

**Ministry of Justice – Department for  
Education linked dataset**

***Feasibility of evaluating early interventions  
for violence prevention:***

***Generating matched control groups***

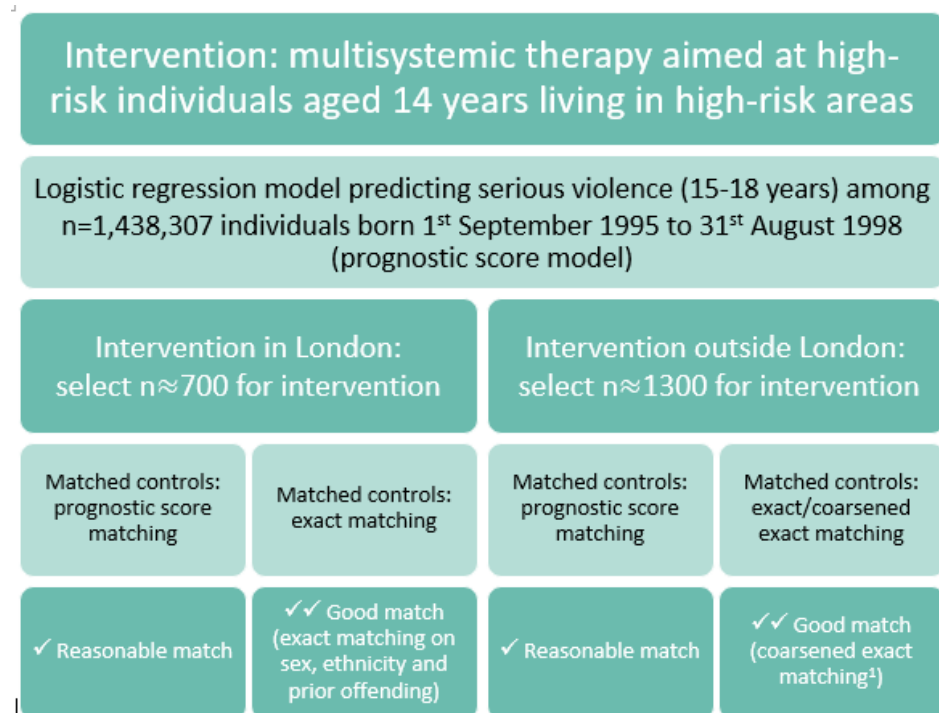
**July 2022**

## Executive summary

Violence prevention research often suffers from having control samples that are a poor match for intervention groups. The substantial increase in the funding of violence prevention initiatives and interest in their effectiveness requires that more rigorous techniques for identifying comparison groups are established. The Ministry of Justice-Department for Education (MoJ-DfE) linked dataset, which combines individual-level educational data for over 15 million people in England with information on criminal justice records (where present), offers the potential to identify well-matched comparison groups for use in evaluations of violence prevention interventions.

The aim of this work was to evaluate whether the linked MoJ-DfE dataset could be used to identify matched controls for interventions aimed at reducing violence in young people. To do this, we used the linked dataset to simulate a violence prevention intervention in England, identified a treatment group and then tested the performance of different matching methods – prognostic score matching<sup>1</sup> and coarsened exact matching – in identifying a matched control group.

Figure 1.1: Summary of approach and findings



- Matched on sex, ethnicity, offending before 14, fixed exclusions in years 5&6 and 7&8, FSM in years 5-6, CiN status, IDACI score in years 7-8 (quartiles) and school-level % FSM (quartiles).

<sup>1</sup> Prognostic score matching is analogous in approach to propensity score matching but uses a prognostic score (i.e. obtained from a model predicting of the outcome) rather than a propensity score (obtained from a model predicting intervention status) (Hansen 2008).

We chose multisystemic therapy as the simulated intervention. Based on effect sizes for this intervention types in the literature, to detect a 25% relative reduction in the risk of serious violence from 15-18 years with power 80% and significance level 5% would require a minimum sample size of approximately 700 if the intervention took place in London and 1300 if it took place outside London (owing to differences in rates of violence).

Given this large scale, we assumed that the intervention would be coordinated by local authorities who would identify young people in their area (aged 14 years) at risk of perpetrating serious violence. Eligibility for the intervention would be based on having an offending record before age 14, one or more permanent exclusions in Years 3 to 8, or a combination of other risk factors. We then sought to identify control groups for this simulated sample using two different matching procedures.

We found that when both intervention and control individuals were selected from high-risk areas, prognostic score matching and (coarsened) exact matching were both successful, resulting in groups that were well-balanced on most key factors. If controls were not restricted to those attending schools in high-risk areas, we found that exact/coarsened exact matching performed better than prognostic score matching. The advantages of exact matching are, firstly, it avoids the need to develop – then later update – a potentially complex prognostic model and, secondly, it can be used to ensure exact balance in the most important covariates.

Our findings would apply for any intervention targeted at an identifiable group of high-risk individuals, including a school-based intervention in which specific individuals (rather than all individuals in the school) were selected for the intervention. For a more universal intervention, exact matching on a few key covariates would be unlikely to be successful because the individuals in the intervention group (and the potential pool of controls) would be much more heterogeneous; either prognostic score matching or coarsened exact matching on all covariates in the prognostic model would have to be used and we have not investigated how well this would work.

The success of the matching is a valuable insight but must be considered alongside several observations. The first is that although the matching generally worked well in our example, there is no guarantee this will always be the case, and matching should not be seen as an equal substitute for the gold standard of a well-designed randomised controlled trial.

Importantly, for the future use of this dataset, we found that, because the risk of serious violence is so different in London compared to other parts of England, an intervention carried out in London would have to select controls from within London; similarly, an intervention outside London would have to select controls outside London. Failing to do this would result in a control group that was very dissimilar from the intervention group and, crucially, a control group with a very different risk of serious violence.

For the same reason, if the dataset were to be used for evaluating interventions, users would need to carefully consider what the inclusion criteria for the intervention group are (particularly if these are not very well-specified) and use these criteria to identify a potential pool of controls – failure to do so will result in a control group that is not comparable to the intervention group in ways that may not be apparent from measured variables.

We developed the prognostic model using data on individuals born from 1995 to 1998. It is possible that the relative importance of different covariates could change over time. As such, the model may need to be periodically updated and checked. Similarly, if definitions of the outcome change, or different outcomes are considered, the prognostic model would have to be developed and tested again.

If the dataset were to be used to evaluate an intervention in practice, it is likely that some individuals in the intervention group would have missing data on some of the covariates needed for matching. This would lead to a loss of power. The potential for successfully identifying appropriate matched groups does not overcome the limitations in the violence prevention field. The use of small samples with a relatively rare outcome when anticipated effect sizes are also small remains a pervasive problem. To ensure that precise estimates of effects can be determined, future violence prevention interventions must increase in scale.

Finally, the time, resources and technical expertise required to prepare the data – that is, to transform it from its raw form to a dataset ready for analysis – is an added cost to the routine use of the linked data for evaluation. There are several ways in which the use of this dataset for evaluation purposes could be made sustainable, efficient, and timely. Perhaps the most efficient of these would be for a government-funded team with experience in handling large administrative datasets to be commissioned to carry out such evaluations.

Further, the utility of the dataset will also depend on how frequently the linkage is refreshed and the dataset updated; as such, publishing a schedule of updates would allow users to incorporate the dataset into their evaluation plans.

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# 1 Introduction

## 1.1 Background

### 1.1.1 Serious violence

Violence in England and Wales has decreased since around 1995, but it still remains a pervasive problem that disproportionately affects young people in the most deprived communities (Office for National Statistics 2021; Yakubovich et al 2019; Brennan 2020; Morgan et al 2020). There were over 700,000 police-recorded incidents of violence with injury in 2019/20, affecting around 0.5% of the population, and the annual number of violent offences involving a knife or other sharp object has increased by over 50% in the ten years to March 2020 (Office for National Statistics 2021).

For perpetrators of serious violence<sup>2</sup> the punishments can be severe and result in substantial and long-term disruption to their lives. The causes of serious violence perpetration are numerous and complex but there is strong evidence that risk develops steadily over time, with young people who witness and experience violence in childhood and who have higher levels of school absence due to truancy, perform poorly in education or are excluded from school being at heightened risk of involvement in serious violence as an adolescent or young adult (Morgan et al 2020; Smith and Wynne-McHardy 2019).

### 1.1.2 Interventions and programmes to tackle serious violence

Increases in violent crime, particularly the murder with knives of young men, resulted in unprecedented levels of funding being allocated to violence prevention initiatives. The Early Intervention Youth Fund, Youth Endowment Fund and Young Londoners Fund have each provided or continue to provide substantial support for voluntary and community sector activities designed to reduce violent crime perpetration and victimisation; these organisations work primarily with young populations. In addition, Violence Reduction Units, based in the eighteen police force areas that have the highest rates of serious violence, are also required to allocate around half of their annual budgets to intervention activity.

With this funding comes a requirement for services and interventions to be evidence-based. This can be reflected in intervention providers selecting intervention activities that have a supporting evidence base, but also in a commitment to evaluating and disseminating the success (or otherwise) of their activities. The most common outcomes relate to serious violence - i.e. arrest and caution/conviction for a violent offence or possession of a weapon. Typically, these outcomes are focused on individuals, but in the case of place-based interventions that target

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<sup>2</sup> Prognostic score matching is analogous in approach to propensity score matching but uses a prognostic score (i.e. obtained from a model predicting of the outcome) rather than a propensity score (obtained from a model predicting intervention status) (Hansen 2008).

areas, the outcomes may relate to number of cases of serious violence in that area over a period of time.

### **1.1.3 Evaluating serious violence initiatives**

While intervention funders and providers commit to an evidence-based approach, evaluation efforts can be limited in practice. Outcomes such as arrest or caution/conviction are routinely collected by police but are not publicly available at the individual level and, in the case of conviction, can take longer to appear in official records than is reasonable for short-term intervention activities.

In addition, identifying a comparison sample for the intervention group requires significant resources and skills that are often beyond those available to most providers. This issue is particularly acute when working with individuals at high risk of violence who are generally rare within the intervention area. A consequence of these limitations is misleading results owing to underpowered analyses, poorly matched controls and regression to the mean (Krisch et al 2015).

### **1.1.4 The MoJ-DfE linked dataset**

In 2020 the Ministry of Justice and the Department for Education completed a data share, linking education and justice data for individuals born from September 1985. Individuals born on or after 31 August 1985 with at least one Police National Computer (PNC) record from 2000 or later were linked. The dataset included offending histories up to the 31 of December 2017 and education data up to the end of the 2017/2018 academic year.

A potential use of the linked MoJ-DfE dataset is to evaluate past and/or future interventions. A central problem with the evaluation of youth interventions is the difficulty in obtaining an appropriate control sample for the treated group. This problem is particularly acute when the outcome is offending because a criminal record is rare in the general population of young people.

Furthermore, when a study is able to identify a control sample, key variables that might affect the suitability of an individual as a control, such as educational attainment, are not readily available. This can result in poorly matched samples, which adversely affects the validity of the study results. Potentially, the large scale of the MoJ-DfE linked dataset and the availability of important, reliably measured variables therein offers a solution to this pervasive problem.

## **1.2 Aims and structure**

The aim of this work was to investigate whether the linked MoJ-DfE dataset could be used to create matched control groups to evaluate interventions aimed at reducing the risk of serious violence among young people. The report is structured as follows. We first describe a simulated intervention that might realistically be used to prevent serious violence. This includes a description of what the intervention would entail as well as how it might be funded and delivered (in terms of selection of an intervention group). We simulated two interventions – one

in London and one outside London. After describing the intervention, we briefly describe the data and the statistical methods used. The results section firstly describes the regression analyses we carried out to model the predictors of serious violence (this model is used in the matching). We then go on to present results regarding the matching process. In Section 4 we summarise our main findings and in Section 5 we briefly discuss the implications of the findings and the main limitations of our work.

## 2 Methods

### 2.1 *Simulated intervention*

To investigate whether the MoJ-DfE linked dataset could be used for evaluation purposes, we simulated a plausible violence prevention intervention. Creating an intervention that has not actually happened in practice allows us to mimic the selection of individuals for intervention and to determine whether suitable matched control groups could be identified.

Beginning with a plausible intervention type, we identified a likely programme funding and allocation mechanism, an appropriate intervention population within the constraints of the available data, and a plausible delivery method and setting. The intervention type informed the anticipated intervention effects with this population which, in turn, informed the required sample sizes, funding allocation and intervention delivery method.

Multisystemic therapy (MST) was selected as the intervention type. MST is a form of behaviour therapy designed for use with adolescents who are exhibiting internalising (e.g. anxiety and depression) and externalising (e.g. aggression and antisocial behaviour) problems (NICE 2011). It is commonly prescribed for young people who have begun to commit crime or who are deemed to be imminently at risk of committing crime.

The intervention is 'multisystemic' in that it recognises that causes and sustaining factors in adolescent offending are present at individual, family, peer, school and neighbourhood levels (van der Stouwe et al 2014) and, accordingly, that interventions should accommodate the diversity in simultaneously contributing factors.

Meta-analyses of MST indicate that this type of intervention is associated with approximately 25% reduction in arrest for a violent offence at one year follow-up compared to a variety of comparison groups (Henggeler et al 1997). However, many of the studies included in this had significant methodological limitations and more robust evaluations with well-matched or randomised control groups indicate lower or no positive effects of MST on later violence (Asscher et al 2014). The uncertainty around the effectiveness of MST for preventing violence, coupled with enthusiasm for this intervention type and standardised delivery method, makes it a suitable intervention type for our simulation.

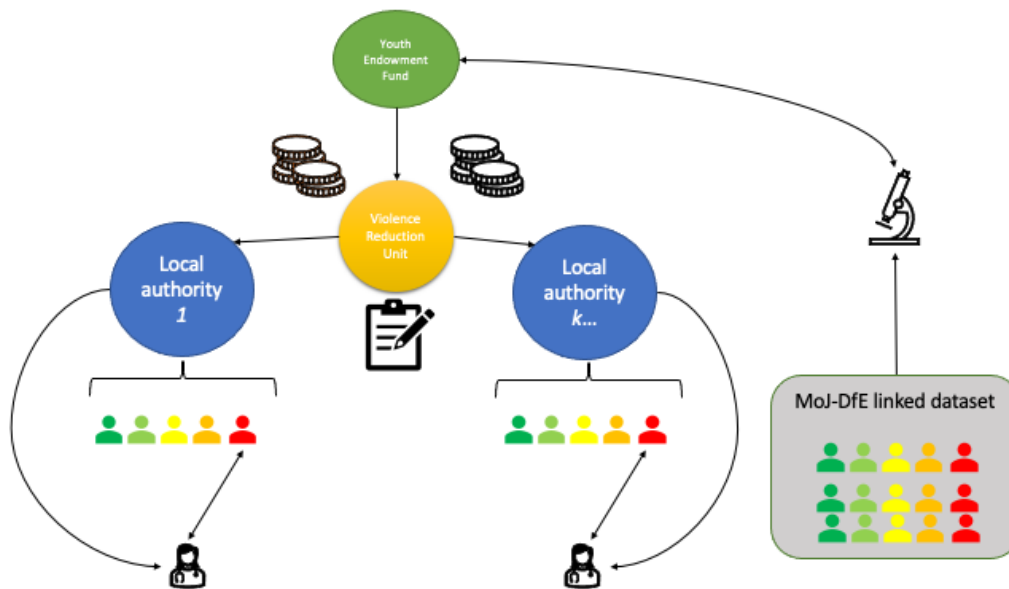
### 2.1.1 Simulated intervention context

Guided by the sample size required to detect a 25% reduction in the risk of serious violence between 15 and 18 years, which was dependent on the area in which it was conducted (700 in the intervention and control group (i.e. 1,400 altogether) in London and 1,300 in each group (2,600 altogether) outside London, where the risk of serious violence is significantly lower) we designed an MST intervention that would plausibly happen within the current funding context.

To achieve the (large) sample size required would require a programme of intervention coordinated by a large funder. Similarly, the intervention would require a large number of qualified clinicians, who would operate as private practitioners or be employed by a sizeable healthcare provider. We envisaged that a Violence Reduction Unit would be an appropriately-sized and resourced funder to be able to coordinate the allocation of funding to intervention providers and to maintain oversight of this programme.

The simulated treatment allocation mechanism was that specific local authorities within the VRU area were allocated funding to intervene with nominated at-risk young people in their communities who were in year 9 in secondary school (age ~14 years). The simulated allocation of funding and selection of individuals is illustrated in Figure 2.1.

Figure 2.1: Simulated intervention: funding and selection of intervention group



Inclusion in the intervention group would be based on either having a history of previous caution/conviction (before age 14), having been permanently excluded from school at between years 3 and 8 (7-13 years), or meeting two or more of the following criteria: one or more fixed term exclusion in years 5 to 6 or two or more fixed term exclusions in years 7 to 8, persistent

absence (>15%) in either year 7 or 8, or being in contact with social services (either being “in need” – assessed by social services as needing support – or in care (“looked after”)) before the end of year 8. These factors were chosen because they have been shown previously to be associated with offending or specifically serious violent offending and, as such, would identify a higher-risk group (Forty et al. 2017; Hawkins et al. 2000; McAra and McVie 2010; Smith and Wynne-McHardy 2019).

We investigated the feasibility of selecting matched control groups for two such interventions: one carried out in London and one outside London (other than location, the interventions were presumed to be the same).

## **2.2 Data**

The dataset used in this project was created from the MoJ-DfE linked dataset using information in the National Pupil Database (NPD) and the PNC. It included 15,029,291 individuals born from 1 September 1985 to 31 August 2007. The PNC data covered offending data up to the end of 2017. From the NPD, we used data from the following datasets: pupil-level census, Key Stage 1 and 2 attainment data, absence, exclusions, children looked after (CLA) (both CLA data included in the NPD and full episode CLA data collected as part of the SSSDA903 return), children in need (CiN), and school-level census and attainment data.

## **2.3 Subjects**

The outcome in our analysis was any serious violent offending from ages 15 to 18 (inclusive)<sup>3</sup>. As such, we needed to exclude anyone born on or after 1 January 1999. Further, some key education datasets were not available across the whole timeframe of the NPD (for example, absence data were available from academic year 2005/2006). To ensure that we had at least some data on Child in Need (CiN) status and to maximise availability of data on key variables such as absence and exclusions, we therefore restricted our analysis to individuals born in academic years 1995/96 to 1997/98 (in other words, children born between 1 September 1995 and 31 August 1998).

## **2.4 Variables**

### **2.4.1 Outcome**

As stated above, the outcome in our analysis was any serious violent offending from ages 15 to 18 (inclusive – i.e. up to their 19 birthday). Those with a caution or conviction (PNC record) for

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<sup>3</sup> This upper age range was chosen largely for pragmatic reasons: had we extended this to 19 or 20, we would have had to exclude additional cohorts, since the PNC data only went up to December 2017 (so those born in June 1998, for example, only had outcome data up to the age of 19.5 years). If we had chosen a shorter period of follow-up the risk would have been lower and therefore the required sample sizes would have been larger.

an offence classified as serious violence (see Appendix B for list of included offences) were counted as having a serious violence offence.

#### **2.4.2 Covariates: predictors of offending from the NPD data**

The covariates listed in Table 1 were measured on individuals born in academic years 1995/96 to 1997/98. For the individual level variables, we considered relevant measures for each school year; for school level variables, we only assessed measures from the school the individual was attending at the time of the spring census while they were in Year 8. We chose year 8 because this corresponded to the age at which they would be selected into the intervention (or control group); matching should not be done on factors measured after selection into an intervention, so this was the latest school year that we could have chosen.

To maximise data availability and minimise the inclusion of many highly correlated variables, combined variables across two school years (more in the case of permanent exclusions) were generated for Income Deprivation Affecting Children Index (IDACI) scores, Free School Meals (FSM) status, Special Educational Needs (SEN) status, and school exclusions. For SEN status and FSM status, the “higher” value across the two years was used (i.e. eligible for FSM; statement of SEN were considered the highest value); if FSM/SEN status was missing in either year, then the non-missing value was taken. For IDACI score, the values observed in years 4, 6 and 8 were used if measured; if these were missing, we took the value from years 3, 5 and 7 (respectively). This therefore gave three variables for each: IDACI score, FSM status and SEN status in years 3/4, 5/6 and 7/8.

For permanent exclusions, we combined all years, creating a single variable that indicated whether an individual had been permanently excluded at least once in years 3 to 8 (inclusive). Similarly, the number of fixed term exclusions from years 5 & 6 and 7 & 8 were added to give the total number across the two years. For years 5 and 6 this was then converted to a binary variable – fixed exclusion in years 5 or 6 (yes/no); for years 7 and 8 we created a categorical variable, with three or more exclusions the highest category. For percent absence, we kept the three years (6, 7 and 8) as separate (Table 2.1).

Table 2.1: Covariates measured on those born in academic years 1995/96 to 1997/98

Measure	Time points (school years), if multiple	Derived variables (if combined)
Individual level		
Birth year	-	-
Sex	-	-
Ethnicity	-	-
Income Deprivation Affecting Children Index (IDACI) score	Years 3-8	IDACI score years 3/4, years 5/6, years 7/8 <sup>1</sup>
Eligible for Free School Meals (FSM)	3-8	FSM years 3/4, years 5/6, years 7/8 <sup>2</sup>
Special Education Needs (SEN) status (No SEN, school support, statement of SEN/EHC plan)	3-8	SEN status years 3/4, years 5/6, years 7/8 <sup>2</sup>
Key Stage1 reading attainment (score in SATs)	2	-
Key Stage2 mathematics attainment (score in SATs)	6	These were summed to give an overall Key Stage 2 attainment score, converted to %
Key Stage2 reading attainment (score in SATs)	6	
Total percent absence (authorised+unauthorised)	6-8	-
Permanently excluded	3-8	Permanently excluded in years 3 to 8
Number of fixed exclusions	5-8	Any fixed exclusion in years 5 & 6, total number years 7 & 8
Child Looked After (CLA) status	Up to Year 8	-
Child in Need (CiN) status	Up to Year 8	-
Any offence classified as triable either way (TEW) or indictment only (IO) before 14 years	-	-
School level		
% FSM	8	-
% White British	8	-
% Total absence	8	-
% obtaining 5 or more A*- C grades at GCSE/equivalent	8	-
Other variables		
Region		-

1. For this variable, the value recorded in year 4, 6, or 8 was taken (if recorded); if missing, the value recorded in year 3, 5, 7 (respectively) was taken.
2. For these variables, the "higher" value across the two years was used (i.e. eligible for FSM; statement of SEN were considered the highest value); if FSM/SEN status was missing in either year, then the non-missing value was taken.

## **2.5 Statistical methods**

### **2.5.1 Modelling predictors of serious violence**

Some of the covariates – particularly ethnicity – were potentially missing not at random (specifically, more likely to be missing if individuals were from an ethnic minority group). Further, there was little evidence that completeness in the covariates was associated with serious violence after conditioning on the observed values of the covariates (odds ratio for being a complete case comparing serious violence to no serious violence aged 15-18 = 0.99; 95% CI: 0.91-1.08;  $p=0.8$ ).

Therefore, because a complete case analysis would produce unbiased estimates of log odds ratios in this scenario, whereas imputation of the missing values for these covariates would be biased (Bartlett et al. 2015; Hughes et al. 2019), all analyses were restricted to individuals with complete covariate data.

Predictors of serious violent offending from age 15-18 were modelled using logistic regression and had three stages. The first two stages were carried out on a random 20% subset of the individuals.

#### **Stage 1: Exploring non linearities<sup>4</sup> in key continuous variables**

Fractional polynomials (Royston and Altman 1994) were used to investigate whether there was evidence for non-linear associations between the outcome and: IDACI score, Key Stage 2 attainment and percent absence. Where there was evidence for non-linearities, the polynomial terms were included in the variable selection stage (Stage 2).

#### **Stage 2: Variable selection**

LASSO (Least Absolute Selection and Shrinkage Operator) models were used to select the best combination of variables for predicting the outcome. LASSO models are a variable selection method that use a penalised regression approach and result in sparse models (Hastie et al. 2015; Tibshirani 1996). The regression coefficients are estimated by maximising the log likelihood, while constraining the sum of the absolute value of the regression coefficients to be below a threshold. They apply a lambda weight which constrains weakly-predictive variables falling below this value to zero, while also shrinking remaining non-zero coefficients towards zero.

All the variables listed in Table 1 except offending before 14 (this variable was added to the final selected model) were included (the derived variables listed on the right where applicable), with polynomial terms for continuous variables if the relationship was identified as being non-linear in Stage 1, plus a limited number of interaction terms. These were identified as plausible a priori

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<sup>4</sup> The term non-linear is used to describe a relationship between two variables in which the outcome (or dependent variable) does not increase or decrease by the same amount for each unit change in the independent variable. When a relationship between two variables is linear, this relationship can be depicted by a straight line on a graph; in contrast, a non-linear relationship would typically be characterised by a curve.

and were interactions between (i) sex and: ethnicity, absence, exclusions, CLA status, CiN status, IDACI score, and attainment; and (ii) ethnicity and region.

### **Stage 3: Final model**

Once the optimal set of covariates was selected, the resulting model (with offending before 14 added to the model at this stage) was used to generate the prognostic score for matching (prognostic score matching is described in Section 2.5.2.4 below). As an additional analysis to illustrate the degree of clustering in the data, this model was also run as a multilevel logistic regression (with individual clustered within schools; the school was chosen as the one which the individual was attending during the spring census of year 8).

## **2.5.2 Selecting the intervention and control groups**

### **2.5.2.1 Selection criteria**

Since the intervention is designed to be targeted at high-risk individuals, only those meeting a set of criteria – outlined in Section 2.1 – were included in the pool of individuals from which the intervention and control groups were selected. These were individuals meeting (A), (B), or two or more of (C) to (F):

- Record for an offence (triable either way or indictment only<sup>5</sup>) before age 14
- One or more permanent exclusions in years 3 to 8
- One or more fixed term exclusion in years 5 to 6 or two or more fixed term exclusions in years 7 to 8.
- Persistent absence (>15%) in either year 7 or 8
- CLA status before the end of year 8
- CiN status before the end of year 8

### **2.5.2.2 Sample size calculations**

The risk of serious violence at ages 15-18 years among individuals meeting the selection criteria (listed above) in Local Education Authorities (LEAs) in the top quintile (in the data) for rates of caution/conviction for serious violence among under 16s in 2007-2008<sup>1</sup> was 24.5% in London and 14.8% outside London (Appendix C: Table C1). To detect a 25% relative reduction in the risk of serious violence from 15-18 years with power 80% and significance level 5% would require a sample size of approximately 700 in London and 1300 outside London.

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<sup>5</sup> MST tends to be used to address more serious offending by this age, so summary offences were excluded. Another possibility would have been to have included individuals with multiple summary offences. Either way, the purpose was to identify a group at higher risk of committing serious violence.

### **2.5.2.3 Intervention groups**

To achieve these sample sizes, the two intervention groups were selected as:

- A) Individuals born during academic years 1995/96 to 1997/98 who attended (in year 8) a school in approximately half the LEAs in London in the top quintile for rates of caution/conviction for serious violence among under 16-year-olds in 2007-2008<sup>6</sup>.
- B) Individuals born during academic years 1995/96 to 1997/98 who attended (in year 8) a school in two LEAs outside London (Birmingham and Leeds) in the top quintile for rates of caution/conviction for serious violence among under 16-year-olds in 2007-2008.

[Note that these individuals would have been attending year 8 during academic years 2008/09 to 2010/11 and would have been between 8 and 13 years during 2007 to 2008, the years we used to identify LEAs with high rates of serious violence among under 16s. They would have been 15-18 years – when we measured their outcomes – between September 2010 and 2017].

### **2.5.2.4 Selection of controls**

Controls were selected from those meeting the selection criteria detailed and born during academic years 1995/96 to 1997/98 using two different matching methods: prognostic score matching and exact matching.

#### **Prognostic score matching**

Prognostic score matching is analogous in approach to propensity score matching but uses a prognostic score (i.e. obtained from a model predicting of the outcome) rather than a propensity score (obtained from a model predicting intervention status) (Hansen 2008). The advantage of this approach is that the same scores can be used for a range of different interventions – given the same outcome. The final model described in Section 2.3.1 was used to generate the prognostic scores then one-to-one nearest neighbour matching, without replacement, was used to select a set of matched controls using `psmatch2` in Stata. <sup>6</sup>To identify these LEAs, we identified all individuals with an arrest for serious violence from 1st January 2007 to 31st December 2008 (among those born from 1993/94 onwards), identified the school and LEA of these individuals, and then calculated an approximate “rate” by counting all pupils in year 8 in each LEA during the year 2007/08 and using this as the denominator.

#### **Coarsened exact matching**

Coarsened exact matching (CEM) is a matching process in which the maximum imbalance after matching is determined in advance (Iacus et al 2012). Each variable (if applicable) is coarsened by recoding so that similar individuals are grouped together. For example, a continuous variable could be categorised into deciles – or grouped in some other way. Exact matching is then applied

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to the resulting coarsened data. This exact matching is done by generating strata of all the different possible combinations of the (coarsened) covariates; individuals in strata where there is at least one control and one intervention subject then form a matched set.

The advantage of CEM is that balance is automatically achieved, including non-linearities, interactions, and any other distributional differences in the covariates (except, of course, differences within the groups generated by the coarsening). To reduce the number of strata (thus decreasing the possibility of having unmatched individuals) we only matched on a subset of covariates, starting with the strongest predictors of serious violence (sex, ethnicity and offending before 14 years) then, if needed, adding additional covariates where the groups were substantially different after matching (again, starting with the strongest predictors of serious violence).

The matching was implemented using `cemin` Stata (Blackwell et al 2009). Because rates of serious violence varied by academic year and because data availability for Looked After Children (CLA) and Children in Need (CiN) meant that the proportions with a CLA or CiN record were higher in the younger cohorts, it was necessary to match exactly on (academic) birth year (for both matching methods). Without doing this, it was not possible to generate balanced groups via the matching. For both interventions, the controls were selected using progressively restrictive criteria until balanced groups were obtained. These are detailed below.

A) Intervention in London

- **Control set 1:** Eligible children nationally
- **Control set 2:** Eligible children from London only
- **Control set 3:** Eligible children from London, restricting to LEAs in top quintile of rates of serious violence in 2007-2008
- Intervention outside London (Birmingham and Leeds)
  - **Control set 1:** Eligible children nationally
  - **Control set 2:** Eligible children from outside London
  - **Control set 3:** Eligible children from outside London, restricting to LEAs in top quintile of rates of serious violence in 2007-2008

### 2.5.3 Comparison of control and intervention groups

Covariate balance was assessed both before and after matching using standardised differences (Austin 2009). For categorical measures, the standardised difference is given by:

$$\frac{(p_{intervention} - p_{control})}{\sqrt{\frac{p_{intervention}(1 - p_{intervention}) + p_{control}(1 - p_{control})}{2}}}$$

where  $p_{intervention}$  and  $p_{control}$  are the proportions of the variable in the intervention and control group, respectively. For numerical variables, the standardised difference is defined as:

$$\frac{(\bar{x}_{intervention} - \bar{x}_{control})}{\sqrt{\frac{s_{intervention}^2 + s_{control}^2}{2}}}$$

Where  $\bar{x}_{intervention}$ ,  $s_{intervention}^2$  and  $\bar{x}_{control}$ ,  $s_{control}^2$  are the mean and variance in the intervention and control group, respectively. There is not an established cut-off for the standardised difference that indicates residual imbalance. Some have suggested that a value of 0.1 or greater suggests important imbalance, although it is argued that better balance is more critical for covariates that are strong predictors of the outcome; as such, a smaller value for the standardised difference may be needed for stronger predictors of the outcome than for less important predictors (Austin 2009). Since balance is guaranteed with exact matching, we only calculated standardised differences after carrying out the coarsened exact matching for variables that were either coarsened or not matched on at all.

We also compared the risk of serious violence in intervention and control groups, again both before and after matching. If the matching were successful, we would expect the outcome risk to be the same in the intervention and control group. Indeed, this comparison was the key metric needed to judge the success or otherwise of the matching (if the risk were the same in the two groups, then remaining differences across covariates would not be important). Thus, for the outcome, we would be looking for a standardised difference very close to zero.

## 3 Results

### 3.1 Sample

There were 2,052,995 children born between 1 September 1995 and 31 August 1998. Of these, 1,845,149 (90%) were included in one or more school census during years 3 to 8, inclusive, (individuals not in any pupil level census from years 3 to 8 would be those who did not attend a state-funded school in England during this period so –for those - we would not have information on any of covariates listed in Table 2.1 except birth year, sex and ethnicity). Of these, 1,438,307 (78%) had complete covariate information.

Appendix C, Tables C2 -C4 give information on missing data and comparisons of characteristics among the whole sample, those included in at least one census and complete cases. Compared to all those with some census data between years 3 and 8, complete cases were less likely to be male, less likely to be Black or Mixed/other and more likely to be White or Asian, less likely to be eligible for FSM, less likely to have a SEN statement / EHC plan, and less likely to have been CLA or CiN before the end of year 8, although most of these differences were relatively small; the distribution of other covariates was similar (Appendix C, Table C4).

### 3.2 Modelling the risk of serious violence

The relationship between percent absence, Key Stage 2 attainment, and IDACI score was found to be non-linear. The best fitting fractional polynomial for percent absence contained the terms -1 and -0.5 (1/percent absence and 1/sqrt(absence)); for Key Stage 2 attainment it contained the terms 3 and 3 (KS2 attainment cubed and KS2 attainment cubed\*ln[ks2 attainment]); and for IDACI score it contained the term -0.5 (1/sqrt[IDACI]). These relationships are shown in Appendix C, Figures C1, C2 and C3.

The lasso model selected the following variables for inclusion: sex, ethnicity, region, birth year, permanently excluded in years 3 to 8, at least one fixed term exclusion in years 5 and 6, number of fixed term exclusions in years 7 and 8, FSM in years 3/4 and years 5/6, IDACI score in years 3/4 and 7/8, SEN in years 3/4, 5/6 and 7/8, CLA status, CiN status, percent absence in year 8, Key Stage 2 attainment, % FSM (school), % White British (school); interactions between sex and: fixed exclusions in years 7 and 8, IDACI scores, CLA status, ethnicity, and absence in year 8; and an interaction between region and ethnicity. Tables 3.1 and 3.2 give the unadjusted risk of serious violence across subgroups defined by the selected variables.

Overall, the risk of serious violence was higher among males, those who had been excluded, individuals whose ethnicity was Black or Mixed/other, those who had been looked after or in need before the end of year 8, those eligible for FSM, those classified as having SEN, those with higher rates of school absence and those with lower attainment at Key Stage 2; it was also higher among those attending a school with a higher percentage of pupils eligible for FSM and

with a lower percentage of White British pupils. There was also regional variation in the risk of serious violence, most notably with a higher risk in London (2.1%, compared to between 0.6% and 1.0% in all other regions).

*Table 3.1: Risk of serious violence 15-18 years by characteristics among complete cases*

<b>Characteristic</b>	<b>Category</b>	<b>Number (%) with caution/conviction for serious violence 15-18 years</b>
Sex	Male	12,313 (1.7%)
	Female	2,108 (0.3%)
Ethnicity	White	10,131 (0.8%)
	Black	1,964 (3.6%)
	Asian	1,116 (1.0%)
	Mixed/other	1,210 (2.5%)
CiN before the end of year 8	No	13,875 (1.0%)
	Yes	546 (7.2%)
CLA before the end of year 8	No	13,425 (0.9%)
	Yes	996 (7.3%)
Region (school attended in year 8)	East Midlands	1,234 (1.0%)
	East of England	1,199 (0.7%)
	London	3,561 (2.1%)
	North East	801 (1.0%)
	North West	1,900 (0.9%)
	South East	1,779 (0.8%)
	South West	828 (0.6%)
	West Midlands	1,732 (1.0%)
	Yorkshire & the Humber	1,387 (0.9%)
Birth year	1995/1996	5,443 (1.1%)
	1996/1997	4,787 (1.0%)
	1997/1998	4,191 (0.9%)
Conviction for TEW or IO offence before 14 years	No	12,019 (0.9%)
	Yes	2,402 (12.6%)
% FSM: school attended in year 8	Quintile 1	1,282 (0.4%)
	2	1,788 (0.6%)
	3	2,554 (0.9%)
	4	3,470 (1.3%)
	Quintile 5	5,327 (2.2%)
% White British: school attended in year 8	Quintile 1	4,764 (2.0%)
	2	2,799 (1.0%)
	3	2,193 (0.8%)
	4	2,066 (0.7%)
	Quintile 5	2,599 (0.8%)

Table 3.2: Risk of serious violence 15-18 years by characteristics among complete cases

Characteristic	Category	Number (%) with caution/conviction for serious violence 15-18 years
FSM years 3 / 4	No	6,924 (0.6%)
	Yes	7,497 (2.7%)
FSM years 5 / 6	No	7,381 (0.6%)
	Yes	7,040 (2.7%)
IDACI years 3 / 4	Quintile 1	1,054 (0.3%)
	2	1,383 (0.5%)
	3	2,575 (0.9%)
	4	3,806 (1.4%)
	Quintile 5	5,603 (2.2%)
IDACI years 7 / 8	Quintile 1	1,239 (0.4%)
	2	1,236 (0.5%)
	3	2,568 (0.8%)
	4	3,456 (1.3%)
	Quintile 5	5,922 (2.2%)
SEN years 3 / 4	None	7,170 (0.7%)
	School support	6,998 (2.2%)
	Statement/EHC plan	253 (1.7%)
SEN years 5 / 6	None	6,525 (0.6%)
	School support	7,454 (2.2%)
	Statement/EHC plan	442 (2.4%)
SEN years 7 / 8	None	6,181 (0.6%)
	School support	7,618 (2.3%)
	Statement/EHC plan	622 (3.0%)
Fixed exclusions years 5+6	None	12,608 (0.9%)
	One or more	1,813 (9.1%)
Fixed exclusions years 7+8	None	7,776 (0.6%)
	1	2,341 (4.7%)
	2	1,280 (8.5%)
	3 or more	3,024 (13.6%)
Permanently excluded years 3-8	No	14,046 (1.0%)
	Yes	376 (15.1%)
KS2 attainment score %	Quintile 1	6,101 (2.0%)
	2	3,867 (1.3%)
	3	2,574 (0.8%)
	4	1,345 (0.5%)
	Quintile 5	534 (0.2%)
Percent absence year 8	Quintile 1	1,180 (0.5%)
	2	1,519 (0.6%)
	3	2,043 (0.7%)
	4	2,982 (1.0%)
	Quintile 5	6,697 (2.1%)

The final model including all individuals is shown in Appendix C; also shown also shown is model excluding offending before 14 years and the multilevel model with individuals clustered within schools.

Tables 3.3 and 3.4 give adjusted odds ratios from models run separately among males and females; these come from models including all variables shown in Tables 3.3 and 3.4 as well as birth year. In these separate models, the interaction between ethnicity and region was no longer important, so these models exclude this. There were several apparent differences.

*Table 3.3: Fully adjusted odds ratios for serious violence separately for males and females*

Characteristic	Category	OR (95% confidence interval)	
		Males	Females
Ethnicity (reference=White)	Black	(2.02, 2.34)	0.98 (0.80, 1.21)
	Asian	1.03 (0.95, 1.97)	0.29 (0.21, 0.40)
	Mixed/other	1.97 (1.83, 2.13)	1.32 (1.10, 1.59)
CiN before the end of year 8	Yes	1.52 (1.32, 1.74)	1.89 (1.52, 2.36)
CLA before the end of year 8	Yes	2.04 (1.84, 2.26)	2.61 (2.19, 3.12)
Region (school attended in year 8) (reference=South East)	East Midlands	1.18 (1.08, 1.28)	1.01 (0.84, 1.22)
	East of England	0.98 (0.90, 1.06)	0.78 (0.64, 0.94)
	London	1.26 (1.16, 1.36)	1.08 (0.89, 1.32)
	North East	1.09 (0.99, 1.21)	0.99 (0.80, 1.22)
	North West	0.99 (0.92, 1.07)	0.77 (0.65, 0.91)
	South West	0.83 (0.76, 0.92)	0.75 (0.61, 0.91)
	West Midlands	0.98 (0.91, 1.06)	0.90 (0.76, 1.07)
	Yorkshire & the Humber	0.93 (0.85, 1.01)	0.96 (0.80, 1.14)
Birth year (reference=1995/96)	1996/1997	0.87 (0.83, 0.91)	0.72 (0.65, 0.80)
	1997/1998	0.84 (0.80, 0.88)	0.66 (0.59, 0.74)
Conviction for TEW or IO offence before 14 years	Yes	3.13 (2.95, 3.32)	2.71 (2.34, 3.15)
% FSM: school attended in year 8	Per 10% increase	1.06 (1.04, 1.08)	1.04 (0.99, 1.08)
% White British: school attended in year 8	Per 10% increase	0.96 (0.95, 0.97)	0.99 (0.96, 1.02)

Among males, the risk of serious violence was approximately twice as high among individuals whose ethnicity was Black or Mixed/other compared to White or Asian individuals. However, among females the risk was much lower among Asian individuals (odds ratio compared to White, OR=0.29; 95% CI: 0.21-0.40), was similar among Black and White individuals and was higher among those who were Mixed/other, but much less so than for males (OR compared to White=1.32; 95% CI: 1.10-1.59).

The association with exclusions, CLA, CiN, absence and attainment was stronger among females than males but the association with previous caution/conviction was weaker. Finally, the

association with earlier deprivation (IDACI score and FSM measured in years 3-4) was slightly stronger for females; conversely, the association with later deprivation (IDACI score in years 7-8 and FSM in years 5-6) was stronger among males.

*Table 3.4: Fully adjusted odds ratios for serious violence separately for males and females*

Characteristic	Category	OR (95% confidence interval)	
		Males	Females
FSM years 3 / 4	Yes	1.30 (1.23, 1.38)	1.72 (1.50, 1.97)
FSM years 5 / 6	Yes	1.29 (1.22, 1.37)	1.18 (1.03, 1.35)
IDACI years 3 / 4	2	1.12 (1.02, 1.23)	1.28 (1.02, 1.60)
	3	1.28 (1.16, 1.41)	1.43 (1.15, 1.80)
	4	1.32 (1.19, 1.45)	1.52 (1.21, 1.91)
	Quintile 5	1.36 (1.23, 1.51)	1.58 (1.25, 2.01)
IDACI years 7 / 8	2	1.11 (1.01, 1.22)	1.00 (0.81, 1.24)
	3	1.22 (1.11, 1.33)	1.16 (0.95, 1.42)
	4	1.28 (1.17, 1.41)	1.18 (0.95, 1.45)
	Quintile 5	1.37 (1.24, 1.51)	1.14 (0.91, 1.42)
SEN years 3 / 4	School support	1.05 (1.00, 1.11)	0.89 (0.78, 1.01)
	Statement/EHC plan	0.77 (0.63, 0.96)	0.65 (0.30, 1.44)
SEN years 5 / 6	School support	1.04 (0.98, 1.11)	1.22 (1.06, 1.40)
	Statement/EHC plan	1.15 (0.91, 1.44)	1.44 (0.59, 3.48)
SEN years 7 / 8	School support	1.23 (1.17, 1.30)	1.27 (1.13, 1.42)
	Statement/EHC plan	0.94 (0.80, 1.11)	1.07 (0.56, 2.05)
Fixed exclusions years 5+6	One or more	1.41 (1.32, 1.50)	1.32 (1.04, 1.68)
Fixed exclusions years 7+8	1	3.24 (3.06, 3.42)	4.57 (4.00, 5.24)
	2	4.46 (4.15, 4.79)	6.01 (4.96, 7.27)
	3 or more	5.55 (5.22, 5.90)	6.92 (5.91, 8.09)
Permanently excluded years 3-8	Yes	1.11 (0.97, 1.27)	1.37 (0.95, 1.98)
KS2 attainment score %	2	0.98 (0.93, 1.02)	0.87 (0.78, 0.98)
	3	0.83 (0.78, 0.88)	0.77 (0.67, 0.88)
	4	0.61 (0.57, 0.66)	0.50 (0.42, 0.60)
	Quintile 5	0.38 (0.34, 0.42)	0.22 (0.17, 0.31)
Percent absence year 8	2	1.14 (1.04, 1.23)	1.14 (0.92, 1.42)
	3	1.24 (1.14, 1.34)	1.33 (1.09, 1.63)
	4	1.47 (1.37, 1.59)	1.62 (1.34, 1.96)
	Quintile 5	1.68 (1.56, 1.80)	2.23 (1.87, 2.67)

### **3.3 Intervention in London**

There were 743 individuals in the intervention group. Using control set one (controls selected from all those eligible nationally) resulted in large imbalances across most covariates, particularly ethnicity and sex (and obviously region, since the intervention group was entirely from London); as a consequence, there was a substantial difference in the risk of serious violence after matching: 17.5% in the control group compared to 25.4% in the intervention group using propensity score matching. Exact matching on sex, ethnicity and offending before age 14 did not improve this (the risk of serious violence in the control group after matching was 16.8%). Because this strategy for selecting controls was clearly unsuccessful in terms of generating balanced groups, these results are not shown.

Tables 3.5 and 3.6 give the covariate and outcome distribution and standardised differences before and after matching with control sets two and three. For the covariates measured at more than one timepoint (IDACI score, FSM, SEN status) we just present results for one of these. The coarsened exact matching was carried out matching only on ethnicity, sex and offending before 14 (so in fact was exact matching – with no coarsening needed).

For control set two the risk of serious violence after matching was 2.6% lower in the control group after prognostic score matching and 0.5% lower after exact matching. Individuals in the prognostic-score-matched control group were more likely to be White (53.8% compared to 36.6% in the intervention group) and less likely to be Black (30.2% compared to 43.6% in the intervention group).

There was also a substantial difference in the percentage of pupils in the school who were White British, both before and after matching (standardised difference after matching = 0.66). After exact matching, the difference in terms of percentage of pupils who were White British was smaller, but still quite marked (standardised difference 0.52).

For control set three, differences in the outcome would arise as a result of heterogeneity between the intervention and control LEAs – i.e. as a result of confounding due to differences between these LEAs (since the intervention group was selected from approximately half the LEAs in the top quintile for rates of serious violence in 2007-2008 and the control group was selected from the other LEAs in this same category). Further, the sample size before matching was only 961, so 77% of these were selected into the matched control group.

As such, the risk of outcome and distribution of covariates were similar before matching to after matching (the risk of serious violence in the control group was 24.8% before matching and 26.4% after prognostic score matching). This was similar to the risk in the intervention group (25.4%) and most covariates were well-balanced.

The main exceptions to this were eligibility for FSM (59.1% in the intervention group in years 5/6 compared to 68.5% in the control group after prognostic score matching, standardised

difference -0.20), IDACI score in years 7/8 (mean 0.44 in the intervention group and 0.50 in the control group, standardised difference -0.33), % pupils eligible for FSM (mean 29.7% in the intervention group and 39.2% in the control group, standardised difference -0.68), and % White British (standardised difference -0.19).

Exact matching on sex, ethnicity, and offending before 14 resulted in 30 individuals in the intervention group remaining unmatched. In the remaining 713 individuals in each group, the risk of violence was similar: 25.0% in the control group and 25.5% in the intervention group. Using exact matching led to very similar results to those obtained using prognostic score matching in terms of covariate differences; it is likely that the majority of the matched controls were the same across these two sets.

Table 3.5: Outcome and covariate balance before and after matching: percentages / mean (SD) and standardised difference - London intervention with control set two

Variable	Level	Intervention n=743	Before		After - PS matching		After - exact matching	
			Control n=2,674	S. diff <sup>1</sup>	Control n=743	S. diff	Control n=743	S. diff
Serious violence	Yes	25.4%	19.5%	0.14	22.9%	0.06	24.9%	0.01
Offence before 14	Yes	52.9%	54.1%	-0.02	55.2%	-0.05		
Sex	M	29.1%	34.5%	-0.12	27.2%	0.04		
Ethnicity	White	36.6%	56.0%	-0.40	53.8%	-0.35		
	Black	43.6%	26.3%	0.37	30.3%	0.28		
	Asian	7.7%	6.1%	0.06	4.0%	0.16		
	Mixed/other	12.1%	11.6%	0.02	12.0%	0.004		
FSM 5/6	Yes	59.1%	61.1%	-0.04	64.6%	-0.11	61.9%	-0.06
SEN 5/6	School support Statement	59.4%	57.0%	0.05	61.4%	-0.04	56.9%	0.05
		6.6%	5.0%	0.07	5.0%	0.07	5.3%	0.06
Permanent exclusion	Yes	9.6%	11.9%	-0.08	11.4%	-0.06	14.0%	-0.14
Fixed excl. 5/6	1+	21.4%	18.9%	0.06	21.7%	-0.007	20.9%	0.01
Fixed excl. 7/8	1	12.5%	14.9%	-0.07	13.9%	-0.04	15.5%	-0.09
	2	16.4%	13.9%	0.07	16.7%	-0.007	14.1%	0.06
	3+	33.8%	30.4%	0.07	34.2%	-0.009	31.2%	0.06
CLA up to y8	Yes	26.7%	24.3%	0.05	24.8%	0.04	27.9%	-0.03
CiN up to y8	Yes	17.2%	17.1%	0.003	17.1%	0.004	19.5%	-0.06
Sqrt % absence y8 <sup>1</sup>	Mean (SD)	3.6 (1.9)	3.7 (1.9)	-0.05	3.8 (1.9)	-0.09	3.5 (1.9)	0.06
KS2 attainment	Mean (SD)	48.9 (18.0)	51.6 (18.7)	-0.15	50.4 (18.4)	-0.08	51.8 (18.6)	-0.16
IDACI 7/8	Mean (SD)	0.44 (0.17)	0.41 (0.19)	0.18	0.42 (0.18)	0.12	0.41 (0.18)	0.14
% FSM	Mean (SD)	29.7 (14.1)	28.3 (15.2)	0.10	30.1 (16.0)	-0.03	29.6 (15.5)	0.007
Sqrt(100-%White British) <sup>1</sup>	Mean (SD)	8.7 (1.2)	7.6 (1.8)	0.72	7.7 (1.7)	0.66	7.9 (1.6)	0.52

1. Transformation applied to obtain a more symmetrical distribution so that means and standard deviations were appropriate measures to use. For percent absence: square root transformation; for percent White British: transformation=square root (100-%White British)

Table 3.6: Outcome and covariate balance before and after matching: percentages / mean (SD) and standardised difference - London intervention with control set three

Variable	Level	Intervention n=743	Before		After - PS matching		After - exact matching	
			Control n=961	S. diff	Control n=743	S. diff	Control n=713 <sup>2</sup>	S.diff
Serious violence	Yes	25.4%	24.8%	0.02	26.4%	-0.02	25.0	0.01 <sup>2</sup>
Offence before 14	Yes	52.9%	52.8%	0.003	52.6%	0.005		
Sex	M	29.1%	33.1%	-0.09	33.1%	-0.09		
Ethnicity	White	36.6%	36.1%	0.01	36.6%	0		
	Black	43.6%	42.4%	0.03	42.8%	0.02		
	Asian	7.7%	8.1%	-0.02	7.7%	0		
	Mixed/ other	12.1%	13.4%	-0.04	12.9%	-0.03		
FSM 5/6	Yes	59.1%	67.5%	-0.18	68.5%	-0.20	66.5%	-0.14
SEN 5/6	School support	59.4%	60.8%	-0.03	62.1%	-0.06	62.6%	-0.07
	Statement	6.6%	5.8%	0.03	5.9%	0.03	5.9%	0.02
Permanent exclusion	Yes	9.6%	10.6%	-0.04	10.9%	-0.04	10.8%	-0.05
Fixed excl. 5/6	1+	21.4%	21.0%	0.009	20.3%	0.03	22.0%	-0.02
Fixed excl. 7/8	1	12.5%	15.4%	-0.08	14.8%	-0.07	15.0%	-0.06
	2	16.4%	15.4%	0.03	16.4%	0.0	15.0%	0.05
	3+	33.8%	29.3%	0.10	28.9%	0.10	29.7%	0.08
CLA up to y8	Yes	26.7%	26.0%	0.02	26.2%	0.009	26.4%	-0.006
CiN up to y8	Yes	17.2%	19.5%	-0.06	19.8%	-0.07	19.9%	-0.06
Sqrt % absence y8 <sup>1</sup>	Mean (SD)	3.6 (1.9)	3.6 (1.9)	-0.02	3.7 (1.9)	-0.06	3.7 (2.0)	-0.04
KS2 attainment	Mean (SD)	48.9 (18.0)	50.4 (19.7)	-0.08	50.0 (19.7)	-0.06	50.2 (19.7)	-0.07
IDACI 7/8	Mean (SD)	0.44 (0.17)	0.49 (0.17)	-0.33	0.50 (0.17)	-0.33	0.50 (0.17)	-0.33
% FSM	Mean (SD)	29.7 (14.1)	38.7 (13.8)	-0.65	39.2 (13.8)	-0.68	38.6 (13.8)	-0.64
Sqrt(100-%White British) <sup>1</sup>	Mean (SD)	8.7 (1.2)	8.9 (0.7)	-0.19	8.9 (0.7)	-0.19	8.8 (0.7)	-0.19

1. Transformation applied to obtain a more symmetrical distribution so that means and standard deviations were appropriate measures to use. For percent absence: square root transformation applied; for percent White British: transformation = square root(100-%White British).
2. Exact matching led to 30 individuals in the intervention group remaining unmatched; the distribution of covariates in the intervention group after matching was therefore slightly different to the distribution before matching (these results are not shown); risk of serious violence in intervention group = 25.5% after matching.

### **3.4 Intervention outside London**

There were 1,338 individuals in the intervention group selected outside London. As with the London intervention, control set one (controls selected from all those eligible nationally) resulted in large imbalances across all covariates, particularly ethnicity and sex; again, these results are not shown. Tables 3.7 and 3.8 give the covariate and outcome distribution and standardised differences before and after matching for control sets two and three.

For control set two the risk of serious violence after matching was 2.3% lower in the control group after prognostic score matching. Individuals in the control group were more likely to be White (92.2% compared to 71.7% in the intervention group) and less likely to be Black, Asian or Mixed/other ethnicity (1.4%, 1.9% and 4.4%, respectively, compared to 8.7%, 9.3%, and 10.2%, respectively, in the intervention group). There were also substantial differences in the percentage of pupils in the school who were White British (standardised difference after matching = 0.98) and the percentage eligible for FSM (standardised difference = 0.74 after matching).

After coarsened exact matching on sex, ethnicity, offending before 14 years, fixed exclusions in years 5&6 and 7&8, FSM in years 5-6, CiN status, IDACI score in years 7-8 (grouped into quartiles), and %FSM (grouped into quartiles), the risk of serious violence was 13.4% in the control group compared to 13.1% in the intervention group. This was based on a sample size of 1115 in each group, as 223 individuals in the intervention group remained unmatched; the risk of serious violence in this unmatched group was much higher than in the group as a whole (22.4%) and the ethnic distribution was also quite different: 10% White, 35% Black, 29% Asian, 26% Mixed/other. There was still quite a large difference in the percentage of pupils who were White British (standardised difference after matching = 0.53).

For control set 3, as for London, differences in the outcome would arise due to of differences between the intervention and control LEAs (since, similarly to London, the intervention group was selected as two LEAs from among those outside London in the top quintile for rates of serious violence in 2007-2008 and the control group was selected from the other LEAs in this same category). Thus, as with London, the risk of serious violence and distribution of covariates were very similar before matching to after matching (the risk of serious violence in the control group was 13.6% before matching and 13.7% after prognostic score matching).

Exact matching on sex, ethnicity, and offending before 14 resulted in 100 individuals in the intervention group remaining unmatched; again, their risk of serious violence was higher than in the group as a whole (22.0%) and, as above, the ethnic distribution was also quite different: 0% White, 41% Black, 51% Asian, 8% Mixed/other. In the remaining 1,238 individuals in each group, the risk of violence was similar: 13.3% in the control group and 13.7% in the intervention group.

Table 3.7: Outcome and covariate balance before and after matching: percentages / mean (SD) and standardised difference – intervention outside London with control set two

Variable	Level	Intervention n=1,338	Before		After – PS matching		After – coarsened exact matching		
			Control n=27,209	S. diff	Control n=1,338	S. diff	Intervention n=1,115	Control n=1,115	S.diff
Serious violence	Yes	14.5%	9.4%	0.16	12.2%	0.07	13.1%	13.4%	-0.009
Offence before 14	Yes	57.3%	60.6%	-0.07	64.7%	-0.15			
Sex	M	31.1%	35.6%	-0.10	27.4%	0.08			
Ethnicity	White	71.7%	92.7%	-0.57	92.2%	-0.56			
	Black	8.7%	1.2%	0.35	1.4%	0.34			
	Asian	9.3%	2.2%	0.31	1.9%	0.33			
	Mixed/ other	10.2%	3.9%	0.25	4.4%	0.23			
FSM 5/6	Yes	61.1%	49.5%	0.23	54.5%	0.13			
SEN 5/6	School support	48.2%	52.4%	-0.08	57.3%	-0.18	47.3%	53.3%	-0.12
	Statement	3.7%	4.9%	-0.06	5.7%	-0.10	4.6%	6.0%	-0.11
Permanent exclusion	Yes	8.4%	7.3%	0.04	9.3%	-0.03	7.2%	7.7%	-0.02
Fixed excl. 5/6	1+	22.7%	17.5%	0.13	23.4%	-0.02			
Fixed excl. 7/8	1	14.4%	12.0%	0.07	12.5%	0.06			
	2	12.0%	11.0%	0.03	12.6%	-0.02			
	3+	37.4%	29.7%	0.16	41.5%	-0.08			
CLA up to y8	Yes	20.0%	23.1%	-0.08	22.5%	-0.06	19.9%	19.4%	0.01
CiN up to y8	Yes	12.3%	18.3%	-0.17	17.0%	-0.13			
Sqrt % absence y8 <sup>1</sup>	Mean (SD)	4.0 (2.0)	3.6 (1.9)	0.17	3.9 (1.9)	0.03	4.0 (2.0)	3.8 (1.9)	0.10
KS2 attainment	Mean (SD)	48.8 (18.4)	50.4 (18.6)	-0.09	49.6 (18.0)	-0.04	49.0 (18.4)	48.3 (18.1)	0.04
IDACI 7/8	Mean (SD)	0.38 (0.19)	0.29 (0.18)	0.45	0.32 (0.19)	0.31	0.36 (0.18)	0.36 (0.18)	0.003
% FSM	Mean (SD)	29.4 (15.5)	17.8 (12.4)	0.83	18.9 (12.6)	0.74	27.1 (14.6)	25.1 (14.9)	0.13
Sqrt(100-%W. British) <sup>1</sup>	Mean (SD)	5.5 (2.4)	3.3 (1.7)	1.03	3.4 (1.8)	0.98	5.0 (2.2)	3.9 (2.1)	0.53

1. Transformation applied to obtain a more symmetrical distribution so that means and standard deviations were appropriate measures to use. For percent absence: square root transformation applied; for percent White British: transformation = square root(100-%White British).
2. Coarsened exact matching led to 223 individuals in the intervention group remaining unmatched; the distribution of covariates in the intervention group after matching was therefore slightly different to the distribution before matching.

Table 3.8: Outcome and covariate balance before and after matching: percentages / mean (SD) and standardised difference – intervention outside London with control set three

Variable	Level	Inter- vention n=1,338	Before		After – PS matching		After – exact matching		
			Control n=2,233	S. diff	Control n=1,338	S. diff	Intervention n=1,238	Control n=1,238	S.diff
Serious violence	Yes	14.5%	13.6%	0.03	13.7%	0.02	13.7%	13.3%	0.01
Offence before 14	Yes	57.3%	59.4%	-0.04	61.7%	-0.09			
Sex	M	31.1%	31.6%	-0.01	32.3%	-0.03			
Ethnicity	White	71.7%	82.5%	-0.26	82.4%	-0.26			
	Black	8.7%	3.7%	0.21	3.8%	0.20			
	Asian	9.3%	3.7%	0.23	3.7%	0.23			
	Mixed/ other	10.2%	10.1%	0.005	10.1%	0.005			
FSM 5/6	Yes	61.1%	65.2%	-0.09	65.6%	-0.10	61.0%	65.1%	-0.09
SEN 5/6	School support	48.2%	56.8%	-0.17	58.5%	-0.21	48.2%	57.3%	-0.18
	Statement	3.7%	3.3%	0.02	3.1%	0.03	3.7%	3.7%	0
Permanent exclusion	Yes	8.4%	6.0%	0.09	5.5%	0.11	7.7%	6.1%	0.06
Fixed excl. 5/6	1+	22.7%	19.7%	0.07	20.3%	0.06	22.1%	19.7%	0.06
Fixed excl. 7/8	1	14.4%	12.1%	0.07	11.1%	0.10	14.3%	11.2%	0.10
	2	12.0%	13.9%	-0.06	13.1%	-0.03	11.3%	15.4%	-0.12
	3+	37.4%	36.1%	0.03	37.1%	0.006	37.5%	36.6%	0.02
CLA up to y8	Yes	20.0%	20.6%	-0.02	19.3%	0.02	20.9%	21.9%	-0.02
CiN up to y8	Yes	12.3%	13.0%	-0.02	12.5%	0.007	13.0%	13.3%	-0.01
Sqrt % absence y8 <sup>1</sup>	Mean (SD)	4.0 (2.0)	4.0 (2.0)	-0.02	4.0 (2.0)	-0.007	4.0 (2.0)	4.0 (2.0)	0.002
KS2 attainment	Mean (SD)	48.8 (18.4)	49.3 (19.2)	-0.03	49.5 (19.1)	-0.04	48.9 (18.3)	49.1 (19.4)	-0.01
IDACI 7/8	Mean (SD)	0.38 (0.19)	0.43 (0.20)	-0.28	0.44 (0.20)	0.29	0.37 (0.19)	0.43 (0.20)	-0.32
% FSM	Mean (SD)	29.4 (15.5)	28.1 (13.3)	0.09	28.0 (13.2)	0.10	28.5 (15.2)	27.8 (13.2)	0.05
Sqrt(100-% W.British) <sup>1</sup>	Mean (SD)	5.5 (2.4)	4.8 (2.0)	0.32	4.7 (2.1)	0.33	5.3 (2.2)	4.8 (2.1)	0.21

1. Transformation applied to obtain a more symmetrical distribution so that means and standard deviations were appropriate measures to use. For percent absence: square root transformation applied; for percent White British: transformation = square root(100-%White British).
2. Exact matching led to 100 individuals in the intervention group remaining unmatched; the distribution of covariates in the intervention group after matching was therefore slightly different to the distribution before matching.

## 4 Summary of findings

### 4.1 Predictors of serious violence

Several different individual, family, school and geographical factors measured in the data were strong predictors of the risk of serious violence between 15 and 18 years. These predictors were consistent with previous research that used two longitudinal survey datasets (Smith and Wynne-McHardy 2019).

Males had a much higher risk of having a record for serious violence. Serious violence was also associated with deprivation – the risk was higher among those with higher IDACI scores (this is a measure based on an individual's postcode, so relates to neighbourhood deprivation) and, independently of IDACI score, higher among those eligible for free school meals.

In terms of other individual-level factors, the risk of serious violence was higher among individuals who had been excluded from school, those with high levels of school absence<sup>7</sup>, those with lower attainment, and among young people who had special educational needs and those who had ever been in need or looked after.

Independently of individual-level factors, the risk of serious violence was also higher for young people attending a school where the percentage of pupils eligible for free school meals was higher and the percentage of pupils who were White British was lower.

There were several differences between males and females in terms of the strength of association between covariates and the risk of serious violence. The most marked difference was for ethnicity. Among males, those whose ethnicity was Black or Mixed/other had approximately double the risk of serious violence compared to White or Asian individuals. The relationship with ethnicity was quite different among females – White and Black females had a similar risk, Asian females had a much lower risk than White or Black females and the risk among those whose ethnicity was Mixed/other was approximately 1.3 times higher than among White or Black individuals.

There was also evidence for geographical differences in the risk of serious violence. The unadjusted risk was 2.1% in London and 0.6% in the South West. Further, the multilevel model suggested that there was clustering by school.

The fact that offending before age 14 was a strong predictor of serious violence independently of all these factors suggests that there are other factors, not measured in the data that predict

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<sup>7</sup> We looked at total percent absence from school. This is a combination of authorised (e.g. as a result of illness) and unauthorised absence; the latter includes truancy but also includes absence due to, for example, a family holiday during term time not authorised by the school.

serious violence. These are likely to be a combination of individual, family, and community-level factors.

## ***4.2 Using the dataset to identify matched control groups***

Our interventions were designed to be targeted at high-risk children living in high-risk areas (i.e. areas with relatively high rates of serious violence); this is what we would expect to happen in practice, although the criteria for inclusion may not always be so well-defined. We found that, because the risk of serious violence is so different in London compared to other parts of England, an intervention carried out in London would have to select controls from within London; similarly, an intervention outside London would have to select controls outside London. Failing to do this would result in a control group that was very dissimilar from the intervention groups and, crucially, a control group with a very different risk of serious violence.

In both areas, coarsened exact matching (exact matching on sex, ethnicity and offending before 14 – in addition to birth year – was sufficient in London, but additional factors were needed in the matching algorithm outside London) performed slightly better than prognostic score matching as this ensured exact balance in the most crucial covariates: sex, ethnicity and previous caution/conviction. Using prognostic score matching led to a control group with a slightly lower risk of serious violence. This would mean that the effect of any intervention would be systematically underestimated.

However, exact or coarsened exact matching resulted in some individuals in the intervention group remaining unmatched; this would inevitably lead to some loss of power. Outside London, these individuals were quite atypical – in comparison to the rest of the intervention group – in terms of the distribution of ethnicity and in terms of their risk of violence. This is in fact an advantage of carrying out matching – because the process – by default – only allows comparison of individuals who could have been in either group (by excluding unmatched individuals).

However, the effectiveness of the intervention is then only estimated on a subsample of the original group and the conclusions cannot not be generalised to the unmatched individuals. This is unlikely to be a problem if the number of unmatched individuals remains relatively small, but would clearly be problematic if a large number of those in the intervention group remain unmatched.

When the controls were also selected only from the high-risk areas, prognostic score matching and exact matching led to very similar samples. This strategy is close to being an RCT in terms of design, except the intervention group came from specific LEAs and the control group from different LEAs (but possibly similar in many respects and, crucially, similar with respect to rates of serious violence).

Finally, because the risk of serious violence varied across the cohorts (this was apparent across the whole range of years included in the dataset) and because, as mentioned in Section 2.4.2.4,

different datasets that make up the NPD – in particular the CLA and CiN data – do not cover the whole period of time, it was necessary to match exactly on birth year in order to obtain balanced groups.

## 5 Conclusions

We carried out an analysis to investigate the feasibility of using the linked MoJ-DfE dataset to evaluate interventions aimed at reducing the risk of serious violence and found that the large sample and large number of variables included in the MoJ-DfE linked data set can, with careful matching, generate well-matched control groups which could be used successfully for evaluation purposes for this outcome. We have specifically evaluated this in the context of a targeted intervention – an intervention targeted at an identifiable group of high-risk individuals.

Our sample size calculations were based on being able to detect a (relative) reduction in the risk of serious violence of 25%. An intervention with a smaller impact would require larger samples, as would a more universal intervention (because the risk of serious violence is much lower in the general population than among a selected high-risk group) or an intervention with a shorter period of follow-up (since the risk of serious violence over a short period – for example, one year – would be lower than the risk over three years, the period of follow-up we considered).

Small, underpowered interventions studies are problematic because they lead to imprecise estimates of the effect of the intervention and thus inconclusive results. Small studies are also more likely to lead to false positive results (find that an intervention is effective when it actually is not). This methodological fact presents a consistent and substantial problem in violence prevention for several reasons: (1) For ethical, practical and financial reasons, the field has tended to rely on natural experiments and quasi-experimental designs to determine the effectiveness of interventions; (2) populations of interest – young people with a significant history of violence – are rare, which limits the number of individuals eligible for intervention; (3) interventions tend to be delivered by small organisations with limited resources; (4) outcomes, such as reoffending, are relatively rare over typical 12-24 month follow-up periods; and (5) effect sizes of interventions tend to be small, as illustrated by the Youth Endowment Fund Evidence and Gap Map (White et al. 2021).

We have shown that the linked dataset could be used to successfully generate matched control groups for evaluating violence prevention interventions. However, it cannot overcome issues of statistical power when effect sizes are small. In order to advance the evidence base for violence prevention, the scale of interventions that used non-randomised study designs needs to increase and/or follow-up periods need to be lengthened. The implications of this for evidence-based violence prevention are significant as it would result in a bias towards the funding of standardised or ‘manualised’ interventions which, by being standardised, may lack the credibility or legitimacy required to engage at-risk young people.

It is worth also noting that an intervention with a smaller anticipated effect size would need to obtain better balance in order to be judged as successful. For example, if an intervention was expected to reduce serious violence by 10% (from 15% to 13.5%, say) a difference of 0.5% between the intervention and control group in the risk of serious violence in the absence of the intervention would represent one third of this effect whereas if an intervention was expected to reduce serious violence by 25% (from 15% to 11.25%, say) then this difference of 0.5% would represent a smaller proportion of this.

Our findings highlight the importance of considering differences between areas in rates of serious violence when selecting control groups. If, as is likely, these rates are higher in the area(s) selected for intervention, systematic differences between the intervention and control group will be present and these may not be addressed using matching. The extent to which this could cause a problem will depend on the disparity in these rates between the intervention and control areas. So, for example, because rates are much higher in London than in other parts of England, selecting controls nationally led to a set of controls that were incomparable with the intervention group, even after matching exactly on key factors such as sex, ethnicity and previous caution/conviction. This happens because the higher rates in London are not explained by factors measured in the data.

As the first team outside government to work with these datasets at a time of significant disruption as a result of the Covid-19 pandemic, the work required to request, access and prepare the data for analysis was substantial. The inventory of reusable resources, such as (programming) code, will increase as the dataset is used by other researchers, making data preparation more efficient and accessible. Users of this resources should be encouraged to share code and to receive acknowledgement for this work. Where possible, subject to quality assurance, a repository of data management and analytical resources should be developed and maintained.

There will always be a necessary lag between the collection of administrative data and its being made available for evaluation purposes. In practice, this results in significant gaps between the delivery of an intervention and the availability of follow-up period data. This is understandable and a necessary cost of having high-quality matched administrative data. The costs of this lag can be mitigated by having a schedule of data release updates that will allow evaluation teams to plan effectively.

Further, to allow the use of the dataset to become routine for evaluation purposes, it might be necessary to find a more efficient mechanism for doing so by developing skillsets in using large administrative datasets within government and academia, sharing resources within these communities and streamlining data access procedures.

This work has some limitations. Firstly, we chose a very defined set of inclusion criteria (i.e. definition of high-risk individuals). In reality, the inclusion criteria for the intervention group

could be less well-specified and could potentially vary from area to area. If that were the case, the potential for systematic differences between the intervention and control group in terms of the risk of serious violence would be greater. Matching would not overcome this problem, however, so those designing interventions will need to clearly state their inclusion and exclusion criteria if these methods are to be used.

We carried out our analyses on complete cases (those with no missing data on all covariates included in the logistic regression model used to generate the prognostic scores). This was felt to be the most appropriate approach because we thought it likely that some covariates, particularly ethnicity, would be missing not at random and because there was little evidence that completeness in the covariates was associated with serious violence after conditioning on the covariates.

Although the differences between complete cases and the whole sample – where present – were generally relatively small and, as such, we do not believe this will have had an overall impact on our findings, missing data does mean that the available pool of controls is smaller. Similarly, if the dataset were going to be used to evaluate an intervention in practice, it is likely that some individuals in the intervention group would have missing data on some of the covariates needed for matching. This could lead to a loss of power.

Exact matching on key characteristics such as previous caution/conviction, sex, ethnicity and possibly other strong predictors of offending in the data (e.g. exclusions) would be an advantage in this context, because – at least for the matching itself – it only requires information to be available on these, although large amounts of missing data on other characteristics would mean that checking covariate balance (on factors other than those matched on) may not be possible. Multiple imputation is a potential solution to this and can be combined with prognostic score matching (Leyrat et al 2019), although those carrying out the evaluation would need to consider carefully whether multiple imputation would be the most appropriate analysis in their specific case and would also need to carry out sensitivity analyses to assess the robustness of the findings to any assumptions made about the missing data mechanism (Lee et al. 2021).

A related limitation of using this dataset for evaluating interventions is that some individuals will be either totally absent from the data or have no data on any covariates. For example, individuals who have attended independent schools or have been home-schooled for the whole of their schooling would only appear in the data at the point of doing exams such as GCSEs, so the methods could not examine the impact of interventions on these people.

Coverage of some of the datasets (for example, the CiN data) means that some past interventions would not be able to be evaluated. However, going forward, the data will be more comprehensive in this respect and therefore this should not be an issue.

Finally, we developed the prognostic model using data on individuals born between 1995/96 and 1997/98. We found that the risk of serious violence changed during that period. It is also possible that the relative importance of different covariates could change over time. Those using the dataset for evaluation in future would therefore need to update the prognostic model (if using prognostic score matching).

In summary, when both intervention and control individuals were selected from high-risk areas, prognostic score matching and (coarsened) exact matching were both successful, resulting in groups that were well-balanced on most factors. If controls were not restricted to those attending schools in high-risk areas, we found that exact or coarsened exact matching performed slightly better than prognostic score matching. Further, exact matching is possibly easier to implement as it avoids the need to develop (and later possibly update) a potentially complex prognostic model. The advantage of this method is that can be used to ensure exact balance in the most important covariates. Matching on sex, ethnicity and previous caution/conviction was crucial and, if using prognostic score matching, these factors could not be omitted from the model. Outcomes other than serious violence would need a different prognostic model and the relative importance of the various factors are likely to be different; the success (or otherwise) of matching for other outcomes would need to be evaluated separately.

Our findings would apply for any intervention aimed at reducing serious violence targeted at an identifiable group of high-risk individuals, including a school-based intervention in which specific individuals (rather than the whole school) were selected for the intervention. For a more universal intervention, exact matching on a few key covariates would be unlikely to be successful because the individuals in the intervention group (and potential pool of controls) would be much more heterogeneous, so either prognostic score matching or coarsened exact matching on all covariates in the prognostic model would have to be used. In this project we did not explore this, so cannot say how successful – or otherwise – these methods would be for such an intervention. If entire schools were to be selected into an intervention group, matching would have to be done at the school level – i.e. to ensure that the distribution of characteristics of pupils within the intervention and control schools were similar. Even though individual-level education data would not be needed to evaluate a school-level intervention, using the linked dataset might still be the most efficient way of doing this; otherwise, a bespoke linkage (to the PNC) would be needed in order to measure outcomes in the intervention and control groups.

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## Appendix B: Codes for serious violence

Detailed offence	Offence	Offence code
<b>Violence against the person</b>		
Not known	Not known	00100
Murder of persons aged 1 year or over	1 Murder	00101
Murder of infants under 1 year of age	1 Murder	00102
Attempted murder	2 Attempted Murder	00200
Making threats to kill	3B Threats to kill	00301
Conspiracy or soliciting, etc., to commit murder	3A Conspiracy to murder	00302
Assisting offender by impeding his apprehension or prosecution in a case of murder	3A Conspiracy to murder	00303
Intentionally encouraging or assisting commission of murder	3A Conspiracy to murder	00304
Encouraging or assisting in the commission of murder believing it will be committed	3A Conspiracy to murder	00305
Encouraging or assisting in the commission of one or more offences of murder believing one or more will be committed	3A Conspiracy to murder	00306
Manslaughter	4.1 Manslaughter	00401
Infanticide	4.2 Infanticide	00402
Child destruction	4.3 Child Destruction	00403
Wounding etc. with intent to do grievous bodily harm etc. or to resist apprehension	5A Wounding with intent to cause grievous bodily harm	00501
Attempting to choke, suffocate etc. with intent to commit an indictable offence (garrotting)	5.2 Other Endangering life - indictable only	00504
Using chloroform, etc., to commit or assist in committing an indictable offence	5.2 Other Endangering life - indictable only	00505
Burning, maiming, etc. by explosion	5.1 Other assault with intent to cause serious harm	00506
Causing, explosions or casting corrosive fluids with intent to do grievous bodily harm	5.2 Other Endangering life - indictable only	00507

Placing, etc. explosives in or near ships or buildings with intent to do bodily harm, etc.	5.2 Other Endangering life - indictable only	00509
Endangering life or causing harm by administering poison	5.2 Other Endangering life - indictable only	00510
Possession etc. of explosives with intent to endanger life	5.2 Other Endangering life - indictable only	00513
Possession of firearms etc., with intent to endanger life (Group I)	5.2 Other Endangering life - indictable only	00514
Possession of firearms etc. with intent to endanger life (Group II)	5.2 Other Endangering life - indictable only	00515
Possession of firearms etc. with intent to endanger life (Group III)	5.2 Other Endangering life - indictable only	00516
Using etc. firearms or imitation firearms with intent to resist arrest etc. (Group I)	5.2 Other Endangering life - indictable only	00517
Using etc. firearms or imitation firearms with intent to resist arrest etc. (Group II)	5.2 Other Endangering life - indictable only	00518
Using etc. firearms or imitation firearms with intent to resist arrest etc. (Group III)	5.2 Other Endangering life - indictable only	00519
Use etc. of chemical weapons	5.2 Other Endangering life - indictable only	00520
Use of premises or equipment for producing chemical weapons	5.2 Other Endangering life - indictable only	00521
Use, threat of use, production or possession of a nuclear weapon	5.2 Other Endangering life - indictable only	00522
Torture	5.1 Other assault with intent to cause serious harm	00527
Administering poison with intent to injure or annoy	8.04 Other assault with injury - indictable	00802
Causing bodily harm by furious driving	8.4 Causing bodily harm by furious driving (MOT)	00804
Assaults on person preserving wreck	8.04 Other assault with injury - indictable	00805
Assaults occasioning actual bodily harm	8.01 Assault occasioning actual bodily harm	00806
Racially aggravated wounding or inflicting grievous bodily harm (inflicting bodily injury with or without weapon)	8H Racially or religiously aggravated wounding or inflicting grievous bodily harm without intent	00833
Religiously aggravated malicious wounding or grievous bodily harm	8H Racially or religiously aggravated wounding or inflicting grievous bodily harm without intent	00840

Racially or religiously aggravated malicious wounding or grievous bodily harm (GBH)	8H Racially or religiously aggravated wounding or inflicting grievous bodily harm without intent	00846
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Racially or religiously aggravated wounding or grievous bodily harm	8H Racially or religiously aggravated wounding or inflicting grievous bodily harm without intent	00859
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**Robbery**

Robbery	34 Robbery	03401
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Assault with intent to rob	34 Robbery	03402
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Not known	Not known	03410
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Theft offences

Burglary in a dwelling with intent to inflict grievous bodily harm - indictable only	28.1 Burglary in a Dwelling - indictable only	02802
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Criminal damage and arson

Arson endangering life	56A Arson endangering life	05601
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**Possession of weapons**

Setting spring guns etc. to injure trespassers	10A Possession of firearms with intent	00803
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Possession of offensive weapons without lawful authority or reasonable excuse	10C.2 Possession of other weapons - triable either way	00811
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Possessing firearm or imitation firearm at time of committing or being arrested for an offence specified in Schedule 1 of the Act (Group I)	10A Possession of firearms with intent	00813
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Possessing firearm or imitation firearm at time of committing or being arrested for an offence specified in Schedule 1 of the Act (Group II)	10A Possession of firearms with intent	00814
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Possessing firearm or imitation firearm at time of committing or being arrested for an offence specified in Schedule 1 of the Act (Group III)	10A Possession of firearms with intent	00815
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Possessing firearm or imitation firearm with intent to commit an indictable offence or resist arrest etc (Group I)	10A Possession of firearms with intent	00816
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Possessing firearm or imitation firearm with intent to commit an	10A Possession of firearms with intent	00817
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indictable offence, or resist arrest etc (Group II)		
Possessing firearm or imitation firearm with intent to commit an indictable offence, or resist arrest etc (Group III)	10A Possession of firearms with intent	00818
Possession of a firearm or imitation firearm, with intent to cause fear of violence (Group I)	10A Possession of firearms with intent	00823
Possession of a firearm or imitation firearm with intent to cause fear of violence (Group II)	10A Possession of firearms with intent	00824
Possession of a firearm or imitation firearm with intent to cause fear of violence (Group III)	10A Possession of firearms with intent	00825
Having an article with a blade or point in a public place	10D Possession of article with blade or point	00826
Having an article with a blade or point on school premises	10D Possession of article with blade or point	00827
Possession of offensive weapons without lawful authority or reasonable excuse on school premises	10C.2 Possession of other weapons - triable either way	00828
Using another to look after, hide or transport a dangerous weapon - offensive weapon, knife or bladed weapon	10C.1 Possession of other weapons - indictable only	00853
Using another to look after, hide or transport a dangerous weapon - a firearm	10A Possession of firearms with intent	00854
Threaten with an offensive weapon in a public place	10C.2 Possession of other weapons - triable either way	00861
Threaten with a blade or sharply pointed article on school premises	10D Possession of article with blade or point	00862
Threaten with an offensive weapon on school premises	10C.2 Possession of other weapons - triable either way	00863
Threaten with blade/sharply pointed article in a public place	10D Possession of article with blade or point	00864
Not known	Not known	08101
Possessing etc. firearm or ammunition without firearm certificate (Group I)	10B.2 Possession of firearms offences - triable either way	08103
Possessing etc. shotgun without certificate (Group II)	10B.2 Possession of firearms offences - triable either way	08104

Trading in firearms without being registered as a firearms dealer (Group I)	81.3 Other firearms offences - triable either way	08107
Trading in firearms without being registered as a firearms dealer	81.3 Other firearms offences - triable either way	08108
Selling firearm to person without a certificate (Group I)	81.3 Other firearms offences - triable either way	08109
Selling firearm to person without a certificate (Group II)	81.3 Other firearms offences - triable either way	08110
Repairing, testing etc. firearm for person without a certificate (Group I)	81.3 Other firearms offences - triable either way	08111
Repairing, testing etc. firearm for person without a certificate (Group II)	81.3 Other firearms offences - triable either way	08112
Falsifying certificate etc. with a view to acquisition of firearm (Group I)	81.3 Other firearms offences - triable either way	08113
Falsifying certificate etc. with a view to acquisition of firearm (Group II)	81.3 Other firearms offences - triable either way	08114
Shortening a shotgun or other smooth bore gun (Group I)	81.3 Other firearms offences - triable either way	08115
Conversion of firearms (Group I)	81.3 Other firearms offences - triable either way	08116
Possessing or distributing prohibited weapons or ammunition (Group I)	10B.1 Possession of firearms offences - indictable only	08117
Carrying firearm in public place etc. (Group I)	10B.2 Possession of firearms offences - triable either way	08126
Carrying loaded firearm in public place etc. (Group II)	10B.2 Possession of firearms offences - triable either way	08127
Trespassing with firearm or imitation firearm in a building (Group I)	10B.2 Possession of firearms offences - triable either way	08129
Trespassing with firearm or imitation firearm in a building (Group II)	10B.2 Possession of firearms offences - triable either way	08130
Possession of firearms by persons previously convicted of crime (Group I)	10B.2 Possession of firearms offences - triable either way	08135
Possession of firearms by persons previously convicted of crime (Group II)	10B.2 Possession of firearms offences - triable either way	08136
Possession of firearms by persons previously convicted of crime (Group III)	10B.2 Possession of firearms offences - triable either way	08137
Supplying firearms to person denied them under Section 21 (Group I)	81.3 Other firearms offences - triable either way	08138

Supplying firearms to person denied them under Section 21 (Group II)	81.3 Other firearms offences - triable either way	08139
Supplying firearms to person denied them under Section 21 (Group III)	81.3 Other firearms offences - triable either way	08140
Failure to transfer firearms or ammunition in person (Group I)	81.3 Other firearms offences - triable either way	08142
Failure to give notice in writing to the Chief Officer of Police of transfers involving firearms (Group I)	81.3 Other firearms offences - triable either way	08143
Failure by certificate holder to notify in writing Chief Officer of Police of deactivation, destruction or loss of firearms or ammunition (Group I)	81.3 Other firearms offences - triable either way	08144
Failure by certificate holder to notify in writing Chief Officer of Police of events taking place outside Great Britain involving firearms and ammunition (sold or otherwise disposed of, lost etc) (Group I)	81.3 Other firearms offences - triable either way	08145
Not known	Not known	08169
Possessing or distributing prohibited weapons designed for discharge of noxious liquid etc. (Group I)	10B.2 Possession of firearms offences - triable either way	08170
Possessing or distributing firearm disguised as other object (Group I)	10B.1 Possession of firearms offences - indictable only	08171
Possessing or distributing other prohibited weapons	10B.2 Possession of firearms offences - triable either way	08172
Offence in relation to the unlawful IMPORTATION of any weapon or ammunition of a kind mentioned in S.5(1)(a),(ab),(aba),(ac), (ad),(ae),(af) or (c) of the Firearms Act 1968	81.3 Other firearms offences - triable either way	08173
Offence in relation to the unlawful EXPORTATION of any weapon or ammunition of a kind mentioned in S.5(1)(a) (ab),(aba),(ac),(ad),(ae), (af) or (c) of the Firearms Act 1968	81.3 Other firearms offences - triable either way	08174
Selling or transferring an air weapon unlawfully	81.3 Other firearms offences - triable either way	08176
Carrying a loaded or unloaded or imitation firearm or air weapon in public place	10B.2 Possession of firearms offences - triable either way	08177
Knowingly being concerned in activity prohibited by Parts 2, 3 or 4 of the Order with intent to evade the relevant prohibition	81.3 Other firearms offences - triable either way	08178

Unship / unload prohibited weapon / ammunition with intent to evade prohibition / restriction	81.1 Firearms offences - Anti-social Behaviour, Crime and Policing Act 2014 - prohibited weapons/ammunition - triable either way	08179
Remove prohibited weapons / ammunition from their place of importation with intent to evade prohibition / restriction	81.1 Firearms offences - Anti-social Behaviour, Crime and Policing Act 2014 - prohibited weapons/ammunition - triable either way	08180
Import prohibited weapons / ammunition with intent to evade a prohibition / restriction	81.1 Firearms offences - Anti-social Behaviour, Crime and Policing Act 2014 - prohibited weapons/ammunition - triable either way	08181
Export prohibited weapon / ammunition with intent to evade prohibition / restriction	81.1 Firearms offences - Anti-social Behaviour, Crime and Policing Act 2014 - prohibited weapons/ammunition - triable either way	08182
Carry / remove / deposit etc. prohibited weapons / ammunition with intent to evade a prohibition / restriction	81.1 Firearms offences - Anti-social Behaviour, Crime and Policing Act 2014 - prohibited weapons/ammunition - triable either way	08183
Knowingly concerned in fraudulent evasion of prohibition / restriction on prohibited weapon / ammunition	81.1 Firearms offences - Anti-social Behaviour, Crime and Policing Act 2014 - prohibited weapons/ammunition - triable either way	08184
Manufacture weapon / ammunition specified in section 5(1) of the Firearms Act 1968	81.1 Firearms offences - Anti-social Behaviour, Crime and Policing Act 2014 - prohibited weapons/ammunition - triable either way	08185
Sell / transfer prohibited weapon / ammunition	81.1 Firearms offences - Anti-social Behaviour, Crime and Policing Act 2014 - prohibited weapons/ammunition - triable either way	08186
Possess prohibited weapon / ammunition for sale / transfer	81.1 Firearms offences - Anti-social Behaviour, Crime and Policing Act 2014 - prohibited weapons/ammunition - triable either way	08187
Purchase / acquire prohibited weapon / ammunition for sale / transfer	81.1 Firearms offences - Anti-social Behaviour, Crime and Policing Act 2014 - prohibited weapons/ammunition - triable either way	08188
Offences under Explosives Precursors Regulations 2014	81.2 Firearms offences - regulated explosives precursors - triable either way	08189
Manufacture an offensive weapon; Possess article for use in connection with conversion of imitation firearm	81.3 Other firearms offences - triable either way	08190

Make / sell / give as gift defectively deactivated weapon - Police and Crime Act 2017	81.3 Other firearms offences - triable either way	08191
Unlawful marketing of knives (selling or hiring)	90 Knives Act 1997 and other related offensive weapons acts / regulations not dealt with elsewhere	09001
Unlawful marketing of knives (offering or exposing to sell or hire)	90 Knives Act 1997 and other related offensive weapons acts / regulations not dealt with elsewhere	09002
Unlawful marketing of knives - having in possession for the purpose of sale or hire	90 Knives Act 1997 and other related offensive weapons acts / regulations not dealt with elsewhere	09003
Publication of any written, pictorial or other material in connection with the marketing of any knife - the material suggesting or indicating knife suitable for combat	90 Knives Act 1997 and other related offensive weapons acts / regulations not dealt with elsewhere	09004
Publication of any written, pictorial or other material in connection with the marketing of any knife - the material is otherwise likely to stimulate or encourage violent behaviour involving use of the knife as a weapon	90 Knives Act 1997 and other related offensive weapons acts / regulations not dealt with elsewhere	09005

**Miscellaneous crimes against society**

Manufacture, possession or control of explosives under suspicious circumstances	59.1 Manufacture, possession or control of explosives under suspicious circumstances	05914
Possessing or making an explosive substance, a noxious or dangerous thing, a machine, engine or instrument with intent to commit an offence under the Offences against the Person Act 1861	59.2 Possessing or making an explosive substance, a machine, etc, with intent to commit an offence under the Offences against the Person Act	05915
Unauthorised possession in prison of knife or offensive weapon	99.9 Other triable either way (non motoring)	06906

## Appendix C: Additional results

*Table C1: Risk of serious violence 15-18 years among individuals classified as either high risk or not high risk (criteria listed in Section 2.3.1.1 of the report) in LEAs in the top quintile of caution/convictions for serious violence (in the data) during 2007-2008*

Birth cohort	London		Outside London		All regions	
	High risk: No	High risk: Yes	High risk: No	High risk: Yes	High risk: No	High risk: Yes
1995/96	821 (3.0%)	253 (25.7%)	582 (1.3%)	336 (16.4%)	1,403 (2.0%)	589 (19.4%)
1996/97	693 (2.4%)	214 (23.1%)	593 (1.3%)	231 (13.6%)	1,286 (1.7%)	445 (16.9%)
1997/98	576 (2.1%)	197 (24.5%)	497 (1.1%)	188 (13.9%)	1,073 (1.5%)	385 (17.9%)
All	2,090 (2.5%)	664 (24.5%)	1,672 (1.3%)	755 (14.8%)	3,762 (1.7%)	1,419 (18.2%)

*Table C2: Comparison of basic demographic characteristics and outcomes among all those born between 1995/96 and 1997/98 and those with some census data between school years 3 and 8 (inclusive)*

Characteristic	Category	All individuals (n=2,052,995)	Those with census data in at least one of years 3 to 8 (n=1,845,149)
Year of birth	1995/96	675,177 (32.9%)	612,960 (33.2%)
	1996/97	691,482 (33.7%)	623,869 (33.8%)
	1997/98	686,336 (33.4%)	608,320 (33.0%)
Sex	Male	1,032,282 (50.3%)	941,679 (51.0%)
	Female	1,013,686 (49.4%)	897,751 (48.7%)
	Missing	7,027 (0.3%)	5,719 (0.3%)
Ethnicity	White	1,494,846 (72.8%) <sup>1</sup>	1,467,583 (79.5%) <sup>1</sup>
	Black	87,214 (4.3%)	80,746 (4.4%)
	Asian	159,913 (7.8%)	150,994 (8.2%)
	Mixed/other	74,797 (3.6%)	69,165 (3.8%)
	Missing	236,225 (11.5%)	76,661 (4.2%)
Offence (TEW or IO) from 15 to 18 years	Yes	65,802 (3.2%)	64,043 (3.5%)
Serious violence offence from 15 to 18 years	Yes	21,254 (1.0%)	20,740 (1.1%)

1. Ethnicity: percent among those with known ethnicity - all individuals: White 82.3%, Black 4.8%, Asian 8.8%, Mixed/other 4.1%; those with census data: White: 83.0%, Black 4.6%, Asian 8.5%, Mixed/other 3.9%.

Table C3: Amount of missing data for key variables among those with some census data between years 3 and 8 (n=1,845,149)

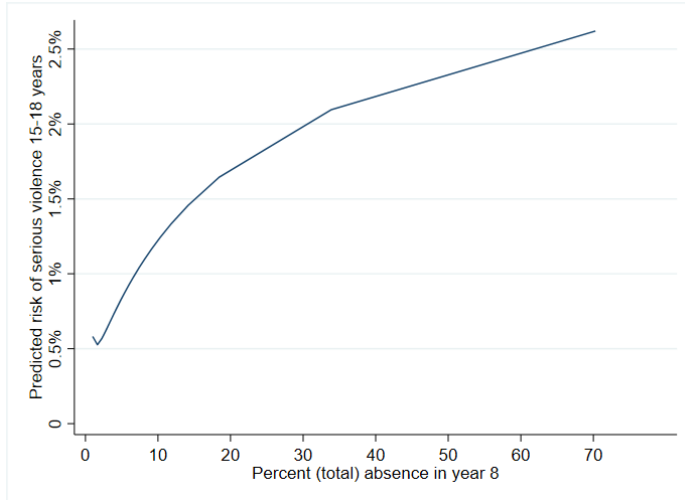
Characteristic	Number with missing data
Individual level	
Sex	5,719
Ethnicity	76,661
FSM year 3 / 4	90,668
FSM year 5 / 6	88,540
FSM year 7 / 8	107,135
SEN status year 3 / 4	90,711
SEN status year 5 / 6	88,545
SEN status year 7 / 8	112,885
IDACI score year 3 / 4	96,134
IDACI score year 5 / 6	93,346
IDACI score year 7 / 8	111,204
KS1 reading score	141,864
KS2 attainment score	200,437
Percent total absence year 6	118,085
Percent total absence year 7	138,417
Percent total absence year 8	134,331
Permanently excluded years 3-8	254,075
Fixed exclusions year 5 + 6	136,918
Fixed exclusions year 7 + 8	141,916
CLA before end year 8	119,678
CIN before end year 8	119,678
School level (measured in the academic year in which the individual was in year 8)	
% FSM	153,506
% White British	153,512

Table C4: Characteristics among all individuals with some census data between years 3 and 8 and complete cases.

Characteristic	Category	All individuals with some census data years 3 to 8 (n=1,845,149)	Complete cases (n=1,438,307)
Sex	Male	941,679 (51.2%)	726,326 (50.5%)
	Female	897,751 (48.8%)	711,981 (49.5%)
Ethnicity	White	1,467,583 (83.0%)	1,227,962 (85.4%)
	Black	80,746 (4.6%)	54,165 (3.8%)
	Asian	150,994 (8.5%)	107,060 (7.4%)
	Mixed/other	69,165 (3.9%)	49,120 (3.4%)
FSM years 3 / 4	Yes	358,102 (20.4%)	279,895 (19.5%)
FSM years 5 / 6	Yes	335,668 (19.1%)	258,074 (17.9%)
IDACI years 3 / 4	Mean (SD)	0.22 (0.18)	0.22 (0.18)
IDACI years 7 / 8	Mean (SD)	0.23 (0.18)	0.22 (0.18)
SEN years 3 / 4	None	1,296,359 (73.9%)	1,103,122 (76.7%)
	School support Statement/EHC plan	410,339 (23.4%)	320,146 (22.3%)
		47,740 (2.7%)	15,039 (1.1%)
SEN years 5 / 6	None	1,274,187 (72.5%)	1,084,535 (75.4%)
	School support Statement/EHC plan	424,493 (24.2%)	335,370 (23.3%)
		57,924 (3.3%)	18,402 (1.3%)
SEN years 7 / 8	None	1,255,879 (72.5%)	1,088,843 (75.7%)
	School support Statement/EHC plan	412,109 (23.8%)	328,372 (22.8%)
		64,276 (3.7%)	21,092 (1.5%)
CiN before end of year 8	Yes	14,752 (0.9%)	7,630 (0.5%)
CLA before end of year 8	Yes	23,779 (1.4%)	13,575 (0.9%)
Fixed exclusions years 5+6	One or more	29,267 (1.7%)	20,022 (1.4%)
Fixed exclusions years 7+8	None	1,590,503 (93.4%)	1,351,404 (94.0%)
	1	69,202 (4.1%)	49,586 (3.5%)
	2	27,420 (1.6%)	14,997 (1.0%)
	3	16,108 (1.0%)	22,320 (1.6%)
Permanently excluded years 3-8	Yes	4,714 (0.3%)	2,485 (0.2%)
KS2 attainment score %	Mean (SD)	64 (19)	64 (19)
Percent absence year 8	Median (IQR)	5 (2-8)	4 (2-8)
% FSM, year 8	Mean (SD)	15 (12.3)	14 (11.6)
% White British, year 8	Median (IQR)	89 (72-94)	90 (75-95)
Region (school attended in year 8)	East Midlands	149,121 (8.8%)	129,799 (9.0%)
	East of England	192,404 (11.4%)	165,305 (11.5%)
	London	228,978 (13.5%)	171,086 (11.9%)
	North East	85,880 (5.1%)	77,201 (5.4%)
	North West	237,089 (14.0%)	207,790 (14.5%)
	South East	266,787 (15.8%)	225,378 (15.7%)
	South West	164,314 (9.7%)	142,208 (9.9%)
	West Midlands	192,505 (11.4%)	166,613 (11.6%)
	Yorkshire & Humber	174,564 (10.3%)	152,927 (10.6%)
Offence (TEW/IO) 15 to 18	Yes	64,043 (3.5%)	46,847 (3.3%)
Serious violence 15 to 18	Yes	20,740 (1.1%)	14,421 (1.0%)

Graphs showing non-linear relationships between percent absence, IDACI scores, Key Stage 2 attainment and serious violence. Note that these analyses were done on a random subset of 20% (n=274,998) individuals.

*Figure C1: Relationship between percent absence in year 8 and serious violence*



*Figure C2: Relationship between Key Stage 2 attainment and serious violence*

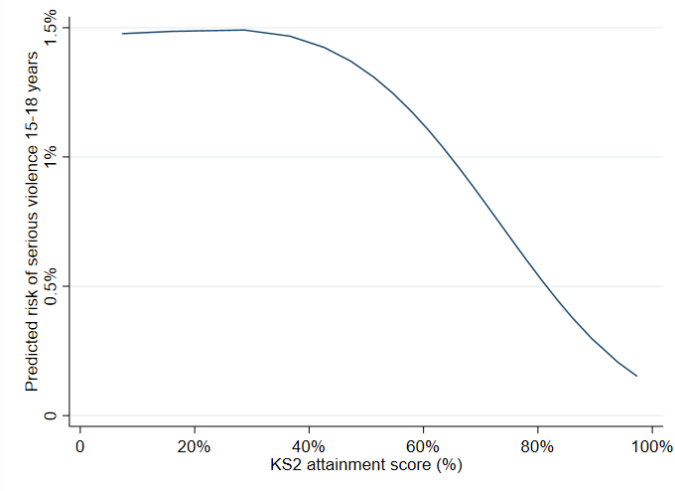
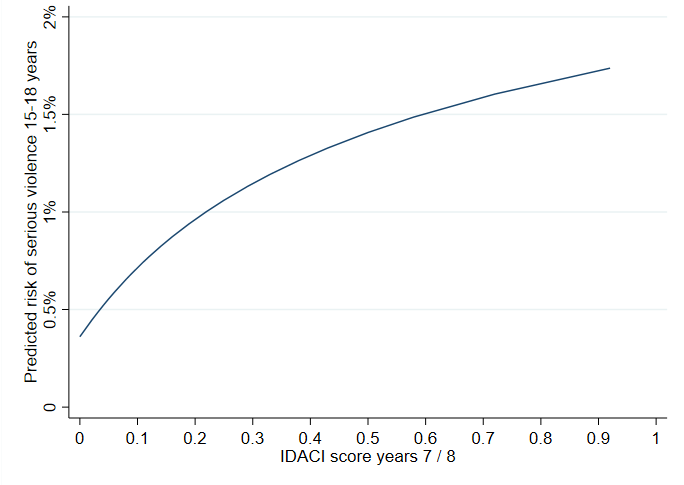


Figure C3: Relationship between IDACI score in year 7/8 and serious violence





1		1.821025	.4900246	2.23	0.026	1.074642	3.085802
pabs8_1		1.068223	.0223885	3.15	0.002	1.025231	1.113017
sex#c.pabs8_1		1.237049	.0642007	4.10	0.000	1.117406	1.369503
pabs8_2		.599136	.0433067	-7.09	0.000	.5199946	.6903223
sex#c.pabs8_2		.4014609	.0706611	-5.19	0.000	.2843304	.5668436
CINby9		1.602191	.0958454	7.88	0.000	1.424933	1.801501
schoolfsm8		1.004911	.0008533	5.77	0.000	1.00324	1.006585
whitebrit8		.9962196	.0005353	-7.05	0.000	.9951711	.9972693
region							
1		1.101022	.0468505	2.26	0.024	1.012922	1.196786
2		.9079142	.0380633	-2.30	0.021	.8362944	.9856676
3		1.217326	.0553162	4.33	0.000	1.113596	1.330719
4		1.072445	.0509563	1.47	0.141	.9770814	1.177115
5		.9599817	.0365003	-1.07	0.283	.891043	1.034254
7		.8179482	.0373102	-4.41	0.000	.7479952	.8944433
8		.9260707	.0383301	-1.86	0.064	.8539116	1.004328
9		.9164068	.037885	-2.11	0.035	.8450823	.9937511
region#ethnic							
1 2		1.359147	.3237639	1.29	0.198	.8521197	2.167864
1 3		.7926911	.1366788	-1.35	0.178	.5653749	1.111403
1 4		1.691198	.258212	3.44	0.001	1.253813	2.28116
2 2		1.624164	.3923581	2.01	0.045	1.01158	2.60771
2 3		1.006322	.1826061	0.03	0.972	.7051452	1.436134
2 4		1.401861	.2269979	2.09	0.037	1.020642	1.92547
3 2		1.619035	.3081589	2.53	0.011	1.114918	2.351092
3 3		.7391338	.097707	-2.29	0.022	.5704291	.9577329
3 4		1.134437	.1426981	1.00	0.316	.8865629	1.451613
4 2		1.003455	.7662638	0.00	0.996	.2246462	4.482259
4 3		.7295596	.2195566	-1.05	0.295	.4044797	1.315906
4 4		.5677483	.2481429	-1.30	0.195	.2410601	1.337169
5 2		1.002122	.2477227	0.01	0.993	.6173126	1.626807
5 3		.6791483	.1036295	-2.54	0.011	.5035956	.9158982
5 4		.9046751	.1489697	-0.61	0.543	.6551311	1.249272
7 2		1.808265	.5259358	2.04	0.042	1.022561	3.19768
7 3		.349365	.1822005	-2.02	0.044	.1257076	.9709506
7 4		.9987949	.2186275	-0.01	0.996	.6503628	1.5339
8 2		1.825284	.3741009	2.94	0.003	1.221441	2.727648
8 3		.8493951	.1170823	-1.18	0.236	.6483036	1.112861
8 4		1.259979	.177679	1.64	0.101	.9557162	1.661107
9 2		1.685375	.4124574	2.13	0.033	1.04324	2.722757
9 3		.7166115	.1076286	-2.22	0.027	.5338768	.9618925
9 4		1.297587	.2134177	1.58	0.113	.9400201	1.791167
_cons		.0419808	.0043023	-30.94	0.000	.0343414	.0513196

Note: \_cons estimates baseline odds.

## Model excluding offending before 14 years

Logistic regression

Number of obs = 1,438,307  
 LR chi2(72) = 35624.33  
 Prob > chi2 = 0.0000  
 Pseudo R2 = 0.2207

Log likelihood = -62909.476

sv15_18	Odds Ratio	Std. Err.	z	P> z	[95% Conf. Interval]
-----					
birthyear					
1996	.8254332	.017186	-9.21	0.000	.7924272 .8598138
1997	.7691288	.0166927	-12.09	0.000	.7370977 .8025518
1.sex	.44553	.0699919	-5.15	0.000	.3274574 .6061766
ethnic					
2	1.375844	.2558107	1.72	0.086	.9556645 1.980766
3	1.355173	.1594089	2.58	0.010	1.076138 1.70656
4	1.696722	.1896378	4.73	0.000	1.362932 2.112259
sex#ethnic					
1 2	.380981	.0357762	-10.28	0.000	.3169354 .4579687
1 3	.2424031	.0368236	-9.33	0.000	.179983 .3264711
1 4	.6091784	.0588307	-5.13	0.000	.5041278 .7361195
pexclude	1.313828	.0823593	4.35	0.000	1.161929 1.485584
fixed78					
1	3.424843	.093566	45.06	0.000	3.24628 3.613228
2	4.933054	.178629	44.07	0.000	4.595083 5.295883
3	6.615634	.1986073	62.94	0.000	6.237601 7.016577
sex#fixed78					
1 1	1.474165	.107917	5.30	0.000	1.277125 1.701605
1 2	1.419225	.1437696	3.46	0.001	1.163652 1.730929
1 3	1.248366	.0991344	2.79	0.005	1.068432 1.458603
fixed56	1.467353	.0450555	12.49	0.000	1.38165 1.558371
ks2_1	1.002882	.0002795	10.32	0.000	1.002334 1.00343
ks2_2	.9987167	.000108	-11.88	0.000	.9985051 .9989284
CLAby9	2.257778	.1132125	16.24	0.000	2.046441 2.49094
sex#CLAby9					
1 1	1.394101	.1228645	3.77	0.000	1.172941 1.65696
fsm34	1.365625	.0376265	11.31	0.000	1.293835 1.441399
fsm56	1.290374	.0352576	9.33	0.000	1.223088 1.361362
sen34					
1	1.028274	.0257568	1.11	0.266	.9790109 1.080016
2	.7256252	.0743143	-3.13	0.002	.5936592 .8869262
sen56					
1	1.065598	.0292567	2.31	0.021	1.009771 1.124511
2	1.160865	.1276322	1.36	0.175	.9358269 1.440017
sen78					
1	1.250423	.0293351	9.53	0.000	1.194229 1.309262
2	1.00426	.082081	0.05	0.959	.855608 1.178737
idac34_1	.6046485	.0663122	-4.59	0.000	.4876982 .7496436
sex#c.idac34_1					
1	.6098652	.160614	-1.88	0.060	.3639666 1.021895
idac78_1	.4190472	.0479961	-7.59	0.000	.3347876 .5245133
sex#c.idac78_1					
1	1.882647	.5049348	2.36	0.018	1.112943 3.184676

pabs8_1		1.130509	.0233497	5.94	0.000	1.085659	1.177213
sex#c.pabs8_1							
1		1.223484	.0630232	3.92	0.000	1.105991	1.353458
pabs8_2		.4800523	.0342539	-10.28	0.000	.417399	.55211
sex#c.pabs8_2							
1		.4202226	.0736357	-4.95	0.000	.2980734	.592428
CINby9		1.654614	.0976127	8.54	0.000	1.473943	1.857431
schoolfsm8		1.005767	.000849	6.81	0.000	1.004104	1.007432
whitebrit8		.9967919	.0005329	-6.01	0.000	.995748	.9978369
region							
1		1.110221	.0469349	2.47	0.013	1.021938	1.20613
2		.9194404	.0383264	-2.01	0.044	.8473087	.9977127
3		1.188631	.0537687	3.82	0.000	1.087783	1.298829
4		1.112439	.0523334	2.27	0.024	1.014454	1.219888
5		.9598055	.036255	-1.09	0.277	.8913136	1.033561
7		.8152419	.036999	-4.50	0.000	.745857	.8910815
8		.8997446	.0370467	-2.57	0.010	.8299869	.9753652
9		.932179	.0382565	-1.71	0.087	.860134	1.010259
region#ethnic							
1 2		1.389998	.3295534	1.39	0.165	.8733803	2.212202
1 3		.8026842	.1380183	-1.28	0.201	.5730388	1.12436
1 4		1.699357	.2572295	3.50	0.000	1.263104	2.286282
2 2		1.675891	.4028969	2.15	0.032	1.04619	2.684609
2 3		1.002555	.1813361	0.01	0.989	.703312	1.429119
2 4		1.384169	.2224887	2.02	0.043	1.01011	1.896748
3 2		1.653157	.3140308	2.65	0.008	1.139256	2.39887
3 3		.7557118	.0996969	-2.12	0.034	.5835283	.9787021
3 4		1.152304	.1440464	1.13	0.257	.9019045	1.472222
4 2		1.055152	.792459	0.07	0.943	.2421201	4.598323
4 3		.7159365	.2143771	-1.12	0.264	.3981018	1.287523
4 4		.5670481	.2468668	-1.30	0.193	.2415715	1.331049
5 2		.961894	.2379561	-0.16	0.875	.5923175	1.562068
5 3		.6815601	.10373	-2.52	0.012	.505773	.9184442
5 4		.9179602	.1498349	-0.52	0.600	.6666313	1.264043
7 2		1.675495	.4887993	1.77	0.077	.9458414	2.968026
7 3		.3540599	.1844907	-1.99	0.046	.1275086	.9831368
7 4		.9802927	.2129834	-0.09	0.927	.6403529	1.500694
8 2		1.837096	.3755928	2.97	0.003	1.230565	2.74258
8 3		.8619666	.1185287	-1.08	0.280	.6583275	1.128597
8 4		1.272258	.1781729	1.72	0.086	.9668717	1.6741
9 2		1.661749	.4053865	2.08	0.037	1.030181	2.680511
9 3		.7088721	.1061432	-2.30	0.022	.5285827	.9506547
9 4		1.297584	.2116658	1.60	0.110	.9425079	1.78643
_cons		.0505557	.0051403	-29.35	0.000	.0414213	.0617045

Note: \_cons estimates baseline odds.

## Multilevel logistic regression with pupils clustered within schools (URN8=school ID number when pupil in year 8)

Mixed-effects logistic regression  
 Group variable: urn8  
 Number of obs = 1438307  
 Number of groups = 3,960  
 Obs per group:  
     avg = 363.2  
 Integration method: mvaghermite  
 Integration pts. = 7  
 Log likelihood = -62137.854  
 Wald chi2(73) = 32412.93  
 Prob > chi2 = 0.0000

sv15_18	Odds Ratio	Std. Err.	z	P> z	[95% Conf. Interval]	
-----						
birthyear						
1996	.8440853	.0178383	-8.02	0.000	.8098371	.8797819
1997	.8029427	.0177671	-9.92	0.000	.7688641	.8385317
offendpre14						
1.sex	3.004328	.0868406	38.06	0.000	2.838855	3.179446
1.sex	.4988324	.0790101	-4.39	0.000	.3657067	.6804191
ethnic						
2	1.369303	.2584765	1.67	0.096	.9458501	1.982333
3	1.341613	.1635199	2.41	0.016	1.056526	1.703627
4	1.697502	.1928126	4.66	0.000	1.358706	2.120777
sex#ethnic						
1 2	.3883053	.0370103	-9.92	0.000	.322139	.4680619
1 3	.2415623	.0369128	-9.30	0.000	.1790434	.3259118
1 4	.6131923	.0596522	-5.03	0.000	.5067463	.741998
pexclude						
pexclude	1.088802	.0705945	1.31	0.189	.9588698	1.236341
fixed78						
1	3.32246	.0929635	42.91	0.000	3.145161	3.509754
2	4.599877	.171756	40.87	0.000	4.275264	4.949136
3	5.769262	.1838666	54.99	0.000	5.419914	6.141127
fixed78#sex						
1 1	1.422728	.1051203	4.77	0.000	1.230919	1.644425
2 1	1.352701	.1391614	2.94	0.003	1.105689	1.654895
3 1	1.185923	.0960307	2.11	0.035	1.011883	1.389898
fixed56						
ks2_1	1.39179	.0438221	10.50	0.000	1.308496	1.480385
ks2_2	1.002875	.0002831	10.17	0.000	1.00232	1.00343
ks2_2	.9987208	.0001093	-11.70	0.000	.9985066	.998935
CLAby9	1.985806	.1025047	13.29	0.000	1.794729	2.197226
sex#CLAby9						
1 1	1.408123	.1270855	3.79	0.000	1.179827	1.680595
fsm34						
fsm34	1.354968	.0376915	10.92	0.000	1.283071	1.430893
fsm56	1.263245	.0348738	8.46	0.000	1.19671	1.333479
sen34						
1	1.031568	.0261387	1.23	0.220	.9815887	1.084093
2	.7609386	.0795543	-2.61	0.009	.6199526	.9339867
sen56						
1	1.064753	.0295933	2.26	0.024	1.008303	1.124364
2	1.17428	.1321834	1.43	0.154	.9417935	1.464157
sen78						
1	1.23332	.0295057	8.77	0.000	1.176825	1.292528
2	.9600217	.0807433	-0.49	0.628	.8141232	1.132067
idac34_1						
idac34_1	.5992869	.0667142	-4.60	0.000	.481811	.7454059

sex#c.idac34_1							
1	.59305	.1570434	-1.97	0.048	.3529293	.9965404	
idac78_1	.4397269	.05118	-7.06	0.000	.3500349	.5524014	
sex#c.idac78_1							
1	1.802709	.4860874	2.19	0.029	1.062686	3.058059	
pabs8_1	1.064434	.0224435	2.96	0.003	1.021342	1.109345	
sex#c.pabs8_1							
1	1.24124	.0645646	4.15	0.000	1.120933	1.37446	
pabs8_2	.6076996	.0442271	-6.84	0.000	.5269146	.7008702	
sex#c.pabs8_2							
1	.3966063	.0699991	-5.24	0.000	.2806251	.560522	
CINby9	1.608929	.0971536	7.88	0.000	1.429348	1.811073	
schoolfsm8	1.005397	.0010182	5.32	0.000	1.003404	1.007395	
whitebrit8	.9962846	.000635	-5.84	0.000	.9950409	.9975299	
region							
1	1.095988	.054382	1.85	0.065	.9944201	1.20793	
2	.9052717	.0435891	-2.07	0.039	.823746	.994866	
3	1.209685	.0648885	3.55	0.000	1.088963	1.34379	
4	1.066557	.059935	1.15	0.252	.9553244	1.190741	
5	.9597219	.0425663	-0.93	0.354	.8798169	1.046884	
7	.8106645	.0421572	-4.04	0.000	.7321093	.8976486	
8	.9245845	.0441644	-1.64	0.101	.8419523	1.015327	
9	.9152833	.0442468	-1.83	0.067	.8325429	1.006247	
region#ethnic							
1 2	1.329959	.324282	1.17	0.242	.8246901	2.144793	
1 3	.7731992	.1414614	-1.41	0.160	.5402055	1.106685	
1 4	1.654611	.2571172	3.24	0.001	1.22018	2.243716	
2 2	1.510231	.3716826	1.68	0.094	.932297	2.446428	
2 3	.9511095	.1827623	-0.26	0.794	.6526289	1.386101	
2 4	1.346536	.220594	1.82	0.069	.976723	1.856371	
3 2	1.639988	.3170365	2.56	0.010	1.122767	2.395476	
3 3	.7505638	.103596	-2.08	0.038	.5726656	.9837258	
3 4	1.144634	.1456236	1.06	0.288	.8920194	1.468787	
4 2	1.042157	.8033966	0.05	0.957	.2300084	4.721963	
4 3	.7988788	.2452079	-0.73	0.464	.4377368	1.45797	
4 4	.5946751	.2609611	-1.18	0.236	.2516212	1.40544	
5 2	1.092279	.2766355	0.35	0.727	.6648968	1.794373	
5 3	.6905717	.1106635	-2.31	0.021	.5044343	.9453944	
5 4	.9200568	.154032	-0.50	0.619	.6626894	1.277377	
7 2	1.917472	.584082	2.14	0.033	1.055465	3.483484	
7 3	.3566759	.1874568	-1.96	0.050	.1273242	.9991637	
7 4	1.010738	.2235505	0.05	0.961	.6551995	1.559206	
8 2	1.917792	.4012395	3.11	0.002	1.272664	2.889945	
8 3	.8860781	.1289289	-0.83	0.406	.6662202	1.178491	
8 4	1.273958	.1819042	1.70	0.090	.9629746	1.68537	
9 2	1.683909	.4222901	2.08	0.038	1.030041	2.75285	
9 3	.7707975	.1227978	-1.63	0.102	.5640702	1.053289	
9 4	1.302536	.2177405	1.58	0.114	.9386361	1.807516	
_cons	.0389858	.0042738	-29.60	0.000	.031448	.0483303	
urn8							
var(_cons)	.0950996	.0088574			.0792318	.1141454	

Note: Estimates are transformed only in the first equation.

Note: \_cons estimates baseline odds (conditional on zero random effects).

LR test vs. logistic model: chibar2(01) = 215.30 Prob >= chibar2 = 0.0000

## Acknowledgements

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