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# **Aggression Replacement Training (ART) for reducing antisocial behaviour in adolescents and adults.**

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## **PROTOCOL**

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# Table of contents

<b>TABLE OF CONTENTS</b>	<b>3</b>
<b>1 BACKGROUND</b>	<b>4</b>
1.1 Description of the condition	4
1.2 Description of the intervention	4
1.3 How the intervention might work	6
1.4 Why it is important to do this review	7
<b>2 OBJECTIVE OF THE REVIEW</b>	<b>9</b>
<b>3 METHODS</b>	<b>10</b>
3.1 Criteria for considering studies for this review	10
3.2 Search methods for identification of studies	11
3.3 Data collection and analysis	13
3.4 Data synthesis	18
<b>4 REFERENCES</b>	<b>20</b>
4.1 references	20
<b>5 FIGURES [IF ANY, OTHERWISE DELETE]</b>	<b>25</b>
<b>6 SOURCES OF SUPPORT</b>	<b>28</b>
6.1 Internal sources	28
6.2 External sources	28
<b>7 APPENDICES</b>	<b>FEL! BOKMÄRKET ÄR INTE DEFINIERAT.</b>
7.1 Heading 2	<b>Fel! Bokmärket är inte definierat.</b>
<b>8 TYPOGRAPHY HEADING 1</b>	<b>FEL! BOKMÄRKET ÄR INTE DEFINIERAT.</b>
8.1 Heading 2	<b>Fel! Bokmärket är inte definierat.</b>

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# 1 Background

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## 1.1 DESCRIPTION OF THE CONDITION

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Antisocial behaviour manifests itself in many different forms. A broad definition encompasses all actions that deviate significantly from established social norms. The type of behaviours included in this definition (e.g., substance abuse, theft, aggression) varies between countries (and many other terms are often used (offender, conduct disorder, delinquency) for the same behavior. Consequently there is no common metric to measure and synthesize prevalence and incidence of antisocial behavior.

Several approaches have been used to treat youth and adults with antisocial behaviour. Previous systematic reviews including meta-analyses suggest that cognitive-behavioral interventions (CBT) are among the more promising rehabilitative treatments for antisocial behaviour in youths (Lipsey et al 2001; Landenberger & Lipsey 2005; Armelius & Andreassen 2007). Within this approach, a number of prevention programmes have been developed to prevent and treat antisocial behaviour. Different cognitive-behavioral approaches have also been found to reduce recidivism with adult violent offenders, but rehabilitation programs for adults are still novel, and few published studies examine the recidivism outcomes of those who take part in such programs (Polaschek & Collie, 2004). Much attention has been paid to the importance of self-control in regulating antisocial, delinquent and criminal behavior and there have been several efforts to create techniques and programmes to improve self-control among particularly children and adolescents (Piquero et al 2010). Aggression Replacement Training (ART) is an example of such a programme.

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## 1.2 DESCRIPTION OF THE INTERVENTION

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The ART-method is a multimodal programme originally developed for aggressive delinquents in residential care in New York, USA. It is a structured programme that combines the use of techniques from cognitive therapy (based on cognitive theories) and behavioral therapy (from learning theory). The main components include anger management, development of social skills and moral reasoning. According to the original manual (Goldstein, Glick & Rainer 1987) ART is a 10-week, 30 –hour intervention, administered to groups of 8-12 youths three times a week. According to the developers (Goldstein et al 1987), aggression has an affective component, a behavioral component and a values component. Thus, ART came to consist of skillstreaming to teach prosocial behaviour (behavioral component), anger control (affective component), and moral reasoning (cognitive component). It is an educational and training approach to replace the antisocial behavior by actively teaching the desirable behaviours (Goldstein et al 1987). Goldstein and his colleagues encouraged the extension and modification of the program to new settings, client groups and outcomes (Goldstein et al 2004). The programme is now available in more or less revised forms for other forms of antisocial behaviour and populations for example adult violent offenders. The programme was for example selected for national implementation in English and Welsh probation service and progressed through their offending behaviour programme accreditation prior to implementation (Lipton et al 2000). The programme has also been implemented and accredited in the Swedish prison and probation service (Kriminalvarden. Retrieved, October, 27, 2010. Available at; <http://www.kriminalvarden.se/sv/Fangelse/Arbete-klientutbildning-och-behandling/ART/>).

### *Skillstreaming*

The behavioral component of ART consists of social skills training, a technique for teaching pro-social behaviour to participants who are weak or lacking these competencies. The teaching of skills serves to displace the out of control destructive behaviours with constructive prosocial behaviour. Theoretically the method is founded in social learning theory and the work of Bandura (1973).

The manual provides a skill checklist of 50 desired skills to identify skills that the participants are missing, which the program then should be focusing on. Flexibility exists to substitute some skills for others according to needs. Each skill is broken down into its behavioral components which are modelled by the trainers and role-played by each trainee during the session. Some skills are complicated like understanding other people's feelings, while others are less complicated like preparing yourself for a difficult conversation (Goldstein et al., 1987).

### *Anger management*

The anger control training component of ART is designed to help make the arousal of anger in chronically aggressive persons less frequent and to provide means to learn self-control when anger is aroused. Just as skillstreaming is designed to teach what one should do in problematic situations, anger control training teaches what one should not do. The anger control training has its foundation in the early anger control work of Novaco (1975) and Meichenbaum (1977).

Anger control training is a multistep sequence in which trainees are first helped to understand how they typically perceive and interpret the behaviour of others in ways that arouse anger. Therefore, in the first lesson, attention is given to identify the external and internal triggers that initiate the anger. The self-control sessions identify triggers and likely consequences of anger and aggression. The self-awareness of triggers and arousing feelings of anger is then used to develop alternative prosocial strategies. The trainer demonstrates the proper use of anger reduction techniques like deep breathing and backward counting,

### *Moral reasoning*

Moral reasoning training is the third component in ART (Gibbs et al 1995). It has its foundation in Kohlberg's (1973) model of moral development with the purpose to raise the individual's level of moral reasoning in order to make more mature decisions in social situations.

In ART moral reasoning is promoted in group discussions of moral dilemmas (social decision making meetings). Basically the trainer presents a moral dilemma where the participants can choose between different alternatives. The trainees choose one position each, motivate individually and discuss with one another. The manual provides ten problem situations designed to create opportunities for participants to consider the perspectives of others.

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## **1.3 HOW THE INTERVENTION MIGHT WORK**

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Goldstein and colleagues argued that aggressive behavior and other forms of antisocial behaviour can be traced to three factors, general shortfall in pro-social behaviour (personal, interpersonal and social-cognitive skills), and low level of anger control and an immature, egocentric style of moral reasoning. The skill streaming component of ART aims to develop social skills which form pro-social behavior. This emotion-oriented section of the programme aims to equip the individual with self-control to manage anger and aggression. The third component of ART addresses the concrete and egocentric thinking typically seen in those who

display aggressive behaviour. The developers claim that these components together provide a programme that will help the participants to function pro-socially (Goldstein et al 1998).

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#### **1.4 WHY IT IS IMPORTANT TO DO THIS REVIEW**

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A number of outcome studies of Aggression Replacement Training have been conducted in the US (Barnoski, 2004; Coleman et al 1992) and in Europe (Hatcher et al 2008; Gundersen & Svartdal, 2006; Moynahan, 2005; Holmqvist et al 2005). Several studies have indicated promising results for the method for example when it comes to recidivism. An outcome evaluation from Washington State showed for example that, when completely delivered, ART has positive outcomes with estimated reductions in 18-month felony recidivism of 24 percent, compared to the control group. Most of the studies were conducted by the programme developers (Goldstein et al 1987; Goldstein et al 1994; Leeman et al 1993).

ART-trials have been included in meta-analytic reviews of effects of a wider array of interventions with juvenile offenders (e.g., Lipsey & Derzon 1998; Armelius et al 2007). Most of them do not report separate results for ART and no review has specifically addressed the programme. Lipsey et al (2007) do report separate results for ART and according to the analysis ART shows positive effects compared to control groups when it comes to recidivism (reported OR > 5). Two independent studies are included. Results of ART outcome studies have also been summarized in non-systematic reviews. Several reviewers suggested that ART is a promising empirically-based treatment for juvenile offenders (Springer et al 2003; Loeber et al 1998). Other reviews conclude that ART is an effective programme (Sherman et al 2002; Cigno & Bourn 1998). The U. S Department of Justice claim in their model program Guide that ART is an effective program (OJJDP Model programs Guide. Retrieved, October 27, 2010. Available at [http:// www.ojjdp.gov/mpg/mpgProgramDetails.aspx?ID=292](http://www.ojjdp.gov/mpg/mpgProgramDetails.aspx?ID=292)).

Aggression Replacement Training is one of the oldest prevention programmes. Since the 1990s it has been provided across North America and Europe within a wide variety of social, educational, correctional services, secure units, community services and prisons. Teachers, counselors, youth care workers, social workers, and

correctional officers are examples of people who become trainers. There have been claims for the effectiveness of the intervention but few studies of ART made by independent researchers have been included in meta-analysis and reviews despite the fact that test searches show that several studies made by independent researchers do exist. No review includes ART for adults. Hence, a systematic review of ART as a separate program should be an important issue of concern for researchers, policy and for practice.

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## 2 Objective of the review

To assess the impacts of ART in residential care and community settings for reducing antisocial behaviour in young and adult people.

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## 3 Methods

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### 3.1 CRITERIA FOR CONSIDERING STUDIES FOR THIS REVIEW

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#### 3.1.1 Types of studies

To be included studies should be experimental, where individuals or groups are randomly assigned to conditions, or quasi-experimental with use of parallel cohorts. Analysis of the absolute effects of ART will involve comparing ART to no treatment and to untreated wait list controls. The relative effects of ART (versus other interventions) will be conducted separately and will include studies that compare ART to other interventions and/or Treatment-As-Usual (TAU). Studies that compare ART with ART and additional components/treatment will be excluded. All follow-up durations reported in the primary studies will be recorded. Both standardized and unstandardized measures will be acceptable measures.

In order to assess whether the evaluator can be regarded as independent, internet searches will be made for each author involved in the included study. An independent evaluator cannot have vested interests in the intervention (e.g. economic or psychological as a developer or program proponent). In other words, the independent evaluator should be “...free of any real or perceived bias introduced by receipt of any benefit in cash or kind, any hospitality, or any subsidy derived from any source that may have or be perceived to have an interest in the outcome...” (Higgins 2008, Box 2.6a).

#### 3.1.2 Types of participants

Participants are males and females (12 years and older) with antisocial behaviour. Both participants in residential care (including prison, secure and open settings) and community settings will be included. Voluntary, mandated as well as sentenced participants will be included.

### **3.1.3 Types of interventions**

Since ART has no trademark, the practical application varies. For inclusion in the review only studies that label the programme they are evaluating “Aggression Replacement Training” (ART) is to be included. Further, the authors have to refer to Goldstein and include a statement that the core principles in the programme are being followed.

### **3.1.4 Types of outcomes**

#### **Primary outcomes**

Any recidivism in antisocial behaviour (criminal behaviour, drug-use, school attendance) that is measured in the studies will be considered.

#### **Secondary outcomes**

Other measures of behaviour (for example social skills, interpersonal skills and anger management), cognition (e.g., moral reasoning) and psychiatric symptoms on standardized tests, for example Social Skills Rating System (Gresham & Elliott 1990). Measures based on unstandardized tests will only be considered if documented psychometric properties are reported.

Analyses will be made for different follow-up periods depending on available data; immediately post-intervention, short-term (up to one year post-intervention and long term (over one year post intervention).

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## **3.2 SEARCH METHODS FOR IDENTIFICATION OF STUDIES**

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### **3.2.1 Search strategy**

Searches will be conducted in electronic reference databases, government databanks and professional websites. There are no restrictions regarding language or date of publication. To identify unpublished reports and ongoing studies, ART- developers and independent investigators will be contacted. Reference lists of included studies

and all reviews will be scanned for new leads. Once a final set of included documents is defined, this list will be sent to lead authors of included studies, together with inclusion criteria, in order to find out if any documents are missing, including grey literature. Conference papers (at least as titles and abstracts) are crucial when publication bias is to be assessed. The following reference databases will be searched:

- ASSIA
- Cochrane (CDSR, DARE, TRIALS, HTA)
- Campbell Library
- Criminal Justice Abstracts
- Proquest Dissertation & Thesis
- ERIC
- Pub Med
- PsycINFO
- Sociological Abstracts
- Social Work Abstracts
- Social Policy and Practice (which includes Social Care Online).

Additional searches will be made by means of *Google* and *Google Scholar* and going through the first 100 or 200 hits.

Search strategies has been developed by using various terms for aggression replacement, aggression control therapy, aggression prevention, positive peer culture, equipping youth to help one another, EQUIP program, Prepare curriculum, PEACE curriculum, Family ART, etc. We will also search for studies including descriptors/keywords describing the three components of ART; the affective component, the behavioral component and the values component. The synonyms from the three categories will be combined with “OR” in every category and with “AND” between categories (see **Table 1 and 2** for examples of search syntaxes for Pub Med and PsycInfo).

It must be emphasized that syntaxes will be modified and tailored for each database and provider. Each tailored search will include controlled terms, terms from a thesaurus or an index (depending on database and database provider), as well as

free-text terms. A set of articles, that needs to be visible, will be used in order to validate each tailored search.

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### **3.3 DATA COLLECTION AND ANALYSIS**

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#### **3.3.1 Selection of studies**

Two reviewers will independently screen titles and abstracts. Selection of primary studies will be made according to criteria described above. Studies considered eligible by at least one of the reviewers on the basis of titles and abstracts, will be retrieved in full text. The full texts will then be appraised by two reviewers. The same persons will decide whether the studies meet the inclusion criteria. Any disagreements about eligibility will be resolved by the review team. Reasons for exclusion will be documented for each study that is retrieved in full text.

More specifically, the selection process will have the following steps (the process will be documented by means of EndNote software and finally stored in RevMan 5.0):

1. Pairs of reviewers will independently select potentially-eligible studies for full-text retrieval on the basis of the inclusion criteria by considering the Title, Abstract, and Subject Terms for each document. A study will be retrieved in full text if reviewers disagree about its potential eligibility. The results will be stored in an EndNote database “Abstract screening”.
2. Pairs of reviewers will independently read documents in full and decide to include or exclude the document on the basis of the inclusion criteria. If reviewers disagree, a third reviewer will have a decisive vote. Primary reasons for exclusion will be documented. The results will be stored in a second EndNote database “Full text inclusion - preliminary”.
3. The complete list of included documents will be sent to a selected group of external international experts together with the inclusion criteria. These experts will be asked whether they know of any eligible studies that are missing studies. They will also be asked if they know of any reasons why any of the included studies should be excluded. Finally they will be asked if they know of any other documents (published or not) on the included studies that could be informative during step 4.
4. Any suggestions from the external international experts will be processed in accordance with step 2 above. The results will be stored in a third EndNote database “Full text inclusion - final”.

5. The final set of included documents will be studied in order to find multiple publications from the same study and multiple studies in single publications. The purpose is to select a set of unique studies. The problem of multiple publications from single studies will be approached by looking at the site and time frame of the evaluation, the intervention, the number of participants in experiment and control groups respectively, etc. The results will be stored in a fourth EndNote database and in RevMan 5.0 “Included unique studies”.

### **3.3.2 Data extraction and management**

Guided by the checklist of items to consider in data collection and data extraction detailed in the Cochrane Handbook (Higgins 2008, Table 7.3.a), at least two independent coders will extract data and store the data in a table in Word-format focusing on populations, interventions, comparisons, and outcomes as basic coding categories (see Table 3). Differences in coding will be resolved by discussion and when not possible a third author will be adjudicated. When necessary the corresponding author of studies will be contacted.

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

Methodological quality and risk of bias regarding included studies will be assessed independently by at least two reviewers on the basis of the revised CONSORT statement and checklist for randomized controlled trials (Altman 2001) and the Cochrane Handbook (Higgins 2008, section 8). Included studies will be assessed on adequate sequence generation, allocation concealment, outcome assessors, incomplete outcome data, selective outcome reporting, and other sources of bias, in a risk of bias table. In all cases, an answer ‘Yes’ indicates a low risk of bias, and an answer ‘No’ indicates a high risk of bias. ‘Unclear’ will indicate an unclear or unknown risk of bias. Given the nature of the method it is unlikely that providers and participants in the intervention can be blinded; quality of blinding will be determined primarily by whether those who assessed and coded outcome measures were blind to treatment conditions. Since studies using quasi-experimental methods will be included information about baseline differences and attempts made to control for them will be examined. Details of each study will be provided in additional tables.

### 3.3.4 Measures of treatment effect

If means and standard deviations are available continuous data will be analyzed to calculate effect size (e.g., from t-tests, or exact p-values). Hedges  $g$  will be used for estimating standardized mean differences (SMD) where scales measure the same clinical outcomes in different ways (e.g., psychiatric symptoms).

If there is a mix of studies with some reporting change scores and others reporting final values, we will contact authors and request the final values. If we do not obtain these values, we will analyze change scores and final values separately (Higgins 2008, chapter 9, section 9.4.5.2).

For dichotomous outcomes we will calculate risk ratios (Relative Risk) with 95 % confidence intervals and p-values. In order to calculate a common metric odds-and risk ratios will be converted to effect size (Chinn 2000). As pointed out in Higgins (2008, section 9.6) there are statistical approaches available which will re-express odds/risk ratios as standardized mean differences allowing dichotomous and continuous data to be pooled together. Even if effect sizes cannot be pooled, study-level effects will be reported in as much detail as possible. Software for statistical analyses will be RevMan 5.0 and STATA 10.0.

### 3.3.5 Unit of analysis issues

The authors will take into account the unit of analysis of the trials to determine whether individuals were randomized in groups (i.e. cluster randomized trials), whether individuals may have undergone multiple interventions at once, whether results were reported at multiple time points, and whether there were multiple treatment groups. The robustness of transforming continuous and dichotomous outcomes into a common metric will be assessed by analyzing each outcome measure separately.

#### *Cluster randomized trials*

It is possible that participants will be randomized to groups in clusters, either when data from multiple participants in a setting are included (creating a cluster within the residential or community setting), or when participants are randomized by treatment locality or clinic. For trials that use clustered randomization, results will be presented with proper controls for clustering (robust standard errors or hierarchical linear models). If appropriate controls are not used and it is impossible

to obtain the full set of individual participant data, the data will be controlled for clustering using the procedures outlined in Higgins (2008). That is, when outcome measures are dichotomous, the number of events and number of participants per trial arm will be divided by the design effect  $[1 + (1 - m) * r]$ , where  $m$  is the average cluster size and  $r$  is the intra-cluster correlation coefficient (ICC). When outcome measures are continuous, the number of participants per trial will be divided by the design effect, while leaving the mean values unchanged. To determine the ICC, the reviewers will use estimates in the primary trials on a study-by-study basis. However, where these values are not reported, the reviewers will use external estimates of the ICC that are appropriate to each trial context and average cluster size by contacting the trial lists and if they are not available, the reviewers will seek statistical assistance from the Cochrane/Campbell Methods Group (Higgins 2008).

#### *Multiple interventions per individual*

If the participants in some of the trials receive ART plus treatment as usual, those studies will be meta-analyzed separately, with the ART plus treatment as usual arm compared to treatment as usual alone. The discussion of those results will take into account the additional treatments received.

#### *Multiple time points*

When the results are measured at multiple time points, each outcome at each time point will be analyzed in a separate meta-analysis with other comparable studies taking measures at a similar time point post-intervention. These will be grouped together as follows: immediately post-intervention, short-term (up to one year post-intervention) and long term (over one year post intervention).

#### *Studies with multiple treatment/control groups*

For trials where there are multiple treatment/control groups, data from the same group will not be analyzed twice. Thus, multiple contrasts from the same study will not be pooled in the same meta-analysis.

The treatment condition will be selected for meta-analysis according to which ones match the inclusion criteria. The comparison condition will be treatment-as-usual, the least active treatment offered, or, if neither of those is an option, the condition that is most focused on the participant in treatment sessions.

When there is more than one control group, we will do pair-wise comparisons. This means that effects will be calculated on the basis the following types of comparisons:

A vs. B, A vs. C, and B vs. C. When A is a defined intervention and C is a no-intervention or placebo-type of intervention, we will use the term “effect”. Yet if A is a defined intervention and B also is a defined intervention, then we will use the term “improved effect” in comparison to the control intervention. When “usual care” is a sufficiently described intervention, we will use the term improved effect, and if “usual care” means no intervention “effect” will be used. In cases when “usual care” is not defined, additional information from the authors or other reliable sources will be used.

### **3.3.6 Dealing with missing data and incomplete data**

Missing data and dropouts will be assessed in the included studies. Reasons, numbers, and characteristics of dropouts will be investigated and reported. Efforts will be made to contact the authors when further information or data are necessary. Any meta-analyses will use data from all original participants when possible, and will report when that is not the case. For studies in which the missing data are not available, a sensitivity analysis will be used to assess potential bias in the analysis and the extent to which the results might be biased by missing data will be discussed.

Although we will seek any important but unreported data from the authors of the original studies, it is sound to assume that this approach is not always successful. As a consequence of this, we will also consider utilizing the imputation methods outlined in White & Higgins (2009).

### **3.3.7 Assessment of heterogeneity**

Heterogeneity among included studies will be examined through the use of the  $\chi^2$ -test, where a low p-value ( $p < 0.1$ ) indicates heterogeneity of treatment effects. The  $I^2$  statistic will also be used to determine the percentage of variability that is due to heterogeneity rather than sampling error or chance (Higgins 2008). The authors will also consider issues such as design quality, publication bias, voluntary or mandatory participation, and differences in participant’s characteristics as possible reasons for any heterogeneity and conduct sensitivity analyses accordingly, where data permit. If heterogeneity is present we will investigate possible sources using the following steps; subgroups analyses, meta-regression and sensitivity analyses.

### 3.3.8 Assessment of reporting biases

Funnel plots will be drawn to investigate any relationship between effect size and standard error when possible. While the visual appraisal of funnel plots will assist us in gaining an understanding of the nature of the data, it is also appropriate with a less subjective appraisal of the evidence for funnel plot asymmetry (Sterne & Egger, 2005). As a consequence of this, we will run a number of statistical tests for funnel plot asymmetry (e.g. Egger's linear regression method).

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## 3.4 DATA SYNTHESIS

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Meta-analysis will be used when event rates or means and standard deviations are available or can be calculated and studies include similar populations (e.g. similar age range, criminal history, setting, etc.), methodology (e.g. randomization, measurements, time points, etc.), and outcome measurements. Thus, when interventions, control groups, participants, and outcomes are sufficiently similar, pooled effects will be calculated. In other cases, when these conditions are not met, results will be shown but not pooled (Higgins 2008, section 16.5.4). When the  $\chi^2$ -test or the  $I^2$  statistic indicates heterogeneity, a random effects meta-analysis will be used. Where studies appear to be homogeneous according to known characteristics and those statistics, a fixed effects model will be used. When meta-analysis is inappropriate, a narrative description of the study results alone will be provided, although general conclusions about the effectiveness of ART would not be possible in that case.

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

#### *Subgroup analysis and investigation of heterogeneity*

Subgroups analyses will be made of ART with different subpopulations which include:

- Service setting (juvenile justice, prisons, and open care)
- Duration of observation period
- Counterfactual condition (“usual services”, other treatment)
- Older versus younger participants.

Since previous research indicate that studies conducted by program developers produce significantly more positive results than those conducted by independent

researchers (Petrosino & Soydan, 2005; Shadish et al 2002) we will also analyze independence from program developers. We will assess results from RCTs separately from results of quasi-experimental studies and separate analyses will examine studies that support intent-to-treat analysis. The analyses will be conducted to study variations in effect sizes between studies.

### 3.4.2 Sensitivity analysis

In order to assess the robustness of the conclusions, sensitivity analyses will be conducted to assess the impact of the quality of the included studies. The quality criteria used in the analyses will be as described above. The analysis will include:

- Comparing results from studies with inconsistencies in the definition, measurement, or reporting of results (e.g. differential attrition, dropouts, lack of intention to treat analysis, outcome measures not taken at consistent time points for all participants) with results from consistent studies.
- Most methods for dealing with missing outcome data requires detailed data for each participant. Since only limited information is typically available in published reports we will focus on the case of incomplete binary outcomes (White & Higgins 2009)
- Reanalyzing the data using different statistical approaches (e.g. using a fixed effects model instead of a random effects model (Higgins 2008) and multivariate meta-analysis (White 2009).
- Independence from program developers will also be considered in the analyses (Petrosino & Soydan, 2005; Shadish et al 2002).

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## 5 Tables

<b>Table 1: Pubmed 100317, Aggression Replacement Training (ART) Hanna Olofsson (information specialist) &amp; Catrine Kaunitz (lead reviewer)</b>		
<b>Nr</b>	<b>Search terms</b>	<b>Numbers of ref. **)</b>
1.	aggression replacement training[tiab] OR aggression replacement therapy[tiab] OR aggression replacement treatment[tiab]	2
2.	Aggression control therapy[tiab]	1
3.	aggression prevention[tiab]	9
4.	Positive peer culture[tiab] OR equipping youths to help one another[tiab] OR equipping youth to help one another[tiab]	0
5.	prepare curriculum[tiab] OR (PREPARE AND prosocial curriculum[tiab]) OR PEACE Curriculum[tiab] OR (PEACE[tiab] AND arthur Goldstein[tw]) OR (Family ART[tiab] AND Goldstein[tw])	1
6.	1 OR 2 OR 3 OR 4 OR 5	11
7.	Social Behavior[MAJR] OR Social Skills Training[tiab] OR Prosocial Behavior[tiab] OR Social Skills Training[tiab] OR Social Skills[tiab] OR prosocial skills training[tiab] OR Skillstreaming[tiab] OR self-management training[tiab]	57740
8.	Anger[MAJR] OR Aggression[MAJR] OR Behavior Control[MeSH] OR Anger Control[tiab] OR anger treatment[tiab] OR Self Control[tiab] OR Aggressive Behavior[tiab] OR Anger management[tiab] OR Aggression control[tiab] OR Self-regulation Skills[tiab]	29311
9.	Moral Development[MeSH] OR Morals[MAJR] OR Moral education[tiab] OR moral reasoning[tiab] OR moral judgement[tiab]	14788
10.	7 AND 8 AND 9	35
11.	6 OR 10	<b>46</b>

**Table 2: PsycInfo via EBSCO 100317****Aggression replacement Training (ART)****Sökning gjord av Hanna Olofsson & Catrine Kaunitz**

Söknr	Termtyp *)	Söktermer	Antal ref. **)
1.	TI	TI "aggression replacement" or AB "aggression replacement" or SU "aggression replacement" or MJ "aggression replacement" or DE "aggression replacement" or KW "aggression replacement" or TX "aggression replacement" or TC "aggression replacement"	55
2.		TI "aggression control therapy" or AB "aggression control therapy" or SU "aggression control therapy" or MJ "aggression control therapy" or DE "aggression control therapy" or KW "aggression control therapy" or TX "aggression control therapy" or TC "aggression control therapy"	5
3.		TI "aggression prevention program*" or AB "aggression prevention program*" or SU "aggression prevention program*" or MJ "aggression prevention program*" or DE "aggression prevention program*" or KW "aggression prevention program*" or TX "aggression prevention program*" or TC "aggression prevention program*"	9
4.		TX "Positive peer culture" OR AB "equipping youths to help one another" OR TI EQUIP OR AB "EQUIP program" OR AB "EQUIP programme"	46
5.		1 OR 2 OR 3 OR 4	112
6.		DE "Social Skills Training" OR DE "Prosocial Behavior" OR DE Social Skills Training OR DE "Social Skills"	14492
7.		prosocial skills training OR Skillstreaming	30
8.		6 OR 7	14501
9.		DE "Anger Control" or DE "Self Control" OR DE Aggressive Behavior	22735
10.		"Anger management" OR "Anger control strategies" OR "Anger control training" OR "Aggression control" OR "Self-regulation Skills"	1077
11.		9 OR 10	23321
12.		DE "Moral Development"	4856
13.		"Moral education" OR "moral reasoning training" OR "moral judgement"	1268
14.		12 OR 13	5518
15.		8 AND 11 AND 14	16
16.		15 OR 5	<b>124</b>

\*)

Table 3: Basic categories for extracting data

Study identification				
Reviewer notes	Population	Interventions	Control	Outcomes
	<p>1) <b>Target population</b></p> <p>2) <b>Eligibility criteria</b></p> <p>3) <b>Sample description</b></p> <p>4) <b>Timeframe</b></p> <p>5) <b>Context:</b> Local, State, Country</p> <p>6) <b>Other</b></p>	<p>1) <b>Intervention</b> ART</p> <p>2) <b>Control 1</b> Usual Care, Placebo, no intervention, etc</p> <p>3) <b>Control 2</b> Usual Care, Placebo, no intervention, etc</p> <p>4) <b>Control n</b> Usual Care, Placebo, no intervention, etc</p> <p>5) <b>Other</b></p>	<p>1) <b>Randomization procedure</b></p> <p>2) <b>Intervention sample</b> (a) Original: <math>N_1=</math> (b) Final: <math>n_1=</math> (c) Attrition at follow up</p> <p>3) <b>Control sample</b> (a) Original: <math>N_2=</math> (b) Final: <math>n_2=</math> (c) Attrition at follow up</p> <p>4) <b>Blinded detection</b></p> <p>5) <b>Adherence</b></p> <p>6) <b>Intention-to-treat analysis</b></p> <p>7) <b>Conflicts of interest (authors)</b></p> <p>8) <b>Overall methodological quality</b></p> <p>9) <b>Other</b></p>	<p>1) <b>Results and conclusion</b> Results favors...</p> <p>2) <b>Primary outcomes:</b> measures of recidivism in antisocial behaviour</p> <p>3) <b>Data</b> (a) counts and proportions (b) means and standard deviations</p> <p>4) <b>Calculated effect sizes</b> (a) Relative Risk (b) SMD</p> <p>5) <b>Secondary outcomes:</b> other measures of behavior (e.g. anger management)</p> <p>6) <b>Secondary outcomes:</b> measures of cognition</p> <p>7) <b>Secondary outcomes:</b> measures of psychiatric outcomes</p> <p>8) <b>Other</b></p>

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## 6 Sources of support

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### 6.1 INTERNAL SOURCES

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### 6.2 EXTERNAL SOURCES

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