

Do the victims of school bullies tend to become depressed later in life?

A systematic review and meta-analysis of longitudinal studies

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Abstract

Purpose – *The purpose of this paper is to investigate the extent to which bullying victimization in school predicts depression in later life and whether this relation holds after controlling for other major childhood risk factors.*

Design/methodology/approach – *As no previous systematic review has been conducted on this topic, effect sizes are based on both published and unpublished studies: longitudinal investigators of 28 studies have conducted specific analyses for the authors' review.*

Findings – *The probability of being depressed up to 36 years later (mean follow-up period of 6.9 years) was much higher for children who were bullied at school than for non-involved students (odds ratio (OR) = 1.99; 95 per cent CI: 1.71-2.32). Bullying victimization was a significant risk factor for later depression even after controlling for up to 20 (mean number of six covariates) major childhood risk factors (OR = 1.74; 95 per cent CI: 1.54-1.97). Effect sizes were smaller when the follow-up period was longer and larger the younger the child was when exposed to bullying. Finally, the summary effect size was not significantly related to the number of risk factors controlled for.*

Originality/value – *Although causal inferences are tentative, the overall results presented in this paper indicate that bullying victimization is a major childhood risk factor that uniquely contributes to later depression. High quality effective anti-bullying programmes could be viewed as an early form of public health promotion.*

Keywords *Bullying, Schools, Adults, Depression*

Paper type *Research paper*

Introduction

Scientific interest in the problem of childhood bullying and its negative short- and long-term effects emerged after the well-publicized suicides of three Norwegian boys in 1982, which were attributed to severe peer bullying (Olweus, 1993). Later research has shown that the prognosis of children who bully and are bullied is not encouraging. When these types of childhood behaviour are not dealt with, they may spiral out of control in adolescence and adulthood, affecting not only the persons themselves but also their future social relations (Ttofi and Farrington, 2008, 2010). An effort to stamp out bullying in childhood can allow individuals at risk to lead normal well-adjusted lives rather than exhibit behavioural and emotional problems in later life.

Children who bully generally do not grow up to be well-adjusted individuals contributing positively to society, as shown in a recent systematic review on the association of school bullying perpetration with later criminal offending based on longitudinal studies (Ttofi *et al.*, 2011). For children who are the targets of bullying, outcomes also seem to be undesirable according to a recent narrative review (Arseneault *et al.*, 2010). However, the authors rightly

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state that the majority of studies on this topic are cross-sectional or have a retrospective longitudinal design. Well-controlled prospective research is required to address the long-term impact of bullying victimization on health measures and also to address the unique contribution of this risk factor across time.

To date, there has been no systematic review or quantitative synthesis of the results of existing longitudinal research on bullying victimization versus later health problems. The current manuscript addresses this gap in the research literature by:

- conducting a systematic review and meta-analysis based on longitudinal studies relating victimization to later depression; and
- including unpublished research on the topic.

We have contacted the principal investigators of a large number of longitudinal studies across the world and initiated new data analyses on this topic.

Objectives of the review

Our objective was to conduct a systematic review of the link between bullying victimization and later depression and calculate standardized effect sizes with the final aim of:

- Establishing the extent to which there is a significant association between bullying victimization and depression later in life.
- Analyzing the unique contribution of school bullying victimization as a risk factor for later depression (i.e. the predictive efficacy across time after controlling for other childhood risk factors).
- Establishing what covariates (e.g. length of follow-up period; number of risk factors controlled for; ages of assessment of bullying victimization and depression) are significantly related to and might explain variability in effect sizes.

Searches

Extensive searches were conducted in 63 journals and 19 electronic databases. In addition, we contacted numerous longitudinal researchers on school bullying and developmental criminology. Further details (e.g. names of journals, key words used in databases, etc.) can be found in a report which is being prepared for the Swedish National Council for Crime Prevention (Ttofi *et al.*, in preparation).

Eligibility criteria for inclusion or exclusion of a study

The criteria for inclusion of reports in this review were as follows:

- The report clearly indicates that it is concerned with school bullying victimization and not with other more general forms of peer victimization.
- A clear measure of depression as an outcome measure is presented.
- The report presents longitudinal data. Subsequently, some papers based on longitudinal studies were excluded because analyses were based on data within one wave, making them essentially cross-sectional in character (Barbarin, 1999; Grills, 2003)*[1].
- We also included follow-up/intervention studies since various bullying prevention programmes targeted both bullying and health-related problems such as depression and anxiety. In this case, we sent e-mails to evaluators of each programme, asking for specific data analyses for the control group which did not receive the intervention. We did not ask for data analyses based on the experimental children because we were concerned that, in the case of efficacious interventions, a reduction in bullying might subsequently be followed by a reduction in health outcomes. Specifically, we asked the evaluators of the programmes to examine whether bullying victimization at the baseline (i.e. before the implementation of the programme) predicted depression at the follow-up

period (i.e. after the implementation of the programme) for the control group only. Other published papers also followed our analytical approach (Fekkes *et al.*, 2006)*. Various evaluators of anti-bullying programmes were responsive (e.g. Dorothy Espelage for the Multimedia Violence Prevention Study[2]; Caroline Hunt for the Confident Kids Programme; Christina Salmivalli for the KiVa Programme; and Rolf Sandell for the SET Project) while others were not able to provide results for the requested data analyses (e.g. the S.S.GRIN Programme, the Beyond Bullying Secondary Programme and the Ongoing Programme[3]).

- Chronologically, the predictor (i.e. bullying victimization) precedes the outcome (i.e. depression). Subsequently, the Shelley (2009)* study and the Moon *et al.* (2008)* study were excluded because of this requirement.
- Study participants are school-aged children in the community and exposure to bullying specifies school years[4]. We did, however, include retrospective studies, in which the study participants are adults and in which a retrospective measure of exposure to bullying is related to outcome measures of interest (i.e. concurrent depression).
- The report has quantitative data that allow calculation of an effect size. For example, the study by Carlisle and Rofes (2007)* was based on qualitative data and was excluded.
- We included published and unpublished reports of the literature including books and book chapters (Olweus, 1993, 1994)*, journal articles, Masters or PhD theses (Blais, 2008; Grills, 2003; Parada, 2006; Singer, 2002; Taylor, 2006)* and conference presentations (Lösel *et al.*, 2008)*. As mentioned earlier, data were also obtained via e-mail communication with Principal Investigators of major longitudinal studies.

Some criteria for exclusion of reports were as follows:

- The predictor is bullying perpetration and not bullying victimization (Espelage *et al.*, 2001; Moffitt *et al.*, 2010[5]; Moon *et al.*, 2008)*.
- The outcome measure (i.e. depression) is part (i.e. a subscale) of a wider theoretical construct (e.g. anti-social behaviour) and effect sizes are not shown for the depression subscale (Farrington and Ttofi, 2011)*[6].
- Our outcome of interest (i.e. depression) is used as a moderator between bullying victimization and another health outcome (Hidaka and Operario, 2006; Nrugham *et al.*, 2008; Roeger *et al.*, 2010)*.

Combining effect sizes within a report

We used ORs as the measure of effect size. Where studies presented other statistics, these were converted into ORs. Within each manuscript more than one effect size could be reported. When choosing an appropriate effect size that would justify inclusion of a report in the meta-analysis, the following rules were set:

- Within a report, if different effect sizes were derived from self-, mother-, teacher-, peer-, and clinician-rated depression, these were combined into one effect size (Arseneault, 2011; Salmivalli, 2010; Shakoor *et al.*, 2011)*. We followed the same strategy for the predictor, i.e. bullying victimization (Salmivalli, 2010)*.
- If a general measure of depression (based on a composite scale) as well as any of the specific items (or sub-scales) were available within a report, then we chose to include the general measure in our meta-analysis, unless we were restricted by what was available in the study[7].
- If the same informant filled in two different instruments on depression which were mutually exclusive (i.e. one not being a subscale of the other), we have combined the relevant effect sizes (Bender and Lösel, 2011; Gladstone *et al.*, 2006)*. We did not find any studies where participants filled in two different instruments on bullying victimization.
- If within a manuscript an effect size was given separately for males and females (Kaltiala-Heino *et al.*, 2010; Nishino *et al.*, 2011; Shelley, 2009)* or for children at different

ages (Hemphill *et al.*, 2011)*[8], we combined the two measures. The same combining strategy was followed when a separate measure was presented for two or more follow-up periods (Nishino *et al.*, 2011; Ozdemir and Stattin, 2011)*. It would have been ideal if we could have examined possible changes in the magnitude of the effect size within each study for different follow-up periods, but not many studies provided this information. We did, however, include the length of the follow-up period in the moderator analyses.

- If for the same outcome measure different effect sizes were reported separately for each informant, but the manuscript also provided a combined measure across all informants, then we chose to report the latter combined measure (e.g. a combined parent-teacher-child depression score for the z-proso longitudinal study by Averdijk *et al.*, 2011)*. We followed the same rule for the predictor (i.e. bullying victimization), giving preference to a combined measure as opposed to a separate measure.

In the Appendix, we list the reports from each longitudinal study. Under the name of each study, we indicate the age of the participants when victimization and depression were measured as well as the number of covariates the authors controlled for when presenting the adjusted effect sizes.

Combining effect sizes across reports relating to the same longitudinal study

The possibility of obtaining more than one (published or unpublished) report relevant to the same longitudinal study could not be excluded. In the study diagram (see Appendix), the reader can see the number of reports/manuscripts relating to each longitudinal study. Some longitudinal studies were represented by one report while others were represented in the systematic review (but not in the meta-analysis) by up to a maximum of ten reports (e.g. the Finnish 1981 Cohort Study).

When different manuscripts relating to the same longitudinal study report discrete effect sizes (because of differences, for example, in the sample size or in the follow-up period that the authors have used), combination of effect sizes across reports is not straightforward as these effect sizes are based on dependent samples. These dependencies must be taken into account, as ignoring them will result in standard errors that are too small, often by a large degree. In this case, the meta-analyst would need to identify independent sets for analysis (Wilson, 2010)[9]. We did face this challenge in the current meta-analysis.

In our Swedish report (Ttöfi *et al.*, in preparation), we explain how we dealt with the issue of combining discrete effect sizes from different reports relating to the same longitudinal study (i.e. the issue of dependency of samples). To give an example, for the International Youth Development Study, we did not take into account the report by Patton *et al.* (2008)* because:

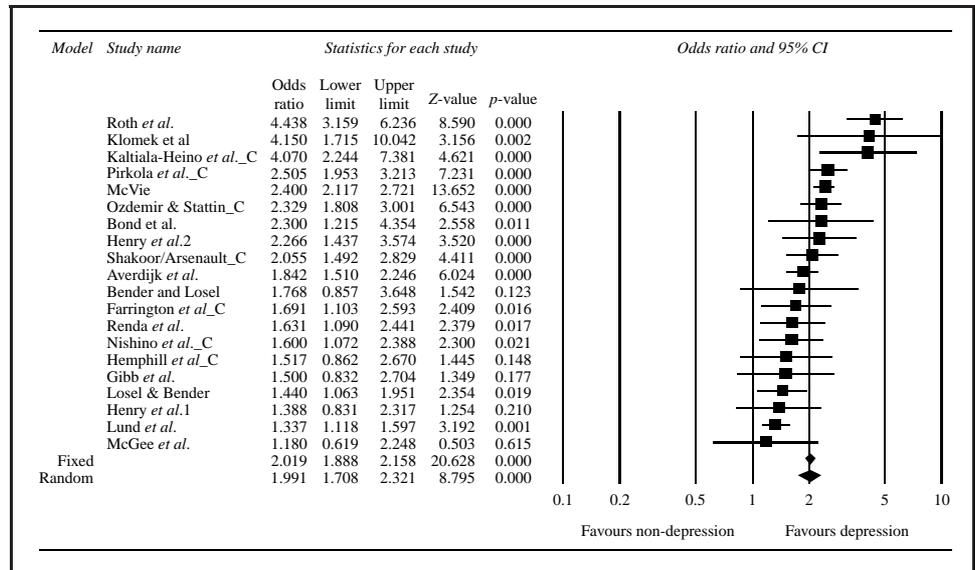
- only adjusted effect sizes are given; and
- primarily, a more recent study is available (Hemphill *et al.*, 2011)* in which both adjusted and unadjusted effect sizes are presented.

Results

School bullying victimization versus later depression: unadjusted and adjusted effect sizes

A total of 29 studies provided an effect size for bullying victimization versus depression. For nine of them (see Appendix), only an unadjusted effect size was available. As mentioned, the effect size that we used was the OR. The summary effect size across the 29 studies was OR = 2.06 (95 per cent CI: 1.79-2.38; $z = 9.23$) for the random-effects model. We used the random-effects model since the heterogeneity test, Q , of 121.41 was highly significant at $p = 0.0001$. When the nine studies with only unadjusted effect sizes were excluded, the summary effect size for the remaining 20 studies – for the random-effects model – was OR = 1.99 (95 per cent CI: 1.71-2.32, $z = 8.80$). Again, there was significant variability in effect sizes across these studies ($Q = 76.33$, $p = 0.0001$). The effect sizes for the majority of studies were significant, as shown in the forest graph in Figure 1.

Figure 1 Bullying victimization and later depression: unadjusted effect sizes

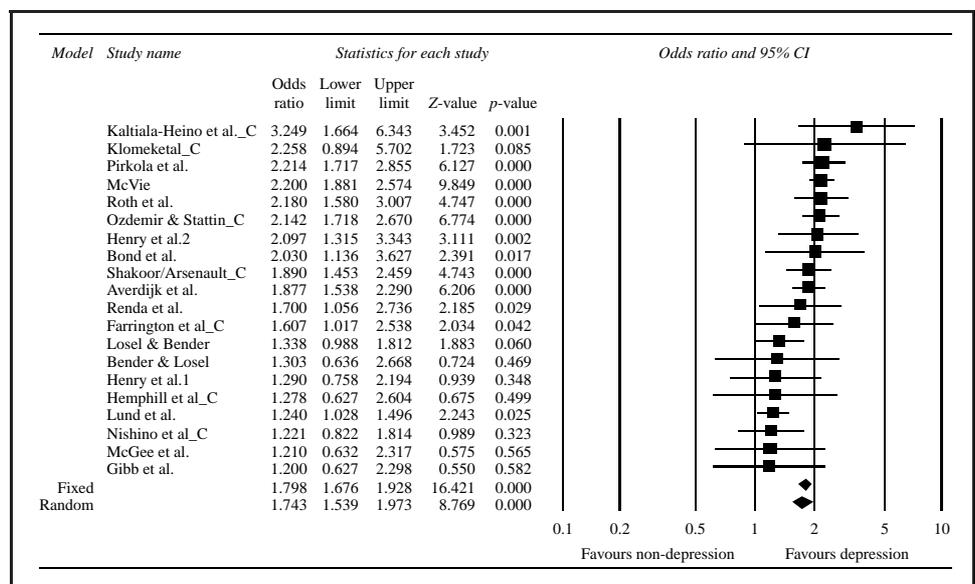


After controlling for covariates, the adjusted summary effect size was reduced to OR = 1.74, but this was still highly significant (95 per cent CI: 1.54-1.97, $z = 8.77$) and with marked precision as shown by the narrow confidence intervals[10]. This is a substantial effect. Assuming that a quarter of children were victimized and a quarter were depressed, OR = 1.74 corresponds to 33 per cent of victimized children being depressed, compared to 22 per cent of other children, a 50 per cent increase in risk. Figure 2 shows the forest graph for adjusted effect sizes. As with the previous forest plot, all the effect sizes were in the expected direction.

Meta-regression

For the adjusted summary effect size, various moderators were investigated to explain the heterogeneity in effect sizes across studies, which was significant ($Q = 45.57, p = 0.001$).

Figure 2 Bullying victimization and later depression: adjusted effect sizes



These included the number of covariates controlled for at baseline (range: 1-20; $M = 6.05$; $SD = 5.05$), the age at which school bullying was measured (range: 8.00-18.00; $M = 12.16$; $SD = 2.75$), the age of participants when depressive symptoms were assessed (range: 10.00-47.00; $M = 19.08$; $SD = 9.53$) and the length of the follow-up period, measured in years (range: 1.00-36.00; $M = 6.91$; $SD = 8.61$). Because $I^2 = 58.31$ per cent, most of the between-study variation reflects real differences rather than random error. Therefore, fixed effects meta-regressions were used.

The age at which bullying victimization was measured was significantly negatively associated with the effect size ($B = -0.028$, $SE = 0.012$, $p = 0.019$) and so was the age at which the outcome measure was taken ($B = -0.006$, $SE = 0.003$, $p = 0.036$). As expected, the length of the follow-up period was significantly negatively related to the effect size with a regression coefficient close to significance ($B = -0.006$, $SE = 0.003$, $p = 0.086$), suggesting (as with the previous coefficient) that the deleterious effects of bullying victimization decrease as time goes by. The relationship between the number of covariates controlled for and the effect size was not in the expected negative direction, but it was not significant ($B = 0.010$, $SE = 0.008$, $p = 0.213$).

Discussion

The results of our systematic review support that bullying victimization in school is not just a temporal and school-related problem but, instead, plays a vital part in the longer-term psychosocial development of the individual. Although not all primary studies showed desirable and significant results, the majority did. Our findings seem to be relatively robust because the fixed-effect and random-effects models revealed rather similar mean odds ratios. This may be due to the substantial sample sizes in many studies.

A key issue is whether school bullying victimization causes later depression or whether depressive individual characteristics enhance the risk of being bullied. The detailed mechanisms of such processes are not yet clear (Arseneault *et al.*, 2010; Lösel and Bliesener, 2003; Ttofi and Farrington, 2010). Although causal inferences should be treated with caution, the current meta-analysis indicates that school bullying victimization is a unique childhood risk factor for later depression, even after controlling for a large number of pre-existing risk factors (a sound strategy for non-experimental longitudinal designs; Murray *et al.*, 2009).

Our review clearly highlights the importance of intervening to save high-risk youth, specifically victims of school bullying. Bullying prevention programmes are effective (Farrington and Ttofi, 2009; Ttofi and Farrington, 2011) and financial support for the implementation of high-quality anti-bullying programmes is justified. These programmes can have longer term effects by reducing the future psychosocial maladjustment of the troubled individual (and associated health, welfare, education, and other costs). In light of evidence on the monetary value of saving a high-risk youth (Cohen and Piquero, 2009), an effective programme for school bullies and victims would have a high benefit:cost ratio.

In any case, the victims of school bullying are children in need. Intervention strategies aiming at tackling school bullying and promoting safer school communities can be seen as a moral imperative (Smith *et al.*, 2003). In addition, we recommend that more efforts should be made to implement effective programmes with individual bullies and victims, perhaps based on child skills training (Lösel and Beelman, 2003) and family-based programmes (Farrington and Welsh, 2003).

Implications for practice

- Exposure to school bullying is a unique childhood risk factor for later depression.
- Effective anti-bullying programmes should be promoted and could be seen as an early form of public health promotion.
- Given the health costs for offering support to depressed adults, anti-bullying programmes are likely to have a high benefit:cost ratio.

Notes

1. Throughout the paper, citations in parentheses indicated with an asterisk refer to manuscripts included in the systematic review but not necessarily in the meta-analysis. These citations are also shown in the Appendix. For the exact references, see Ttofi *et al.* (in preparation); they are not presented here because of space limitations.
2. The study shows specific results on school bullying and not just aggression. Dorothy Espelage provided the zero-order correlation coefficient for Time 1 bullying perpetration versus Time 2 depression for the sixth graders who were part of the control group (e-mail: December 3, 2010). Adjusted effect sizes could not be provided.
3. We were not able to find the e-mail address of Randie Taylor at all.
4. We have excluded the paper by Jordanova *et al.* (2007) after confirmation by Robert Stewart that exposure to bullying under "lifetime events" did not specify school bullying victimization (e-mail communication, January 13, 2011).
5. E-mail communication with Retate Houts, July 22, 2010.
6. Depression is part of the composite measure of life success.
7. For example, the Due *et al.* (2009) paper deals with depression based on Becks' (2001) Major Depression Inventory, but effect sizes of interest are shown based on a single item.
8. With the exception of those studies where younger and older children were based on different cohorts (Henry *et al.*, 2010).
9. E-mail communication with David B. Wilson (October 25, 2010).
10. Two studies (see Appendix) provided only adjusted effect sizes. Across the 22 studies, the adjusted summary effect size was OR = 1.77 (95 per cent CI: 1.57-1.99; $z = 9.50$); $Q = 51.83$, $p = 0.0001$.

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Appendix. 71 reports on depression from 49 longitudinal studies

(A). Included studies

Adolescent Mental Health Cohort Study (Kaltiala-Heino *et al.*, 2010); longitudinal prospective:

- Depression at age 17; bullying victimization at age 15; controlling for four covariates.

Australian Temperament Project (Renda *et al.*, 2011); longitudinal prospective (For depression, results were provided via e-mail communication with Jennifer Renda (July 16, 2010).)

- Depression at age 23.5; bullying victimization at age 13.5; controlling for 20 covariates.

Christchurch Health and Development Study (Gibb *et al.*, 2011); longitudinal prospective.

- Depression at age 23; bullying victimization at age 14; controlling for 16 covariates.

Confident Kids Program (Berry and Hunt, 2009); follow-up/intervention study (The authors have provided standardized regression coefficients for bullying victimization at baseline (before the implementation of the programme) versus depression at the follow-up for the control group only (e-mail communication with Caroline Hunt, May 26, 2010). Bullying victimization was a continuous variable.)

- Depression at age 13.21; bullying victimization at age 13.04; unadjusted effect size only.

Danish Longitudinal Health Behaviour Study, Young Cohort (Due *et al.*, 2009); longitudinal retrospective (Further to our e-mail correspondence (January 14, 2011), Pernile Due has agreed to provide adjusted effect sizes in due course. For the moment, we can only report the results presented in the published paper.)

- Depression at age 27; bullying victimization at age 15; unadjusted effect sizes only.

Danish Longitudinal Retrospective Study (Lund *et al.*, 2008); longitudinal retrospective.

- Depression at age 41; bullying victimization at age 18; controlling for two covariates.

Dutch Anti-bullying Programme (Fekkes *et al.*, 2006); follow-up/intervention study:

- Depression at age 10.5; bullying victimization at age 10; unadjusted effect sizes only.

Edinburgh Study of Youth Transitions and Crime (McVie, 2010); longitudinal prospective.

- Depression at age 14; bullying victimization at age 13; controlling for ten covariates.

E-risk Longitudinal Study (Shakoor *et al.*, 2011; Arsenault, 2011); longitudinal prospective (Adjusted effect sizes provided by Louise Arsenault via e-mail communication (January 21, 2011).)

- Depression at age 12; bullying victimization at age 9.25; controlling for five covariates.

Erlangen-Nuremberg Development and Prevention Study (Lösel and Bender, 2011); longitudinal prospective:

- Depression at age 13.7; bullying victimization at age 9; controlling for five covariates.

Erlangen-Nuremberg Study of School Bullying (Bender and Lösel, 2011; Lösel and Bliesener, 2003; Lösel *et al.*, 2008); longitudinal prospective:

- Depression at age 24.64; bullying victimization at age 15.54; controlling for three covariates.

European TMR Network Project (Singer, 2002); longitudinal retrospective:

- Depression at age 23.5; bullying victimization at age 11; controlling for four covariates; adjusted effect sizes only.

Finnish Cohort Longitudinal Study (Haavisto *et al.*, 2004; Klomek *et al.*, 2008; Sourander *et al.*, 2007, 2009); longitudinal prospective:

- Depression at age 18; bullying victimization at age 8; controlling for one covariate.

Gatehouse Project (Bond *et al.*, 2001); follow-up/intervention study (The authors have controlled for the implementation group):

- Depression at age 14; bullying victimization at age 13; controlling for five covariates.

Health 2000 Project (Pirkola *et al.*, 2005); longitudinal retrospective:

- Depression at age 47; bullying victimization at age 11; controlling for three covariates.

International Youth Development Study (Hemphill *et al.*, 2011; Patton *et al.*, 2008); longitudinal prospective:

- Depression at age 16.9; bullying victimization at age 14.4; controlling for eight covariates.

Japanese Longitudinal Study (Nishino *et al.*, 2009, 2011); longitudinal prospective:

- Depression at age 13.81; bullying victimization at age 12.5; controlling for four covariates.

KiVa Anti-bullying Programme (Salmivalli, 2010); follow-up/intervention study (Results given via e-mail communication with Christina Salmivalli (March 29, 2010)):

- Depression at age 10.5; bullying victimization at age 9.5; unadjusted effect sizes only.

Longitudinal Retrospective Study at the Mood Disorders Unit Outpatient Depression Clinic in Sydney, Australia (Gladstone *et al.*, 2006); longitudinal retrospective:

- Depression at age 43; bullying victimization at age 10.5; unadjusted effect sizes only.

Longitudinal Retrospective Study of Adult Twin Pairs (Gladstone and Parker, 2006); longitudinal retrospective:

- Depression at age 40.7; bullying victimization at age 8.5; unadjusted effect sizes only.

Longitudinal Retrospective Study of American University Students (Roth *et al.*, 2002); longitudinal retrospective:

- Depression at age 19.36; bullying victimization at age 12; controlling for one covariate.

Longitudinal Retrospective Study of Japanese University Students (Matsui *et al.*, 1996); longitudinal retrospective:

- Depression at age 19.4; bullying victimization at age 13.5; controlling for two covariates; adjusted effect sizes only.

Mater-University of Queensland Study of Pregnancy and Its Outcomes (McGee *et al.*, 2011); longitudinal prospective:

- Depression at age 20.9; bullying victimization at age 13.9; controlling for three covariates.

Metropolitan Area Child Study (Henry *et al.*, 2010); Study 1; longitudinal prospective (Results obtained via e-mail communication with David Henry (July 16, 2010). The two reports are based on two independent cohorts):

- Depression at age 10; bullying victimization at age 8; controlling for four covariates.

Metropolitan Area Child Study (Henry *et al.*, 2010); Study 2; longitudinal prospective:

- Depression at age 13; bullying victimization at age 11; controlling for four covariates.

Pittsburgh Youth Study (Farrington *et al.*, 2011); longitudinal prospective:

- Depression at age 14.27; bullying victimization at age 10.98; controlling for ten covariates.

SET Project (Kimber *et al.*, 2008a, b); follow-up/intervention study (Special data analyses results (only adjusted effect sizes) provided via e-mail communication with Rolf Sandell (e-mail: March 19, 2010)):

- Depression at age 14.5; bullying victimization at age 13.5; controlling for three covariates; adjusted effect sizes only.

Seven Schools Longitudinal Study (Ozdemir and Stattin, 2011); longitudinal prospective:

- Depression at age 14.49; bullying victimization at age 13.2; controlling for two covariates.

Six-month follow-up study in Canada (Shelley, 2009; Shelley and Craig, 2010); longitudinal prospective:

- Depression at age 11.5; bullying victimization at age 11; unadjusted effect sizes only.

Swedish Community Samples (Olweus, 1993, 1994); longitudinal prospective:

- Depression at age 23; bullying victimization at age 16; unadjusted effect sizes only.

z-proso Longitudinal Study (Averdijk *et al.*, 2011); longitudinal prospective:

- Depression at age 11; bullying victimization at age 8; controlling for 11 covariates.

(B) Excluded studies

Beyond Bullying Secondary Programme (March *et al.*, 2004; Parada, 2006; Parada *et al.*, 2008) (We were unable to find the address for correspondence of Roberto Parada. Various e-mails have been sent to Herbert Marsh since January 15, 2011, but we did not receive any response.)

Birth to Ten Longitudinal Study in South Africa (Barbarin, 1999).

British National Survey of Psychiatric Morbidity (Jordanova *et al.*, 2007) (Excluded further to e-mail correspondence with Robert Stewart (January 13, 2011).)

Cambridge Study in Delinquent Development (Farrington and Ttofi, 2011).

Dunedin Longitudinal Study (Moffitt *et al.*, 2010) (E-mail communication with Renate Houts, July 22, 2010).

Finnish Cohort Longitudinal Study (Kumpulainen and Rasanen 2000; Kumpulainen and Roine, 2002; Kumpulainen *et al.*, 2000, 2001; Klomek *et al.*, 2009; Sourander *et al.*, 2000).

Follow-up Study of Canadian Students (Blais, 2008).

Health Omnibus Survey in South Australia (Roger *et al.*, 2010).

Longitudinal Retrospective Study of English GBQ Men (Rivers, 2001, 2004; Rivers and Cowie, 2006) (Potentially includable upon further information obtained.)

Longitudinal Retrospective Study of Japanese GBQ Men (Hidaka and Operario, 2006).

Longitudinal Study in Korea (Moon *et al.*, 2008).

Multimedia Violence Prevention Study (Espelage *et al.*, 2001) (The study shows specific results on school bullying and not just aggression. Dorothy Espelage has provided the zero-order correlation coefficient for Time 1 bullying perpetration versus Time 2 depression for the sixth graders who were part of the control group (e-mail: December 3, 2010).)

Norwegian Follow-up Study (Nrugham *et al.*, 2008).

Owning Up Bullying Prevention Programme (Taylor, 2006) (We were unable to find the address for correspondence of the author and get in touch with him.)

Pilot Study of Adult Males in the USA, the UK and Australia (Carlisle and Rofes, 2007).

S.S.GRIN Intervention Study (DeRosier, 2004, 2007; DeRosier and Marcus, 2005) (E-mail communication with Melissa DeRosier, January 4, 2011.)

Three-year Follow-up Study in Australia (Rigby, 1999, 2001) (Bullying victimization, anxiety and depression were part of the baseline data. Questions on depression, but not on anxiety, were excluded from the follow-up period upon request from the schools.)

Two-year Follow-up of Virginia Students (Grills, 2003).

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