/).
- Department for International Development (www.dfid.gov.uk).
- United Nations Office on Drugs and Crime (www.unodc.org/).

Data collection and analysis

Selection of studies
The references from the electronic database searches will be imported into Endnote Reference Management Software and then screened. AG and DW will independently screen titles, abstracts, and keywords and exclude ineligible studies based on topic, design, population, setting, or intervention (if specified in sufficient detail to exclude the possibility of violence prevention). AG and DW will then review the remaining full texts where additional ineligible studies will be excluded using the same criteria. If any discrepancy occurs regarding eligibility for inclusion, AG and DW will consult with SM.

Data extraction and management

We will translate any non-English publications. AG and DW will independently extract data from each eligible study using an electronic data extraction form and enter the data into Review Manager 2012 software (Review Manager 2012). The review authors will not be blinded to the names of the journals, the authors, the institutions or the results when extracting data and assessing methods. Every effort will be made to contact authors of all eligible studies to confirm study details, obtain missing data, and identify relevant unpublished outcomes. The review authors will extract data on the following for each included study:

- study design: aim of study, description of study design, risk of bias assessment criteria;
We will refer any disagreements to SM. We will address any disagreements through discussion. In instances where there is an unresolved issue, SM will independently extract data according to the same process undertaken by AG and DW. We will document all differences and how these were resolved.

Assessment of risk of bias in included studies

For each included study, AG and DW will independently assess the risk of bias. We will address any disagreements through discussion and in the case that this cannot be resolved, we will contact the study authors for further information. We will assess risk of bias according to study design. We will assess RCTs using The Cochrane Collaboration's tool for assessing risk of bias (Higgins 2011). We will refer any disagreements not resolved through discussion to SM. The tool will assess the following domains as either 'low', 'high', or 'unclear' risk of bias (where the risk of bias is uncertain or unknown).

**Selection bias**

Random sequence generation: was the method used to generate the allocation sequence adequate?
- **Low risk of bias** if investigators describe any truly random process (e.g. coin tossing, computer random number generator).
- **High risk of bias** if any investigators describe a non-random process (e.g. odd or even date of birth, allocation by judgement of clinician).
- **Unclear risk of bias** if there was insufficient information about sequence generation.

Allocation concealment

Was the allocation method adequately concealed to prevent biased allocation to the interventions?
- **Low risk of bias** if participants and investigators could not foresee the assignment due to central allocation (e.g. telephone, web-based) or sequentially numbered, opaque, sealed envelopes.
- **High risk of bias** if participants and investigators could possibly foresee the assignments due to open random allocation, unsealed or non-opaque envelopes, allocation due to date of birth or case record number.
- **Unclear risk of bias** if there was insufficient information about method of concealment.

Performance bias

Blinding of participants and personnel: were adequate measures taken to prevent participants and study personnel having any knowledge to which group participants were allocated?
- **Low risk of bias** if participants and personnel were blinded and it is unlikely that the blinding could have been broken or if the outcome was not likely to be influenced by a lack of blinding.
- **High risk of bias** if participants or personnel were not blinded or it is likely that any blinding could have been broken and the outcome was likely to be influenced by lack of blinding.
- **Unclear risk of bias** if there was insufficient information about blinding.

Detection bias

Blinding of outcome assessment: were adequate measures taken to prevent the outcome assessors having any knowledge of which intervention a participant received?
- **Low risk of bias** if outcome assessors were blinded and it is unlikely that the blinding could have been broken or if the outcome measurement was not likely to be influenced by a lack of blinding.
- **High risk of bias** if outcome assessors were not blinded or it is likely that any blinding could have been broken and the outcome measurement was likely to be influenced by lack of blinding.
- **Unclear risk of bias** if there was insufficient information about blinding.

Incomplete outcome data

Will be assessed for each outcome: were the data from each outcome complete and, if not, was attrition and exclusion adequately described and explained?
- **Low risk of bias** if there were no missing outcome data, reasons for the missing data were unlikely to be related to the true outcome, the missing outcome data were equal in both groups, missing data have been imputed using statistical methods, the extent of missing data was not large enough to have a clinically significant effect.
- **High risk of bias** if the reason for missing data was related to the outcome, the extent of the missing data was large enough to have a clinically significant effect, missing data values have been imputed inappropriately or 'as-treated' analysis done with
substantial departure of the intervention received from that assigned at randomisation.

- **Unclear risk of bias** if insufficient information was provided (e.g. no reasons for missing data provided, number of participants randomised not given).

### Reporting bias

Selective reporting: does all collected data appear to be reported in the results section or have data from some measures been omitted?

- **Low risk of bias** if all the study's pre-specified primary and secondary outcomes have been reported in the specified way or all the expected outcomes were reported.
- **High risk of bias** if not all of the study's pre-specified primary and secondary outcomes have been reported or not reported completely, one or more primary outcome was reported using measurements, methods, or analysis that was not pre-specified, or one or more reported primary outcome was not pre-specified.
- **Unclear risk of bias** if there was insufficient information on all outcomes to allow judgement for 'low risk' or 'high risk' of bias.

### Other bias

For each described study, we will assess whether it was free from other problems that could put it at risk of bias (e.g. baseline imbalances, use of insensitive instruments to measure outcomes, deviation from study protocol).

We will include cluster-randomised trials, which have specific risks of bias (Puffer 2003). In particular, we will consider the following areas:

- Recruitment bias: have individuals been recruited to the trial after the clusters have been randomised? If this is the case, this could lead to selection bias.
- Baseline imbalance: has baseline comparability of the clusters been reported or statistical adjustment been carried out?
- Loss of cluster from trials: have any clusters been omitted from the analysis or have outcomes for individuals within clusters been omitted?
- Analysis of clustering: have the trials been analysed correctly, taking clustering into account?
- Comparability with individually randomised trials: are cluster-randomised trials creating a 'herd effect' and thus overestimating the results of an intervention?

As there is no universal tool that is able to encapsulate all domains of risk of bias in NRTs (Deeks 2003), we will follow the recommendations of Chapter 13 of the Cochrane Handbook of Systematic Reviews of Interventions (Reeves 2011). First, NRTs will initially be evaluated using The Cochrane Collaboration's tool for assessing risk of bias as detailed previously (Higgins 2011). We will then use EPOC's risk of bias criteria for NRTs and CBA studies (EPOC 2013), which is an amended version of The Cochrane Collaboration's tool for assessing risk of bias providing extra guidance for assessing NRTs and CBA studies and provides an additional item for risk of contamination and additional items to assess the risk of confounding and selection bias. We will assess these domains as follows.

Was the study adequately protected against contamination?

- **Low risk of bias** if allocation was by community, institution, or practice, and it is unlikely that the control group received the intervention.
- **High risk of bias** if it was likely that the control group received the intervention.
- **Unclear risk of bias** if it was possible that communication between intervention and control groups could have occurred.

Were baseline outcome measurements similar?

- **Low risk of bias** if participant outcomes were measured prior to the intervention and there were no important differences between groups.
- **High risk of bias** if important differences were present at baseline and not adjusted for in the analysis.
- **Unclear risk of bias** if no baseline measurements are presented.

Were baseline characteristics similar?

- **Low risk of bias** if baseline characteristics of the study and control providers were reported and similar.
- **High risk of bias** if there was no report of characteristics or if there were differences between control and intervention providers.
- **Unclear risk of bias** if baseline characteristics were mentioned but no data were presented.

ITS studies will then be appraised using the EPOC Group's tool for assessing risk of bias in ITS studies across the following domains (EPOC 2013).

Was the intervention independent of other changes?

- **Low risk of bias** if there are compelling arguments that the intervention occurred independently of other changes over time and the outcome was not affected by confounding variables and historic events.
- **High risk of bias** if the intervention was not independent of other changes at the time.

Was the shape of the intervention effect (i.e. change in slope or change in intercept) pre-specified?

- **Low risk of bias** if point of analysis was the point of intervention or a rational explanation as to why the point of analysis was not the point of intervention was provided. If appropriate, this will also include an explanation if the point of analysis was not the point of intervention.
- **High risk of bias** if the point of analysis was not the point of intervention and no rational explanation was provided.

Was the intervention unlikely to affect data collection?
The scale contains eight items across the domains of selection, comparability, and exposure for the assessment of case-control studies and eight items across the domains of selection, comparability, and exposure for the assessment of NRTs that have no specific tool.

Finally, as the biggest risk of bias in NRTs is selection bias, we will follow the guidance in the *Cochrane Handbook of Systematic Reviews of Intervention* (Reeves 2011), and have identified a priori the following potential confounding factors:

- **Low risk of bias** if the intervention itself was unlikely to affect data collection (i.e. sources and methods of data collection were the same before and after the intervention).
- **High risk of bias** if the intervention itself was unlikely to affect data collection (i.e. changes in source or methods of collection were reported).

We will attempt to obtain any missing information related to 'risk of bias' (e.g. details on randomisation, omitted data) from study authors. Particular weight will be given to selection bias as it may have the greatest detrimental effect on interpreting results (Chan 2005). We will present the results of these assessments in 'Risk of bias' tables, alongside a rationale for the judgement and this information will be considered when interpreting results.

**Measures of treatment effect**

As per the recommendations of the *Cochrane Handbook of Systematic Reviews of Intervention* (Higgins 2008), we will not combine results for NRTs and RCTs in the meta-analysis. However, if the different types of NRTs do not appear heterogeneous in terms of the following: characteristics of their populations (i.e. age, gender, prior involvement in violent behaviour); interventions (i.e. intensity or additional components to the social development programme, such as mentoring or diversionary activities); or methods (i.e. outcomes and duration of follow-up), we will combine data for NRTs in a separate analysis.

The effect size used in the review will be dependent upon study design and outcome (i.e. dichotomous or continuous). In the case of RCTs and non-equivalent comparison group designs, we will calculate a pooled odds ratio (OR) together with a 95% confidence interval (CI) for dichotomous outcomes. Where overall results are significant, we will calculate the number needed to treat for an additional beneficial outcome (NNTB) or harmful outcome (NNTH) to produce one outcome by combining the overall OR with a range of assumed control risks. For continuous outcomes, we will use the standardised mean difference (SMD) when studies use different outcome scales measuring a similar construct (e.g. scales measuring level of aggression). Where studies use the same outcome scale, we will use the mean difference (MD).

For ITS studies, we will use changes in slopes and changes pre- and post-intervention as a measure of effect. If this information is not available, we will extract data from the original papers (or requested from study authors if this is not possible) and then reanalyse using the methods recommended by Ramsay 2003 for inclusion of ITS studies in a systematic review. This will involve using a time series regression (adjusting for autocorrelation) to first examine the difference between pre- and post-intervention regression lines, and second to estimate changes in slopes.

To explain statistical heterogeneity between studies and to examine the influence of age and sex, we will conduct a Bayesian meta-
regression.

**Unit of analysis issues**

We will categorise statistical comparisons on primary and secondary outcomes into one of four specific follow-up periods (immediately after the intervention; between one and six months; between six and 18 months; up to five years) and conduct meta-analyses within these categories. Additionally, as it is plausible that outcomes may be measured at a community or an individual level, we will categorise outcomes according to level and conduct meta-analyses separately.

**Cluster-randomised trials**

We may identify studies that randomise groups of individuals to the same intervention. If trials are identified that used clustered randomisation, we will assume that study investigators have presented their results after appropriately controlling for clustering effects (e.g., using robust standard errors or hierarchical linear models). If it is unclear whether a cluster-randomised trial has used appropriate controls for clustering, we will contact the corresponding author of the study for further information. In instances where appropriate controls have not been used, we will request individual participant data and reanalyse using multilevel models controlling for clustering. We will perform met-analyses of effect sizes and standard errors in Review Manager 5 (Review Manager 2012) using the generic inverse method (Higgins 2008). In order to avoid unit-of-analysis errors, where possible, analysis will be made at the level of the individual while accounting for clustering in the data. Where cluster-randomised trials are included in any analyses along with individually randomised trials, we will adjust their sample size using the methods described in the Cochrane Handbook for Systematic Reviews of Interventions (Higgins 2011) and Klar 2001, using an estimate of the ICC derived from the trial (if possible) or from another source. If ICCs from other sources were used, we will note this and carry out sensitivity analyses to investigate the effect of variation in ICC. We will synthesise the findings from individually randomised and cluster-randomised trials provided there is little heterogeneity between the study designs, and the interaction between the effect of intervention and the choice of randomisation unit is considered unlikely.

**Multi-arm trials**

In the case of multi-arm studies, all intervention groups will be mentioned in 'Characteristics of included studies' tables; however, only the intervention groups directly relevant to the review will be included in the analysis. If we identify any studies containing more than two relevant groups, we will combine the data from the multiple intervention and control groups into single intervention and control groups to make single pair-wise comparisons, as recommended by Higgins 2011. However, if this prevents investigation of potential sources of heterogeneity, we will analyse intervention groups separately against a common control group. In order to ensure that data from a comparator group does not over-contribute to a particular meta-analysis that includes data from each of its other intervention groups, we will reduce comparator data proportionately (Higgins 2011). For example, where studies include two intervention groups and one comparison group, we will halve data from the comparator group for both the numerator (i.e. the number of events) and the denominator (i.e. the number of study participants in the group).

**Dealing with missing data**

AG will contact study authors to request missing or unreported data on group means, standard deviations, details of attrition, and details of interventions received by the control group. Furthermore, if the study only provides data for participants who completed the study or who followed the protocol (i.e. did not include data on participants who did not adhere to the intervention or who were withdrawn), AG will contact study authors to obtain the missing data, to ensure all analyses are conducted in accordance with ITT principles. We will carefully describe all remaining missing data, attrition rates, and details of the intervention for each study in the 'Risk of bias' tables, and will conduct a sensitivity analysis to assess the potential for bias in the primary meta-analysis (Higgins 2008). We will discuss the extent to which the results might be affected by missing data in the 'Discussion' section of the review.

**Assessment of heterogeneity**

Due to the inclusion of NRTs and the nature of the interventions, we expect to find variation in both study design and intervention type. Therefore, we will group together studies that appear homogeneous in terms of study design and intervention type prior to data analysis. We will then assess heterogeneity within these groups by comparing the distribution of important participant characteristics (e.g. age, gender, country of origin, socioeconomic status) and study characteristics (i.e. randomisation concealment, blinding, losses to follow-up, additional components to the social development programme) between studies. We will examine the resulting forest plots for heterogeneity and perform a Chi$^2$ test. As the results of Chi$^2$ test can be dependent on the number of studies, we will also assess statistical heterogeneity using the $I^2$ statistic, which quantifies inconsistencies, as recommended by the Cochrane Handbook for Systematic Reviews of Interventions (Higgins 2011).

**Assessment of reporting biases**

If we identify more than 10 studies, we will create a funnel plot with intervention effect on the horizontal axis and the standard error on the vertical axis (Higgins 2011). An asymmetrical plot
may indicate publication bias; however, it may also indicate true heterogeneity (i.e. the size of effect size differs according to study size) or poor methodological quality of studies (Egger 1997). We will inspect plot asymmetry visually at first and then conduct formal tests for funnel plot asymmetry. For continuous outcomes, we will use the test proposed by Egger 1997, and for dichotomous outcomes, we will use the test by Harbord 2006 if between-study heterogeneity is low or the test by Rückert 2008 if heterogeneity is high.

**Data synthesis**

As we expect heterogeneity in methodology between NRTs and RCTs, and in accordance with both The Cochrane Collaboration's (Higgins 2011) and The Campbell Collaboration's (Shadish 2004) recommendations, we will separate NRTs and RCTs in the main meta-analysis. As detailed in the Assessment of heterogeneity, when the interventions are the same or sufficiently similar in 1. type of outcome assessed, 2. methodology (i.e. study design, randomisation method, time points), and 3. intervention components (i.e. whether the social development classes have a mentoring or diversionary activities component), we will synthesise results in a meta-analysis. Only those interventions that we consider to be conceptually similar in purpose will be combined in the analysis (i.e. social development programmes that have a mentoring or diversionary activity component will be analysed separately). We will carry out the statistical analyses using Review Manager 5 (Review Manager 2012). We anticipate that there will be considerable heterogeneity between studies in terms of the interventions and populations studied, and we will, therefore, use a random-effects meta-analysis for combining data. However, if we identify severe forest-plot asymmetry, we will conduct both fixed-effect and random-effects analyses, on the assumption that neither model is appropriate. If the fixed-effect and random-effects models do not agree, this will be reported. Following control and sensitivity analysis, we will calculate overall effect sizes and standard errors using the generic inverse variance method (Higgins 2011). As included studies may have both dichotomous and continuous measures for the same construct, we will convert the ORs from the dichotomous measure to an SMD as long as we can presume the underlying continuous measure has an approximately normal or logistical distribution (Higgins 2011). If this is not the case, we will conduct two separate analyses. If included NRTs are not sufficiently homogeneous, we will present the results in forest plots but the pooled estimate will be suppressed. Additionally, if suitable numerical data are not available (i.e. mean and standard deviations) or there is severe heterogeneity in either programme delivery, outcomes assessed or methodology and a pooled effect would not be reliable, then a meta-analysis will be considered inappropriate and we will provide only a narrative description of the study results, and we will make no general conclusions about the effectiveness of the intervention.

**Subgroup analysis and investigation of heterogeneity**

Dependent on the examination of heterogeneity and sufficient data, we will undertake exploratory subgroup analysis to assess whether effect size may vary according to the following factors:

1. age of study participants (participants in the lower age range of 12 to 18 year olds (12 to 15 year olds) versus in the higher age range of 12 to 18 year olds (16 to 18 year olds) as violence peaks at approximately 17 years (Office of the Surgeon General (US) 2001));
2. sex of study participants (male versus female as violence is more prevalent among males (Dahlberg 2002));
3. socioeconomic status (as the prevalence of violence is higher in more deprived areas (Dahlberg 2002));
4. intervention characteristics (i.e. duration (brief versus extended), components (i.e. social development classes or social development classes plus mentoring or social development class plus diversionary activities)).

We will use only primary outcomes in subgroup analysis. We will examine the forest plots of subgroup analyses visually to look at whether there is overlap of 95% CIs for the effects in different groups; non-overlapping CIs will be taken to suggest a difference between subgroups. We will also conduct formal statistical analyses to examine any possible differences between subgroups classifying whole trials by interaction tests as described in the Cochrane Handbook for Systematic Reviews of Interventions (Higgins 2011) and that are available in Review Manager 5 (Review Manager 2012). Results of interaction tests will be reported in the text.

**Sensitivity analysis**

As discussed in Dealing with missing data, we will perform sensitivity analyses to evaluate the effect of missing data on the results of the primary meta-analysis. The sensitivity analyses will be performed according to the following imputations. First, assuming that all missing data represented successful outcomes, and second assuming that all the missing data represented failure.

As we intend to include cluster-randomised trials, there may be heterogeneity between randomisation units so we will use a sensitivity analysis to investigate this. Additionally, if the ICC is obtained from an external study, we will analyse the effect of variation in ICC.

Due to the inclusion of NRTs, we will perform a sensitivity analysis to assess the effects of removing such trials, as, by their nature, they will be judged to be at high risk of bias.

In addition, we will carry out sensitivity analysis for primary outcomes by study quality. Studies will be divided into subgroups according to whether they are at low risk of bias as opposed to unclear or high risk of allocation concealment bias, attrition bias, and assessment bias to evaluate what impact this would have on the treatment effect. We will make the judgements on level of bias according to The Cochrane Collaboration's tool for assessing risk.
of bias as detailed in Assessment of risk of bias in included studies (Higgins 2011).

ACKNOWLEDGEMENTS

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Universal community-based social development interventions for preventing community violence by young people 12 to 18 years of age

(Protocol)

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Universal community-based social development interventions for preventing community violence by young people 12 to 18 years of age

Protocol

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CONTRIBUTIONS OF AUTHORS

All authors were involved in writing the protocol.

Anna Gavine and Damien Williams will assess titles and abstracts for inclusion, assess study quality, and extract data.

Steve MacGillivray will act as an arbiter if differences of opinion occur regarding study inclusion and quality assessment.

All authors will be involved in data analysis and interpretation, and writing the final review.

DECLARATIONS OF INTEREST

Two of the review authors (Anna Gavine and Damien Williams) are currently involved in an evaluation of a large community-based intervention for the prevention of violence in young people aged 11 to 18 in Glasgow, Scotland. The intervention, Glasgow’s Community Initiative to Reduce Violence (CIRV), has been set up, and is run, by the National Violence Reduction Unit (VRU). The initiative was developed following a scoping exercise by the VRU, who identified Cincinnati’s Initiative to Reduce Violence as being successful in the prevention of youth violence (Engel 2008) and adapted it to meet Glasgow’s needs (CIRV 2010). It is a separate piece of work from this review.

Anna Gavine’s institution received a studentship from Strathclyde Joint Police Board, to pay PhD university fees and stipend. Funds are available to her for travel expenses for data collection, and attendance at conferences and courses. This review is being undertaken at the same time as the PhD.

Damien Williams’s institution received funding from Strathclyde Joint Police Board for a PhD studentship to evaluate the under 16’s component of the Community Initiative to Reduce Violence in Glasgow’s East End. Funding was also awarded from the VRU (Scotland) for a Master’s studentship, to undertake an evaluation of an assets-based approach in Kilmarnock. They also provided funding for a review of interventions for knife crime offenders. The Scottish Institute for Policing Research and Wellcome Trust awarded funding to Damien’s institute for studentships to undertake an evaluation of components of the CIRV in Glasgow’s East End. Damien was awarded the Elizabeth Russell Prize in 2011 and received payment for this award.

Steve MacGillivray - none known.

SOURCES OF SUPPORT
Internal sources

- No sources of support supplied

External sources

- Strathclyde Joint Police Board, UK.
  Studentship to Anne Gavin's institution to pay PhD university fees, stipend, and travel expenses for data collection, and attendance at conferences and courses.
- Strathclyde Joint Police Board, UK.
  PhD studentship to Damian Williams's institution to evaluate the under 16's component of the Community Initiative to Reduce Violence in Glasgow's East End.
- Violence Reduction Unit (VRU) (Scotland), UK.
  Funding for a Master's studentship to undertake an evaluation of an assets-based approach in Kilmarnock. Also provided funding for a review of interventions for knife crime offenders.
- The Scottish Institute for Policing Research and Wellcome Trust, UK.
  Awarded funding to Damien's institute for studentships to undertake an evaluation of components of the Community Initiative to Reduce Violence (CIRV) in Glasgow's East End.