THE CAMPBELL COLLABORATION

# Protocol for a systematic review:

# School-based interventions for reducing disciplinary school exclusion: A systematic review

## Sara Valdebenito, Manuel Eisner, David Farrington, Maria Ttofi & Alex Sutherland

Submitted to the Coordinating Group of:

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| ☒ | Crime and Justice |
| ☒ | Education |
| | ☐ Disability |
| ☐ | International Development |
| | ☐ Nutrition |
| ☐ | Social Welfare |
| ☐ | Other: |

Plans to co-register:

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|---|---|---|---|
| ☐ | No | | |
| ☐ | Yes | ☐ Cochrane | ☐ Other |
| ☒ | Maybe | | |

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## I. BACKGROUND

Discipline problems are frequent in schools and they may have a harmful effect on pupils' learning outcomes. A lack of discipline and the subsequent potential increase in school disorder (e.g., bullying, substance use) can seriously threaten the quality of instruction that teachers provide, hamper pupils' acquisition of academic skills (e.g., low achievements or excessive referrals for special education) and subsequently reduce their attachment to the educational system (Sugai et al., 2000).

As such, discipline represents a serious concern for parents, head teachers and teachers, demanding significant efforts and resources from schools (Kaplan et al., 2002). The most recent PISA report (OECD, 2010) recognises that schools registering higher levels of disciplinary problems compel teachers to reduce time for learning while dealing with disciplinary issues. For instance, the Teaching and Learning International Survey (TALIS) pointed out that teachers spend at least 20% of lesson time dealing with disruption and administrative tasks. On a global average, 13% of teachers' time is spent maintaining order (OECD, 2009). In the United Kingdom, the Behaviour Survey 2010 states that 80% of school teachers felt their ability to teach effectively was impaired by students' poor behaviour (Massey, 2011).

Schools use different procedures to manage discipline, including a range of punitive responses (e.g., loss of privileges, additional homework or detention hours). Among them, exclusion is normally seen as one of the most serious punishments for offences. Although types and lengths vary from country to country, school exclusion (also known as school suspension)[1] can be broadly defined as a disciplinary sanction imposed in reaction to students' behaviour (i.e., violations of school policies) by the responsible authority. In concrete terms, exclusion entails a removal from regular teaching for a period of time during which students are not allowed to take part in classroom lessons or be present on school premises. Specifically, fixed-term exclusions consist of a limited number of hours or days (Cornell et al., 2011), whereas permanent exclusion involves the pupil being transferred to a different school or educated outside the regular education system (Spink, 2011; Webb & Vulliamy, 2004).

Even if school policies suggest the use of exclusion as a measure of last resort, reserved for only the most serious and persistent offences (Skiba & Peterson, 1999; Gregory & Weinstein, 2008; Skiba et al., 2012), research evidence suggests that behaviours provoking this type of punishment can also involve minor offences (Munn, Cullen, Johnstone, & Lloyd, 2001; Skiba, 2014). Fenning et al. (2012) provide a case in point: Their research concluded that suspension and expulsion were the most common types of punishment imposed as a response to minor problems such as tardiness and school truancy. These findings were also

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[1] In this document school exclusion and school suspension are used synonymously.

confirmed by Liu (2013) who found that 48% of suspensions lasting a maximum of 5 days targeted minor disorder or disruptive behaviours.

In terms of prevalence, data provided by the UK Department for Education (academic year 2011/12) show that in England fixed-term exclusion affects 3.5% of the school population whereas permanent exclusion reaches only 0.06%. The national figures suggest that students in secondary-level education (6.8% of the school population) as well as those in special education (14.7%) are the most likely to experience fixed-term exclusion (DfE, 2013).

International comparisons of exclusion prevalence rates are not available in the literature examined. Differences in use, extent and recording (i.e., unreported exclusions) make a global estimation challenging. However, a comparative analysis among a sample of high- and middle-income countries allows an overview of variations in exclusion names, types and lengths as well as in the accountable authority responsible for—and with control over—exclusions. Table 1 describes exclusion in ten different countries.

*Table 1: Comparative description of school exclusion in a sample of high- and middle-income countries.*

| Country | Name given | Type of exclusions | Length (for fixed exclusions) | Who takes the decision? |
|---|---|---|---|---|
| **Australia[2] (New South Wales)** | Suspension | Short suspensions | 4 school days. | School principal |
| | | Long suspensions | Up to 20 school days. | |
| | Expulsion | | | |
| **Chile[3]** | Suspension | Fixed. It should be implemented inside the school premises | The law does not limit the duration of fixed suspensions. Each school community should issue their own disciplinary code and define disciplinary sanctions and their duration. | Disciplinary board |
| **Colombia[4]** | Suspension | Fixed Definitive | Each school community should issue their own disciplinary code and define disciplinary sanctions and their duration. Normally fixed exclusion can last 3 days. | Discretionary |

[2] Information retrieved from "Suspension and Expulsion of School Students" New South Wales Government. Updated in October 2014 https://www.det.nsw.edu.au/policies/student_serv/discipline/stu_discip_gov/suspol_07.pdf

[3] http://www.supereduc.cl/. Additionally the information can be found in Torche & Mizala (2012)

[4] In Colombia, each school must define school exclusion length. This is established in the Ley General de Educación Nº 115, February 1994. http://www.mineducacion.gov.co/1621/articles-85906_archivo_pdf.pdf. Additional information can be retrieved from http://www.mineducacion.gov.co/1621/article-86906.html

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|---|---|---|---|---|
| **Costa Rica** | Retiro de Clases | Fixed Permanent | Up to 8 school days. | School Principal School Board |
| **England**[5] | Exclusion | Permanent Fixed (in-school, out-of-school) | 1–45 days per year. After 5 days of fixed out-of-school exclusion, the school must provide alternative education. | Discretionary Head teacher |
| **France**[6] | Exclusion | Temporary exclusion from the classroom Temporary exclusion from school Permanent exclusion | Maximum of 8 days. | Disciplinary board. The student should be represented on the disciplinary board. |
| **Finland**[7] | Exclusion | In-school exclusion and out-of-school exclusion with the obligation to provide education at home by schools. Permanent exclusion does not exist in the local laws. | In-school exclusion: 2 hours as a maximum. Out-of-school exclusion: No more than 3 months. It is a very infrequent measure. | Teacher and head teacher using a formal procedure. In cases of out-of-school exclusion, a personal plan of education must be provided and local social services should be informed. |
| **Norway**[8] | Exclusion | Fixed exclusion, expulsion for the rest of the year and loss of rights to education. | Primary education (level 1–7): Exclusion from specific lessons or for the rest of the day. Secondary education (level 8–10): Maximum of 3 days. Expulsion and loss of right are defined in the Educational Law but its use is extremely rare or null. | Head teacher in consultation with the pupil's teacher unless the local authority defines a different procedure. |
| **New Zealand**[9] | Stand-down | Stand-down | The student is removed from school for 5 school days in a term or 10 school days in a year. | School principal through a formal procedure that includes the obligation to inform the family, the Education Authority and the school board. |
| | Suspension | Suspension | The student is removed from school for no more than 7 days. Maximum of 10 days in a year. | |

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[5] In England, exclusions are regulated by the Education Act of 2002

[6] In France, school exclusions are regulated by the Code de l'education

[7] Basic Education Act 628/1998 (Amendments up to 1136/2010)

[8] LOV 1998-07-17 nr 61: Law on Primary and Secondary Education (The Education Act)

[9] In New Zealand, the guidance for suspensions is based on the Education Act of 1989 and the Education Rules 1999 (Stand-down, Suspension, Exclusion, and Expulsion)

| | | | | |
|---|---|---|---|---|
| **US, Washington DC**[10] | Suspension | Suspension (short-term and long-term) is a restriction from attending school or school activities. | Short-term suspension: Maximum of 10 consecutive days. Long-term suspension: More than 10 consecutive days. | The head teacher informally communicates short-term suspensions to the student/parents. Long-term suspensions and expulsion require a formal process (i.e., written notice by school district). |
| | Expulsion | Expulsion makes this restriction indefinite in time. | | |
| **US, Virginia**[11] | Suspension | Suspension (short-term and long-term) is a restriction from attending school or school activities. | Short-term suspension: 10 consecutive or 10 cumulative school days in a school year Long-term suspension: More than 10 school days but less than 365 calendar days. | Imposed by either the school principal, any assistant principal, or, in their absence, any teacher. Suspension should entail a formal process. Student must be heard. |
| | Expulsion | Expulsion makes the restriction last longer in time. | A student is not permitted to attend school within the school division and is ineligible for readmission for 365 calendar days after expulsion. | Imposed by a committee of the school board. Includes a formal process, written notice and appeal. |
| **US, Texas**[12] | Suspension | In-school suspension (e.g., use of seclusion units) Out-of-school suspension | In-school suspension lasts between 1 class and several days. Out-of-school suspension should be no longer than 3 days. | Low-level offences are dealt with on a discretionary basis (according to a defined code of conduct) by the school office or the designated administrator (usually the principal or vice-principal). Higher-level offences require mandatory removal from the classroom. Rules for a due process are defined. |
| | Expulsion | In the case of serious offences a student can be expelled from school. | At least 1 year Disciplinary Alternative Education Program (DAEP) for students removed for over 3 days (no maximum period provided). | |

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[10] In the US, procedures and definitions of school suspension vary among states. In this case we are exemplifying by describing the case of Washington State. For more details see www.k12.wa.us/Safetycenter/Discipline/pubdocs/Suspension-expulsion-rights.pdf

[11] See the specific section for Virginia, p. 10–16 in https://safesupportivelearning.ed.gov/sites/default/files/discipline-compendium/School%20Discipline%20Laws%20and%20Regulations%20Compendium.pdf

[12] See the specific section for Texas, p. 14–27 in https://safesupportivelearning.ed.gov/sites/default/files/discipline-compendium/School%20Discipline%20Laws%20and%20Regulations%20Compendium.pdf

The comparative data incorporated in the table above suggest heterogeneity in the application of exclusion among countries. For instance, in the US, Norway and England, educational systems distinguish between fixed and permanent exclusion. However, in some educational systems, such as Finland's, the law prescribes only fixed-term exclusions. In terms of length, England limits fixed-term exclusions to a maximum of 45 days per school year while New Zealand's legislation allows exclusions for a maximum of 10 days per year. On the other hand, it is important to note that in some countries (e.g., France), there are specific laws that define and regulate exclusion whereas in others (e.g., Chile and Colombia), the authority to define the length of the sanction is given to each school. We anticipate that these differences will be addressed in the proposed review.

## 1.1   Predictors of school exclusion

From a normative point of view, school exclusion is a punitive response for misbehaviour. In that sense, behavioural problems seem to be the most obvious predictor for exclusion. In fact, Reinke, Herman, Petras, & Ialongo (2008) illustrate the aforementioned role of problem behaviour in exclusion by conducting a latent class analysis. Participants in the subclass of boys exhibiting behavioural problems only (i.e., isolating other academic/learning difficulties) were almost 4 times more likely to be suspended ($OR$ = 3.42; $95\%CI$ 1.36–8.58; $p$ < .05) than their non-problematic peers. Similarly, Pas, Bradshaw, Hershfeldt, & Leaf (2010) found that after controlling by student, teacher, classroom, and school level covariates, the strongest predictor for out-of-school suspension was disruptive behaviour ($OR$ = 4.83 ; $95\%CI$ 4.10–5.68; $p$ < .05).

Despite the role of behaviour in school exclusion described above, research from the last 40 years suggests that it is not the unique or even the most prominent predictor. In fact, previous findings illustrate a more complex scenario where exclusion is disproportionately predicted by gender, ethnicity, age, economic background and special education needs (Yudof, 1975; Costenbader & Markson, 1998; Monroe, 2005; Nickerson & Spears, 2007; Noltemeyer & Mcloughlin, 2010; Mcloughlin & Noltemeyer, 2010; Skiba, Horner, May, & Tobin, 2011). In the following paragraphs we offer an overview of the role of these variables in predicting school exclusion.

### Gender as a predictor of exclusion

Data provided by the Department for Education in England (DfE) 2011/12 suggests that male pupils are around three times as likely to be punished by exclusion than female pupils (DfE, 2013). The same trend can be observed in the recent study published by Liu (2013) based on longitudinal data from 13,875 American students. The study reports the predominance of males being excluded, but recognises that the proportion of females excluded tends to increase from elementary (23.7%), to secondary (32.7%) and high school (35.2%). More specifically, Bowman-Perrott et al. (2013: 91), based on a sample of 2,597

pupils, concluded that the predominance of males in exclusion rates (OR = 2.28) was even larger in the case of pupils with learning disabilities (OR = 4.31).

### Ethnicity

Research outcomes suggest a clear and consistent disproportionality in the prevalence of ethnic minorities as a target for disciplinary exclusion (Gregory et al., 2010). In the US, different sources of data show that school exclusion overly affects minorities such as Afro-Caribbean (Noltemeyer & Mcloughlin, 2010), Latino (Skiba et al., 2011) and American Indian students (Gregory et al., 2010) in comparison with their white peers. In the UK, data from the DfE (2012) showed that: "The rate of exclusions was highest for Travellers of Irish Heritage, Black Caribbean and Gypsy/Roman ethnic groups. Black Caribbean pupils were nearly 4 times more likely to receive a permanent exclusion than the school population as a whole and were twice as likely to receive a fixed period exclusion". Notably, recent multivariate analysis points out that racial disproportionality in exclusion still remains significant after controlling by behaviour, number and type of school offences, age, gender, teacher's ethnicity and socio-economic status (Fabelo et al., 2011; Noltemeyer & Mcloughlin, 2010; Rocque & Paternoster, 2011; Skiba, Michael, Nardo, & Peterson, 2002).

### Age as a predictor of exclusion

The likelihood of being punished by exclusion increases with age, being more frequent during adolescence. In England, 52% of permanent exclusions are imposed on pupils aged between 13 and 14 (DfE, 2013). In the case of American students, results follow a similar trend. In fact, data reported by Liu (2013) pointed out that suspensions reach a peak in ninth grade (i.e., 14 to 15 years old). Also based on a sample of American students, Raush & Skiba (2004) concluded that the number of out-of-school suspensions was significantly higher in secondary schools when compared to elementary ones.

### Socio-economic status (SES)

Low SES has also been identified as a predictor of high rates of disciplinary exclusion. The UK Department for Education (DfE) (2012) compared the rates of exclusion by eligibility for free school meals (FSM). Those eligible for FSM were 4 times more likely to be punished by a permanent exclusion and around 3 times more likely to get a fixed-period exclusion than children who were not eligible. In the US, Nichols (2004), based on a sample of 52 schools (37,000 students), found a similar pattern but the correlation between FSM and exclusion was higher and more significant for pupils in middle school ($r$ = .84; $p$ <. 01) than for elementary ($r$ = -.12) or high school pupils ($r$ = .48). In Australia, Hemphill et al. (2010), using multilevel mixed-effects logistic regression ($N$ = 8,028 students), concluded that pupils settled in low SES neighbourhoods were exposed to higher rates of exclusion (8.7%) when compared with pupils in high SES areas (2.9%). However, the evidence still seems to be inconclusive in this respect. Recently, Skiba et al. (2012), using a multilevel approach, tested data from 365 schools and a total number of 43,320 students. They concluded that

when comparing those students eligible for free or reduced-cost lunch with their non-eligible peers, the first were more likely to get out-of-school exclusions (OR = 1.269). However, contrary to expectations, the eligibility for free or reduced meals resulted in a negative predictor of permanent exclusion (OR = 0.025).

### *Special educational needs (SEN)*

Although an increasing amount of research has focused on predictors of school exclusion, analysis on the role of SEN still seems to be limited. In 2007, Achilles et al. exceptionally differentiate the role of three different SEN, namely emotional/behavioural disorders (EBD), attention-deficit/ hyperactivity disorder (ADHD), and learning disability (LD). Higher rates of exclusion were more likely among those with EBD (OR = -1.49; $p <$. 001) compared with ADHD (OR = -2.58; $p < .001$) and LD (OR = -5.44; $p < .001$). Recently, Bowman-Perrott et al. (2013), using three waves from the Special Education Elementary Longitudinal Study (SEELS), confirmed that children identified with emotional or behavioural disorders (*OR* = 3.95) and attention-deficit or hyperactivity disorders (*OR* = 4.96) were more likely than children with learning disabilities (*OR* = 2.54) to get suspended or expelled from school. In the same direction, Sullivan, Van Norman, & Klingbeil (2014) confirmed the idea of differences among disabilities. In their conclusions, those presenting EBD were by far more at risk of exclusion (OR = 6.78) than those presenting other health impairments. In fact, when controlling for race and gender, this trend remained stable and significant.

### *1.2   Negative outcomes linked to school exclusion*

Supporters of zero tolerance policies have pointed out that the use of exclusion can persuade students to take account of their behaviour and limit the motivation for rule-breaking (Bear, 2012). However, most of the research has consistently documented the negative impact of these types of sanctions (Hemphill et al., 2006; APA Zero Tolerance Task Force, 2008; Sharkey & Fenning, 2012; Chin et al., 2012). In particular, previous research suggests that school exclusion is related to serious negative externalities on at least three dimensions of young people's development: behavioural, academic, and future social inclusion.

### *Behaviour*

Some literature related to the question of the relationship between exclusionary punishment and behaviour suggests that harsh punishments such as exclusion could result in a spiral into more defiant behaviour of students. Raffaele-Mendez (2003), for instance, found a moderate and significant correlation ($r = .39$) between out-of-school exclusion (grades 4 to 5) and subsequent exclusion (grade 6). Similarly, Theriot et al. (2010) found that pupils punished by in-school and out-of-school exclusion were slightly more likely to get the same punishment again ($OR_{in\text{-}school}$ = 1.25; $p < .001$; $N$ = 9706 and $OR_{out\text{-}of\text{-}school}$ = 1.32; $p < .001$; $N$ = 9706).

Using longitudinal data, Arcia (2006: 366) concluded that dropout was another behavioural consequence of exclusion. In fact, "43% of students who were suspended 21 or more days dropped out 3 years after their ninth-grade enrolment". Similarly, Cratty (2012: 649) found a positive correlation between out-of-school suspensions and dropout rates. In particular, "those who had an early record of multiple exclusions registered 60% dropout during high school" when compared with non-excluded students.

The use of exclusion, in turn, is linked with more serious behavioural outcomes such as antisocial conduct, delinquency and entry into the juvenile justice system. Longitudinal research carried out by Hemphill et al. (2006: 736) argues that "school suspensions significantly increased antisocial behaviour 12 months later, after holding constant established risk and protective factors ($OR$ = 1.5; $95\%CI$ 1.1-2.1; $p$ < .05; $N$ = 3655)". In terms of the involvement of school excludees in the criminal justice system, Costenbader & Markson (1998: 67) found significant differences between excluded students and those never excluded. In their view, "while 6% of the students who had never been suspended reported having been arrested, on probation, or on parole, 32% of the externally suspended subsample and 14% of the internally suspended subsample responded positively to this question. Males reported significantly more involvement with the legal system than did females". Meanwhile, Challen & Walton (2004), studying a population of boys in the criminal justice system, concluded that more than 80% had been previously excluded from school[13].

### *Academic achievements*

Evidence suggests that periods of exclusion may have detrimental effects on pupils' learning outcomes. Exclusion is accompanied by missed academic activities, alienation as well as demotivation in relation to academic goals (Brown, 2007; Michail, 2011). In particular, Hemphill et al. (2006) found that excluded pupils were slightly more prone to fail in the academic curriculum when compared with non-excluded students ($OR$ = 1.3, $95\%$ CI 1.1-1.5, $p$ < .01). Along similar lines, Arcia (2006) produced a longitudinal retrospective study regarding the associations between exclusions and achievements from fourth to seventh grade. After three years, non-excluded students displayed substantially higher reading achievement scores when compared with their non-excluded peers. In fact, seventh-grade students who were excluded 21 days or more achieved scores similar to fourth-grade students not excluded. Finally, Raffaele-Mendez (2003) added that those excluded were also less likely to graduate from high school on schedule.

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[13] The strong link between school exclusion and subsequent drop-out/entry into the juvenile justice system has been termed the "school-to-prison pipeline" (Snyder & Sickmund, 2006; Fenning et al., 2012; Chin et al., 2012 Christle et al., 2005; Nicholson-Crotty et al., 2009). It describes the escalating punitive consequences of harsh discipline in school and its exclusionary practices.

### *Future social inclusion*

Some studies have pointed out that young people excluded from school can also register a high risk of becoming "Not in Education, Employment, or Training" (NEET) in the future. In 2007, Brookes et al. stated that students who had been excluded, were 37% more likely to be unemployed during adulthood. Speilhofer et al. (2009) showed that among individuals who were in sustained NEET, the majority had experienced previous prior exclusions and truancy. More precisely, Massey (2011) argued that approximately one out of two excluded children will be NEET within two years of their exclusion.

Research has also illustrated the long-term implications of exclusion for society as a whole. In economic terms, the cost of excluding children from school can increase the demand on public resources. Although the literature on this matter is still limited, Brookes et al. (2007) produced a report regarding the costs of permanent exclusion in the United Kingdom. The analysis encompasses an estimation of costs for the individual as well as for the educational, health, social and criminal justice services. Overall the cost, in 2005 prices, of permanently excluding a student was estimated to amount to £63,851 per *year* to society.

While there is a stark link between the aforementioned negative outcomes and school exclusion, these should not be regarded as causal. Notwithstanding decades of research on school exclusion and its impact on later behaviour, we are still in an initial stage for testing causal associations in these matters. The association between exclusion and these negative outcomes may simply reflect underlying behavioural tendencies that lead to conduct problems, exclusion and poor outcomes later in life, namely the antisocial syndrome depicted by Farrington (1997). In fact, school exclusion and the behaviours described here as "negative outcomes" could be explained by the same underlying factors or personality traits characterising the syndrome.

Despite the lack of empirical support for a causal association, some criminological theories have been able to explain the connection between punishment and the reproduction of deviant behaviour. Labelling theory, for example, suggests that those punished (e.g., by exclusion) and labelled as "deviant" may start behaving in ways that conform with their newly formed self-image: For example, by being more limited in their interactions with integrated students, and shunning conventional social systems such as the school (Krohn, Lopes, & Ward, 2014: 179). Likewise, Sherman's defiance theory (1993) elucidates the circumstances in which punishment can evolve into more antisocial behaviour (i.e., defiance) instead of compliance with rules. In his view, punishment can increase the prevalence, incidence, or seriousness of future offending when offenders deny responsibility, and when they perceive sanctions as unfair, stigmatising and imposed by an illegitimate authority.

Finally, despite all these findings and the rationale around the negative outcomes linked to school exclusion, it is important to mention that, so far, there is no evidence demonstrating that exclusion is effective for improving school discipline (Skiba, 2014). What is more, in the

short term, exclusion seems to directly deny students' right to access school as well as reducing adult supervision for those who are most at risk of taking deviant paths, or most in need of teachers' support.


## 1.3  Intervention programmes

The prevalence of exclusion and its adverse correlated consequences has caught the attention of policy makers and programme developers. As a result, a range of interventions has been designed/implemented to improve school discipline. In the present review, we plan to include any type of school-based intervention aimed at reducing school exclusion as a punishment for inappropriate behaviour. Included interventions may be those targeting individual risk factors or school-related factors, as well as those using a more comprehensive strategy that includes parents, teachers, school administrators and also the community.

Interventions targeting individual risk factors include, for instance, cognitive-behavioural approaches such as *anger management programmes* or *skills training for children* (e.g., Humphrey & Brooks, 2006). Another type of intervention focusing on student behaviour or more precisely students' skills for conflict resolution are *restorative justice programmes* (e.g., Shapiro et al., 2002; Cantrell et al., 2007). In general, these interventions target motivated children and train them in practical skills to deal with anger, solve conflicts or become more assertive in social relationships. Such interventions are normally organised in a curriculum and implemented in schools during school hours. The curriculum involves a package of group or one-to-one sessions using a wide range of techniques such as instruction, modelling, role-play, feedback and reinforcement, among others (Gottfredson, Cook, & Na, 2012; Schindler & Yoshikawa, 2012).

At the classroom level, interventions may target *teachers' abilities in classroom management* (Pane et al., 2013). Essentially, the training for teachers encompasses instructional (i.e., guidelines for teaching rules, maintaining attendance) and non-instructional skills (i.e., group management techniques, reinforcing positive conduct, techniques to explain expected behaviour) aimed at improving the learning process, preventing misbehaviour and encouraging positive participation by pupils (Averdijk, Eisner, Luciano, Valdebenito, & Obsuth, 2014).

Some schools offer *mental health services* independently or via community agencies. Experienced clinicians are located in schools in order to contribute to the school through individual, group, and/or family therapy. Clinicians can also be available for teacher consultation on matters related to students' behavioural and emotional issues. All these interventions would target a reduction in out-of-school exclusion (Bruns et al., 2005).

Alternatively, *Comprehensive prevention strategies* target, students, families, teachers and school managers as well as the community as a whole (e.g., Bradshaw et al., 2012; Pritchard

& Williams, 2001; Flay & Allred, 2003; Snyder et al., 2010). A well-known comprehensive programme is the School-Wide Positive Behavioural Interventions and Supports (SWPBIS). The programme intends to provide support for positive conduct by building proactive school-wide disciplinary procedures (i.e., improving school climate and reducing problem behaviours). SWPBIS incorporates a multi-level approach (i.e., whole school prevention, group-based intervention for problematic pupils and personalised, tailored interventions for high-risk students). The basic elements of the programme are: i) building a school culture for both social and academic attainment, ii) early prevention of problem behaviour, iii) teaching social skills to all students, iv) using behaviour support practices, and v) actively using data for decision-making. Research reports promising results, although more and stronger evaluation designs need to be undertaken (Gottfredson et al., 2012; Maag, 2012).

## 1.4   *Previous reviews*

In 2013/14 we conducted a systematic search of reviews and meta-analyses assessing the effectiveness of school-based programmes for many different outcomes (Averdijk et al., 2014). The results suggested that there has been no previous meta-analysis aimed at assessing the effectiveness of interventions (i.e., different types of approaches) at reducing disciplinary school exclusion. Probably the most similar study is the one published by Burrell et al. (2003), who conducted a meta-analysis on the effectiveness of mediation[14] programmes in educational settings. Among many other outcomes, the analysis suggested that these interventions had a desirable effect ($r$ = -.287, $K$ = 17, $N$ = 5,706, $p$ < .05) on administrative suspensions, expulsions and disciplinary actions. However, in this meta-analysis the outcome of suspension was reported along with other disciplinary actions, and the study did not compare mediation with any other intervention (as proposed in the present meta-analysis). The authors also call for a cautious interpretation given the high heterogeneity of primary results. A similar type of analysis was followed by Durlak, Weissberg, Dymnicki, Taylor, & Schellinger (2011) and Gottfredson, Wilson, & Najaka (2002). In both studies, school exclusion was coded as an outcome, but the final meta-analysis did not report the impact of the intervention specifically with relation to the targeted outcome.

Likewise, Solomon et al. (2012) conducted a meta-analysis exclusively testing the effectiveness of a singular intervention, namely, the School-Wide Positive Behavioural Interventions and Supports (SWPBIS) programme. Despite a small number of included studies reporting data on exclusion, the review does not report effect sizes by measuring their increase/decrease. Rather, the review reports effect sizes on the reduction of office

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[14] Peer mediation programmes focus on the development of non-violent conflict resolution skills training to facilitate the achievement of agreements. Peer mediators act as neutral third parties, assisting other students in the resolution of interpersonal conflicts through non-violent means (Burrell et al., 2003; Daunic et al., 2000).

discipline referrals and problematic behaviour.

In addition, two narrative reviews have been recently produced regarding intervention reducing disciplinary exclusion. Spink (2011) explored qualitative, quantitative and mixed methods studies. Overall 10 reports were found. The review concluded that multiagency interventions were the most frequent and that they could have a positive effect on reducing exclusion of pupils who are at risk. As expected, the study did not report a meta-analysis of effect sizes. In 2012, Johnson produced another narrative review identifying programmes that may be an alternative for suspension and exclusion in school systems. The search strategies were not clear enough to allow replication and, again, the nature of the design does not allow the calculation of effect sizes.

### 1.4   What are the benefits of this systematic review and meta-analysis?

Despite a growing body of research on the negative side effects of exclusion, no previous meta-analysis based on a comprehensive systematic review has been conducted to synthesize previous evidence assessing the impact of school-based interventions for reducing disciplinary exclusion. The current review addresses this gap by meta-analysing results from existing published and unpublished studies, providing a statistical assessment of the overall effect of school-based interventions at reducing exclusion.

This meta-analytic investigation has clear implications for policy making. The results provided by the present study would produce a much-needed evidence base for school managers, policymakers and researchers alike. These results can contribute to tackling the adverse developmental, social and economic effects of school exclusion mentioned in the previous pages as well as potentially identifying alternative and less punitive ways to tackle school discipline.

## II. OBJECTIVES

The main goal of the present research is to systematically examine the available evidence for the effectiveness of different types of school-based interventions for reducing disciplinary school exclusion. Secondary goals relate to comparing different approaches (e.g., school-wide management, classroom management, restorative justice, cognitive-behavioural interventions) and identifying those that could potentially demonstrate larger and more significant effects.

We also aim (potentially) to run analysis controlling for characteristics of *participants* (e.g., age, ethnicity, level of risk); *interventions* (e.g., theoretical bases, components); *implementation* (e.g., facilitators' training, doses, quality); and *methodology* (e.g., research design).

The research questions underlying this project are as follows:

- Do school-based programmes reduce the use of exclusionary sanctions in schools?

- Are some school-based approaches more effective than others in reducing exclusionary sanctions?

- Do participants' characteristics (e.g., age, gender, ethnicity) affect the impact of school-based programmes on exclusionary sanctions in schools?

- Do characteristics of the interventions, implementation, and methodology affect the impact of school-based programmes on exclusionary sanctions in schools?

## III. METHODOLOGY

### 3.1 Criteria for including and excluding studies

### Types of study designs

We will include studies based on experimental designs or randomised controlled trials (RCT) with at least one experimental group (i.e., participants receiving the treatment) and one control group (i.e., comparison group). Participants should be randomly allocated to each condition.

The control condition in this review may involve, for instance, a control group with no intervention, a control group with intervention as usual, a wait-list control group or a placebo group. Trials involving clustered samples will also be included. However, we plan to

correct the combination of individually and clustered data (see the unit of analysis sub-heading).

We will also include quasi-experimental designs (QED) that involve both control group and pre-post test. To be included, the treatment and control group should be selected in a way that the effect of selection bias is statistically controlled. The design should report clearly the method used to ensure statistical equivalence, taking into account for instance major risk factors and demographic characteristics (e.g., Propensity Score Matching, Matching through Cohort Controls). Studies where there is a large difference between the treatment and control group at pre-test will be excluded as they will not help in distinguishing intervention effects from other effects (Piquero, Farrington, Welsh, Tremblay, & Jennings, 2008; Shadish, Cook, & Campbell, 2002).

Quasi-experimental studies based on one-group pretest-posttest design, repeated measures panel designs or the one-group post-test-only designs will be excluded from the present review.

In the final review, analysis and results based on RCT and QED will be reported separately. Since we plan to code effect size from primary studies indicating whether they have been adjusted for other covariates, when data allows, we will report adjusted and unadjusted effects in the final review separately.

### Types of participants

Included reports should sample a general population of students in primary and secondary schools irrespective of nationality, language, and cultural or socio-economical background. Samples from countries other than the UK will be included as long as they represent equivalent school levels.

By targeting primary and secondary schools, the sample will consist of children aged 4 to 18. However, we expect the bulk of studies to be targeting pupils aged about 10 to 15, where research suggests the largest number of exclusions takes place (e.g., Liu, 2013; Raush & Skiba, 2004; DfE, 2012).

Reports involving students who present special education needs, disabilities or learning problems but settled in mainstream schools will also be included.

Reports involving students with serious mental disabilities or those in need of special schools will not be included. The rationale for this decision rests in the idea that for the present review, pupils included should represent a general population of students.

Students in college or upper levels of education will be excluded from our review.

### Types of interventions

We understand as school-based all the interventions delivered in schools, supported by schools, or which have at least one component implemented in the school setting.

In the present review we will target school-based interventions aimed at preventing/reducing school exclusion or at least measuring exclusion rates as an outcome. Intervention can cover a wide range of psychosocial strategies, targeting individuals (e.g., students, teachers) or the whole school community (e.g., School-Wide Positive Behavioural Interventions & Supports, or SWPBIS). Types of intervention can include, for instance, those focused on instructing students to identify risky behaviours and expanding their alternatives for responding appropriately to risks or harms (e.g., Life Skills Training); interventions focusing on managing classrooms (e.g., rewards schemes), cognitive-behavioural treatment (e.g., anger management), counselling and social work, and mentoring programmes; interventions inspired by restorative justice principles (e.g., peer mediation, restorative conferences, restorative circles); and interventions targeting teachers' skills to improve the quality of their management in the classroom (see examples on page 52 -53, Appendix 2). Programmes combining some of these strategies will also be included, as in the systematic review developed by Wilson, Gottfredson, & Najaka (2001). Since there is no previous review analysing school-based prevention programmes for reducing exclusionary discipline, we want to keep a wide range of school-based interventions that could be effective for reducing exclusionary practices.

We will exclude studies where the intervention is not school-based or school-supported, or where exclusion is not measured. We plan to exclude interventions designed for children or adolescents who have committed a crime, namely specialised interventions aimed at reducing reoffending or reconviction (e.g., Reasoning & Rehabilitation). Those interventions will be excluded because they exceed the strategies used by schools to prevent exclusion and their levels of specialization make them not a priority for a general population of students. We will also exclude school-based prevention programmes targeting outcomes related only to students' physical health (e.g., AIDS/ HIV prevention programmes, prevention of pregnancy, programmes to develop healthy nutrition).

Different types of interventions will not be synthesised together. We anticipate that at least, interventions targeting individuals will be analysed independently of those more comprehensive in nature (i.e., school wide approaches).

### *Types of outcome measures*

Studies will be eligible if they address school exclusion as an outcome. As mentioned above, school exclusion is defined as an official disciplinary sanction imposed by an authority and consisting of the removal of a child from their normal schooling. This removal should happen as a reaction to student behaviour that violates the school rules or is illegal. School exclusion can be fixed or permanent depending on the country and it can be implemented on or off school premises. We will include a range of possible search terms for exclusion that incorporate different languages and terminology from several jurisdictions. In general all of

them will target one or both of the following disciplinary sanctions: i) fixed-term exclusions (e.g., in-school or out-of-school), and ii) permanent exclusions (i.e., expulsion). Analysis of these two different outcomes will be carried out independently.

We will exclude other disciplinary sanctions implemented in schools if they do not share the criteria described above. For instance, we will exclude disciplinary sanctions such as loss of privileges, extra work, break/lunch detention, and after-school detentions. They do not imply exclusion from school or exclusion from regular teaching hours and in that sense they are not covered by this review.

For any identified study that reports findings on school exclusion as an outcome, we will also code effects of the intervention on specific behaviour domains. We will be specially focused on internalising and externalising problem behaviour (Achenbach & Edelbrock, 1979; Achenbach, 1978; Farrington, 1989). The construct of externalising behaviour suggests a set of manifest problems that are exhibited in children's outward behaviour and reflect the child negatively acting on the external environment (Liu, 2004). In the research literature as well as in our review, externalising behaviour would involve defiant or delinquent behaviours (e.g., self reported crime or offences), hyperactive (e.g., ADHD) and aggressive behaviours (e.g., bullying, intimidation, harassment, fights). Internalising behaviour problems, by contrast, affect the individual's internal psychological environment. They would be expressed by inhibition (e.g., social withdrawal), anxiety (e.g., fear, shame) or depression (Eisenberg et al., 2001).

By coding secondary outcomes we aim to assess the extent to which reductions in problem behaviour are a mediator of treatment effects on school exclusion. Indeed, interventions may affect exclusion in two different ways. The first is by improving behaviour that led to an exclusionary measure. The second possibility is that behaviour stays the same but that the school develops an alternative strategy to deal with the disciplinary problems.

### Timeframe

Databases and journals will be searched from 1980 onwards with the aim of including more contemporary interventions or prevention programmes.

### Publications

To be eligible, studies can be either published or unpublished reports. Sources included would be book chapters, journal articles, government reports, and also academic MSc and PhD theses. Additionally, it is important to mention that when needed, some information would be obtained through email communication with the authors or researchers in charge of a given study (e.g., statistical results)

*Language*

Eligible studies can come from any country or be written in any language as long as the title, abstract and key words are written in English. The inclusion of non-English studies will also be contingent upon resources and availability of translation services.

## 3.2  Search strategy

The proposed review will intend to locate and retrieve the most complete collection of empirical studies (e.g., from different countries and databases, published or unpublished). A great effort will be made to implement an exhaustive search, capable of reducing potential publication bias that could influence overall effect sizes. All searches will be conducted using a selected set of keywords. The latter will cover four main dimensions: type of study, type of interventions, population and outcomes. Table 2 describes the proposed key words for searching in the four dimensions.

*Table 2: Proposed key words*

| Type of study | Interventions | Population | Outcomes |
|---|---|---|---|
| Evaluation | Disciplinary methods | Schoolchildren | School exclusion |
| Effectiveness | Token economy | Pupils | School exclusion reduction |
| Intervention | Classroom management | Children | Suspension reduction |
| Programme | program/intervention/stra | Adolescents | Out-of-school suspension |
| Programme effectiveness | tegies | School-aged children | In-school suspension |
| Impact | School management | Student | Out-of-school exclusion |
| Effect | Early interventions | Youth | In-school exclusion |
| Experimental evaluation | School support projects | Adolescent | Suspended |
| Quasi-experimental evaluation | Skills training | Young people | Suspension |
| RCT | | | Expelled |
| Random evaluation | | | Expulsion |
| Efficacy trial | | | Outdoor suspension |
| | | | Stand-down |
| | | | Exclusionary discipline |
| | | | Discipline |

The above-mentioned key words will be combined using Boolean operators (e.g., AND, OR, NOT), wildcards and truncation symbols with the aim of running effective searches. Since different electronic databases accept different symbols, we will create specific combinations of terms, using key words and symbols as appropriate. We will keep a precise record of each search, including for instance the key words used, their combination, the date the search is performed, the sources consulted to identify eligible studies (e.g., electronic databases, list of references, hand searches), the total number of studies located and total number of studies retrieved.

We will use the electronic software Endnote for administering all relevant bibliographic references.

### *Electronic searches of bibliographic databases*

The following list details the electronic databases to be searched involving published (e.g., ISI web of knowledge, PsycINFO) and unpublished reports (e.g., Dissertation Abstracts) as well as reports in languages other than English (e.g., Scientific Electronic Library Online – SciELO).

- Australian Education Index (AEI)
- British Education Index (BEI)
- The Campbell Collaboration Social, Psychological, Educational and Criminological Trials Register (C2-SPECTR)
- BMJ controlled trials
- CBCA Education (Canada)
- ClinicalTrial.gov
- Criminal Justice Abstracts
- Cochrane Central Register of Controlled Trials (CENTRAL)
- Database of Abstracts of Reviews of Effects (DARE)
- Dissertation Abstracts
- Educational Resources Information Center (ERIC)
- EThOS (Beta)
- EMBASE
- Google and Google Scholar
- Index to Theses Database
- Institute of Education Sciences - What Works Clearinghouse
- ISI Web of Knowledge
- MEDLINE
- The National Dropout Prevention Center/Network
- The Netherlands National Trial Register (NTR)
- OpenGrey
- PsychINFO
- Sociological Abstracts
- Social Sciences Citation Index (SSCI)
- Scientific Electronic Library Online (SciELO). Electronic database collecting scientific production from developing countries (Spanish and Portuguese)
- World Health Organisation International Clinical Trials Registry (WHO ICTRP)

For each database we will run pilot searches including the key terms depicted in Table 2. They will help to adjust the terms, synonyms, and wildcards as appropriate. The pilot searches will also be helpful in creating combinations of terms that will capture relevant sets of studies. Some examples of these combinations are stated in Appendix 3.

In order to produce a transparent report of the methodological decisions, we will keep a record of the electronic searches (e.g., date of searches, number of reports found, retrieved, key terms included, synonyms and wildcards used when appropriate). We plan to generate electronic alerts to be aware of the most recent publications in the field published during the whole process timetable of the present review.

If our initial searches are older than 12 months at the time of final publication, we will re-run searches in order to publish an updated review.

### Contacting key authors

We plan to identify and contact key authors requesting information on primary studies that could be potentially integrated in this systematic review and meta-analysis.

Additionally, in the event that papers found do not offer sufficient statistical data, the main authors will be contacted with a request for more detail.

### List of references

We propose to review reference lists of previous primary studies or reviews related to the intervention/outcomes (e.g., Burrell et al., 2003; Gottfredson et al., 2012; Johnson, 2012; Wilson, Tanner-Smith, Lipsey, Steinka-Fry, & Morrison, 2011). Previous experiences demonstrate that this exercise produces an extra stock of manuscripts (e.g., Farrington & Ttofi, 2010).

### Hand searches

We will hand-search journals specialised in education research or evaluation research if they are not available online.

## 3.3 Description of methods used in primary research

### Type of studies

In the field of education research, the number of experimental studies is limited. For many reasons, these types of studies are not always feasible: Large-scale trials are expensive and demanding, the availability of the sample is sometimes restricted, there are methodological difficulties in randomising school populations (individuals versus clusters), and there are some ethical concerns about the children in the control group who are not receiving treatment although they could potentially need it. We hope to find the most reliable randomised controlled trials, but knowing that they could be a minority, quasi-experiments will also be included. Studies using a quasi-experimental design should involve pre- and post-intervention measures as well as a control condition. More details have been provided in the sub heading types of study design.

We plan to run moderator analysis controlling by the type of designs implemented in the primary sources.

### Unit of analysis

For the purposes of this systematic review, we plan to include primary studies involving pupils and clusters of pupils as units of analysis. One key issue emerges when meta-analysis includes studies randomising clusters or units. Participants nested in the same cluster tend to share similarities (Intra Class Correlation – ICC). When this correlation is not accounted for, standard errors, confidence intervals and *p*-values will tend to be to small. These conditions affect the meta-analysis in two different ways. First, the primary trial gets a mistakenly high weight. Second, the pooled result produces a meta-analysis with an overly small standard error. In order to avoid the combination of individual and clustered data we plan to follow the strategy proposed by the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins & Green, 2011). The handbook suggests that the effective sample size in a cluster-randomised trial can be obtained dividing the original sample size by the *design effect*, which equals 1+(*M*-1)*ICC. In this equation, *M* is the average cluster size and ICC is the intra-cluster correlation coefficient.

### 3.4   Criteria for determination of independent findings

We will use several strategies to deal with dependency in the data extracted from primary studies since violations of the assumptions of independence in meta-analysis would lead to incorrect estimates of the variance for the overall effect sizes (Higgins & Green, 2011; Romano & Kromrey, 2009).

First of all, as previously mentioned, the sources included in this review will be book chapters, journal articles, government reports, and academic MSc and PhD theses. In some cases, the same data would be published in more than one source (e.g., a book chapter and a journal article). In order to avoid the overestimation of the effect sizes, data will be coded just once. Following Lipsey & Landenberger (2006), in the above-mentioned cases we will code the most frequent result across the set of sources. In cases where this criterion is not enough, contact with the main author is proposed.

Secondly, included studies could report multiple time points, for instance, multiple follow-up measures. The inclusion of multiple follow-ups would create statistical dependence because the different measures are based on the same subjects (i.e., correlated with each other). In this event, we plan to calculate effect sizes separately for those studies reporting short-term and long-term follow-up measures. In fact, we will code all the follow-up measures in the data collection instrument.

Thirdly, we expect that some primary studies could report multiple outcomes for school exclusion (e.g., fixed-term exclusion versus permanent exclusion or out-of school exclusion versus in-school exclusion). In those cases outcome measures could be based on the same set of students. In the event that we find a sufficient number of studies, we could potentially

perform more than one meta-analysis representing for instance fixed-term and permanent exclusion separately. Another alternative to dealing with the interdependent measures will consist of computing the mean of the outcomes and using this mean score as the unit of analysis. In general we will follow Borenstein, Hedges, Higgins, & Rothstein (2009) Part 5 for complex data structures.

### 3.5 Details of study coding categories

### Coded variables

For the purposes of this meta-analysis, studies will be coded in terms of *publication features* (e.g., author, year of publication, language), *methodology* (e.g., research design, sampling methods, attrition), *participants* (e.g., age, ethnicity, gender), *characteristics of the intervention* (e.g., setting, doses, training), *role of the evaluator* (e.g., dependent, independent evaluator), and the *outcomes measured* (e.g., school exclusion). Appendix 1 offers a detailed scheme of the variables to be codified.

### Coding process and coding reliability

The process for screening studies for inclusion/exclusion will be organised in two stages. First, we will identify our targeted studies based on titles, abstracts and key words. The second stage of the screening will be based on the reading of the full text, including any relevant retraction statements and errata notes. Retraction statements and errata would be important for assessing study limitations or study quality (Higgins & Green, 2011).

Two trained coders (i.e., one of the authors and a trained research assistant) will be in charge of the coding process. They will work independently deciding the inclusion/exclusion of reports following the predefined criteria in section 3.1. The participation of two independent coders is aimed at reducing bias and reducing the risk of making mistakes.

Coders will be in charge of extracting data from each included study using the data collection instrument in Appendix 1. This information will be input into an electronic database (STATA 13) to produce descriptive/inferential statistics.

In order to test the coding reliability we will monitor consistency of the extracted data in the full sample of studies. In the event of discrepancies between coders, principal investigators will take part in the decision-making process. Discrepancies will be solved by consensus. We will keep a record of discrepancies, involving the independent coding plus the final agreement.

The procedures for searching manuscripts as well as the screening for each manuscript's inclusion or exclusion will be documented in detail. In the final review we will use these

details to produce a flow chart such as that suggested by PRISMA (Liberati et al., 2009) and a table reporting the characteristics of included and excluded reports.

### 3.6 Statistical procedures and conventions

### Effect size metrics

We will use the Comprehensive Meta Analysis software to carry out calculation of effect size. In the event of less common statistical outcomes we would potentially use the Campbell Collaboration effect size calculator and/or expert consultation.

Since measures of school exclusion are mainly expressed in raw frequencies of exclusion, percentages, proportions and rates (e.g., number of days suspended/excluded from school divided by 100 students), we will use odds ratios (ORs) as the main metric for the primary outcome. Consequently, our results will express the ratio of the odds of being excluded from school (event) for those in the treatment and control groups. Odds ratios computation will be carried out on a log scale (i.e., natural logarithm) with the purpose of maintaining symmetry in the analysis. Log odds ratio and the standard error of the log will then be converted back to original odds ratio metric to facilitate substantive interpretation, as advised by Borenstein et al. (2009). In all the cases, ORs will be reported along with 95% confidence intervals.

Secondary outcomes (i.e., externalising/internalising behaviour) will be more likely measured using continuous scales. If sufficient data is reported we will calculate standardised mean difference (SMD) or Cohen's *d* as the main metric for secondary outcomes. In the event of a small sample size, SMD will be corrected by transforming the point estimate into Hedges' *g,* using the formula in Lipsey & Wilson (2001). The estimated parameters will be reported along with 95% confidence intervals.

On the assumption that we find a mixture of both binary and continuous data for our targeted outcomes, we will save the original metric in the data collection instrument and transform the less frequent effect size into the more common metric for a given outcome. In the case of results expressed in raw data along with log-transformed data we will proceed to transformation following specialised criteria and formulas such as those proposed by Higgins & Green, (2011) in section 9.4 of the *Cochrane Handbook for Systematic Reviews of Interventions*. We will run sensitivity analysis for any potential transformation.

Following Chandler, Churchill, Higgins, Lasserson, & Tovey (2013), effect sizes will be coded in a way that a positive effect will reflect the outcomes favouring the treatment group. We will report when directions are reversed.

Since our review has a wide scope, it will potentially synthesize primary studies with a broad range of variation (e.g., differences in the delivery of the intervention, differences in the theoretical frameworks for interventions, differences in their administration, dosages), we

anticipate the use of the random effect inverse variance weighted models for meta-analytical calculations. The random effect model is the more appropriate when effect sizes are not homogeneous or consistent with the idea that they come from a singular population (Borenstein et al., 2009). Under a random model the variance will include the original (within-studies) variance plus the between-studies variance, tau-squared.

To illustrate our analysis, we will provide summary forest plots displaying the estimated effect sizes along with their 95% confidence intervals. In the event that our data allow the analysis between intervention effects and covariates, we plan to include moderator analysis (i.e., categorical models and or meta-regression) and its respective plots.

### *Missing data*

Following Lipsey & Wilson (2001), in those reports where key statistical information is missing, we will attempt to obtain data from principal investigators. When that is not possible, the study will be excluded from calculations of effect sizes. All the studies excluded for this reason will be identified and systematically reported.

### *Assessing risk of bias in included studies*

We plan to control the risk of bias in included studies by using the Cochrane Collaboration's tool for assessing risk of bias (Higgins & Green, 2011). The instrument involves seven specific domains, namely: i) sequence generation, ii) allocation concealment, iii) blinding of participants and personnel, iv) blinding of outcome assessment, v) incomplete outcome data, vi) selective outcome reporting, and vii) other issues. Each of these domains will be judged on a 3-point scale (i.e., low risk, high risk, unclear risk).

Risk of bias of studies involving quasi-experimental designs will be analysed using the ACROBAT-NRSI, a Cochrane Risk of Bias Assessment Tool for Non-Randomised Studies of Interventions (Stern, Higgins, & Reeves, 2014). This is an adaptation of the previously mentioned tool, which was originally designed for randomised controlled trials only. However both are focused on external validity. The ACROBAT-NRSI involves seven domains, namely: i) bias due to confounding, ii) bias in selection of participants into the study, iii) bias in measurement, iv) bias due to departures from intended interventions, v) bias due to missing data, vi) bias in measurement of outcomes and vii) bias in selection of the reported results. Each domain includes questions that facilitate the judgement of each single report. Each of these domains would be judged on a 5-point scale (i.e., low, moderate, serious, critical and no-information risk).

We will report RCT and QED results separately. We will conduct sensitivity analysis for the different levels of bias risk detected (e.g., low/high).

### Sensitivity analysis

Since meta-analysis involves a wide range of decisions, we will conduct sensitivity analysis to test the robustness of these decisions (Higgins & Green, 2011). The use of this technique can contribute to increasing the confidence in the pooled effects produced by the analysis. When possible, we will run sensitivity analysis isolating randomised controlled trials and quasi-experimental designs, distinguishing the role of low/high/unknown risk of bias (i.e., quality of the primary studies), differences between adjusted and unadjusted effect sizes, and the differences between published and unpublished data. In the event of outliers accounting for heterogeneity, we will also re-run analysis for controlling their presence in the pooled effect sizes calculated. It will be also necessary to run sensitivity analysis on the statistical procedures to compute effect sizes (e.g., transforming effect sizes), and the inclusion of reports presenting missing/incomplete data, among others.

### Exploring and assessing heterogeneity

We will report weighted mean effect sizes, under a random model using 95% confidence intervals and accompanied with graphical representation (i.e., forest plots). For investigating heterogeneity we will use the estimates suggested by Borenstein et al. (2009), namely, Tau-squared, Q-statistic and I-squared.

Tau-squared or the difference between the total variance or variance observed and the within-studies variance, will be estimated and reported along with 95% confidence intervals. Tau-squared > 1 suggests the presence of substantial heterogeneity (Borenstein et al., 2009).

The final calculation of the Q-statistic will be reported including its value, degrees of freedom and *p*-values. Significant *p*-values will provide evidence of heterogeneity in intervention effects.

Bearing in mind that Q can appear distorted when the number of studies meta-analysed is small (Higgins et al., 2003) we will also report I-squared ($I^2$). $I^2$ informing high percentages will be interpreted as an indication of high heterogeneity, meaning that the study-to-study dispersion is due to real differences in true effect size and not attributable to random error.

### Moderator analysis

On the condition that we retrieve and include a sufficient number of studies, we will perform analysis to explore the potential role of some specific moderators (covariates) explaining the potential heterogeneity involved in our results. Based on theory and our knowledge of previous research we have anticipated a number of *potential effect modifiers* that should be extracted from the selected studies and coded on the data collection instrument (Appendix 1). Those moderators would potentially cover the following aspects:

Participants' demographic characteristics. Previous research suggests that school children from ethnic minorities are more likely to be excluded than Caucasians (e.g., Skiba et al.,

2011). Also, boys are over-represented in exclusion rates when compared with girls (Bowman-Perrott et al., 2013). We will try to explore the role of ethnicity and gender as moderators of overall effect sizes.

Behavioural problems. Previous findings report that the effect of school-based prevention programmes can vary depending on pre-existing behavioural problems (e.g., Ferguson, Miguel, Kilburn, & Sanchez, 2007; Lösel & Beelmann, 2006). We plan to explore the role of behavioural problems at moderating overall effect sizes.

Theoretical bases of the interventions. We would be interested in testing whether the theoretical background of interventions (e.g., Cognitive-Behavioural, Restorative Justice) can moderate the effect of intervention in reducing disciplinary exclusion.

Quality of the intervention. Previous research testing the effectiveness of prevention programmes settled in schools demonstrates that well-implemented programmes—those including training, monitoring and supervision—display better results (e.g., Durlak et al., 2011; Gottfredson & Wilson, 2003; Lösel & Beelmann, 2006).

As previously mentioned, in the event that our data present an acceptable statistical power we will explore heterogeneity by running meta-regression. For this purpose we will use CMA version 3, which allows meta-regression with categorical and continuous data simultaneously.

In the event that we use moderator analysis involving categorical variables, we anticipate the estimation of models analogous to ANOVA. If we have at least five studies comparing groups based on categorical variables, we will run analysis under random-effect model. It is foreseeable that we would use separate estimate of tau-squared (i.e. variance component) for each group. As has been said before, in our review it is difficult to assume that the true effect between-studies is the same for all groups. We will follow methodological guidelines provided by Borenstein et al. (2009).

### *Publication bias*

To test publication bias, funnel plots of standard error will be produced. Given that the interpretation of funnel plots can be subjective (e.g., Borenstein et al., 2009), we plan the inclusion of additional statistical tests on the potential publication bias (e.g., Fail Safe N, Trim-and-Fill).

### *3.7  Treatment of qualitative research*

We do not plan to include qualitative research.

## IV. REVIEW AUTHORS

**Lead review author:**
Sara Valdebenito
PhD Candidate. Institute of Criminology, University of Cambridge
Sidgwick Avenue, Cambridge CB3 9DA
United Kingdom
Phone: +44 1223 767373
Email: sv331@cam.ac.uk

**Co-author(s):**
Professor Manuel Eisner
Institute of Criminology, University of Cambridge
Sidgwick Avenue, Cambridge CB3 9DA
United Kingdom
Phone: +44 1223 335374
Email: mpe23@cam.ac.uk

Professor David Farrington
Institute of Criminology, University of Cambridge
Sidgwick Avenue, Cambridge CB3 9DA
United Kingdom
Phone: +44 1223 767186
Email: dpf1@cam.ac.uk

Dr Maria Ttofi
Institute of Criminology, University of Cambridge
Sidgwick Avenue, Cambridge CB3 9DA
United Kingdom
Phone: +44 1223 767186
Email: mt394@cam.ac.uk

Dr Alex Sutherland
Institute of Criminology, University of Cambridge
Sidgwick Avenue, Cambridge CB3 9DA
United Kingdom
Phone: +44 1223 746519
Email: as2140@cam.ac.uk

## V. ROLES AND RESPONSIBLIITIES

**Content**: Professor Manuel Eisner has extensive experience conducting research on child prevention of antisocial behaviour. He is currently leading a large-scale longitudinal study conducted in Swiss schools, the Zurich Project on the Social Development of Children (z-proso). He has also conducted an important number of independent randomised controlled trials on school backgrounds. Dr Sutherland is currently working on projects relating to school exclusion and on underachievement in schools. Sara Valdebenito, Professor Eisner and Dr Sutherland are involved in a large-scale cluster-randomised controlled trial, the London Education and Inclusion Project.

**Systematic review methods**: Professor David Farrington and Dr Maria Ttofi have been involved in previous large-scale projects for the Campbell Collaboration. They have produced an important number of meta-analyses in fields related to education and risk-focused prevention. PhD candidate Sara Valdebenito has recently conducted two systematic reviews related to school bullying. During 2015 she will be delivering a course on meta-analysis for the Social Sciences Research Centre at the University of Cambridge.

**Statistical analysis**: Sara Valdebenito will conduct the statistical analysis, with Professor Farrington and Dr Ttofi acting as advisors during this process.

**Information retrieval**: The review team has conducted academic research for several years. All of them are familiar with experimental and quasi-experimental designs as well as databases where studies can be accessed. This project has been allocated resources for hiring assistance during the retrieval process. Sara Valdebenito will be in charge of the research assistant (i.e., MPhil or PhD student) training for the coding process. Dr Sutherland will provide advice on the coding of studies and the use of statistical software.


## VI. SOURCES OF SUPPORT

Professor Manuel Eisner and Sara Valdebenito have been awarded a grant by the Nuffield Foundation for conducting the proposed systematic review. Terms and conditions agreed with the sponsor involve the submission of results during 2016.


## VII. DECLARATIONS OF INTEREST

None of the researchers involved in the team present financial interest in this review. None of them have been involved in the development of interventions or systematic reviews on the scope of the present one. Three authors (Sara Valdebenito, Professor Manuel Eisner and Dr Alex Sutherland) are currently involved in the London Education and Inclusion Project cluster-randomised controlled trial (ISRCTN 23244695). The study is designed as an independent evaluation and the authors have no financial or other links to the evaluated programme.


## VIII. PRELIMINARY TIMEFRAME

The approximate date for submission of the systematic review is February 2016

# IX. PLANS FOR UPDATING THE REVIEW

We plan to produce an updated version of the review every three years. The lead author will be in charge of coordinating and producing the revised versions.

# X. AUTHOR DECLARATION

## Authors' responsibilities

By completing this form, you accept responsibility for preparing, maintaining and updating the review in accordance with Campbell Collaboration policy. The Campbell Collaboration will provide as much support as possible to assist with the preparation of the review.

A draft review must be submitted to the relevant Coordinating Group within two years of protocol publication. If drafts are not submitted before the agreed deadlines, or if we are unable to contact you for an extended period, the relevant Coordinating Group has the right to de-register the title or transfer the title to alternative authors. The Coordinating Group also has the right to de-register or transfer the title if it does not meet the standards of the Coordinating Group and/or the Campbell Collaboration.

You accept responsibility for maintaining the review in light of new evidence, comments and criticisms, and other developments, and updating the review at least once every five years, or, if requested, transferring responsibility for maintaining the review to others as agreed with the Coordinating Group.

## Publication in the Campbell Library

The support of the Coordinating Group in preparing your review is conditional upon your agreement to publish the protocol, finished review, and subsequent updates in the Campbell Library. The Campbell Collaboration places no restrictions on publication of the findings of a Campbell systematic review in a more abbreviated form as a journal article either before or after the publication of the monograph version in *Campbell Systematic Reviews*. Some journals, however, have restrictions that preclude publication of findings that have been, or will be, reported elsewhere and authors considering publication in such a journal should be aware of possible conflict with publication of the monograph version in *Campbell Systematic Reviews*. Publication in a journal after publication or in press status in *Campbell Systematic Reviews* should acknowledge the Campbell version and include a citation to it. Note that systematic reviews published in *Campbell Systematic Reviews* and co-registered with the Cochrane Collaboration may have additional requirements or restrictions for co-publication. Review authors accept responsibility for meeting any co-publication requirements.

**I understand the commitment required to undertake a Campbell review, and agree to publish in the Campbell Library. Signed on behalf of the authors**:

**Form completed by:** Sara Valdebenito          **Date:** 25/03/2015

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**DATA-CODING INSTRUMENT**
*School-based interventions for reducing
disciplinary school exclusion. A systematic review*
[Variable names in brackets]

Contents

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## Section A. Codification

Instruction: use one data-coding instrument for each manuscript. When more than one manuscript reports the same research project, select one of them as the principal (e.g., the older) and give it an ID number. The following manuscripts should use the same ID but it must be registered in the Crossref field.

**[STUDYID]** Study ID number:
**[CROSSREF1]** Cross reference document identifier:
**[CROSSREF2]** Cross reference document identifier:
**[CROSSREF3]** Cross reference document identifier:
**[DATESCR]** Date of screening:
**[CODER]** Coder Initials:

## Section B. Bibliographical information

Before completing this section, please be sure that the manuscript is correctly uploaded in the reference manager programme.

**[AUTHOR]** Name of the main author(s):
**[AFFIL]** Main author affiliation:
**[DATEPUB]** Year of publication:
**[DATEFIEDW]** Year of fieldwork (usually reported in a range):
**[COISTATEMENT]** Has the paper included a conflict of interest statement?
  - ⊚  1. Yes
  - ⊚  0. No

**[LANGPUB]** Language of the publication:
  - ⊚  1. English
  - ⊚  2. German
  - ⊚  3. Italian
  - ⊚  4. Spanish
  - ⊚  5. Portuguese
  - ⊚  999. Other:_____

**[COUNTPUB]** Country of publication:
  - ⊚  1. UK
  - ⊚  2. USA
  - ⊚  3. Canada
  - ⊚  4. Australia
  - ⊚  999. Other:_____
  - ⊚  99. Unknown

**[TYPUB]** Type of publication:
  - ⊚  1. Journal
  - ⊚  2. Book/book chapter
  - ⊚  3. Masters thesis
  - ⊚  4. PhD/doctoral thesis
  - ⊚  5. Technical/governmental report
  - ⊚  6. Conference proceedings
  - ⊚  999. Other:_____

**[AUTDIS]** Main author discipline:
- ⊚ 1. Education
- ⊚ 2. Social Work
- ⊚ 3. Psychology
- ⊚ 4. Criminal Justice
- ⊚ 5. Sociology
- ⊚ 6. Psychiatry/Medicine
- ⊚ 999. Other:_____
- ⊚ 99. Unknown

**[LOCAT]** How was the study/report located?
- ⊚ 1. Electronic database
- ⊚ 2. Web search
- ⊚ 3. Reference in a book/paper. Please specify:
- ⊚ 4. Hand search in specialised journal
- ⊚ 5. Peer/expert suggestion
- ⊚ 999. Other. Specify:_____

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## Section C. Ethics

**[CONSENT]** Did the study declare the use of "consent agreement forms"?
- ⊚ 1. Yes
- ⊚ 0. No
- ⊚ 999. Other:_____
- ⊚ 99. Unknown

**[SIGNCONS]** Who signed the consent?
- ⊚ 1. Students
- ⊚ 2. Parents
- ⊚ 3. Teachers
- ⊚ 4. Schools
- ⊚ 5. Parents and student
- ⊚ 999. Other. Specify:_____
- ⊚ 99. Unknown

## Section D. Design

The present systematic review includes randomised control trials as well as quasi-experimental reports (before/after measure plus a control or comparison group). If the control/comparison group is randomly allocated, non-randomly allocated or matched and no intervention expected to produce impact is provided to it, you will be able to code that group as CONTROL. Subsequently, the TREATMENT group could be understood as the group that receives the intervention, no matter if that condition has been randomly allocated or not.

Please select always the data that is related with the sample effectively analysed.

**[DESTYPE]** What kind of design is this paper based on?
- ⊚ 1. Randomised controlled trial (true experiment)
- ⊚ 2. Before-and-after with control/comparison group/s
- ⊚ 3. Instrumental variable
- ⊚ 4. Propensity score matching
- ⊚ 5. Interrupted time series
- ⊚ 6. Pre/post measures with unmatched control/comparison group
- ⊚ 7. Inverse probability weighting
- ⊚ 999. Other. Specify:_____

**[RANDUNIT]** Units of randomization
- ⊚ 1. Individuals
- ⊚ 2. Clusters/groups (classroom, schools)
- ⊚ 999. Other. Specify:_____
- ⊚ 99. Unknown

**[ANALUNIT]** Unit of analysis
- ⊚ 1. Students
- ⊚ 2. Clusters/groups (classroom, schools)
- ⊚ 999. Other. Specify:_____
- ⊚ 99. Unknown

**[COMPVAR]** Variables measured to create comparability? (e.g., variables used to match the control and treatment groups)

_____

_____

**[MAINSTAT]** What is the main statistical analysis used to produce the final results?
- ⊚ 1. Multilevel modelling
- ⊚ 2. Differences of means
- ⊚ 3. MANOVA
- ⊚ 4. Chi-squared
- ⊚ 5. Propensity Score Matching
- ⊚ 999. Other. Specify:_____

## Section D. Sample

**[SAMPSELECT]** How was the sample selected?
- �267 1. Randomly
- �267 2. Assessment
- �267 3. Self-selection
- �267 999. Other. Specify:_____

**[INSAMP]** Initial sample size (i.e., individuals/schools):

**[NUMBFOLL]** Nº of follow-up:

**[FOLLSAMP1]** Follow-up 1 sample size:

**[FOLLSAMP2]** Follow-up 2 sample size:

**[FOLLSAMP3]** Follow-up 3 sample size:


**[NSCHOOL]** Initial number of schools:

**[NSFOLL1]** Follow-up 1 sample size:

**[NSFOLL2]** Follow-up 2 sample size:

**[NSFOLL3]** Follow-up 3 sample size:


**[NCLASS]** Initial number of classes:

**[NCFOLL1]** Follow-up 1 sample size:

**[NCFOLL2]** Follow-up 2 sample size:

**[NCFOLL3]** Follow-up 3 sample size:


Please code here the information on attrition described in the manuscript:

|  | Total number of students at Baseline | Total number of students at Follow-up |
|---|---|---|
| Treatment | **[NTREBAS]** | **[NTREFOLL]** |
| Control | **[NCONTBA]** | **[NCONTFOL]** |

**[MEANAGE]** Mean age and standard deviation of overall sample at beginning of intervention:

**[GENDER]** Gender
- �267 % of males
- �267 % of females
- �267 99. Unknown

**[LOCAT]** Location of program
- �267 Urban area
- �267 Suburban area
- �267 Rural area
- �267 Mixture of areas
- �267 99. Not enough information to determine

**[GRADEX]** Grade level of students
- �267 % of students in Elementary school or equivalent
- �267 % of students in Secondary school or equivalent
- �267 % of students in High school or equivalent
- �267 4. Other:
- �267 99. Unknown

**[ETHNI]** Predominant ethnicity[15]
- ⊚ 1. % of Caucasian:
- ⊚ 2. % of Black:
- ⊚ 3. % of Hispanic:
- ⊚ 4. % of Asian:
- ⊚ 5. % of other mixed background:
- ⊚ 99. Unknown

**[COUNTRY]** Please state the name of the country where schools and sample of students were located when tested.

_____ (99 if unknown)

**[LUNEX]** Socio-economic status

% of students receiving free/reduced school lunch:

99. Unknown

**[SENEX]** Special Educational Needs

% of students declaring SEN:

99. Unknown

---

## Section E. Primary Outcome (School Exclusion)

**[EXCLUSION] Is the manuscript reporting outcomes for school exclusion?**
- ⊚ 1. Yes
- ⊚ 0. No

**[TYPEXC]** Type of exclusion measured
- ⊚ 1. In-school exclusion
- ⊚ 2. Out-of-school exclusion
- ⊚ 99. Unknown

**[CHEKTIP]** Duration of school exclusion measured
- ⊚ 1. Days of Fixed-term exclusion
  (Expressed in number or days, frequencies, percentages)
- ⊚ 2. Days of Permanent exclusion
  (Expressed in number or days, frequencies, percentages)
- ⊚ 99. Unknown

**[ICCEXCLU]** If the statistical analysis include cluster in MLM, please register the ICC for Exclusion:

---

[15] Based on Lipsey & Wilson (2001)

## Section F. Secondary outcomes

**[BEHAVMES]** Did the study include measures on behaviour domains?
- ⦿ 1. Yes
- ⦿ 0. No
- ⦿ 99. Unknown

What types of the following behaviours are measured?

- **[PROSO]** Pro-social behaviour (e.g., helping, empathy). Specify:_____
  - ⦿ 1. Yes
  - ⦿ 0. No

**[MEPROSO]** Measure(s) used to test the behaviour (name):

**[ALPHAPROSO]**
- ⦿ Reliability test. Specify alpha value:_____
- ⦿ Non reported

| Groups | Effect size before | Effect size after |
|---|---|---|
| Control or comparison | **[PROBC]** | **[PROAC]** |
| Treatment | **[PROBT]** | **[PROAT]** |

**[PAGEPROSO]** Number of the page from where you extract statistical data:

**[ICCPROSO]** If the statistical analysis include cluster in MLM, please register the ICC for behavioural outcomes:

- **[INTERNAL]** Internalising problem behaviour

(e.g., anxiety, depression, attention-deficit and hyperactivity disorder (ADHD), attention deficit, hyperactivity). Specify:_____
  - ⦿ 1. Yes
  - ⦿ 0. No

**[MINTERNAL]** Measure (s) used to test the behaviour (name):

**[ALPHAINTERNAL]**
- ⦿ Reliability test. Specify alpha value:_____
- ⦿ Non reported

| Groups | Effect size before | Effect size after |
|---|---|---|
| Control or comparison | **[PROBC]** | **[PROAC]** |
| Treatment | **[PROBT]** | **[PROAT]** |

**[PAGEINTERNAL]** Number of the page from where you extract statistical data:

**[ICCINTERNAL]** If the statistical analysis include cluster in MLM, please register the ICC for behavioural outcomes:

- **[NAEXTERNAL]** Non-aggressive externalising problem behaviour

(e.g., stealing, lying, graffiti, illegal drugs). Specify: _____

      ⑥ 1. Yes

      ⑥ 0. No

**[MNAEXTERNAL]** Measure used to test the behaviour (name):

**[ALPHANAEXTER]**

    ⑥ Reliability test. Specify alpha value:_____

    ⑥ Non reported

| Groups | Effect size before | Effect size after |
|---|---|---|
| Control or comparison | **[PROBC]** | **[PROAC]** |
| Treatment | **[PROBT]** | **[PROAT]** |

**[PAGENAXTERN]** Number of the page from where you extract statistical data:


**[ICCNAEXT]** If the statistical analysis include cluster in MLM, please register the ICC for behavioural outcomes:


- **[AAGRESEXT]**    Aggressive externalising problem behaviour

(e.g., Opposition/defiance, physical aggression, indirect aggression, instrumental aggressions/dominance, reactive aggression, school bullying). Specify:_____

      ⑥ 1. Yes

      ⑥ 0. No

**[MAGRESSEXT]** Measure used to test the behaviour (name):

**[ALPHAAEXT]**

    ⑥ Reliability test. Specify alpha value:_____

    ⑥   Non reported

| Groups | Effect size before | Effect size after |
|---|---|---|
| Control or comparison | **[PROBC]** | **[PROAC]** |
| Treatment | **[PROBT]** | **[PROAT]** |

**[AGRESPAGE]** Number of the page from where you extract statistical data:


**[ICCAGREEX]** If the statistical analysis include cluster in MLM, please register the ICC for Behavioural outcomes:

## Section G. Base-line measurements

**[DATABAS]** Date of baseline assessment:

What measures were used?

**[SRMES]** Self-report
- ⊚  1. Yes
- ⊚  0. No
- ⊚  99. Unknown

**[TRMES]** Teachers' report
- ⊚  1. Yes
- ⊚  0. No
- ⊚  99. Unknown

**[SCHRMES]** School records
- ⊚  1. Yes
- ⊚  0. No
- ⊚  99. Unknown

**[PAREP]** Parents
- ⊚  1. Yes
- ⊚  0. No
- ⊚  99. Unknown

**[OMES]** Other:_____

**[EXCBL]** Frequency of exclusion at baseline (register any measure given by the study)

## Section H. Programme delivered

This section aims to codify data on the delivery process. Be aware that sometimes final reports do not describe all the data related to delivery. In those cases it would be helpful to search for registered protocols or earlier publications reporting more data on this.

**[PRONAME]** Name of the programme:

**[PROCURRI]** Was the program curricular?
- ⊚  1. Yes
- ⊚  0. No
- ⊚  99. Unknown
- ⊚  999. Other. Specify:_____

**[PROEND]** The programme was conducted for:
- ⊚  1. Research ends
- ⊚  2. Demonstration ends
- ⊚  3. Routine
- ⊚  99. Unknown
- ⊚  999. Other. Specify:_____

**[PROSIT]** Primary programme site:
- ⊚  1. Public school
- ⊚  2. Private school
- ⊚  3. Other, (specify):_____
- ⊚  99. Unknown

**[PROSCH]** Was at least one of the components of the intervention was settled at school?
- ⊚  1. Yes
- ⊚  0. No
- ⊚  99. Unknown

**[PRODEL]** Who delivered the programme?
- ⊚  1. External facilitators
- ⊚  2. School facilitators
- ⊚  3. Both
- ⊚  99. Unknown

**[PDBACK]** Deliverer's background 1
- ⊚  1. Social worker
- ⊚  2. Psychologist
- ⊚  3. Teacher
- ⊚  4. Police officers
- ⊚  5. Peers
- ⊚  999. Other. Specify:_____
- ⊚  99. Unknown

**[PDBACK]** Deliverer's background 2
- ⊚  1. Social worker
- ⊚  2. Psychologist
- ⊚  3. Teacher
- ⊚  4. Police officers
- ⊚  5. Peers
- ⊚  999. Other. Specify:_____
- ⊚  99. Unknown

**[TRAINBEF]** Did the deliverer receive training BEFORE implementing the programme?
- ⊚  1. Yes.
- ⊚  0. No.
- ⊚  99. Unknown

**[THOURS]** How long was the training in hours?:_____

**[TRAINDUR]** Did the deliverer receive training DURING the implementation?
- ⊚  1. Yes.
- ⊚  0. No.
- ⊚  99. Unknown

**[THOURS2]** How long was the training in hours?:

What type of intervention was delivered? If the manuscript indicates a mixture of interventions you can select more than one using TYPEPRO 1, 2 and 3.

|  | **[TYPEPRO1]** | **[TYPEPRO2]** | **[TYPEPRO3]** |
|---|---|---|---|
| 1. Mentoring programme |  |  |  |
| 2. Restorative programme |  |  |  |
| 3. Skills training programme |  |  |  |
| 4. School-wide systemic intervention |  |  |  |
| 5. Classroom management |  |  |  |
| 6. Counselling/therapy |  |  |  |
| 999. Other |  |  |  |

Theoretical background of the intervention. If the manuscript indicates a mixture of theories, you can select more than one using THEORY 1, 2 and 3.

|  | [THEORY1] | [THEORY2] | [THEORY3] |
|---|---|---|---|
| 1. Cognitive behavioural |  |  |  |
| 2. Learning theory |  |  |  |
| 3. Restorative theories |  |  |  |
| 4. Organisational theories or principles |  |  |  |
| 99. Unknown |  |  |  |
| 999. Other (Specify) |  |  |  |

[PROCONT] What happened to the control group?
- ⊚ 1. No intervention
- ⊚ 2. Wait-list control
- ⊚ 3. Minimal contact
- ⊚ 4. Treatment as usual
- ⊚ 5. Alternative treatment
- ⊚ 5. Placebo
- ⊚ 999. Other. Specify:_____

[PROFORM] Delivery format:
- ⊚ 1. Manualised programme
- ⊚ 2. Unstructured programme
- ⊚ 3. Mixed
- ⊚ 99. Unknown
- ⊚ 999. Other. Specify:_____

What was the programme dosage?

[PRODOSW] AVERAGE Duration in weeks:

[PRODOSH] AVERAGE Hours per week:

[PROFREQ] What was the frequency of the programme counted?
- ⊚ 1. Less than a week
- ⊚ 2. Once a week
- ⊚ 3. Twice a week
- ⊚ 4. 3-4 times a week
- ⊚ 5. Daily
- ⊚ 99. Unknown

[EVROLE] What was the "evaluator" role?
- ⊚ 1. Deliver the programme
- ⊚ 2. Designed the programme
- ⊚ 3. Both design and delivery
- ⊚ 4. Independent evaluator
- ⊚ 99. Unknown

[MONITOR] Was the programme implementation monitored?
- ⊚ 1. Yes
- ⊚ 0. No
- ⊚ 99. Unknown. Not enough information

[IMPROB] Does the report provide information about implementation problems?
- ⊚ 1. Yes, there were clear problems which are reported
- ⊚ 0. No, non-reported problems, reasonably well implemented
- ⊚ 2. Possible problems based on the description of the intervention
- ⊚ 99. Unknown. Not enough information

**[PROCOST]** Is the cost of the intervention mentioned?

  ⊚   1. Yes
  ⊚   0. No

**[AMOUNT]** Cost:

**[UNITCURR]** Currency:

## Section I. Follow-up measurement

**[DATEFALL]** Date of follow up:

Multiple follow-ups

**[MONTHFO1]** N° of months from baseline to 1st follow-up:

**[MONTHFO2]** N° of months from baseline to 2nd follow-up:

**[MONTHFO3]** N° of months from baseline to 3rd follow-up:

**[MONTHFO4]** N° of months from baseline to 4th follow-up:

What measures were used?

**[POSTSR]** Children/adolescent self-report

  ⊚   1. Yes
  ⊚   0. No

**[POSTTR]** Teachers' report

  ⊚   1. Yes
  ⊚   0. No

**[POSTSR]** School records

  ⊚   1. Yes
  ⊚   0. No

**[POSTPR]** Parents report

  ⊚   1. Yes
  ⊚   0. No

**[POSTO]** Other:_____

**[FREQEXFOLL]** Frequency of exclusion at follow-up (register any measure given by the study)

## Section J. Effect sizes of intervention on school exclusion

- Effect size: outcomes expressed in continuous data.

**[CSSEX]** Sample size for the ES (Treatment group)

**[CSSCON]** Sample size for the ES (Control group)

**[MEANEX]** Mean (Treatment group)

**[MEANCON]** Mean (Control group)

**[MEANADJ]** Are the Means adjusted?

  ⊚   1. Yes.
  ⊚   0. No

**[ADJBY]** Adjusted by (describe):_____

**[SDEX]** Standard deviation (Treatment group)

**[SDCON]** Standard deviation (Control group)

**[SEEX]** Standard error (Treatment group)
**[SECON]** Standard error (Control group)

**[CORREX]** Correlation coefficient + *p* value (Treatment group)
**[CORRCON]** Correlation coefficient + *p* value (Control group)

**[SMDTREAT]** Standardised mean difference + confidence intervals


- Effect size: outcomes expressed in dichotomous data.

**[DSSTRE]** Sample size for the ES (Treatment group)
**[DSSCONT]** Sample size for the ES (Control group)

**[NUMTRE]** Treatment group; number of successful cases:
**[NUMCON]** Control group; number of successful cases:

**[PROPTRE]** Treatment group; proportion of successful cases:
**[PROPCON]** Control group; proportion of successful cases:

**[ORTRE]** Treatment group; odds ratios:
      Confidence Intervals:
      *p*-value:
**[ORCON]** Control group; odds ratios:
      Confidence Intervals:
      *p*-value:
**[ORADJ]** Are the odds ratios adjusted?
    ⑥  1. Yes.
    ⑥  0. No
Adjusted by (explain):_____

**[CHISC]** X$^2$ value with *df:*

**[PAGEEFFECT]** Number of the page from where you extract statistical data:

- Effect sizes at follow-up

**[ESFOLLOW1]** Calculated effect at follows up 1:_____
[ESFOLL1] Number of months after intervention for follow-up 1:_____

**[ESFOLLOW2]** Calculated effect at follows up 2:_____

[ESFOLL2] Number of months after intervention for follow-up 2:_____

**[ESFOLLOW3]** Calculated effect at follows up 3:_____
[ESFOLL3] Number of months after intervention for follow-up 3:_____

**[ESFOLLOW4]** Calculated effect at follows up 4:_____
[ESFOLL4] Number of months after intervention for follow-up 4:_____

**Eligibility checklist**

| | | |
|---|---|---|
| Does this paper measure school exclusion as an outcome? | ⑥ YES | ⑥ NO |
| Does the intervention is school based? (or at least one component in the school) | ⑥ YES | ⑥ NO |
| Are the target individuals school students? | ⑥ YES | ⑥ NO |
| The report is based on an experimental, quasi-experimental design? | ⑥ YES | ⑥ NO |
| Is this report informing statistical results able to be transformed in effect sizes? | ⑥ YES | ⑥ NO |
| Is this report included? | ⑥ YES | ⑥ NO |
| Reasons for exclusion: | | |

## APPENDIX 2: DIFFERENT TYPES OF INTERVENTIONS

| Paper | Level | Name of programme | Principles, main focus | Targeted population |
|---|---|---|---|---|
| Bradshaw et al., (2012) Effects of School-Wide Positive Behavioural Interventions and Supports on Child Behaviour Problems | School Level | School-Wide Positive Behavioural Interventions and Supports (SWPBIS) | Altering staff behaviour and developing systems and supports to meet children's behavioural needs | Kindergarten to Fifth Grade |
| Breunlin, Cimmarusti, Bryant-Edwards, & Hetherington, (2002) Conflict Resolution Training as an Alternative to Suspension for Violent Behaviour | Family | Alternative to Suspension for Violent Behaviour | Conflict resolution strategies used to teach alternatives to violence. Skill-building and thinking skills components are grounded in conflict resolution theory. | Pupils suspended |
| Flay, Allred, & Ordway, (2001) Effects of the *Positive Action* Program on Achievement and Discipline: Two Matched Control Comparisons | Comprehensive (child, teachers, school staff, parents) | Positive Action | Teaching positive behaviours to reinforce positive self-concept. Physical and social/emotional components taught in curriculum. | Elementary school pupils |

| | | | | |
|---|---|---|---|---|
| Farrell, Meyer, & White, (2010)Evaluation of Responding in Peaceful and Positive Ways (RIPP): A School-Based Prevention Program for Reducing Violence Among Urban Adolescents | Individual | Responding in Peaceful and Positive Ways (RIPP) | Social cognitive learning principles tackling behaviour, interpersonal characteristics and environmental characteristics. | Sixth Grade Pupils |
| Pritchard & Williams, (2001) A Three-Year Comparative Longitudinal Study of a School-Based Social Work Family Service to Reduce Truancy, Delinquency and School Exclusions | Comprehensive (family, child, school, teachers, other agencies) | No specific name of Programme- referred to as a school-based child and family social work service. | Classroom support, counselling, parental liaison, consulting with teachers, supporting external agencies, group work with children, community work. Long-term follow-up support for a range of social work issues. | Social workers were based in primary and secondary schools. |
| Schellenberg & Parks-Savage, (2007) Reducing Levels of Elementary School Violence with Peer Mediation | Individual | Peace Pal | Grounded in social learning theory; applies a cognitive behavioural approach to the mediation process; teaches conflict resolution and resolves disputes. | Grades 3-5 (8-11 years old) |

## APPENDIX 3: EXAMPLE OF ELECTRONIC SEARCH

**ISI Web of Knowledge**

#1  **TOPIC:** (experiment*) *OR* **TOPIC:** (evaluation) *OR* **TOPIC:** (random*) *OR* **TOPIC:** (intervention) *OR* **TOPIC:** (effective*) *OR* **TOPIC:** (efficacy) *OR* **TOPIC:** (quasi) *OR* **TOPIC:** (impact) *OR* **TOPIC:** (RCT)

#2 **TOPIC:** (school) *AND* **TOPIC:** (*exclusion)

#3 **TOPIC:** (school) *AND* **TOPIC:** (*exclusion)

#4 **TOPIC:** (school) *AND* **TOPIC:** (*suspension*)

#5 **TOPIC:** (school) *AND* **TOPIC:** (expelled)

#6 **TOPIC:** (school) *AND* **TOPIC:** (expulsion)

#7 **TOPIC:** (school) *AND* **TOPIC:** (stand-down)

#8 **TOPIC:** (school management) *OR* **TOPIC:** (classroom management) *OR* **TOPIC:** (school support project*) *OR* **TOPIC:** (skills training) *OR* **TOPIC:** (disciplinary methods) *OR* **TOPIC:** (token economy) *OR* **TOPIC:** (program*) *OR* **TOPIC:** (*intervention*) *OR* **TOPIC:** (strateg*)

#9 **TOPIC:** (schoolchildren) *OR* **TOPIC:** (*children*) *OR* **TOPIC:** (school-age*) *OR* **TOPIC:** (adolescent*) *OR* **TOPIC:** (pupil*) *OR* **TOPIC:** (student)

#3 AND #1 AND #8 AND #9

#1 AND #2 AND #3 AND #4 AND #5 AND #6 AND # 7 AND #8 AND #9