

The Impact of Youth Clubs Closures on Youth Offending in England

Secondary Data Project Analysis Plan

Evaluating institution: Institute for Fiscal Studies

Principal investigator: Carmen Villa



Analysis Plan for YEF Secondary Data Analysis Projects

Project summary

Project title ¹	The Impact of Youth Club Closures on Youth Offending in England
Research Team	Institute for Fiscal Studies
Principal investigator	Carmen Villa
Analysis plan author(s)	Carmen Villa, Ilina Logani
Overarching research question ²	What impact do youth clubs have on children and young people's involvement in crime in different parts of England?
Supporting research question(s) ³	Do the effects of youth clubs differ by crime, individual or area attributes? How has the provision of youth clubs changed across England in the last decade? What are the determinants of youth club closures or openings across England?
Dataset(s) to be used	Own database created from FOI data, MOJ-DfE database, Police UK, Understanding Society, CSEW
Population characteristics	Cohorts born between 1992 and 2009 (aged between 10 and 18 years old in the years the data spans)
Years data spans	2010/11 to 2019/20
Geographic coverage	England

¹ Please make sure the title matches what's in the header.

² In simple terms written for a none-expert, what's the main thing this research projects sets out to answer?

³ What are the supporting research questions that will be tested in support of addressing the primary research question. This should not exceed three on the cover sheet and more detail can be added below if there are further questions to be addressed.

Primary outcome(s) investigated (if relevant)	Offending as measured in MOJ records
Main method(s) to be used or tested	Difference-in-differences

Analysis plan history

Version	Date	Reason for revision
1.0 [<i>original</i>]	04/11/2025	

Table of contents⁴

1. About the project.....	5
1.1. Background to the project.....	5
1.2. Research aims.....	7
1.3. Research question(s).....	7
1.4. Hypotheses.....	11
1.5. Key concepts.....	11
Policy.....	12
Exposure to youth clubs.....	12
Intervention	12
Factors influencing effects	12
Bias.....	13
Control Conditions.....	13
Outcomes.....	13
2. About the datasets.....	165
2.1. Overview of datasets used	15
2.2. Secondary data source(s).....	16
2.3. Primary data collection.....	20
2.4. Linking datasets.....	21
2.5. Access and data protection.....	21
2.6. Ethics.....	21
3. Methods.....	22
3.1. Research design.....	22
3.2. Participants.....	24
3.3. Variables and measurement.....	24
Outcome measures	26
Exposure measures	27
3.4. Sample size	27

⁴ To update, right click table > select 'Update Field' > select 'Update entire table'.

3.5.	<i>Research methods and analysis</i>	27
3.6.	<i>Missing data</i>	29
3.7.	<i>Additional analysis and robustness checks.....</i>	30
3.8.	<i>Other potential limitations or sources of bias.....</i>	31
4.	Project management	34
4.1.	<i>Risks and mitigations.....</i>	34
4.2.	<i>Timeline</i>	35
5.	References.....	35

1. About the project

1.1. Background to the project

This project is set to explore the **causal effects of youth clubs on crime**. Youth clubs, also known as youth centres, are community-based programmes that provide safe spaces for children and teenagers to meet, socialise, and participate in a variety of extracurricular activities, ranging from sports and games to workshops, arts, and mentoring. These programmes are offered after school hours (and, often, during weekends and school holidays), are overseen by youth workers, and are usually heavily subsidized or free of charge for young people in underprivileged neighbourhoods. While the target audience for youth clubs are usually individuals aged 10 to 18, some youth clubs also provide services for even younger children, while others allow individuals up to age 25 to attend if they have special educational needs or disabilities. Some clubs provide dedicated sessions or affinity spaces for children and teenagers with specific identities, such as for girls or for LGBTQIA+ users; most clubs have open spaces to socialise and areas with pool or ping-pong tables and board games.

As such, youth clubs are positioned to provide a supervised, supportive, and affordable environment after school for children and teenagers to develop personally, build life skills and social connections, access key mentorship through relationships with youth workers on site, and connect with their local community. These aspects might be particularly beneficial for more vulnerable children and teenagers, who according to survey data are more likely to attend. On the other hand, peer interactions at the youth club might also be linked to negative peer effects (e.g. pressure to commit crimes, learning criminal abilities, etc.). For instance, research on peer interactions in schools has shown that some crimes can increase when young people interact (Jacob and Lefgren, 2003).

Previous causal studies in localised settings and assessing small samples have showed that after-school clubs delivered in schools to selected groups can be beneficial for young people (Gottfredson et al. 2004, Dinarte-Diaz & Egana del Sol 2024). Other studies, focusing on disadvantaged youth, show that summer jobs can reduce reoffending (Heller 2014, Modestino 2019, Davis & Heller 2020) and improve educational outcomes (Modestino & Paulsen 2023), while programs targeting socio-emotional skills can lower arrest rates (Heller et al. 2017). The only causal study focusing on youth clubs specifically is Villa (2024), which looks at London. The author studies the closure of youth clubs in 2010 to 2019 due to austerity cuts in London, using difference-in-differences models comparing youth affected by closures (residing in areas where all youth clubs close) to those unaffected (residing in areas where all youth clubs remained open). The paper showed that closures increased youth offending by around 13%, and that the increases were driven by acquisitive crimes, drugs and violence. The datasets used in Villa (2024) were administrative records from the Metropolitan Police, local crime data from Police UK, and data from Understanding Society.

This paper answers a call for a proposal which **explicitly asked to expand Villa (2024)** to other parts of England (Youth Endowment Fund, 2025). This question is relevant given that the closures were not unique to London and due to the important socioeconomic differences between London and the rest of the country. Do the pervasive results of closures generalise to rural areas or smaller cities? Do the effects of youth club closures depend on local attributes? These are important policy questions as national and local governments continue to invest in youth crime prevention strategies and in planning both openings and closures of youth clubs.

This paper will be the first to provide credible causal estimates on the effects of community-based after-school clubs on crime across (many areas of) England. Our main outcome of interest is **children's offending** (where children are individuals aged 10 to 18 years old). We will use **official records** from the Ministry of Justice to measure our first outcome of interest. As secondary outcomes, we will assess effects on **local crimes** as recorded by Police UK and, if statistical power is sufficiently high, we will explore **self-reported** victimisation in the Crime Survey of England and Wales. As part of this project, we will also assess changes in after-school activity participation using Understanding Society.

Understanding the causal effects of youth clubs is complex and complicated by analytical issues. First, there is very limited data on youth clubs or youth club usage. Second, attendance at youth clubs correlates with socioeconomic attributes which might also affect offending, as shown in survey data; moreover, youth clubs tend to locate in lower-income neighbourhoods, which are characterised by higher crime. These correlations imply that regressions of offending variables on attendance at youth clubs might suffer from omitted variable bias and/or reverse causality. It is therefore difficult to disentangle cause from correlation.

Like Villa (2024), we will exploit the quasi-experimental variation in youth club availability in the U.K. between 2010 and 2019 to estimate the causal effects of youth clubs on crime. In 2010, the U.K. government announced substantial welfare cuts as part of its 2010 Spending Review. These cuts impacted a broad spectrum of public policy, significantly reducing funds available to local authorities to spread among their various needs. Youth services were among the most hit areas of social policy, due in part to relaxed laws regarding the statutory duty of local authorities and in part to the abolishment of several grants specifically intended for these services. Because of these austerity reforms, spending on youth services fell by 72% across the U.K. between 2010 and 2019, from an initial £1.4 billion, leading to widespread closures of youth clubs across England and Wales. Youth club closures were staggered, with decisions on which youth clubs to close taken by local authorities. The closure of youth clubs in the U.K. between 2010 and 2019 thus provides a natural experiment, acting as a shock to youth club usage.

1.2. Research Aim

The key objective of this project is to understand **whether youth clubs causally affect crime, with a focus on youth offending**. There is evidence in support of this hypothesis for London, but not for the rest of England. This research project aims to bridge that gap.

1.3. Research question(s)

The main research question is the following:

1. What causal impact do youth clubs have on children and young people's involvement in crime in different parts of England ?

Our paper will use the (arguably) exogenous closure of youth clubs as a shock to youth club availability, which allows to assess effects of these closures on affected youth while leaving other offending determinants unaffected.

A key contribution of this project will be the **creation of a database on youth club availability** across England in 2010 to 2019 using Freedom of Information (FOI) data. At the time of writing, the FOI requests have been made but not processed. Building the data requires manually revising each FOI response to homogenise the data across the country. Our expectation is that **not all local authorities (LAs) will have the necessary information to construct the treatment variable**. Our best estimate is that the data will be of sufficient quality (including the postcode of each youth club, whether it was open in 2010, and whether it closed between 2010 to 2019) for $\frac{1}{2}$ to $\frac{2}{3}$ of LAs. We expect that most LAs will be able to provide years of closure but not more precise dates. We cannot ex-ante understand whether the LAs which are able to provide data are representative of the rest of the country. To understand whether they might be similar to areas which are not included in the sample we propose to describe socioeconomic attributes in an appendix and interpret plausible differences and similarities.

Our identification strategy is a **difference-in-differences research design** which will compare children residing in areas affected by youth club closures to children residing in areas near open youth clubs. Notice that this strategy omits areas which are far from youth clubs, hence avoiding some potential issues on selection (as areas without youth clubs tend to be richer). We will follow Villa (2024), who defined treated areas as those where all nearby youth clubs closed and control as those where all nearby youth clubs remained open. This implies that youth residing in areas where we do not have information on whether there are youth clubs will be omitted from the analysis. In that context, nearby was determined as 40 minutes on foot and computed using a routing API. These definitions are discussed in Table 1.5 and Table 3.4. In London the areas included in the sample based on the radius restriction of 40 minutes

represented 44% of all LSOAs. We will assess whether youth club closures are correlated with austerity in other dimensions, namely the loss in individual welfare transfers, as measured by Beatty and Fothergill (2016), and changes in spending in schools or family services, as measured in public data from the Department for Education.

Our primary outcome is an **individual's probability of offending** (appearing as guilty in MOJ-DfE records) in a given year. We believe this is the most accurate and complete measure of detected youth offending across England. A limitation of MOJ-DfE is that low level offences which are recorded or detected by police, but which do not end up in court, are not included in the data.

We believe the minimum detectable effect size (MDE) on **individual's probability of offending** using MOJ-DfE data will be slightly above 7%. This MDE is based on our estimate of having at least 1 million observations in sample (as described in Table 2.2.2) and assuming baseline offending probabilities of 1 per 1,000, significance levels of 0.05, power of 80% and 30% of observations being treated in simple two-samples mean power calculations.⁵

We will assess whether the parallel trends assumptions holds and the robustness of the estimates to changes in sample, controls, and estimation. We will also evaluate heterogeneity in effects by individual level and area attributes (see question 3 below). Our preferred specification will depend on the outcome of these robustness checks.

As secondary outcomes, we will assess whether youth club closures affected **local crime rates** as measured in Police UK data. These data focus on where crimes occur rather than whether individuals residing near youth clubs commit crimes. Analyses of these data will be done at the lower super output area (LSOA) and year level, which is the lowest level publicly available. Villa (2024) did not detect changes in local crime rates in the London study.

If statistical power is sufficiently high, we will also leverage **victimisation** data from the Crime Survey for England and Wales (CSEW). At the time of writing we cannot assert whether this will be possible. Villa (2024) did not include CSEW in her work, as the sample in London is very small (around 1,500 non-missing answers for many questions, where victimisation is a low-probability event). Therefore, we will only be able to understand whether these analyses are

⁵ Actual MDE might be slightly higher as our estimates will take into account potential autocorrelation in standard errors at the LSOA or MSOA levels. This reduces statistical power and therefore increases the MDE. Without the data we would have to make assumptions on the level of intra-cluster correlation to provide precise estimates.

possible once we finish the data collection efforts for youth clubs across England and we understand how many LAs were compliant.

Supplementary regressions analysing changes in **participation in after-school activities** will also be provided, using Understanding Society. Villa (2024) was able to detect effects for London; hence, we hope that we will be able to detect effects as we enlarge the sample too.

2. How do the effects differ by crime type?

For our **main outcome**, youth offending as measured using MOJ-DfE data, we will assess heterogeneity by crime type using the Home Office categorisation. For the more common crimes, we will define variables which measure **the probability of offending in a given category in a given year**.

Given simple two-mean calculations assuming $\alpha=0.05$, power of 0.80 and a sample of 1 million in MOJ-DfE where 30% are treated and the rest are control, the estimated MDEs are 0.07 percentage points in the probability of yearly offending for Violence Against the person (baseline mean 0.03), acquisitive crimes (baseline mean 0.07) and drug offences (baseline mean 0.04).

For our **main outcome**, youth offending (in any category) as measured using MOJ-DfE data, we will perform **heterogeneity analyses by individual and local area attributes**. Given the existing evidence suggesting that vulnerable children are more likely to attend youth clubs, we expect indicators of lower socioeconomic status to mediate effects of youth clubs.

At the individual level, we will explore effects by eligibility to **free school meals** (FSM) (which Villa 2024 shows is a key determining factor of detrimental effects of youth club closures on educational attainment), speaking **English as an Additional Language** (EAL, which includes migrant children), and **Ethnicity**. These are binary indicators (e.g. a pupil is either FSM or not).

At the local area level, we will assess differences in effects by **rural/urban status** (a binary indicator particularly relevant given our interest in youth clubs beyond London) and average **local deprivation**, as measured by the ONS Census in 2011 (before most youth clubs closed). For this last (continuous variable – the proportion of population with one or more levels of deprivation) our analysis will compare effects in areas below or above median attributes.

3. How has the provision of youth clubs changed across England in the last decade?

This question will be answered in our interim analysis, upon presenting the FOI data collection efforts. For as many LAs as possible, we will **describe the evolution** of youth clubs in the decade.

4. What are the correlates of youth club closures or openings across England?

This question will be answered in our interim analysis, where we will compare areas where youth clubs closed to areas where youth clubs remained open in terms of observable socioeconomic attributes from the ONS census in 2011. This will allow us to understand whether there is **selection into treatment** (e.g. some areas being prioritised by local authorities based on their attributes) and **posit how that might bias any results**. We propose to assess the correlation between closure and local deprivation, local unemployment, proportion of Black, Asian, or Minority Ethnic population, and proportion of population renting from the council, which are key metrics of local economic status (jointly).

5. Which areas of England lack information to answer the above-mentioned questions and should strengthen their data collection and measurement around youth provision?

This question will be answered in our interim analysis, upon presenting the FOI data collection efforts. For each LA in England, we will **state whether data was collected**, whether data is questionable, or whether data did not exist.

Table 1.2. How will the questions be addressed at each stage?

Question Number ⁶	Interim report	Final report
1	We will present preliminary results using MOJ-DfE and Police UK. We will present an assessment of statistical power regarding CSEW. For our main analysis, we will assess the parallel trends assumption.	We will present the final estimates and Understanding Society. For our main analysis, we will complete robustness checks relating to sample, controls, and estimation.
2	We will present preliminary results on the heterogeneous effects by crime type in MOJ-DfE.	If the robustness checks to question 1 highlight high sensitivity to some changes (sample, controls or estimation), we will perform more conservative robustness checks on the results on heterogeneity by crime.
3	We will present preliminary results on the heterogeneous effects by	If the robustness checks to question 1 highlight high sensitivity to some

⁶Question numbers should follow the ordering in the section above.

	individual attributes and local area attributes in MOJ-DfE.	changes (sample, controls or estimation), we will perform more conservative robustness checks on the results on heterogeneity by crime.
4	Full analysis completed.	
5	Full analysis completed.	
6	Full analysis completed.	

1.4. Hypotheses

We hypothesise that **youth club closures increased children’s offending across England** (on average). This hypothesis is based on the analysis in Villa (2024), who found that closures in London led to youth offending rises. The likely mechanism described in Villa (2024) is that youth clubs provide safe spaces and structured activities that help reduce time spent in risky environments or peer groups, and that they are not easily substitutable. In the absence of other studies, this is our best guess.

We hypothesise that youth club closures affected **violent crimes, drug offences and acquisitive crimes**. This is again based on Villa (2024), the only paper that informs our priors.

We hypothesise that **local crimes—measured by crimes committed in the area near the youth club—did not increase**, as based on Villa (2024), although in this instance we are unsure whether Villa (2024) will hold. The explanations in Villa (2024) were that youth club closures did not affect crime attractors or deterrents in the areas where youth clubs were; rather, the “new” youth offenders were committing crimes in the same areas where crimes were committed before, which are orthogonal to treatment definitions. More explicitly, the proportions of child offenders and rates of child offending increased in Villa (2024), but the locations in which this crime occurred did not change in the case of London. **This might differ outside of London** which may have very different crime opportunities than smaller cities or rural areas.

We hypothesise that attendance at after-school activities as measured in Understanding Society declined, as in Villa (2024).

We hypothesise that **effects on youth offending might be larger for pupils from lower socioeconomic backgrounds**. In that light, effects on FSM, EAL and minority pupils might be larger.

We hypothesise that **effects on youth offending might differ in rural/urban areas and in areas with different economic opportunities**. We do not have an expectation of whether youth clubs should matter more in rural or urban areas. The channels could go either way (more alternatives in cities might mitigate effects of youth clubs if they are substitutable for other leisure; but more inequality in cities also means higher crime opportunities). We hypothesise that **effects might be larger in areas with higher deprivation indicators**.

1.5. Key concepts

Policy

The policy we assess is the **availability of youth clubs for young people in their local area**.

Exposure to youth clubs

We define **youth exposed** to youth clubs as those living **nearby** youth clubs. It is impossible to assess whether a specific person attended youth clubs, or a specific youth club. Hence, we rely on the **assumption that youth living nearby are more likely to attend**, which is supported by the knowledge of youth workers.

Villa (2024) defines nearby as within 40 minutes on foot, measured from the centroid of the LSOA of residence of the youth to the youth club using a routing API. We will follow the same definition and also assess sensitivity of the effects to changes in the 40-minute radius (20, 30 or 50 minutes will be assessed). Any noise in the definition of exposure attenuates our estimates (bias toward zero).

Given the exposure definition our estimates will measure an Intention to Treat Effect. This captures direct effects of youth clubs (on attending youth), and indirect effect (e.g. on non-attending peers). It is impossible to distinguish direct from indirect effects with this data and in this context.

Intervention

The **intervention** we measure is the closure of youth clubs in an area between 2010 and 2019. Following Villa (2024), we will use a binary treatment definition and define **treated areas as those where all youth clubs closed within 40 minutes on foot**. The comparison group will be areas where all nearby youth clubs (40 minutes on foot) remained open over the same period. Using areas near youth clubs rather than areas without youth clubs allows to capture the fact that youth clubs tend to be in more deprived areas.

The year of treatment will be the year of the first youth club closure in the area.⁷ The year of closure will be that provided to us by local authorities and should compose the specific year of closure, not the year quality was reduced, for instance opening fewer hours or offering fewer activities. Noise in the year definition likely also biases our estimates toward zero.

We use youth club closures as a natural experiment, treating them as a shock to youth club usage.

Factors influencing effects

We will assess heterogeneity in effects by youth attributes and by local area attributes. Individual level characteristics tested will be: **ethnicity**, eligibility for **free school meals**, and speaking English as an Additional Language. Local area attributes relate to characteristics of the LSOA as measured in the 2011 Census: rural/urban status and local deprivation. If additional variables are made available by DfE (e.g. SEN status, and CLA) we can also assess heterogeneity across these dimensions.

Bias

There could be variation in how youth clubs were delivered across areas — in size, quality, and the exact services offered. However, those are not possible to measure. Hence, this project focuses on **closures vs. continued provision**, rather than trying to measure variation in quality.

Any selection into treatment based on unobservable characteristics (like youth club quality) could bias the estimates. If closing clubs were similar, estimates in this paper would be unbiased. If closing clubs were of lower quality, the effects would be a lower-bound. If, improbably, closing clubs were higher quality – e.g., if councils had closed the best clubs – the effects would be overstated. These limitations will be clearly stated in the final report.

Control conditions

The control group is areas where **youth club provision remained stable regarding** the number or **quantity** of youth clubs provided. It is possible that these areas were also affected in so far as quality concerns (which is unobserved). Any unobserved change to control areas in terms of the quality of provision would bias our estimates. Based on existing knowledge, the control group was also negatively affected by austerity. Interviews with youth workers suggest that

⁷ We argue that the parallel trends assumption is more difficult to justify when there have already been closures in an area. We believe the approach proposed (where the earliest closure in sample starts the treatment period) makes the Intention To Treat assumptions more plausible.

most youth clubs would have seen funding decreased. This means our estimates would be biased toward zero (attenuated).

Outcomes

Our key outcome is the probability of a children offending in a given calendar year. The data will be obtained from MOJ-DfE records and hence composes detected crimes which ended in a trial in court. **This is the most accurate measure of detected offending.** This will be a binary indicator (=1 if individual found guilty of a crime in a calendar year and =0 otherwise). Given improvements in recording in the last decades and computerisation, we believe this is very high-quality data and that measurement error (for instance, due to human error) is minimal. **Any crimes which were not recorded by the police, which were recorded but did not end up in court, or where the offender was not known will not be captured.** We will assess effects on both cautions and convictions.

Our secondary outcome is local crime rate, where local refers to the place where the crime was committed. This is the sum of crimes detected by the police in an LSOA over total population and is available in the raw data from Police UK.

If the sample in CSEW is sufficiently high, we will assess self-reported victimisation. There is a specific question which asks whether the individual in the sample has been a victim of a crime in a given survey year. We will code a binary indicator (=1 if has been a victim, and 0 if they answered the question stating they had not been a victim).

In addition, we will also assess the effects on the probability of attending structured after-school activities using Understanding Society, which serves as a proxy for a first-stage effects of closures on time use and youth club usage. These activities include, but are not restricted to, youth club attendance, and can include attendance at after-school private sports clubs, after-school paid tutoring, and organisations such as the Scouts or Guides.

Table 1.5 Definitions of key concepts

Terms	Definition used
Youth club	Open-access youth club, universal provision of after-school activities to young people (usually aged 10 to 18 years old). This includes youth centres (in a dedicated bespoke space) and youth clubs (which

		might take place in community-based spaces where other activities occur). Constructed from FOI data.
Nearby		Within 40 minutes on foot from a person's home address.
Treated individuals		Individuals residing in areas where all nearby youth clubs closed between 2010 and 2019, as in Villa (2024).
Control individuals		Individuals residing in areas where all nearby youth clubs remained open between 2010 and 2019, as in Villa (2024).
Areas		Lower Super Output areas, 2011 boundaries as defined by Ordnance Survey. https://geoportal.statistics.gov.uk/datasets/ons::lower-layer-super-output-areas-december-2011-boundaries-ew-bfc-v3/about
Incidence of offending	of	Binary indicator of an individual being deemed guilty in a given year as per MOJ records. https://www.gov.uk/guidance/ministry-of-justice-data-first
Incidence of offending (convictions)	of	Binary indicator of whether an individual receives a conviction in a given year as per MOJ records.. https://www.gov.uk/guidance/ministry-of-justice-data-first
Incidence of offending (cautions)	of	Binary indicator of whether an individual receives a caution in a given year as per MOJ records. https://www.gov.uk/guidance/ministry-of-justice-data-first
Violent crimes		Violence against the person offences, as defined by the Home Office. https://www.met.police.uk/police-forces/metropolitan-police/areas/stats-and-data/stats-and-data/met/crime-type-definitions/
Drug offences		Drugs, as defined by the home office. https://www.met.police.uk/police-forces/metropolitan-police/areas/stats-and-data/stats-and-data/met/crime-type-definitions/
Acquisitive crimes		The sum of Thefts, Burglary, Robbery and Shoplifting as defined by the Home Office.
Local crime rate		Total crimes occurring in an LSOA over total resident population, from Police UK. Non-negative number. https://data.police.uk
Incidence of victimisation	of	=1 if individual reports having been a victim of a crime in a survey year in the Crime Survey for England , 0 if individual reports not having been a victim of a crime. Missing responses or individuals who do not want to respond excluded. https://www.crimesurvey.co.uk/en/surveyresults.html

Incidence of attendance at organised after-school activities	=1 if individual reports attending structured after-school activities in a survey year in youth questionnaire in Understanding Society, 0 if individual reports never attending. https://www.understandingsociety.ac.uk
FSM status	=1 if individual is eligible to Free School Meals in MOJ-DfE database in a given year, 0 otherwise.
EAL status	=1 if individual has English as an Additional Language in the MOJ-DfE database in a given year, 0 otherwise.
Ethnicity	Ethnic code as defined by ONS and as reported in MOJ-DfE. We will focus on broad categories: White, Black, Asian, Other. We will clarify what effect sizes we can accept or reject if these are within -20 and +20%.
Rural or urban	Determined by ONS. We will use the categorisation as per the 2011 Census at the LSOA level. https://www.ons.gov.uk/methodology/geography/geographicalproducts/ruralurbanclassifications/2011ruralurbanclassification
Proportion of deprived population	Determined by ONS. Proportion of population deprived in one or more dimensions at the LSOA level. https://www.ons.gov.uk/datasets/TS011/editions/2021/versions/6

2. About the datasets

2.1. Overview of datasets used

This project will use a combination of new and existing data sources to study the impact of youth clubs on crime.

The new data will be used to define treatment at the LSOA level. We will create a new dataset **tracking the opening and closure of youth clubs** across as many local authorities in England as possible, between 2010 and 2019. This will be merged with other sources at the LSOA level.

The main database used is MOJ-DfE, which contains individual level education data matched to court outcomes. Our key **outcome is youth offending**. The study population is people aged

between 10 and 18 in the years 2010 to 2019 and living in England. The analysis will be conducted at the individual level.⁸

As secondary analyses, we will assess effects on **local crime rates** using Police UK data, publicly available at the LSOA-year level. If statistical power allows, we will also assess self-reported **victimisation** using CSEW.⁹ We will also present estimates of the effects of youth club closures on **attendance at structured after-school activities** using Understanding Society.

2.2. Secondary data source(s)

Table 2.2.1 Dataset Description – FOI database of youth clubs

Name of dataset	Database of youth club availability
Data owner(s)	Carmen Villa
Type of data	Longitudinal panel at the LSOA level
Availability of data	Constructed from publicly available FOI data
Team member(s) who will have access	Ilina Logani, Carmen Villa
Population/geographic coverage or sampling frame	We have requested records from all local authorities in England
Years covered or survey waves	2009/10 to 2019/20
Exclusion criteria	We will remove any areas for which we cannot be certain of the number of youth clubs (for which we cannot collect data using FOI information provided).
Expected population/sample size (following exclusion criteria)¹⁰	We expect that the data will cover between 1/2 and 2/3 of local authorities (and hence, LSOAs) in England

⁸ This implies that there is heterogeneity by treatment exposure (with some people being affected for longer). Our analyses will control for cohort effects and present average effects across people who have been exposed for different times to the treatment.

⁹ Rather than hypothesize MDEs under assumptions which cannot be tested without having the full database, we propose to include in the paper those results where we can at least bound the effects between -20 and +20%.

¹⁰ This may not be known at this stage of the project. Please provide your best estimate or range based on your knowledge of the dataset.

Documentation	NA
---------------	----

Table 2.2.2 Dataset Description – MOJ-DfE database

Name of dataset	MOJ – DfE database
Data owner(s)	Department for Education, Ministry of Justice
Type of data	Longitudinal yearly panel at the individual level
Availability of data	Data sharing agreement with MOJ-DfE via ONS SRS is in progress
Team member(s) who will have access	Ilina Logani, Carmen Villa
Population/geographic coverage or sampling frame	Universe of pupils studying in England and born between 1992 and 2009 (aged between 10 and 18 years old in the years the data spans) residing in areas where we can define treatment.
Years covered or survey waves	2009/10 to 2019/20
Exclusion criteria	We will remove any areas for which we cannot be certain of the number of youth clubs (for which we cannot collect data using FOI information provided).
Expected population/sample size (following exclusion criteria) ¹¹	Our best estimate comes from Villa's (2024) figure on total 15/16-year-olds in the NPD in 2010-2019 after considering areas near open or closed youth clubs – a total of 350,000 unique pupils. Hence, we expect that the England sample for a larger age group (10 to 19 between 2010 and 2019) corresponds to at least 2 million individuals (assuming about 15% of children reside in London). Beyond the radius restriction (which is incorporated above) we need to consider that we will only be able to gather data for ½ to 2/3 of areas. Hence,

¹¹ This may not be known at this stage of the project. Please provide your best estimate or range based on your knowledge of the dataset.

	the sample of unique individuals in the regressions might oscillate between 1 million and 1.3 million.
Documentation	https://www.gov.uk/guidance/ministry-of-justice-data-first

Table 2.2.3 Dataset Description – Police UK

Name of dataset	Police UK
Data owner(s)	Home Office
Type of data	Longitudinal yearly panel at the LSOA level
Availability of data	Publicly available
Team member(s) who will have access	Ilina Logani, Carmen Villa
Population/geographic coverage or sampling frame	Universe of areas in England for which we can define treatment
Years covered or survey waves	2009/10 to 2019/20
Exclusion criteria	NA
Expected population/sample size (following exclusion criteria) ¹²	Between ½ and 2/3 of LSOAs in England
Documentation	https://data.police.uk

Table 2.2.4 Dataset Description – CSEW

Name of dataset	Crime Survey for England and Wales
Data owner(s)	Office for National Statistics

¹² This may not be known at this stage of the project. Please provide your best estimate or range based on your knowledge of the dataset.

Type of data	Cross-sectional individual level data
Availability of data	Special license
Team member(s) who will have access	Ilina Logani, Carmen Villa
Population/geographic coverage or sampling frame	Residents in sample living in areas of England for which we can define treatment
Years covered or survey waves	2009/10 to 2019/20
Exclusion criteria	NA
Expected population/sample size (following exclusion criteria) ¹³	The number of respondent-year observations in the youth questionnaire for England on questions related to youth crime is about 15,000. However, we would have to select only the subset for which it is possible to define treatment (between ½ and 2/3 of all areas), and the subset which live near areas with youth clubs (whether all open, or all close) – which represents 40% of areas. Hence, the final sample might oscillate between 3,300 and 4,400.
Documentation	https://www.crimesurvey.co.uk/en/surveyresults.html

Table 2.2.4 Dataset Description – Understanding Society

Name of dataset	Understanding Society
Data owner(s)	UK Data Service
Type of data	Longitudinal individual level data
Availability of data	Special license

¹³ This may not be known at this stage of the project. Please provide your best estimate or range based on your knowledge of the dataset.

Team member(s) who will have access	Ilina Logani, Carmen Villa
Population/geographic coverage or sampling frame	Residents in sample living in areas of England for which we can define treatment
Years covered or survey waves	2009/10 to 2019/20
Exclusion criteria	NA
Expected population/sample size (following exclusion criteria)¹⁴	The London sample in Villa (2024) for individuals in Understanding Society after considering residents near open or closed youth clubs closed comprised slightly less than 1,000 unique pupil-year observations – for the preferred sample which used 40-minute radii. Hence, we expect that the England sample for a larger age group (10 to 19 between 2010 and 2019) corresponds to about 6,500 individual-years (assuming about 15% of children reside in London). Our best estimate is that, after considering that we will not be able to gather data on youth club availability for all areas, the sample of unique individuals in the regressions will oscillate between 3,250 and 4,500 respondent-year observations.
Documentation	https://www.understandingsociety.ac.uk

2.3. Primary data collection

This project will collect primary data from local authorities via Freedom of Information (FOI) requests to define treatment. These FOI requests will relate to the provision of youth clubs and will ask about location (including postcode) and years of operation in 2010 to 2019. Our FOI requests will explicitly request dates when youth clubs opened or closed within the specified period. We expect that for most youth clubs, local authorities will provide the year of closure but not the month, hence our analysis will be done using yearly data. We will use as year of treatment the first year in which the youth club was, presumably at least for some months closed. In event study designs we will use the year before closure as the reference point.

¹⁴ This may not be known at this stage of the project. Please provide your best estimate or range based on your knowledge of the dataset.

This will be used to build a new database of youth club provision across England from 2010 to 2019. The data will cover the location and operational years of youth clubs, and will be collected at the local authority level.

This data collection does not involve individuals, so consent from data subjects is not required. All information gathered will relate to services rather than personal data.

2.4. Linking datasets

We will link our newly collected database with MOJ-DfE records, Police UK, CSEW and Understanding Society at the LSOA level. The pertinent LSOA will be the LSOA of a **pupil's residence**. Any noise in the treatment definition (e.g. if a pupil attends the youth club nearest to their school and not to their home) attenuates our estimates (bias toward zero). Proximity to a youth club from a LSOA centroid and each youth club within a 5-kilometre boundary (to decrease dimensionality) will be calculated using the HERE routing API.

2.5. Access and data protection

The data which will be constructed from FOI answers is publicly available.

The data from MOJ-DfE will be accessed through the ONS SRS. At the time of writing, we have submitted our application. Some of the data requested to MOJ-DfE include sensitive data (FSM, EAL, ethnicity). Our application argues that these variables are central to answering the research questions mentioned above and understanding the differential impacts of youth clubs in vulnerable groups. The legal basis for accessing the data is legitimate interest. The legitimate interest in question is research and statistical purposes: the conduct of non-commercial, robust social and economic research to inform research and policy. Our request has been supported by the Social Research Association, an independent agency.

Access to Police UK data is public and downloadable via the following link: <https://data.police.uk>.

Access to CSEW has been granted to the research team through the UKDS SDS under a secure license agreement.

Access to Understanding Society has been obtained through Special License Agreements with the UK Data Service.

2.6. Ethics

This project, and in particular our application to use administrative records from MOJ-DfE, which is the only withstanding application (and perhaps the one with higher risks given the size of the dataset and the variables required), has received positive assessment by the Social Research Association (SRA Ethics Appraisal Service, 2025, Case 30).

3. Methods

3.1. Research design

Table 3.1: Research design

Research design		Quasi-experimental
Datasets used		New database of youth clubs using FOIA, MOJ-DfE administrative records, Police UK, CSEW, Understanding Society
Population of interest		Individuals aged 10-18 between 2010 and 2019
Size of sample population		Between 70,000 and 700,000 individuals expected
Stratification variable		Lower-Super Output Area of pupil's residence (unit of treatment)
Primary outcome	Variable	Incidence of offending in a given year
	Measure	=1 if a individual deemed guilty in a given year (between 2010 and 2019) as per MOJ-DfE records, 0 otherwise, we will assess effects on overall crimes, cautions and convictions
Secondary outcomes	Variable	Local crime rate in a given year.
	Measure	Count of crimes in a LSOA and year between 2010 to 2019 divided by total population as per Police UK data
	Variable	Self-reported victimisation in a given year.
	Measure	=1 if individual reports having been a victim of a crime in CSEW, 0 if individual reports not having been a victim of a crime (omits missing values)
	Variable	Participation in after-school activities in a given year.
	Measure	=1 if individual reports having attended after-school activities at least once in a calendar year as per Understanding Society, 0 if individual reports never attending (omits missing values)
Main method to be used		Difference-in-difference analysis

The objective of this proposal is to understand the **causal effects of youth clubs on children's offending**.

To that end, we will leverage the **closure of youth clubs** across as many areas as we can gather data for across England . We will use difference-in-differences research designs and compare areas affected by closures to areas unaffected.

We will follow the following definition as in Villa (2024):

- Treated: areas where all youth clubs within 40 minutes on foot closed between 2010 and 2019
- Control: all youth clubs within 40 minutes on foot remained open between 2010 and 2019

We will use the year of first closure as the first treatment year and discuss any variation when areas are treated more than once. We will also discuss sample selection and potential confounders (general austerity).

Our main regression will:

- Estimate effects using two-way fixed effects controlling for LSOA of residence and year, using OLS. Additional estimators are proposed as robustness checks.
- We will cluster standard errors at the LSOA or MSOA levels (as in Villa 2024). MSOAs are slightly larger geographic units and clustering at those levels might capture spatial autocorrelation.

For the interim analysis we will:

- Describe youth club availability across England , assess the determinants of youth club closures descriptively, and report what areas will be part of the analysis.
- Present preliminary results of the effects of youth club closures on youth offending across England using MOJ-DfE.
- Present preliminary results of the effects of youth club closures on local crime rates across England using Police UK.
- Include discussions on our ability to analyse victimisation using CSEW (statistical power).

For the final analysis we will:

- Assess the identification assumptions. In particular we will assess parallel trends using F-tests over pre-treatment coefficients and portraying event studies, and evaluate the assumption of homogeneous effects over time and no dynamics to assess whether new DiD models are necessary over the baseline specification using TWFE. If the F-tests and visual examination of event studies suggest plausible violations, we will assess the sensitivity of our results to other assumptions (e.g. small violations à la Rambachan and Roth, 2023).
- Assess the robustness for our main outcome (children's offending) to changes in sample, estimation, and controls.
- Include heterogeneity analyses on children's offending by crime type from MOJ-DfE.

- Include heterogeneity analyses children's offending of MOJ-DfE by individual and local area attributes. This will be done assessing effects on subsamples by attribute.
- Include estimates of changes in attendance at after-school activities from Understanding Society.

Our robustness checks will:

- Focus on our primary outcome.
- Assess the robustness to changes in sample (e.g. areas within 20, 30 or 50 minutes on foot of closed or open youth clubs).
- Assess the robustness to inclusion of individual level controls (e.g. age and gender).
- Assess the robustness to changes in estimators, in particular assessing whether estimators which are robust to heterogeneous analyses in settings with staggered treatment introduction (e.g. using the stacked estimator).

If the parallel trends assumption is violated:

- We will assess effects on the subsample where the parallel trends assumption is most plausible. This might involve matching a subset of the treated and control areas either on observable attributes or pre-treatment trends (synthetic difference-in-differences).

If some of the robustness checks show sensitivity:

- We will discuss these results and bound our estimates (present plausible ranges).

3.2. Participants

The cohorts of interest are individuals who were in the target age of youth clubs (between 10 and 18 years old) in the 2010 to 2019 period. This means we are primarily interested in youth who were born between 1992 and 2009. We will only use data for individual for whom we can define treatment using FOI data. Local authorities which are excluded from the analysis will be listed.

Given that we cannot directly observe what youth club an individual attended, or would have attended, we rely on the assumption that individuals are more likely to attend youth clubs nearby their homes. This appears to be corroborated in conversations with youth workers. Hence, our treatment and control definitions will exclude individuals residing too far from youth clubs. Too far will initially mean further than 40 minutes on foot, following Villa (2024). We will assess sensitivity of the results to this definition. As discussed above, our treated areas will be those where all nearby youth clubs closed and control areas those where all

nearby youth clubs remained open (and where youth clubs were already open since 2010).¹⁵ This approach ensures that, in so far as possible, we identify residents of areas which are left very underprovided of youth clubs. Any noise in the treatment definition (e.g. if individuals do not care about proximity to youth clubs to attend) attenuates our estimates.

3.3. Variables and measurement

Table 3.3 Measurement of key concepts

Concept ¹⁶	How the concept will be measured and encoded	Variable source, definition and derivation or specification	Analysis required at interim stage to test assumptions
Youth club	We have asked local authorities about all the youth clubs or youth centres in their area. Youth club comprises both youth centres and youth clubs. From searching the types of institutions the local authorities shared, these seem to be open-access spaces offering activities to youth ages 10 to 18.	Freedom of Information Requests to local authorities in England .	Description of new FOI data obtained.
Nearby	Proximity to a youth club. This will be measured computing the commuting distance on foot between a youth club and each LSOA population weighted centroid as of	The location of youth clubs comes from FOI requests. We will use the postcode centroid. The population-weighted centroids are obtained from ONS.	Description of results using 40 minute radial boundary from the postcode centroid will be included in interim report. Additional results

¹⁵ Openings were a rare event in the 2010 to 2019 period. In Villa (2024), “there were only 34 new openings during the decade, of which 17 also closed before 2019”.

¹⁶ This should align directly with the names and list of concepts defined in table 1.3

	2011 (before most youth clubs closed). We will follow Villa's definition of "nearby", which is within 40 minutes on foot.		will be provided in final report.
--	---	--	-----------------------------------

Outcome measures

Primary outcome

We are interested in understanding whether youth clubs affect individual's crime engagement. Our main outcome of interest is the **yearly likelihood of being found guilty of a crime** in official records from the Ministry of Justice (MOJ).

The advantage of this source is that it is measurable for all pupils in the DfE, that recording is homogeneous across individuals in the sample, and that criminalisation in these records has real implications (either punishments or at least a criminal record). Other data with information on being a suspect but not being found guilty or self-reported engagement in crime might not have the same consequences or be measured as consistently. A disadvantage of this source is that any crimes that do not end up in court, for which there is not sufficient proof to deem the person guilty, or which are not detected do not appear in these records. This means we are probably only identifying a small part of all crimes that might be affected by youth club closures.

In the data each MOJ case appears as a single row. For our main variable we will identify whether a given individual has been found guilty of a crime occurring each year and code a binary outcome. We will assess effects overall, and by cautions vs convictions.

We plan to look at feasibility of more granular outcomes (e.g. offending specifically related to violence) during the interim phase.

Secondary outcomes

We will create secondary measures of crime engagement disentangling crimes by type using the MOJ data described above. This allows to understand whether youth clubs affect violence to different extents than other prominent crime types. A crime category is captured in variables as per the Home Office crime categorisation. We will focus on Violence Against the Person crimes, Drug Offences and Acquisitive crimes. The last category includes Theft, Shoplifting, and Robbery.

We will also measure **local crime rates**. This variable is created using Police UK data. For each LSOA and year the data inform of the crime rates. The advantage of this data is that it allows us to understand whether youth club closures affect crimes occurring around the youth club. The previously cited measures instead focus on whether individuals residing near youth clubs become more or less likely to offend. This helps understand whether all individuals in the neighbourhood are affected after closures (e.g. more likely to be victimised) and/or whether youth clubs affect crime attractors and deterrents. It also allows us to consider crime if the identity of the offender is unknown (80% of all crimes). A disadvantage of this data is that we cannot distinguish crimes committed by youth from other offences. An additional limitation is that not all crimes are reported or recorded.

We will also attempt to capture **victimisation** from the CSEW. In the data there is a question asking people whether they were victimised in a given survey year or not. We will code as 1 the people who report having victims and as 0 the people who report not having been victims. We will omit from the computation any individuals who have not answered or responding that they didn't know whether they had been victims or that they didn't want to answer. A caveat of this data is that there might be differences in self-reporting and own biases when assessing crime victimisation. An advantage is that it might capture information which is not recorded by the police (and hence not recorded later by MOJ either).

Last, we will assess effects on the probability of attending structured afterschool activities as measured in Understanding Society. We will code as 1 an individual who reports attending occasionally or more, and as 0 individuals who report never attending.

Exposure measures

We will assess effects in our main outcome variable – the likelihood of offending (in any crime category) in a given year) by **ethnicity**, eligibility for **free school meals**, and speaking English as an Additional Language. These variables are defined in the MOJ-DfE database. The first, ethnicity, refers to the Ethnic group. We will attempt to assess differences by White and Black, Asian, or Minority Ethnic. If statistical power is sufficient, we will assess any differences between Blacks, Asians and Other ethnicities. We do not expect to have statistical power to explore differences at more granular ethnic group levels (e.g. Black Caribbean). Free school meals indicates eligibility to receive free school meals. This is conditional on parents receiving certain benefits. We estimate that the bottom 25% of the income distribution is eligible. EAL indicates whether English is the primary language of a pupil or not and is a proxy for (parental) migrant status.

We will also assess differences in our main outcome by local area attributes. We will focus on whether an area is rural or urban as defined by ONS, and by local deprivation, also defined by ONS.

3.4. Sample size

We estimate that we will be able to construct the treatment variable on youth club availability from FOI data for $\frac{1}{2}$ to $\frac{2}{3}$ of England . The number is unknown at the time of writing, as we haven't processed the FOI information.

Hence, we estimate that our main sample, which uses MOJ-DfE records will have between 70,000 and 700,000 individuals within the cohorts specified and time frame in sample. The low range is the sample in London, for which we already have data. The upper range is our rough estimate of the number of individuals which might be in sample for most of England. MOJ-DfE have confirmed that they do not hold data on Wales.

We will assess effects on all individuals within the cohorts and time frame specified. No other restrictions will be imposed beyond obtaining access MOJ-DfE.

For our main outcome, which has a baseline mean of 1% (as offending each year is rare), the lower possible sample (1 million, which is the London sample in Villa (2024)) allows to detect effects larger than 7 percent at the 95%, as discussed above. These calculations use simple two-sided mean tests, assume significance level of 5% and statistical power of 80%.

3.5. Research methods and analysis

Descriptive analysis

Two research questions in our paper will be answered descriptively. Those are questions 4,, and 6.

Question 4. How has the provision of youth clubs changed across England in the last decade?

Question 5. What are the determinants of youth club closures or openings across England ?

Question 6. Which areas of England lack information to answer the above-mentioned questions and should strengthen their data collection and measurement around youth provision?

To answer question 4, we will use our newly collected FOI database on the location of youth clubs across as many areas as possible within England . We have already made the Freedom of Information requests and we estimate that we will be able to obtain the location and years of operation of youth clubs from 2010 to 2019 for (we estimate) $\frac{1}{2}$ to $\frac{2}{3}$ of all local authorities. The final number depends on the quality of information and FOIA teams within local authorities. With this information, we will plot descriptives on youth club availability by year in the sample.

To answer question 5, we will combine the above-mentioned data with information from the ONS 2011 Census that allows us to run a regression of the likelihood of a LSOA experiencing a closure or not based on local observables. We propose to estimate the regression using OLS and to use heteroskedasticity-robust standard errors.

To answer question 6, we will use our newly collected FOI database to state which areas of England lack data on the location and status of youth clubs and which areas have only questionable data. Analyses of the attributes of areas included vs excluded in the analyses might be informative on the generalisability of results.

Inferential analysis/ identification strategy

The remaining research questions – numbered 1, 2, and 3 -- will be answered through a causal lens. The identification strategy we will leverage is **difference-in-differences** whereby we compare areas where youth clubs closed to areas where youth clubs remained open. Treatment is constructed with the above-mentioned data, gathered through FOIs.

Question 1. What causal impact do youth clubs have on children and young people's involvement in crime in different parts of England?

Question 2. How do the effects differ by crime type?

Question 3. How do the effects differ by individual and area attributes?

We will assess the effects of youth clubs on crime through Equation 1, using individual-level data. P is the probability of offending for individual n residing in block i in year t . X is a vector of controls including age, gender, ethnicity and FSM status. *After x Closure* is 1 if an individual resides in an area where all nearby youth clubs closed in years after closure. We control for yearly trends which are constant across space (μ_t) and for local block (LSOA) characteristics which are constant across time (μ_i). The coefficient β captures the average treatment effect on treated areas (ATT); u is the idiosyncratic error term.

$$P_{n(i)t} = \mu_i + \mu_t + \eta X_{n(i)t} + \beta \text{After } x \text{ Closure}_{it} + u_{n(i)t} \quad (1)$$

As described above, we will follow the treatment definition in Villa (2024) and denote treated LSOAs as those where all nearby youth clubs closed. Control LSOAs will be those where all nearby youth clubs remained open. Nearby in this setting means within 40 minutes on foot, measured using APIs that take into account the urban structure (in particular we will rely on the HERE API).

Our identification strategy relies on a **parallel trends** and no-anticipation assumption. While this assumption is inherently untestable (there always exists error in estimation, and there is a debate with regard to how long before the intervention trends need to be parallel), we will include event studies to present evidence in support or against the assumption.

The population of interest will be all individuals aged 10 to 18 between 2010 and 2019 who are in the DfE-MOJ records, and who live near a recorded youth club—whether open or closed—between 2010 and 2019. We expect this to include the near-universe of pupils.

3.6. Missing data

The key limitation in our study is local authorities' ability to provide data on the youth clubs within their area. From conversations with some authorities, we are aware of constraints in data gathering ability and in FOIA staffing. Our aim is to include as many areas in England as possible – we cannot include Wales as they do not appear in the MOJ-DfE. We will compare the attributes of areas in sample and areas not in sample (because they don't have data) to understand plausible differences and similarities and how that might affect the generalisability of the estimates. We have requested FOI answers from all councils and boroughs in those nations. Data for London has already been processed and most local authorities were compliant. It is impossible to assert how many local authorities will be compliant in other regions. From conversations with the sector we expect some areas to be unable to provide information or answers. We will detail in the research output which areas have not been able to provide necessary information and clearly state the coverage. In addition, our analyses by definition use only areas within a certain proximity (40 minutes on foot) to youth clubs. This means an additional restriction on the total number of observations included in the study.

The second limitation has to do with the nature of MOJ-DfE data. Only crimes which are detected, which end up in court, and for which the offender is found guilty appear in the sample. This implies many crimes are not accounted for in the data. We expect the bias to be uncorrelated with the closure of youth clubs.

If the sample is too small, we might not be able to answer some of the above-mentioned heterogeneity analyses—namely, by crime type or by individual attribute. While we cannot assert what samples would allow us to detect different MDEs without having seen the data, we propose to report results where confidence intervals allow to provide bounded estimates of the effects ranging between -20% and 20% (otherwise, we would deem any estimates as uninformative).

3.7. Additional analysis and robustness checks

First, one of our key questions relates to assessing differences in the areas in which youth clubs closed or stayed open. This allows to understand potential selection into treatment, pertinent confounders, and potential generalisability of the results.

Should the analyses reflect clear potential confounders, we propose to adjust our identification strategy to account for these differences. For instance, in Villa (2024), youth clubs which remained open were more likely to be in more densely populated areas, which was resolved in estimation allowing differentially populated areas to differ in trends.

Second, once we have determined the appropriate baseline equation (e.g. accounting for selection if need be) we will assess the robustness of the results to:

- Sample selection
- Control conditions
- Estimators

Sample selection relates to the areas which are used in estimation. We propose to begin selecting areas within 40 minutes on foot (from either open or closed youth clubs), and to assess the robustness of the estimates to different radii.

Controls relates to assessing how the results change including or omitting individual level controls and accounting for unobserved heterogeneity at the local authority and year level. This second approach would account for changes, for instance, in yearly spending across boroughs in different services and only compare areas affected or unaffected by closures within the same borough. The baseline specification always includes area (LSOA) and year fixed effects.

Estimators relates to assessing whether TWFE is accurate at measuring effects, or whether different methods are required. Recent literature has highlighted limitations of TWFE in staggered settings, which might require assessing whether heterogeneity robust estimators, like the stacked design by Cengiz et al. (2019) yield different results. In the context of Villa (2024), TWFE was not biased, but this might differ in our study. This will be checked by comparing TWFE estimates to some of the newly proposed methods (as Cengiz, for instance).

Regarding the assumption of parallel trends, if the baseline analyses reveal potential differences in trends across treated and control areas Rambachan and Roth (2023), we will perform additional analyses on the subset of areas for which the assumption appears more justifiable, and use those as our preferred estimates. This will involve matching techniques either on observable attributes of different areas or on trends prior to the intervention (synthetic difference-in-differences). Some examples of pertinent matching attributes are proximity to youth clubs at baseline, local unemployment rates, and proportion of children residing in the area.

3.8. Other potential limitations or sources of bias

The first limitation relates to data completeness in the FOIA data. We will attempt to capture information on youth club closures across as many local authorities as possible in England . However, capturing the full universe of closures might not be feasible. This means that generalisability to areas where we do not have data can be challenged (external validity).

The second limitation relates to data completeness in MOJ-DfE records. While we believe this captures the near universe of pupils in the data, it is possible that there are structural differences in the likelihood of being deemed guilty. We need to assume that these differences are constant within a given LSOA over time. Wales does not appear in the MOJ-DfE.

The third relates to statistical power. While we believe this is the best possible strategy and data to answer the research question, we cannot be certain until we attempt estimation.

The fourth as mentioned relates to a) the impossibility of asserting whether youth attended youth clubs and b) whether they attended a specific youth club. Our analysis relies on the assumption that youth who live closer are more likely to attend. We believe this is a reasonable assumption. Any noise implies our estimates are a lower bound on potential true effects.

While we will assess the correlation of youth club closures to general austerity, and control for year and area fixed effects, as well as run additional analyses including local authority and year fixed effects (interacted), there might remain additional unexplained changes at the LSOA and year level that correlate with youth club closures which we cannot observe or control for.

Any contamination (e.g. youth in areas affected attending youth clubs which remained open) would bias our estimates toward zero (noise in the treatment definition).

Table 3.10: Threats to internal validity

Threat	Risk level	Comments
Confounding policies / concurrent interventions – general austerity	Low	The closure of youth clubs coincides (temporally) with austerity in many other areas of public policy. Therefore, if the areas which were more heavily affected by youth club closures were also more likely to be affected by austerity in other areas, the effects might not be attributable to youth club closures specifically. We will mitigate this risk first, by controlling for local unemployment or Income Deprivation Affecting Children Index (IDACI) year on year, which might account for local economic decline in an LSOA. Second, as discussed in the robustness

		checks, we will complement our analyses with additional estimates controlling for heterogeneity at the local authority and year level. These analyses will effectively compare areas within boroughs, which means any attributes constant at the borough year level (like levels of spending in specific policies) will be considered.
Confounding policies – Violation of parallel trends	Medium	The identification strategy relies on a parallel trends assumption. This means that any differences in levels (crime levels, education levels etc.) are accounted for in estimation and that estimates are unbiased insofar as the differences in levels did not change over time. Therefore, differences in treated and control areas are only an issue in so far as they affect potential generalisability of the results. In Villa (2024) treated areas tended to be richer, which suggested that the effects estimated might be a lower bound. We will assess differences in local attributes at baseline and discuss generalisability and bounds in the paper. The parallel trends assumption is essential for the causal interpretation of results. We will assess whether the parallel trends assumption holds using event studies. If parallel trends do not hold, our preferred estimates might require focusing on the subset of areas for which the assumption are plausible. This might involve matching treated and control areas on observable outcomes above mentioned or on pre-trends (synthetic difference-in-differences). We will clearly state for which areas we can provide valid estimates and for which we cannot. We believe this mitigates the risks of this key assumption being violated while allowing us to provide plausible bounds on true effects.
Contamination, spillover effects and misclassification of interventions	Low	Contamination or spillovers in this setting would be present if individuals being treated (affected by youth club closures) attend youth clubs in other areas, or vice versa. In the absence of better data, we need to assume that proximity to a youth club matters (which is supported anecdotally). Any noise in the treatment definition means that our estimates would be attenuated (bias toward zero). Another type of misclassification would occur if local authorities do not provide sufficient data, perhaps withholding information on openings or closures. There is no possible way of being certain that the information in the FOI is truthful, and the data we will gather is arguably the best indicator of youth club availability. This would similarly introduce attenuation bias.
Implementation fidelity	Low	In this context implementation fidelity means that youth clubs effectively closed. Our concern, based on previous experience is that, if anything, quality of provision might have begun to decline in youth club closures prior to the closures (perhaps due to progressive loss in funding) in unobservable ways. This implies that our reference timing is imperfect and that our estimates are once again, biased toward zero.

Attrition and missing data	Low	We will be using administrative records from MOJ-DfE. Hence, we are not relying on experimental evidence or different waves of data gathering but on information which already exists and is stored by the data owners. We believe this is the most complete dataset to date and that missing observations (e.g. missing control data, or offending data) due to attrition or MOJ-DfE's inability to track participants are a small proportion. We will report whether a substantial number of individuals are missing control data, for instance. The more pressing missing data in this paper relates to FOI information. We will aim to gather details for as many LAs in England as possible. We estimate we will be able to gather data for ½ to 2/3 of all local authorities.
Measurement of outcomes and treatment	Low	We will be using administrative records from MOJ-DfE. This is the most complete and accurate possible source for recorded offending in England. We believe using this database as opposed to self-recorded survey data minimises important reporting biases, while allowing us to study the complete sample, and to focus on an outcome which has real implications (through a criminal record). Beyond accurate measurement of outcomes, it is worth discussing differences in measurement of treatment. It is possible that youth clubs which closed differ from youth clubs which remain open in attributes which are not measurable. If youth clubs that closed were of higher quality or larger, for instance, then our estimates might be an upper bound on the true effects. Contrarily, if the closed youth clubs were smaller or of lower quality (more plausible), then our estimates are a lower bound. This will be clearly stated in the paper.
Selective reporting	Low	This analysis plan presents a thorough description of the research aims, research processes and hypotheses tested. The only restrictions we have proposed to impose relate to only reporting results where we can be confident of the findings – hence, where we can at least bound the size of the effect between at least -20% or +20%. Therefore, this analysis plan should serve as an almost “pre-registry”, which should minimise publication bias or other such concerns.

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	Have engaged early with data owners. Our project is currently in the 5 th revision by MOJ and DfE.
2	Discontinuation of the research team	Medium	Ilina Logani will be applying to PhDs and might need to cease working in 1/09/2026. We aim to get as much work done as needed by then. Afterwards, we will seek additional support hiring a new RA and Carmen Villa will contribute with more of her time.

4.2. Timeline

Table 4.2 Timeline

Date	Activity	Staff responsible/leading
September '25	Preparation of analysis plan, hiring of RA (Ilina Logani), data requests to MOJ-DfE	Carmen Villa
October '25 – January '26	Gathering FOI data	Ilina Logani will lead the processing, quality assurance will be done by Carmen Villa
January – September '26	Preparation of interim report, regressions, hiring of new RA support if Ilina Logani pursues PhD	Ilina Logani and Carmen Villa
September–December '26	Finalisation of interim report	Carmen Villa

January – March '27	Additional analyses and preparation of final report	Carmen Villa
---------------------	---	--------------

5. References

- Beatty, C. & Fothergill, S. (2014), 'The local and regional impact of the UK's welfare reforms', *Cambridge Journal of Regions, Economy and Society* 7(1), 63–79.
- Cengiz, D., Dube, A., Lindner, A., & Zipperer, B. (2019). The effect of minimum wages on low-wage jobs. *The Quarterly Journal of Economics* 134(3), 1405-1454.
- Davis, J. M. & Heller, S. B. (2020), 'Rethinking the benefits of youth employment programs: The heterogeneous effects of summer jobs', *Review of Economics and Statistics* 102(4), 664–677.
- Dinarte-Diaz, L., & Egana-del Sol, P. (2024), 'Preventing Violence in the Most Violent Contexts: Behavioral and Neurophysiological Evidence from El Salvador', *Journal of the European Economic Association* 22(3), 1367–1406.
- Gottfredson, D. C., Gerstenblith, S. A., Soul'e, D. A., Womer, S. C. & Lu, S. (2004), 'Do after school programs reduce delinquency?', *Prevention Science* 5, 253–266.
- Heller, S. B. (2014), 'Summer jobs reduce violence among disadvantaged youth', *Science* 346(6214), 1219–1223.
- Heller, S. B., Shah, A. K., Guryan, J., Ludwig, J., Mullainathan, S. & Pollack, H. A. (2017), 'Thinking, fast and slow? Some field experiments to reduce crime and dropout in Chicago', *Quarterly Journal of Economics* 132(1), 1–54.
- Jacob, B. A. & Lefgren, L. (2003), 'Are idle hands the devil's workshop? Incapacitation, concentration, and juvenile crime', *American Economic Review* 93(5), 1560–1577.
- Modestino, A. S. (2019), 'How do summer youth employment programs improve criminal justice outcomes, and for whom?', *Journal of Policy Analysis and Management* 38(3), 600–628.
- Modestino, A. S. & Paulsen, R. (2023), 'School's out: How summer youth employment programs impact academic outcomes', *Education Finance and Policy* 18(1), 97–126.
- Rambachan, A., & Roth, J. (2023). A more credible approach to parallel trends. *Review of Economic Studies*, 90(5), 2555-2591.

Youth Endowment Fund. (2025) 'Using secondary data analysis to learn about violence prevention', <https://youthendowmentfund.org.uk/grants/secondary-data-analysis/>. Last accessed 21st August 2025.

Villa, C. (2024) 'The Effects of Youth Clubs on Education and Crime'. IFS Working Paper.