

The Two-Child Limit: impact on children's education and justice outcomes

Secondary Data Project Analysis Plan

Evaluating institution: Manchester Metropolitan University

Principal investigator(s): Will Cook



Project summary

Project title	The Two-Child Limit: impact on children's education and justice outcomes
Research Team	Manchester Metropolitan University
Principal investigator	Will Cook
Analysis plan author(s)	Will Cook
Overarching research question	What are the impacts of the two-child benefit limit on children's school and justice outcomes in affected families?
Supporting research question(s)	RQ1. Has the Two Child Limit causally affected predictors of serious youth violence. RQ2. Has the Two Child Limit causally affected youth offending/ serious youth violence? RQ3. Are there moderating factors that influence whether the Two Child Limit causally affects the above outcomes?
Dataset(s) to be used	The National Pupil Database and the National Pupil Database linked to the Police National Computer (MoJ-DfE data share)
Population characteristics	Pupils educated in state schools in 2024
Years data spans	2013/14-2024/25
Geographic coverage	England
Primary outcome(s) investigated (if relevant)	RQ1: - SEND identification for Social, Emotional and Mental Health Needs RQ2: - Any offending record

Main method(s) to be used or tested	Difference in Difference Regression Discontinuity

Analysis plan history

Version	Date	Reason for revision
1.X [<i>latest</i>]		
1.1		
1.0 [<i>original</i>]		<i>[leave blank for the original version]</i>

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1. About the project

1.1. Background to the project

In April 2017, the government implemented the so called “two child limit”; (“2CL”). The 2CL refers to the restriction of per child welfare benefits (Child Tax Credit or the Child element of universal credit, “CTC/UC”) to the first two children within a family and applied to households with third or higher order children born on or after the 6th of April 2017. This reduced the amount eligible to those households by a figure that is now £3,514 p.a.; estimated to be worth around 10% of affected families’ household income (Latimer and Waters, 2024). This project aims to test whether this policy has had a causal impact on youth violent crime and the predictors of it, such as school exclusions and absence.

There is a substantial amount of research that relates poverty to increased propensity towards adverse outcomes, including involvement in violent crime. However, the recent YEF evidence review of the relationship between poverty, crime and violence could not establish that poverty causes violence as it did not find research which examined this (Clemmow et al., 2025). The 2CL presents a rare opportunity to perform causal analysis on this question. This is possible due to the quasi-random nature of the policy’s implementation, the size of the income difference that it induced, and the number of children that have been affected by the policy.

The project is intended to provide evidence that may inform policy around:

- Unconditional cash transfers as a way of improving social outcomes, as opposed to service provision
- Universal basic income provision
- The provision of indirect transfers (e.g. free school meals)

More broadly this project will provide evidence for future impact assessments into welfare changes by ensuring potential societal costs and benefits related to crime are recognised; it is notable that the government’s impact assessment of the 2CL was not able to refer to any evidence of social impacts.

Aside from the main findings of the project itself, there will be two positive externalities generated by the project. First, via the production of the descriptive statistics, the project will produce the first detailed profile of the young people that are most likely to have been affected by the 2CL – e.g. their ethnicity and the types of schools they attend, etc. Secondly, the project will produce flags of those pupils most likely to have been affected by the 2CL. These new variables will be made available to researchers in the future, allowing for the

assessment of impacts over the life course.

1.2. Research Aim

The research aim is to analyse whether the 2CL has impacted offending and predictors of offending.

1.3. Research question(s)

The research is framed around four research questions:

RQ0. [Checking of identifying assumptions]

- Is the density of cases of third born children for the samples analysed continuous over the 6th of April 2017 birth date threshold
- Are the baseline characteristics of these children continuous over the 6th of April 2017 birth date threshold?

RQ1. Has the Two Child Limit causally affected predictors of serious youth violence?

RQ2. Has the Two Child Limit causally affected youth offending/serious youth violence?

RQ3. Are there moderating factors that influence whether the Two Child Limit causally affects the above outcomes?

Table 1.1. How will the questions be addressed at each stage?

Question Number	Interim report	Final report
RQ0	- Descriptive statistics - Continuity assumption tests	- Revision of interim report results if required. - Additional continuity assumption tests focussed on main findings if required.
RQ1	-Graphical analysis (pre-post charts)	- Full statistical results (RDD and Diff in Diff Models) - Robustness checks

RQ2	-Graphical analysis (pre-post charts)	- Full statistical results (RDD and Diff in Diff Models) - Robustness checks
RQ3	- Descriptive statistics	- Results for RQ1 and RQ2 presented by subgroup

1.4. Hypotheses

For the development of the research hypotheses, we focus on evidence around the impacts of household income on outcomes, as this is to where most evidence relates. However, it should be noted that changes in welfare eligibility may induce behaviour change that may maintain household income by obtaining income from other sources, e.g. claiming other welfare benefits, or entering employment. The main theoretical mechanism by which welfare cuts may induce adverse child outcomes is the Family Stress Model – this contends that children directly experience economic stress via unmet material needs, whilst at the same time economic stress affects adult-adult and adult-child relationships which results in family breakdown and adverse parenting practices (Neppl. et al, 2016). Through this model, economic pressures affect the development trajectory of children, leading to accumulating and reinforcing adverse outcomes, both as a response to, and as a result of, economic stress. On the basis of this theory the following hypotheses are proposed for each RQ:

RQ1

Hypothesis: The 2CL has an adverse effect on the predictors of offending.

Evidence: In general, most research studies support the view that lower household income is related to adverse child outcomes, the majority of this evidence is associational, but there are a few studies that implement causal designs (Micheltmore, 2025). However, recent RCT evidence of cash transfers from the US challenges this view and has tended to find null effects of cash transfers on child outcomes (Noble et al., 2025).

RQ2

Hypothesis: The 2CL increases the proportion of young people offending for the families it affects.

Evidence: A recent evidence review by YEF (Clemmow et al, 2025) identifies an association between poverty and youth crime in studies published since 1990, however it is emphasised

that this is associational rather than causal evidence. There is some evidence that cuts in UK welfare generosity is related to area level crime increases, particularly for violent crime (Giulietti & McConnell, 2022). However, there is limited causal evidence of the impact of welfare changes/household income on crime at the individual level and the author is not aware of any study estimating the causal effect of these factors on youth offending.

RQ3

Hypothesis: Boys are more likely to be affected by the 2CL than girls.

Evidence: The Clemmow et al. (2025) review found that gender and family structure moderate the link between poverty and youth crime, with boys most responsive to the effect of deprivation on crime. Family structure is not measured in the data being used in this project.

1.5. Key concepts

Policy

The policy to be evaluated is the 2CL however, it is important to note that there is no variable in the datasets we use that identify whether a family is directly impacted by the 2CL (i.e. with 3 or more children, with at least one born on or after April the 6th, 2017, eligible for CTC/UC). In the modelling we approach this by defining the treatment group in two ways:

‘Large families’ – these are all families in our data with third or higher order children born on or after the 6th of April 2017. Defining the treatment group in this way will provide population level effects of the policy – the so called ‘reduced form’ estimates, i.e. by how much the 2CL affected the outcomes for larger families in the aggregate. For simplicity, this group is referred to as ‘Group A’ in this document (along with those third or higher order children born prior to the 6th of April 2017, to be used as a control group).

‘Eligible families’ - those households that are most likely to be those affected by the 2CL, these being those that comply with the following conditions in the National Pupil Database:

- Third or higher order children born on or after the 6th of April 2017
- Have a child in the January 2017 Schools Census who is registered as being eligible for Free School Meals.

Defining the treatment group in this way is an attempt to isolate the individual level treatment effect of the policy. It is defined in this way because those eligible for free school meals would also be eligible for the child element of universal credit (or child tax credit). We use the eligibility as at January 2017 (and not later) to avoid any bias arising from the 2CL affecting

the propensity to claim free school meals. This group (along with that comply with the above conditions, born prior to April the 6th 2017) is referred to as 'Group B' in this document.

The control group will be defined according to the method employed. For the difference in difference approach, the comparator group will be households with a *second* born child born between September 2016 and August 2017 as these households were not impacted by the changes to CTC/UC. For the RDD approach, the concept of a control group is not strictly defined, however it can be loosely thought of as the families (Group A and Group B) with third or higher order children born in the months leading up to the 6th of April 2017. A summary of the different samples and treatment/control definitions are provided below:

Table 1.2. Summary of analysis samples

Method	Treatment/Control	Definition
Regression Discontinuity	Treatment	Group A: All families with 3 or more children with the third or higher order children born on or after 6th of April 2017 Group B: All families with 3 or more children with the third or higher order children born on or after 6th of April 2017 AND who were recorded as eligible for FSM in the January 2017 Schools Census.
Regression Discontinuity	Control	Group A: All families with 3 or more children with the third or higher order children born before 6th of April 2017 Group B: All families with 3 or more children with the third or higher order children born before 6th of April 2017

		AND who were recorded as eligible for FSM in the January 2017 Schools Census.
Difference in Difference	Treatment	Group A: All families with 3 or more children with the third or higher order children born on or after 6th of April 2017 but prior to September the 1st 2017. Group B: All families with 3 or more children with the third or higher order children born on or after 6th of April 2017 but prior to September the 1st 2017, AND who were recorded as eligible for FSM in the January 2017 Schools Census.
Difference in Difference	Control	Group A: All families with 3 or more children with the third or higher order children born before 6th of April 2017 [Pre-period treatment group] but on or after the 1st of September 2016; All

		families with second born children born during the 2016/17 academic year. [Never treated group] Group B: All families with 3 or more children with the third or higher order children born before 6th of April 2017 but on or after the 1st of September 2016 AND who were recorded as eligible for FSM in the January 2017 Schools Census. [Pre-period treatment group]; All families with second born children born during the 2016/17 academic year AND who were recorded as eligible for FSM in the January 2017 Schools Census [Never treated group]
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We refer in this document to the second, third or higher order child born in the academic year 2016/17 (i.e. the child that determines the treatment status of the family), as the 'index' child.

Outcomes

Our outcomes are split between predictive factors of serious violent crime (RQ1) and crime outcomes (RQ2).

RQ1 outcomes are based on those available in the National Pupil Database that correspond to known risk factors of serious violent offending based on MoJ/DfE (2022) and Rollings et al. (2025). We have selected the primary outcome to be ever having been identified as having Social, Emotional and Mental Health Needs (SEMH) as primary or secondary SEND type. This SEND type is defined as:

“Children and young people may experience a wide range of social and emotional difficulties which manifest themselves in many ways. These may include becoming withdrawn or isolated, as well as displaying challenging, disruptive or disturbing behaviour”

(SEND code of practice, 2015)

This has been selected as an outcome because it has been estimated that 58% of children who had been cautioned or sentenced for a serious violence offence were identified as having SEMH needs (MoJ/DfE, 2022) and have been identified as a high-risk group for criminal and violent behaviour (Ramaiah et al, 2025). What links this outcome to the two child limit is that household income in early childhood has been found to be independently associated with children's behavioural problems in England (Tamura et al, 2020), while evidence from the US indicates that increases in household income has a causal impact on reducing emotional and behavioural disorders (Akee et al, 2018).

Table 1.2. RQ1 outcomes – predictors of serious violent crime

Concept	Outcome variable description
Emotional or mental health needs	Primary or Secondary SEN recorded as Social, Emotional and Mental Health needs.
Behaviour	Permanent Exclusion
Behaviour	Suspension

Absence	Absence rate by year group
Absence	Persistent Absence (>10% absence)
Attainment	KS2 Maths Scaled Score
Attainment	KS2 Reading Scaled Score
Attainment	Phonics screening check score
Social Care	Child in Need
Social Care	Child Looked After

RQ2 outcomes will be binary outcomes of recorded offences for an individual:

- 'Any offending' will be defined as whether an individual has any offence record on the PNC.
- 'Violent offending' will include any offence included in the 'violence against the person' and 'Possession of weapons' offence groups as detailed in Home Office (2022)
- 'Serious violent offending' will be defined using the offence codes as per MoJ/DfE (2022).

2. About the datasets

2.1. Overview of datasets used

The research will use the NPD alone (RQ0, RQ1 & RQ3) and the NPD linked to the PNC (RQ2 & RQ3) for the purposes of the research.

Table 2.1 Dataset Description – *National Pupil Database*

Name of dataset	National Pupil Database
Data owner(s)	DfE
Type of data	Administrative, multi-cohort, longitudinal pupil level data
Availability of data	Available via a data sharing agreement with the DfE
Team member(s) who will have access	Will Cook Research Associate (to be recruited)
Population/geographic coverage or sampling frame	The universe of pupils educated in English state schools between 2022 and 2024.
Years covered or survey waves	Cohorts born between 2008-2020
Exclusion criteria	None
Expected population/sample size (following exclusion criteria)	~6.5million linked to c.16,000 primary and c.3000 secondary schools
Documentation	https://www.gov.uk/guidance/apply-for-department-for-education-dfe-personal-data

Table 2.2 Dataset Description – *Police National Computer*

Name of dataset	Police National Computer
Data owner(s)	MoJ
Type of data	Administrative, offence level
Availability of data	Available via a data sharing agreement with the MoJ
Team member(s) who will have access	Will Cook Research Associate (to be recruited)
Population/geographic coverage or sampling frame	Offences recorded for individuals from the age of ten in England (from 2018 onward).
Years covered or survey waves	Offences from 2018- April 2025
Exclusion criteria	N/A
Expected population/sample size (following exclusion criteria)	Estimated ~500,000 offences
Documentation	https://www.gov.uk/guidance/ministry-of-justice-data-first

2.2. Linking datasets

The link between the NPD and the PNC is done as part of the MoJ/DfE data share. Our understanding is that this dataset will be updated in time for our analysis to cover offences up until April 2025.

In order to conduct the analysis of the 2CL we need to be able to identify individuals who are the third or higher born in their family within the cohorts of interest. As it stands, the NPD does not have this information. Therefore, we have commissioned FFT Education Datalab to create sibling and household identifiers in the NPD. FFT Education Datalab have created sibling identifiers in the NPD previously, the last iteration being for the 2015/2016 Schools

Census. These were based on identifying pupils living at the same address with the same surname -see Holt & Allen (2016) for details of the method used; we propose to use an identical method for this project.

This work will require access to sensitive name and address information and is being conducted with the support of the DfE as an enhancement to the NPD. Once the sibling identifiers have been created, they will then be linked to the main DfE datafile. We will then request both i) the NPD data alone , and ii) the NPD linked to the PNC including these new identifiers.

2.3. Access and data protection

The data will be accessed via the ONS Secure Research Service, and we will comply with the conditions of access and output use. The data analysed will be personal data and include the special categories of ethnicity data and health data (i.e. the SEND categories in the NPD). As a university conducting research the legal basis for processing the data is public task. We will complete a Data Protection Impact Assessment where legal conditions around access will be fully assessed.

2.4. Ethics

The research will be submitted for Manchester Metropolitan University institutional ethics approval in Autumn 2025.

We will make the analysis code available on the Manchester Metropolitan University research repository (e-space). The siblings IDs created as part of this project will be made available to researchers on application to the DfE.

3. Methods

3.1. Research design

Table 3.1: Research design

Research design		Secondary analysis using quasi-experimental design approach to test causal impact of reducing welfare benefits on offending and the predictors of offending.
Dataset(s) used		National Pupil Database linked to the Police National Computer.
Population of interest		Pupils educated in state schools in 2024
Size of sample population		Estimated to be up to 6.5million pupils (including all siblings) linked to c.16,000 primary and c.3000 secondary schools
Primary outcome	Variable	1. SEND identification for Social, Emotional and Mental Health Needs (SEMH) 2. Any offending
	Measure (instrument, scale, source)	1. Binary variable of ever having been identified as SEMH for primary or secondary SEN type. 2. Binary variable for any offending record on the PNC
Main method(s) to be used or tested		i) Difference-in-difference (DiD) analysis ii) Regression discontinuity design (RDD)

3.2. Variables and measurement

Table 3.2 Measurement of key concepts

Concept	How the concept will be measured and encoded	Variable source, definition and derivation or specification
Social, Emotional and Mental Health needs	Primary or Secondary SEN recorded as Social, Emotional and Mental Health needs at any point	NPD Schools Census

Behaviour	Suspension by individual years of age (e.g. suspension by age 8)	NPD Exclusions, binary variable of exclusions categorised as 'Suspension.'
Behaviour	Permanent Exclusion by individual years of age.	NPD Exclusions, binary variable of exclusions categorised as 'Permanent.'
Absence	Absence rate by year group (e.g. absence rate in year 2)	NPD Absence, calculated as OverallAbsence_6HalfTerms_ab[yy] divided by SessionsPossible_6HalfTerms_ab[yy]
Absence	Persistent Absence (>10% absence) by year group (e.g. persistent absence in year 2)	Binary variable derived from absence rate (>10%=1 ; 0 otherwise)
Attainment	KS2 Maths Scaled Score	NPD KS2
Attainment	KS2 Reading Scaled Score	NPD KS2
Attainment	Phonics screening check score	NPD Phonics
Social Care	Child in Need	NPD-CIN, binary variable (1=in dataset; 0 not in dataset)
Social Care	Child Looked After	NPD-CLA, binary variable (1=in dataset; 0 not in dataset)
Offending	Any Offending	PNC, binary variable of whether the pupil is recorded as having offended in the PNC
Offending	Violent Offending	PNC, binary variable of whether the pupil is recorded as having offended in the PNC in the categories contained with the 'violence against the person' and 'possession of weapons' offence group.
Offending	Serious Violent Offending	PNC, binary variable of whether the pupil is recorded as having offended in the PNC in the categories identified within MoJ/DfE (2022) as corresponding to serious violent offending.

Table 3.3 Details of other variables used in the analysis

Use in modelling	Variable	Variable source, definition and derivation or specification
Control/Subgroup	Gender	NPD Schools Census
Control/Subgroup	Ethnicity	NPD Schools Census
Control/Subgroup	EAL Status	NPD Schools Census
Control	Age in months	NPD Schools Census
Control	Month of birth	NPD Schools Census
Time (DiD)/Running variable (RDD)	Date of birth of index child	NPD Schools Census, this will be specified as the month of birth for the DiD analysis and the date of birth for the RDD analysis.
Sibling groups	Family Identifier	Derived from NPD using groups with common addresses and surnames.
Treatment	Treatment (Group A)	Derived from the family identifier and date of birth
Treatment	Treatment (Group B)	Derived from the family identifier and FSM status of any family member as at January 2017, and date of birth.
Subgroup	School FSM quintiles	Derived from NPD Schools Census
Subgroup	Cohort % affected by 2CL terciles	Calculated based on the 2CL identifier
Subgroup	Index of Deprivation Affecting Children Index (IDACI) quintiles	NPD Schools Census

3.3. Sample

It is difficult at this point to specify the sample size for analysis, but it is possible to give an indication. For the Group A treatment group we do know that in England and Wales c.7000 third children and c.19000 second born children were born in April 2017 alone according to the Census 2021. We estimate around 40% of these children would be eligible for CTC/UC.

The sample will also vary from model to model according to the outcome analysed. For some outcomes there will not be data for younger siblings – for example KS2 attainment, and we expect that crime outcomes will mostly only be available for the oldest siblings (the PNC recorded offences from the age of 10).

3.4. Research methods and analysis

Descriptive analysis: We will conduct descriptive analysis to both check on the identifying assumptions of the modelling (see section 3.7) and to explore the characteristics of those most likely to have been affected by the 2CL. For the latter purpose we will produce summary statistics of the overall sample, the Group A sample, and the Group B sample. We will also estimate a logistic regression model with the 2CL indicator as the dependent variable and gender, ethnicity, EAL status, school %FSM and IDACI score as explanatory variables, to explore the profile of the 2CL affected group (sample restricted to third or higher order sibling being born after April the 6th 2017).

Difference in difference: This approach will exploit the sharp ‘before and after’ of the implementation of the policy for households with a third or higher order child born before or after the 6th of April 2017. For this approach we will compare the before and after change in outcomes for households with a third or higher order child born in the 2016/17 academic year with a comparator group: households with a second born child born in the 2016/17 academic year.

The sample used will therefore be restricted to households that fit into the above categories. The basic modelling approach will be linear probability models of the form:

$$y_{it} = \alpha_0 + \beta_1(threeplus)_i + \beta_2(post)_t + \gamma(threeplus * post)_{it} + \epsilon_{it} \quad (1)$$

Where y_{it} refers to outcome y for pupil i born in period t ; *threeplus* is a binary variable of whether the pupil is in a household where the third or higher order child born in the 2016/17 academic year (coded to zero for the second born child comparator); *post* is a binary variable of whether the index pupil’s date of birth is on or after the 6th of April 2017; α, β and γ are parameters to be estimated, with γ being the difference in difference estimate of the impact of the 2CL on outcomes.

The models will be estimated both for all families with second and third or higher order born children as well as for a sample with a further restriction: those households with a child eligible for free school meals as recorded by the January 2017 schools census (i.e. the two samples that correspond to the Group A and Group B treatment groups outlined in section 1.5). Though not necessary for identification, we will include a limited selection of control variables: gender, ethnicity, EAL status, birth order, school fixed effects and cohort fixed effects (for siblings). The control variables are necessarily limited: commonly used NPD covariates, such as FSM eligibility, care experience, SEND status, and any variables related to

the residence of a child, are potentially impacted by the 2CL and are therefore inappropriate as controls. Model estimates will be presented with and without control variables.

Extensions to this model will include:

- i) An event study specification of the difference in difference model, that is, estimating separate difference in difference estimates for each month of birth. This specification will serve as a basis to assess the credibility of the parallel trends assumption and changes in sample composition around the cutoff date.
- ii) Extending the estimation window beyond those households with children born in the academic year 2016/2017. Particularly attention will be paid however to the possibility of diverging long term trends with the comparator group and compositional change amongst households with third or higher born children.
- iii) An alternative comparator group, replacing the households with second born children with households with third or higher born children born in the academic year 2015/2016, and modifying the definition of *post* in the above model to refer to those born between the 6th of April and the 31st of August in either academic year 2015/2016 or 2016/17.

Regression Discontinuity Design

The 2CL policy applies to families with third or higher order children born on or after the 6th of April 2017. This provides a sharp and natural cut off point to implement RDD methods. We will estimate regression discontinuity models with the birth date of the third or higher order child as the running variable and test for whether there is a discontinuity in outcomes at the cut off. The sample will be restricted to households with third or higher children born in the academic year 2016/2017. The models will be of the form:

$$y_{it} = \alpha_0 + \beta_1(DOB)_i + \beta_2(DOB * post)_{it} + \gamma(post)_t + \epsilon_{it} \quad (2)$$

Definitions as per model (1), *DOB* is the pupils date of birth, with the 6th of April 2017 = 0 (and 5th of April 2017 = -1; 7th of April 2017 =1, etc). We will follow current best practice in RDD estimation by using local linear regressions either side of the cutoff to generate the estimate γ . Estimation will be restricted to a window of data around the cutoff, the bandwidth, selected using a data-driven approach that minimizes the coverage error rate.

The regressions will be weighted using a triangular weighting function that places higher weights on observations closest to the cutoff. To ensure valid inference, we will use robust bias-corrected standard errors as per Calonico, Cattaneo & Farrell (2021). The modelling will be implemented using the *rdrobust* package in Stata/R.

As with the difference-in-difference models, the models will be estimated separately for the group A and group B samples. Control variables will be similarly limited as described above.

For all models we will report the estimate of γ alongside the corresponding robust standard error clustered at the school level corresponding to the age of the child (e.g. if a child is in secondary school, at the level of the secondary school). We will correct for the fact that we are testing two primary outcomes using the Benjamini–Hochberg procedure. We will not correct for multiple testing for the other outcomes studied.

3.5. Subgroup analysis

We will investigate RQ3 by re-estimating each model by restricting the sample according to each category contained with the variables listed as ‘Subgroup’ in Table 3.3.

3.6. Missing data

Missing pupil data might arise for instance if a pupil is educated outside of state schooling in England for a period of time covered by the analysis, or if a pupil is absent for an attainment test. We will generate a missingness indicator for each outcome variable. We will estimate our models with this missingness indicator as an outcome, to test whether missingness might drive bias in our estimates. If we find a significant treatment effect in this modelling, we will investigate further to assess whether the missingness is associated with a systematic change in the sample around the policy cutoff.

3.7. Additional analysis and robustness checks

The key identification assumption in both the difference in difference and the RDD approaches is that there is no sorting of cases around the policy implementation date that varies with variables that are related to the outcomes of interest. The main threat to this assumption and therefore the validity of the research is that the composition of the sample changes around the cutoff. This may be because parents brought forward conception of third or higher order births in order to avoid the cut in benefits or were deterred from the birth of a third or higher order child altogether. Some assurance of support for the validity of this

assumption is provided by existing research: no evidence of a discontinuity in the birth rate as a result of the 2CL (Reader et al., 2025) and no evidence of any effect on abortions (Grollman et al, 2025), and no evidence of discontinuities in key NPD variables (Cattan et al, 2025).

Difference in difference

- i) Validity checks: We will implement standard tests for parallel trends in the pre-treatment period and anticipatory effects. We will also test identical difference in difference models to the ones estimated but using baseline covariates as outcomes, this will test whether the characteristics of the sample change after the policy implementation date.

RDD

- i) Validity checks (RQ0): We will apply graphical and statistical tests of the continuity of the density of cases, using the test outlined in Cattaneo et al. (2018). The test of the continuity of baseline covariates will be conducted by estimating RDD models with control variables as outcomes. We will also estimate the RDD models with the sample of households with a second child born in the academic year 2016/17 to check whether any estimated effect is not driven by something other than the 2CL that may have affected other families.
- ii) Robustness checks: We will test for whether our conclusions change when: i) the bandwidth is varied (by estimating the RDD models using 0.5, 1.5, and 2 times the optimal bandwidth); ii) we use a uniform weighting kernel; iii) we specify a 2nd order polynomial for the local linear regression; iv) when we exclude baseline covariates.

3.8. Race equity

It is well known that certain racial groups may be overrepresented in recorded crime data for reasons that may not accurately reflect differential levels of offending. While this does not necessarily affect the robustness of the analysis presented here, we will ensure that this fact is highlighted when presenting statistics of differential crime rates between ethnic groups. As suggested in YEF (2025), care will be taken to analyse the results according to the ethnic group using the most appropriate ethnicity variables available. This implies using the detailed rather than the broad (5 category) ethnicity codes in the NPD, and using the ethnicity coded in the NPD rather than the PNC. This is because the PNC codes are derived from Police observation and as a result contain a substantial number of cases with 'unknown' ethnicity and likely error.

4. Project management

4.1. Risks and mitigations

Table 4.1 Risks and mitigations

Number	Risk	Likelihood (Low/Medium/ High)	Mitigation
1.	Fail to gain sign-off on data access from data controller	Low	We have engaged early with data owners and have received informal support for our proposal; DfE is supporting our work to generate new sibling identifiers as an enhancement to the NPD
2.	Failure of identifying assumptions (particularly sorting around the policy date)	Medium	Existing research indicates that this is unlikely. However, should this risk be realised when the data is analysed, we will investigate sample restrictions that will ensure assumptions are met and investigate applying recent methodological advances in RDD that address this type of issue.
3.	Sparse offending data given age of pupils in dataset	Medium	We have confirmed with MoJ that the MoJ-DfE data share will be updated to include offences up to April 2025. We estimate that the modal eldest siblings in our analysis will have offending data up to age 13 – the point at which the age-crime curve is rising steeply.

4.2. Timeline

Using the table template below, please include a project timeline with the main activities, dates and who's in the project team. Where possible include specific dates or date intervals.

Table 4.2 Timeline

Date	Activity	Staff responsible/leading
June 2025-October 2025	- Submit applications for the NPD data. - Write and publish research protocol	Will Cook
October 2025 – March 2026:	- Develop sibling/household IDs - Clean data for analysis - Transfer sibling/household IDs into NPD-PNC - Analysis coding on simulated data/Initial analysis - Interim report publication	Will Cook/ FFT Education Datalab
March 2026 – November 2026:	- Deliver proposed analysis & robustness checks	Will Cook
October-November 2026	- Final report production - Dissemination activities	Will Cook

The project team comprises:

- The project lead, Will Cook;
- Quality assurance, Steve Morris
- A Post Doctoral Research Associate
- Data production, FFT Education Datalab

The Post Doctoral Research Associate (PDRA) will be recruited for the project. The advertised job description will specify skills in the analysis of large datasets and the application of econometric methods. The PDRA will perform the majority of the analysis under the direction of the project lead. They will document the analysis code and will contribute to writing up the project outputs.

FFT Education Datalab are recognised expert analysts of education data, particularly the NPD, and work closely with the DfE on a routine basis. The Chief statistician at FFT Education Datalab, Dave Thomson, will lead on their work for this project. Dave developed the sibling indicators that currently exist in the NPD.

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