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Exclusion from School and Risk of Serious Violence: A Target Trial Emulation Study

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Evidence for or against a causal effect of school exclusion on offending is inhibited by random allocation not being available on ethical grounds. To advance understanding of the connection between school exclusion and offending—specifically, serious violent offending—we emulate a randomized controlled trial using a target trial framework and a linkage of national education and justice data. Across more than 20,000 matched pairs of excluded and not excluded children exclusion was associated with at least a doubling of risk for perpetrating serious violence (hazard ratio 2.05, 95% CI: 1.83, 2.29) and homicide/near-miss homicide (2.36, 95% CI: 1.04, 5.36) within 12 months of target trial entry. We discuss the implications of these findings for theory and policy in education and criminal justice as well as discussing the extent to which the observed relationships can be considered causal.

KEY WORDS: school exclusion, violence, target trial

INTRODUCTION—OVERVIEW

Youth violence is one of the most pressing crime-related issues of the twenty-first century. In the United States, homicide is among the top four causes of death in 1- to 34-year-olds (Centers for Disease Control and Prevention 2023). In England and Wales, among children entering the criminal justice system for the first time in 2022/23, 38 per cent had perpetrated violence and possession of weapon offences (Youth Justice Board 2024). Serious violence, particularly that perpetrated by and against children and young adults, is both a criminal justice and a political priority with at least £600 million having been spent on or allocated to violence prevention initiatives by HM Government since 2016.

Preventing and responding to serious violence involving young people—as perpetrators and victims—is a major priority of contemporary policing but the causes and implications of violence stretch beyond criminal justice to many other areas of life, such as health and education. For example, in the United Kingdom, people aged 0–24 years accounted for 38 per cent of all admissions to hospital for violent injury with a sharp object in 2020/21 (Office for National

Statistics 2023) and around 20 per cent of children report having skipped school because they felt unsafe (Youth Endowment Fund 2024). The emergence of a public health approach to violence has emphasized that violence prevention strategies must look beyond criminal justice institutions to understand and prevent the circumstances, events and experiences that increase the risk of a young person becoming involved in violence.

The UK Serious Violence Duty (Home Office 2022) made it a requirement for some statutory agencies—policing, justice, health, local authorities, housing services, family support services, fire and rescue and education—to collaborate to prevent violence. With regard to education, the guidance states that involvement in education is an important protective factor against violence and that education providers have 'a vital role in preventing and reducing serious violence by facilitating early intervention, prevention and safeguarding children and young people in their care' (Home Office 2022: 95). This position is supported by evidence indicating that school engagement and connectedness (Rose *et al.* 2024), educational attainment (Jackson 2009) and a supportive school environment (Bonell *et al.* 2013) all serve protective functions in healthy child development. By extension, reduced school engagement (Rocque *et al.* 2017), poorer attainment (Katsiyannis *et al.* 2008) as well as challenging school experience (Smith 2006) and environment (Lo *et al.* 2011) are all associated with worse behavioural and social outcomes, including criminal justice involvement.

The use of exclusions from school—temporarily or permanently banning a student from attending a school because they have breached the school's disciplinary code—is one of the most contentious issues in education. While it is framed as a necessary tool of last resort (Tillson and Oxley 2020; Department for Education 2024) for maintaining a safe and effective classroom and school environment, critics claim that it is practised unfairly and inconsistently (Graham *et al.* 2019), contravenes a school's duty of care (Parsons 2005) to the excluded child, exacerbates the problems (Caslin 2021) that led to the child's exclusion and acts as a trigger for worsening behavioural outcomes (Holt 2011), such as offending. There is a wealth of research that indicates an association between school exclusion and offending (Gerlinger *et al.* 2021), a phenomenon collectively known as the 'school-to-prison pipeline', but the evidence for a causal relationship has remained weak due to a number of intractable methodological challenges (Hirschfield 2018; Farrell *et al.* 2020).

In this study, we aimed to advance the evidence around the links of permanent school exclusion with serious violent offending by emulating a randomized controlled trial using a target trial framework, thus overcoming some of the limitations of previous research.

SCHOOL EXCLUSION AND SERIOUS VIOLENCE

School exclusion in the United Kingdom

In the United Kingdom, a child can be suspended or permanently excluded from a school on disciplinary grounds for behaviour in and outside school. This step is usually taken in response to repeated disruptive behaviour or breaches of school rules or to safeguard other children or staff. All suspensions and exclusions must be legal, take into account the school's duty of care to the child and must be recorded formally.

Across the United Kingdom, exclusion practices vary considerably. While Scotland has, in practice if not officially, abolished permanent exclusion (there was a single permanent exclusion among almost 700,000 students in 2020/21), around 1 in 2,000 students were permanently excluded in England that year. In Northern Ireland and Wales, around 1 in 15,000 and 1 in 3,500 students were excluded (respectively). Furthermore, local authorities and schools can set their own exclusion policies and practices and there are differences in the use of exclusion across areas (Cathro *et al.* 2023).

When a child is excluded, the local authority must arrange alternative education provision within 6 school days. During this interim period, the child's parent/guardian is legally responsible for ensuring that the child is not in a public place during school hours. Alternative education provision could include a new school, a pupil referral unit or other approved provider (Department for Education 2016). Around 1 year following exclusion, approximately 40 per cent of excluded children who are still required to attend school are attending a pupil referral unit, one-quarter are attending a new mainstream school, around 1 in 10 are in some other form of education and just under a quarter do not attend any form of alternative provision (FFT Education Data Lab 2023). Attendance rates in pupil referral units are poor: the overall absence (Department for Education 2024) rate in 2022/23 was 41 per cent compared to 7 per cent in mainstream education and 82 per cent of children were persistently absent (ibid.) compared to 21 per cent in mainstream education. Alternative provision school environments and governance are, in general, less thorough than in mainstream schools (ibid.). The combination of imported behavioural and psychological needs, absence, weak governance and insufficient resources to deal with these complex needs means that alternative provision often provides a weaker educational experience for excluded children (Ofsted 2016).

Risk factors for exclusion

In England in 2020/21, male pupils were approximately 3.5 times more likely to be excluded than female pupils (Department for Education 2024). The peak age for exclusion was 14 years. White, Black, Mixed and Minoritized ethnicity students were excluded at similar rates (between 0.04 per cent and 0.06 per cent per year) with Asian students and ethnicity unclassified students being excluded at lower (0.02 per cent) and higher annual rates (0.08 per cent), respectively. Students receiving special education needs provision and who were eligible for free school meals were excluded around four times more frequently than students without special education needs/not eligible for free school meals. An analysis (Jay *et al.* 2023) of over one million students in the United Kingdom aged between 14 and 16 years found that experience of social care before age 12 and having special education needs were strongly predictive of exclusion. These findings align with earlier analysis of administrative data (Paget *et al.* 2018) but reflect a significant decrease in disproportionate rates of exclusion across ethnic groups in the 2010s.

Other risk factors for being excluded from school in the past year identified through a longitudinal cohort study (ALSPAC) in Bristol, UK (ibid.) included lower socio-economic status, mental health difficulties, communication and language difficulties, involvement in bullying, lower parental engagement with education, maternal mental illness, low educational attainment, having a poor relationship with their teacher and antisocial activities.

School exclusion, offending and serious violence

A persistent criticism of school exclusion is that it is a cause of later criminal behaviour, particularly violence. As described below, there is a strong correlation between the two. The section that follows details the theoretical and empirical connection but also notes the substantial limitations in theory and evidence that leave unanswered questions about how one may cause the other.

Theoretical background

Several theories can explain how exclusion could lead to the perpetration of serious violence. At the individual level, the formal exclusion process may label (Berridge *et al.* 2001) a person as a 'troublemaker' or 'lost cause', an identity that they may adopt (Lemert 1951), or that leaves them 'angry, dejected and isolated' (Billingham and Irwin-Rogers 2022: 91). Removal from school also means removal of or impaired access to learning and behavioural support, particularly for

those with special education needs. Time spent outside the supervision provided by formal education creates greater opportunity for criminal activity (Hirschfield 2018) or being criminally exploited (Commission on Young Lives 2022), which is exacerbated by associating with similarly-excluded (Berridge *et al.* 2001) peers who may be involved in offending. The stigma of exclusion may damage that person's relationship with their family, peers and community (Holt 2011), which could undermine the availability of social support at this crucial period or at later turning points in life. In the longer term, exclusion damages employment prospects by impeding the potential to achieve qualifications (Sutherland and Eisner 2014), which can later restrict opportunities and create strain that may lead to offending. Finally, macro-socioeconomic factors, such as economic austerity, may change the causal relationship between school environment and offending (Farrell *et al.* 2020).

While the theoretical connections between exclusion and offending are highly plausible and observational research consistently provides compelling evidence of a connection between exclusion and later violence, a counter-argument to the position that exclusion is a cause of later offending is that their relationship is confounded by one or more common causes. This counter-argument is plausible: in 2022/23, for example, physical assault against another pupil (15.3 per cent of all exclusions) and physical assault of an adult (11.9 per cent) were two of the four most common reasons for exclusion (Department for Education 2024).[1] It is clear that school exclusion and perpetrating serious violence are likely to share many common risk factors. As such, the influence of confounding is the primary challenge to determining whether the observed association represents a causal relationship.

Empirical evidence for a school exclusion-offending relationship

Given the weight of observational evidence that exclusion and offending are connected, any attempt to randomize the allocation of exclusion would violate the principle of equipoise and likely be considered unethical. With randomized controlled trials unavailable, several studies have attempted to use observational data to estimate the impact of exclusion on offending or serious violence.

A meta-analysis (Gerlinger *et al.* 2021) of observational studies found consistent evidence of a positive association between exclusionary practices (including in and out-of-school fixed-term and permanent exclusions) and offending. Although the study was limited to the US literature and the authors report evidence of file-drawer bias, which may have artificially inflated the observed effect, the relationship was, nonetheless, compelling. Even when using the Supplementary Tables to limit the exposure to out of school suspensions and exclusions, restricting the outcome to violent offending and the study design to longitudinal research (that is able to make a temporal distinction between exposures and outcomes and thus is better at providing evidence on the direction of an association), a strong relationship can still be observed.

UK evidence

Although there are differences in exclusion policies across the world (Deakin et al. 2018), primarily in the prevalence and length of exclusions, the reasons for exclusion and the way in which they are executed are similar enough that exclusions can be viewed as a homogeneous exposure between countries. However, the same cannot be said for how crime occurs, how it is defined and how it is captured in administrative data across jurisdictions, meaning that evidence on the effect of exclusion on violence may not be generalizable between countries. Thus it is important to consider evidence from the United Kingdom.

1 Persistent disruptive behaviour (38.5 per cent) and verbal abuse of an adult (12.35 per cent) were the first and third most common reasons for permanent exclusion, respectively (Department for Education 2024).

A joint report by Department for Education and Ministry of Justice (2022), using a linkage of national education and justice records, demonstrated that permanent exclusion from school is a relatively common event in the lives of teenagers later convicted of serious violence. Of the 1.63 million pupils in England who were aged 16 years between 2012 and 2015, approximately 1 per cent had been permanently excluded. Of these, 15 per cent had a caution or conviction for serious violence before age 18, compared to around 1 per cent of those who were not excluded.

This report (ibid.) also provides information about the timing and ordering of offending and school exclusion. Of children who were convicted or cautioned for serious violence and who were permanently excluded at any point during their time in school, the exclusion preceded the offending by at least 1 month in 62 per cent of cases and followed the offence by at least 1 month in 21 per cent of cases, demonstrating that the association is not universally in one direction. In a subsequent multilevel analysis (Fuller and McNally 2023) using the same dataset, the authors concluded that being permanently excluded after year 7 (age 11–12 years) was not a strong predictor of being convicted or cautioned for serious violence. However, their models included factors that could have been on the causal pathway between exclusion and violence, such as other offending and attendance at alternative provision, so the results are difficult to interpret.

A longitudinal study (McAra and McVie 2012) of people in Edinburgh that combined self-report and administrative records found that having been excluded by age 14 years was associated with an increased risk (odds ratio 1.5; 95% confidence interval: 1.1–2.3) of having perpetrated serious offending at 15 years.

As with the international evidence, these UK studies were limited by their vulnerability to the influence of confounding variables. In an attempt to overcome this, Cathro *et al.* (2023) leveraged changes in school disciplinary policy to estimate the causal effect of exclusion on later custodial outcomes. A school becoming an 'academy' (Gerlinger *et al.* 2021) was typically associated with stricter disciplinary codes, resulting in increased rates of school exclusion. Thus, the authors claimed, the rapid change caused by 'academization' could serve as an instrumental variable to help isolate the effect of exclusion on offending because it held all factors constant except the probability of exclusion. They compared outcomes for students who attended an academy as a Year 10 (aged 14–15 years) pupil in the year of or the year following conversion to those among students whose school had not yet converted to an academy when they were in Year 10. The research indicated that permanent exclusion increased the probability of receiving a custodial sentence in the next 2 years by 33 per cent.

Methodological challenges in the exclusion-offending literature

As described above, the evidence for the link between exclusions and offending comes, necessarily, from observational studies. Typically, these have compared offending outcomes in students who were excluded to those for students who were not. Acknowledging that there may be underlying differences in the excluded and not excluded groups, most have attempted to adjust for these differences—for example, via the inclusion of relevant covariates in regression models. However, this does not address the intractable problem of unmeasured confounding factors. This is particularly an issue when using administrative data. These have large samples sizes, low risk of attrition and consistently measured records of exclusion and offending alongside indicators of attainment, learning difficulties and school-level measures, but do not capture personal experience and attitudes, peer relationships and exposures outside school, such as family breakdown, peer influence and undetected offending. In contrast, cohort studies are often based on self-report, capture a wider range of experience, behaviour and outcomes, particularly, in this case, undetected offending. However, these often have low rates of rare exposures and outcomes, likely have greater measurement error and, frequently, study attrition is correlated with exclusion and offending.

Although the study by Cathro *et al.* (2023) overcomes some of these limitations and is the most rigorous in terms of its ability to determine the effect of exclusion on offending, it too has limitations. Firstly, it relies on the assumption inherent in instrumental variable studies that 'academization' and the change in disciplinary policies had no effect on discipline in the school, nor did it affect the likelihood of offending except through its influence on exclusion. Farrell *et al.* (2020) have presented evidence to the contrary. Second, it is unlikely that academization affected the probability of exclusion for all behaviours but instead created a lower threshold for exclusion: while students at the most extreme levels of disciplinary failure will not have been affected by academization, students whose behaviour was borderline eligible for exclusion will have been. A further limitation is that there was no data on when the offence occurred. Given that there is a delay—often lengthy—between offending and sentencing taking place, it is possible that some of the offences in fact occurred prior to academization. Finally, while a custodial sentence is a valid indicator of more serious offending, particularly for children aged 15–17, it is plausible that having been excluded from school may have affected the likelihood of a custodial sentence being imposed in court.

Theoretical challenges in the exclusion-offending relationship

The plausible mechanisms by which exclusion might cause offending remain unclear, which, in turn, creates further methodological challenges to modelling a causal relationship.

Timing

In particular, the timing of exclusion is an important consideration. There may be time-varying factors that impact how exclusion affects offending. Children excluded earlier and later in their lives may have very different paths into offending (although the child-level predictors (Paget *et al.* 2015) of exclusion by ages 8 and 16 years are similar), but these are not accommodated in the theoretical explanations. Furthermore, even the time of year at which a child is excluded offers different opportunities to be involved in offending (Rock *et al.* 2008). Self-report measures of exclusion simply capture exclusion that happened within a wide time band, such as the preceding year, as opposed to precise timing. Understanding the mechanism would assist in identifying comparison groups with equivalent periods of exposure.

If exclusion does cause offending, it is also unclear what the time to onset is likely to be. Some authors have described exclusion as a 'trigger' that can rapidly result in offending. This is consistent with routine activities (Cohen and Felson 1979) explanations whereby exclusion removes the supervision afforded by the school, with differential association theories whereby exclusion results in a more criminogenic peer group and with labelling theories (Lemert 1951) where the exclusion may result in a change to identity. However, the literature does not provide anticipated timings through which such mechanisms would occur. A reasonable expectation is for these effects to occur within 1 year of an exclusion, but each could plausibly take longer to emerge and the effects could extend this. Alternatively, strain effects (Merton 1938), whereby exclusion from school limits the opportunity to succeed within society's dominant norms and structures, would take longer to emerge. The lack of theoretical clarity presents a challenge for causal modelling: too brief a timeframe could miss important effects, and be obscured by residual effects of unmeasured offending in the pre-exclusion period; too long a timeframe could mean that the 'signal' of exclusion is lost amid a range of other contributing factors. Observational studies have commonly (Hirschfield 2018) focused on the short-term relationship, typically using follow-up periods of 1–2 years. This may be driven by theoretical assertions that exclusion immediately imbues a 'trouble-maker' identity and damages social support as well as creating greater

opportunity for offending and association with criminal peers. It may also reflect a convenient path of least resistance to identifying a 'signal'.

Types of offending

Although there is some evidence that adolescents and young adults are diverse in their offending behaviour, it is still valuable to explain how exclusion could influence different offending types. As externalizing behaviours—persistent disruptive behaviour and assault—account for almost two-thirds of school exclusions, it is plausible that violence against the person would be the most common offending consequence. Similarly, as externalizing behaviour is more visible to school authorities than internalizing behaviour, this type of behaviour may be more likely to result in exclusion than other forms of disruption that are correlated with offending.

School-level effects

As exclusion takes place within the disciplinary environment of a school, the probability of exclusion and the culture of a school are highly correlated. Consequently, it is plausible that offending arises as a consequence of being exposed to a more punitive disciplinary environment rather than the effect of exclusion. Theories of the exclusion-offending relationship have not been specific about how these two competing effects should be considered but it is important as any changes in the use of exclusions will affect school policy.

Research questions

In this study, we examine the associations of permanent exclusion from school with (i) serious violence and (ii) homicide or near-miss homicide and investigate whether there is any evidence that these associations could be causal.

METHODS

Study design: Target trial emulation

In recent years, a new approach for using observational data to estimate causal effects in the absence of randomized experiments has emerged from epidemiology. A target trial (Hernán *et al.* 2022) framework is a set of guidelines for designing and analyzing observational data in a way that closely matches the conditions of a (hypothetical) randomized controlled trial and avoids some of the obstacles to making robust causal inference that are present in many observational studies. The framework first encourages researchers to imagine what a randomized controlled trial for the focal effect would look like and then attempt to emulate this with observational data. Considerations include 'eligibility criteria, treatment strategies, treatment assignment, the start and end of follow-up, outcomes, and causal contrasts' (ibid.: 2446). Researchers use observational data to identify eligible samples, allocating them to their intervention condition based on their observed exposure, following them up for equivalent time and, as much as possible, adjusting for confounders across the arms of the 'trial'. Although the problem of unmeasured factors cannot be addressed, such a framework addresses other challenges seen in the existing evidence on the exclusion-offending relationship.

Data sources

We used data from the Ministry of Justice (MoJ) and Department for Education (DfE) data share carried out in 2020, linking information held in the National Pupil Database (NPD) and the Police National Computer (PNC). This share included just over 15 million individuals born from September 1985 to August 2007. The PNC data covered offending data (convictions and

cautions) up to the end of 2017. From the NPD, we used data from the following datasets: pupil-level census, Key Stage 2 (KS2) attainment data,[2] absence, exclusions, Child in Need (CiN)[3] data and Child Looked After (CLA)[4] data.

Pre-registration, data access and ethical approval

Our research questions, methods and analysis plan were pre-registered on the Open Science Framework (https://osf.io/t8skb/). Access to the data was approved and facilitated by the MoJ and DfE; this included the completion of an ethical self-assessment. The study was approved by the Faculty of Health Sciences Research Ethics Committee at the University of Bristol (REF: 10845). This was a secondary data analysis of pseudonymized administrative data collected by the DfE and MoJ. The DfE and MoJ have privacy notices explaining how they process any personal or other data that they collect. The data were accessed via the ONS Secure Research Service (SRS). All results were assessed for statistical disclosure risk by the ONS. The text and results of this report were reviewed by the MoJ and DfE in line with the terms of access to and use of the data (Department for Education 2022).

Outcome definitions

The outcome definitions consist of Home Office offence codes and descriptions. The definition of near-miss homicide is one used by the Home Office and College of Policing (https://www.college.police.uk/guidance/homicide-problem-solving-guide/scanning#near-miss-data/).

Our definition of serious violence has been used previously (see https://www.college.police.uk/guidance/homicide-problem-solving-guide/scanning#near-miss-data/ -care-and-offending) and includes the following categories of offence: indictable only offences classified as violence against the person, indictable only offences classified as robbery, and indictable only or triable either way possession of weapons offences. The definition of homicide and near-miss homicide is provided in Supplementary Table A.S1.

Permanent exclusion

The exposure was having been permanently excluded from school. A permanent exclusion can begin on any school day in a person's school career. By law, the day and reason for any permanent exclusion must be recorded by a school and this information shared with the local authority and the DfE.

Target trial specification and emulation

Key elements of the protocol for the target trial and how each of these was emulated are outlined in Table 1.

Sample and eligibility criteria

We included all pupils born between September 1994 and August 2004 who appeared in the school census in Year 6 (10–11 years) and in the autumn census in Year 7 (11–12 years) and who had no conviction or caution for any offence before the start of Year 7. 'Eligibility' for

2 Key Stage 2 refers to the school years between the ages of 7 and 11 years.

3 Under Children Act 1989 s 17, a child will be considered in need if they are unlikely to achieve or maintain or to have the opportunity to achieve or maintain a reasonable standard of health or development without provision of services from the local authority; their health or development is likely to be significantly impaired, or further impaired, without the provision of services from the local authority; or they have a disability.

4 A child is 'looked after' if, for a period of 24 hours or more, there is no person who has parental responsibility for them; they have been lost or abandoned; or the person who has been caring for them is prevented (whether or not permanently, and for whatever reason) from providing them with suitable accommodation or care.

Table 1. Outline of protocol for a target trial to estimate the effect of exclusions on serious violence and homicide/near-miss homicide and how we emulated each element

Element	Target trial specification	Emulation
Eligibility criteria	Individuals aged 11–16 enrolled in a state secondary school who have engaged in behaviour deemed to be 'exclusion-worthy', had no conviction or caution for any offence before secondary school and no previous conviction or caution for serious violence	Individuals aged 11–16 enrolled in a state secondary school, had no conviction or caution for any offence before secondary school, had previously been suspended from school for at least 1 school day, had not previously been convicted or cautioned for serious violence and had not had a permanent exclusion in the previous 12 months
Treatment groups	Intervention group: individuals are permanently excluded from school Control group: individuals are not excluded from school	As per target trial
Treatment assignment	Random, unblinded assignment to either arm at enrolment	As per observed data: excluded individuals matched 1:1 with an unexcluded individual
Follow-up period	Follow-up starts on the day of randomization and ends when a serious violence / homicide or near-miss homicide offence occurs, or 12 months after randomization	As per target trial
Outcomes	Serious violence; homicide or near-miss homicide (defined above)	As per target trial
Causal contrast	Per-protocol effect Intention-to-treat effect	Observational equivalents of these (main analysis = per protocol)

inclusion in the target trial means not just that exclusion was available as a punishment, but that they engaged in 'exclusion-worthy' behaviour. Such behaviour is not captured in administrative data except when it results in exclusion. Thus, for each 2-week period[5] of the school year, pupils were eligible for inclusion if they had at least one suspension from school lasting at least two school sessions (1 school day) prior to that period, had not previously been convicted or cautioned for serious violence and had not had a permanent exclusion in the previous 12 months.

Comparison groups

During each 2-week period in which eligibility was assessed, every eligible pupil who was permanently excluded was matched (1:1) with an eligible pupil who was not excluded during that period. Pupils were matched exactly on: gender, ethnicity and duration of prior suspensions (grouped as: < 2 days, 2 to < 5 days, 5 to < 10 days, 10 or more days), number of previous convictions or cautions (grouped as: 0, 1, 2, 3 or more), percent unauthorized absence in the previous school year (grouped as: < 1 per cent, 1 per cent to < 5 per cent, 5 per cent to < 10 per cent, 10 per cent or more), Special Education Needs status (None, school action/school action plus, EHC plan) and whether or not they attended a school in London. Additional covariates extracted were KS2 attainment score (total of English and maths, converted to a percentage of

5 Local authorities and schools vary in terms of exact term dates. For simplicity, we divided each month of the school year (September—July) into two periods: 1–14th of the month and 15th—end of the month, acknowledging that these periods will including varying numbers of school days, depending on the month, year and local authority. We have referred to these throughout as 2-week periods.

the total possible score and grouped into quintiles); the following characteristics recorded during the previous school year: Free School Meals (FSM) status, Income Deprivation Affecting Children Index (IDACI), CLA status, CiN status; and the following school-level characteristics: % eligible for FSM, grouped into quintiles, and % White British, grouped into quintiles. School-level characteristics, including region, were those recorded in the autumn census of the current year, or the previous year if this was missing. This was to ensure that, for the majority of individuals, school-level characteristics applied to the school they were attending at the time of the exclusion.

Outcomes and follow-up

Outcomes were a conviction or caution for (i) serious violence and (ii) homicide or near-miss homicide. (Note that dates recorded in the data are dates on which the offence took place, as required for this analysis, not the date of conviction/caution.) Each pupil was followed up from study entry (the date of exclusion for each matched pair) until the outcome of interest or 12 months from study entry, whichever occurred first. Follow-up was censored for the matched pair (both the excluded pupil and their matched control) if the matched control was excluded. When this occurred, the matched control was assigned to the excluded group and a (new) matched control for that pupil was found.

Statistical analysis

The cumulative incidence of each of the outcomes was estimated using Kaplan–Meier survival analysis. Cox models were used to estimate hazard ratios comparing those permanently excluded to those not excluded. Robust standard errors were used to take account of the fact that some individuals were included in both the excluded and non-excluded groups and some individuals were excluded—or included as a control—more than once.

Sensitivity analyses

We carried out three sensitivity analyses. Firstly, we excluded pairs in which the offence took place within 1 week of the exclusion, under the assumption that serious violence happening immediately after an exclusion could plausibly have been linked to pre-existing circumstances which were a cause of the exclusion as well as the violence.

Second, we used a fixed bias analysis (Lash *et al.* 2021) to carry out a sensitivity analysis to unmeasured confounding. This consists of adjusting the observed association (e.g. relative risk) between an exposure and outcome by calculating what that relative risk would have been had the unmeasured factor or factors been measured and, in general, requires making assumptions about the relationships between the unmeasured factor and both the outcome and the exposure, as well as assumptions about the distribution of the factor (e.g. its prevalence in the case of a binary confounder). For our analysis, we assumed a prevalence of the unmeasured confounder among those not excluded of 10 per cent. We varied the strength of association between this confounder and exclusion (with odds ratios ranging from 2 to 10) and the strength of association between this confounder and the two outcomes (again, odds ratios ranging from 2 to 10). We used the episens (Orsini *et al.* 2008) command in Stata to carry out this analysis. For simplicity, we worked with odds ratios rather than hazard ratios in this analysis. For rare outcomes (such as serious violence and homicide/near-miss homicide) the odds ratio and hazard ratio will be similar.

In the third sensitivity analysis, we did not censor pairs when the control was later excluded. This would be similar to carrying out an intention-to-treat analysis in a randomized controlled trial, whereby individuals are analyzed according to the treatment to which they were randomized.

RESULTS

Following statistical disclosure control guidelines (Griffiths *et al.* 2019), all numbers in this report have been rounded to the nearest 10.

Descriptive results

Table 2 summarizes the numbers eligible for inclusion. Altogether, there were just over 6.7 million individuals born between September 1994 and August 2004. Just under 5.5 million of these appeared in the school census in Years 6 and 7 and had no conviction or caution prior to secondary school. Among these, 653,430 met the eligibility criteria at least once during Years 7–11.

Supplementary Table S1 shows key characteristics among the 653,430 meeting eligibility criteria and among all those born between September 1994 and August 2004. Crucially, those meeting eligibility criteria were 10 times more likely than the all-pupil population to be excluded at some point during Years 7–11. They were also more likely to be male, to be eligible for Free School Meals, to have SEN, to be Looked After or a Child in Need, to have a conviction or caution for serious violence by the age of 18 years, and—on average—had lower attainment in Key Stage 2 and higher IDACI scores.

There were 30,290 permanent exclusions among the 653,430 pupils who were eligible at some point during Years 7–11 (Table 3). Among these, 6,020 (20 per cent) had missing data on at least one covariate (matching factors or other potential confounders), a further 3,510 (12 per cent) did not meet eligibility criteria at the time of their exclusion and 480 (2 per cent) were not matched to a control. In the final analysis, there were therefore a total of 20,630 eligible exclusions matched to an unexcluded control (Table 3). Supplementary Tables S2 and S3

Table 2. Numbers in the study

Year of birth	All	In census Years 6 and 7	No conviction or caution prior to secondary school	Met eligibility criteria at least once during Years 7–11
Total	6,705,830	5,443,710	5,418,840	653,430

Table 3. Total numbers of exclusions among those ever eligible, those eligible at the time of exclusion and those matched

Not eligible							
School year	All	Missing data	Did not have at least one suspension lasting 2 + sessions	Exclusion in past year	Convicted / cautioned for serious violence	Not matched	Matched
7	3,450	1,160 (33%)[a]	310 (9%)	k	40 (1%)	80 (2%)	1,890 (54%)
8	6,580	1,290 (20%)	470 (7%)	k	200 (3%)	110 (2%)	4,480 (68%)
9	8,540	1,490 (17%)	460 (6%)	k	370 (4%)	160 (2%)	6,020 (70%)
10	8,820	1,550 (18%)	410 (5%)	k	460 (5%)	110 (1%)	6,250 (71%)
11	2,900	520 (19%)	120 (4%)	k	190 (6%)	30 (1%)	2,030 (70%)
Total	30,290	6,020 (20%)	2,120 (7%)	k	1,240 (4%)	480 (2%)	20,630 (68%)

[a] Note that this proportion is higher because although absence data started in 2005/2006, this was mainly for secondary schools, thus Year 6 absence is missing for all those who were in Year 7 in 2005/2006 (born 1994/1995).

k: Used when a result which is not zero would appear as zero due to rounding.

show the characteristics of all individuals who were eligible for inclusion at the start of Years 7 and 10, respectively, compared to those with no missing covariate data. The distribution of characteristics was similar in both cases. These school years were chosen because Year 7 was the starting point for our analysis and Year 10 was the school year in which the highest number of exclusions occurred.

Supplementary Tables S4–S8 show characteristics before and after matching for Years 7–11 (respectively). Before matching, excluded individuals were much more likely than unexcluded individuals to have had: prior convictions and/or cautions, a greater number of school suspensions, more unauthorized absences, and were more likely to be eligible for FSM, be a child in need, have SEN (school action or school action plus but not have an EHC plan). In addition, excluded individuals were slightly more likely to be White British, and KS2 attainment was—on average—slightly lower and IDACI scores slightly higher. In some school years, those who were excluded were more likely to be male and slightly more likely to be Looked After; the former was particularly the case for Years 7 and 11. School-level characteristics, including region, were generally quite similar prior to matching. After matching, differences in most factors were eliminated or reduced, although there were small remaining differences for some factors in some school years and the distribution of individuals across regions became more imbalanced (Supplementary Tables S4–S8).

The final dataset of 20,630 exclusions and their matched controls included 38,230 individuals. In 19,020 cases, the matched control was never excluded and, altogether, 17,810 individuals were included only as controls (< 100 three or four times, 1,030 twice, 16,700 once). Similarly, 18,880 individuals were only included as excluded (230 twice) and 1,540 individuals were included at least once as a control and once as excluded. Overall, follow-up was censored (when the unexcluded control was excluded) in 4 per cent of the matched pairs (Table 4).

Main results

Of the 20,630 pairs, 50 were excluded from the analysis of serious violence and fewer than 5 from the analysis of homicide and near-miss homicide because the offence took place on the same day as the exclusion. There were 990 serious violence offences and 20 homicides or near-misses in the 12-month period following exclusion among the excluded group compared to 500 and < 10 (respectively) among the unexcluded controls (Table 5). Figure 1 shows Kaplan–Meier survival curves for serious violence comparing those excluded to those not excluded. After adjusting for covariates, the hazard ratio was 2.05 (95% CI: 1.83, 2.29) for serious violence and 2.36 (1.04, 5.36) for homicide/near-miss.

Table 4. Numbers censored by school year

School year	Total number of matched pairs	Percentage where the unexcluded control was excluded within the 12-month follow-up (so follow-up for the pair was censored)
7	1,860	5%
8	4,480	6%
9	6,020	4%
10	6,250	2%
11	2,030	[k]
	20,630	4%

k: Used when a result which is not zero would appear as zero due to rounding.

Sensitivity analyses

Excluding pairs where the serious violence took place within 7 days of the exclusion slightly reduced the hazard ratio for serious violence—adjusted hazard ratio = 2.01 (95% CI: 1.79, 2.25).

Supplementary Table S9 shows the results of the fixed bias analysis. For example, an odds ratio for exclusion of 4 and an odds ratio for serious violence and homicide or near-miss homicide of 4 would be expected to reduce the odds ratio for exclusion-serious violence to 1.37 and the odds ratio for exclusion-homicide/near-miss to 1.50.

Table 5. Rates of serious violence and homicide/near-miss and hazard ratios (HRs) comparing excluded to unexcluded

Outcome	Excluded		Not excluded		Unadjusted HR (95% CI)	Adjusted[a] HR (95% CI)
	Number of events	Years follow-up	Number of events	Years follow-up		
Serious violence	990	19,680	500	19,960	2.01 (1.80, 2.24)	2.05 (1.83, 2.29)
Homicide and near-miss	20	20,210	<10	20,210	b	2.36 (1.04, 5.36)

[a] For serious violence: adjusted for school year, CLA and CiN status, exact number of prior offences, exact duration of prior suspensions, region (of school), % White British (quintiles) and % FSM (quintiles); for homicide/near-miss: adjusted for exact number of prior offences, exact duration of prior suspensions, CLA and CiN (we did not adjust for the complete set of variables because numbers were very small; however, further adjustment for other variables—individually—did not change the estimate of the hazard ratio).

[b] Unadjusted hazard ratio and its confidence interval supressed for disclosure control purposes.

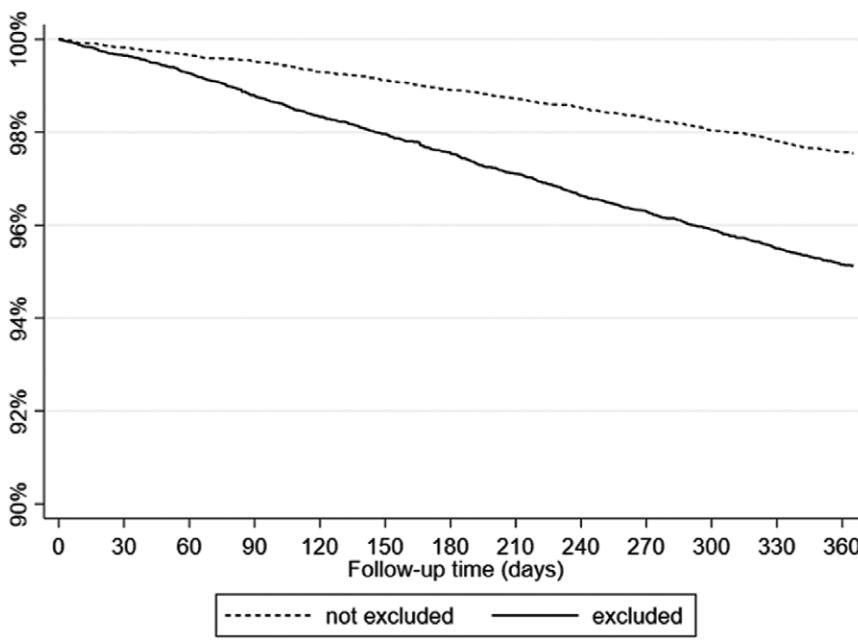

Fig. 1 Kaplan–Meier survival plot: serious violence by exclusion

Table 6. Hazard ratios when pairs were not censored if the non-excluded control was excluded

Outcome	Unadjusted HR (95% CI)	Adjusted HR (95% CI)
Serious violence	1.99 (1.79, 2.21)	2.02 (1.81, 2.25)
Homicide and near-miss	1.80 (0.85, 3.79)	1.92 (0.88, 4.18)

Finally, not censoring pairs when the control was excluded slightly reduced the estimated hazard ratios for serious violence and resulted in a larger reduction in the hazard ratio for homicide/near-miss homicide (Table 6).

DISCUSSION

Summary of findings

Compared to their matched controls, children who were permanently excluded were twice as likely to have a conviction or caution for serious violence and over twice as likely to have one for homicide or near-miss homicide within 12 months of their exclusion.

Over a 12-month follow-up period, approximately 20,600 matched pairs were convicted or cautioned for 1,500 serious violent offences and almost 30 homicide or near-miss homicide offences. Among the eligible sample, 6 per cent were convicted or cautioned for serious violence by the age of 18 years. This compares to 1 per cent among all those born in the same 10-year period, indicating that the eligible sample, regardless of exclusion status, perpetrated serious violence at a considerably higher rate than the population from which they were drawn. The many matching and control variables employed in this study design would have been taxing for a sub-national data set but, in both cases, the matching performed well and the majority of excluded children were matched to a control in the same time period.

By matching periods of follow-up, applying eligibility criteria and matching or controlling for potential confounders, this study has overcome some of the limitations of past observational studies in this area, resulting in the most compelling evidence to date of a direct link between exclusion and perpetration of violence. Nonetheless, the potential influence of factors not measured within this data set prevents our making a claim that this link is causal. In the sections that follow, we discuss the strengths and limitations of this work, how it advances our understanding of the exclusion-violence link and the implications for education and violence prevention policy.

These findings are broadly consistent with those of the existing literature. For example, using a different outcome Cathro *et al.* (2023) found that changes in exposure policy increased the risk of custody by 33 per cent and a cohort study found a 1.5 times increased risk of self-reported offending following exclusion from school (McAra and McVie 2012).

Strengths and limitations

This study leveraged a data linkage between education and criminal justice records in England to support the emulation of a randomized controlled trial to examine the link between permanent exclusion and serious violence. To our knowledge, this is the first published attempt to emulate a randomized controlled trial—a target trial—both for an educational exposure and for a criminal justice outcome. In settings such as criminal justice, where forms of punishment and social control are common practice, but cannot ethically or practically be allocated randomly, target trial emulation offers a promising new way to advance understanding of effective policy.

It offers a framework for overcoming some of the sources of bias commonly found in the literature. The potential for applying target trial emulation in criminal justice in England is enhanced by the recent availability of population-wide criminal justice data linked to education and family courts data (Ministry of Justice 2022). We encourage others to explore the potential of using this method.

As exclusion was not allocated randomly, we cannot claim that the observed relationship is causal. A component of the target trial framework is to ensure that the control cohort is eligible for the intervention. In the case of school exclusion, this requires them not just to be attending school but to have engaged in exclusion-worthy behaviour (but were perhaps in a school that had a higher threshold for exclusion so were not excluded). Such information is not collected in routine administrative data sets and cannot be confirmed. For exclusion, eligibility for inclusion in the study was limited to those who had a past suspension and individuals were matched on an extensive range of covariates. However, it is likely that there are many individuals included as controls who never engaged in behaviour that could have resulted in a permanent exclusion while inclusion in 'official' records of indiscipline could have a stigmatizing effect that exacerbated the risk of further disciplinary punishment (Carlen *et al.* 1992).

We carried out a sensitivity analysis in which we did not censor pairs when the matched control was excluded. Individuals within these pairs are likely to have been, on average, more similar to each other (with respect to unmeasured factors) than to those in pairs in which the matched control was not excluded within the 12-month follow-up period. The fact that the relative risk estimates were reduced when not censoring these pairs suggests the presence of at least some unmeasured confounding. Unfortunately, the likely impact of unmeasured confounding cannot be quantified. That said, our sensitivity analysis to unmeasured confounding indicated that an unmeasured confounder would have had to have quite a strong relationship with both the exposure and the outcome to fully explain the increased risk of serious violence and homicide/near-miss homicide among those excluded or persistently absent. By matching on strong predictors of serious violence, notably prior offending, we will have accounted for many unmeasured differences between the groups. However, there are also many ways in which excluded children could differ from their matched controls despite having a similar offending history. Plausible candidates include parental involvement in the child's education, individual personality or behavioural factors, current mental health and family effects such as parental and sibling education and history of exclusion and/or school absence. Furthermore, and as alluded to above, we cannot rule out the influence of unmeasured precipitating factors or events that were influential both in the exclusion and the emergence of serious violence.

We suggest that a particular limitation is the absence of a measure of parental or guardian involvement in a child's education. Exclusion is a course of last resort and a parent or guardian is likely to have been invited to be involved in the decisions to exclude a child (Department for Education 2024). That involvement can often be the deciding factor in the decision to exclude or not. Similarly, parental engagement or the absence of guardianship is a predictor of violence, creating a potentially confounding association between exclusion and violence. As a construct such as parental engagement is unlikely to be measured directly in administrative data, adjusting for it requires alternative data sets. Longitudinal cohort studies may offer a viable alternative source of data but will have its own shortcomings in terms of the exchangeability of excluded and non-excluded children.

Finally, this work is more firmly focused on understanding the link between exclusion and serious violence than other studies of the exclusion-offending association. While this is a strength in helping to narrow down the theory about how exclusion and violence may be connected, the diversity of offending in late adolescence and early adulthood (Piquero 2000), could mean that we have taken too narrow a focus and may be missing a wider exclusion-offending association. Our

observation in relation to the homicide outcome is particularly vulnerable to this because of the rarity of the outcome, meaning that it is likely susceptible to a very wide range of factors.

Implications

The outcome variables—serious violence, homicide and near-miss homicide—were selected to provide a high bar for the seriousness of the outcome. Mid-to-late adolescence is the peak time for the prevalence of criminal offending across the life course but, as self-report and official records indicate, the gravity of these offences is generally low (Youth Justice Board 2024). Between three-quarters and almost all of the children included in our sample (depending on the school year) had no previous convictions or cautions for any offence. Thus, their subsequent conviction or caution within 12 months for serious violent offending reflects an alarming change in behaviour or at least a rapid escalation in the seriousness of offending behaviour.

In addition to providing some of the most robust evidence to date of the association between exclusion and serious violent offending, this study has implications for our understanding of why that association might exist. As noted earlier, there are several competing theories for an exclusion-offending link and the veracity of each needs to be determined if the risk of violence following exclusion is to be effectively addressed. Our observation that the risk of serious violence increased within 12 months suggests that (if the link is indeed causal) violence results from changes to proximal, environmental factors, such as increased opportunity for offending through reduced supervision, the removal of school-based support and changes in peer groups or exposure to exploitation. There could be a separate mechanism acting via longer-term impacts on educational outcomes and socio-economic strain, which would typically take more time to emerge. As discussed above, by restricting follow-up to 12 months, we were unable to investigate this and detecting such a signal over the longer term would present significant challenges.

This evidence of a link between a child's exclusion and their risk of perpetrating serious violence should escalate discussion of how 'exclusion-worthy' behaviour in schools can be understood and addressed. One possible response to this evidence might be a proposal that exclusion from school in England should be minimized further or even prohibited as has been the case in Scotland and, to a lesser extent, Northern Ireland. Indeed, the minimization of exclusions is currently advocated by the HM Government (2018) while the Department for Education views them as a 'last resort' (Department for Education 2024: 3). However, a recent analysis of exclusion reduction in Glasgow (Billingham and Gillon 2024) suggested that many of the circumstances which facilitated that reduction—such as a shared sense of 'crisis', a pro-inclusion Zeitgeist across policy domains, and an unfragmented education system—tend to be absent in the London context (as well as, arguably, in England as a whole).

By focusing only on outcomes for the individual, we have not considered or weighed the implications for school safety, the learning environment or staff and student morale that are frequently identified as necessary reasons for powers of exclusion. There is little robust literature exploring the effect of excluding a disruptive or aggressive peer on the wellbeing, safety or attainment of their classmates, nor on the impact on the school staff and environment, but a recent survey of young people found that 20 per cent had skipped school due to feeling unsafe (Youth Endowment Fund 2023). This rate was doubled among children who were eligible for free school meals. It is unclear whether the exclusion of individuals would affect this rate—analogously, for example, fear of crime is not connected to incarceration rates (Corradi and Baumer 2022)—but recent variations in exclusion rates at national and local levels may provide some insight into the wider effect of exclusions.

The inability to account for unmeasured factors through a randomized controlled trial means that the causal connection will likely never be firmly established. Consequently, in the debate about the effect of exclusions on serious violence or the effect of disruptive or violent peers on

classmates, both sides will always have a counter-argument to each other's claims. However, exclusion is not a zero-sum game and it is plausible that this debate would never be settled with evidence alone. Over the longer term, addressing the common causes of exclusion and violence should be the greater priority. It is clear that the children 'eligible' for exclusion in this study had many risk factors for violence. Compared to the total cohort, those eligible were more likely to be male, spent more days suspended, came from more deprived households and more deprived areas, had greater special educational need and were more likely to have been a child in need or looked after by their local authority. While exclusion may have served to exacerbate the excluded child's situation, they were already at much higher risk for being involved in violence and warning signs for a child's exclusion will have been clear in many cases both within and beyond the school. A more inclusive system preventing violence through school exclusion is one that can predict, accommodate and, ultimately, reduce the occurrence of exclusion-eligible events and one that addresses the circumstances that make exclusion a viable and acceptable response to school discipline. That system is also one that can reduce the events outside school that lead to exclusion from it and views schools as being part of a system that can both produce and prevent violence. The Serious Violence Duty, with a focus on early intervention through multi-agency collaboration, presents an opportunity to prevent violence by reducing the factors and events outside school that precipitate exclusion from it. As noted in the Timpson Report (Timpson 2019), although exclusion from school may be a trigger and a forebear of serious violence, preventing violence cannot be the responsibility of schools alone.

CONCLUSION

In summary, we have shown that permanent exclusion is associated with approximately a doubling of the risk of having a conviction or caution for both serious violence and for homicide or near-miss homicide in the 12-month period following the exclusion. We cannot determine whether these associations are causal and it is highly likely that they could be at least partly due to unmeasured confounding. However, we have demonstrated that an unmeasured confounder would have to be strongly related to both the exposure and outcome in order to fully explain the associations. Regardless of this, children who were permanently excluded were already at higher risk of serious violence offending at the point of exclusion. This, together with the fact that questions of causality are likely never to be resolved, suggests that targeting these upstream common causes of exclusion and violence should be prioritized.

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SUPPLEMENTARY MATERIAL

Supplementary material is available at *British Journal of Criminology* online.

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