# THE EFFECTS OF YOUTH CLUBS ON EDUCATION AND CRIME

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## JOB MARKET PAPER

**Abstract**

Youth clubs are community-based after-school programmes, typically offered free of charge to teenagers in underprivileged neighbourhoods. I provide the first causal estimates of their effects on education and crime, leveraging quasi-experimental variation from austerity-related cuts, which led to the closure of 30% of youth clubs in London between 2010 and 2019. I use difference-in-differences research designs and novel data to compare neighbourhoods affected by closures with those unaffected. Teenagers in areas affected performed nearly 4% worse in national high-school exams. Youths aged 10 to 17 became 14% more likely to commit crimes. Youth clubs provide key support in a lasting manner, particularly to teenagers from low-income backgrounds. The effects are due to youth clubs offering unique amenities that support positive behaviours rather than mere incapacitation. Closing youth clubs was not cost-effective; for every £1 saved from closures, there are associated losses of nearly £3 due to forgone returns to education and crime costs.

*JEL Codes*: H72, I38, J13, J24, K14, K42.

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# I. INTRODUCTION

Idle time during adolescence is connected to an array of negative outcomes, from higher crime engagement (Lochner & Moretti 2004, Akee et al. 2014) to poorer mental health (Allcott et al. 2020, Braghieri et al. 2022, Burnell et al. 2024). After-school activities are an opportunity for youths to learn new skills, develop friendships and foster healthy habits. However, these are often privately provided and costly, which might create barriers to participation for lower-income youths.

Several countries have developed publicly funded after-school clubs either fully free or highly subsidised, but their effects remain poorly understood. Concentrating deprived youths could have unintended detrimental consequences through peer effects (Bayer et al. 2009, Dinarte 2020). Conversely, these spaces might foster youth development by providing a safe space, mentoring youths and providing positive programmes such as music or sports. Empirical evidence is limited due to data scarcity as the best usage estimates come from a handful of surveys. There are also empirical challenges in establishing causal effects because attendance at youth clubs may correlate with socio-economic factors that influence the outcomes of interest.

In this paper, I combine several novel data sources and plausibly exogenous variation in youth club availability to provide the first causal estimates of the effects of youth clubs on education and crime. I also explore potential channels explaining the effects and evaluate their cost-effectiveness. I study London (UK), where youth clubs are offered free of charge to youths aged 10 to 19 in community-based bespoke spaces. At the youth club, trained youth workers organise sports activities, homework clubs and workshops, and act as someone young people can confide in. In 2009, approximately 40% of children aged 11 to 16 residing in London attended at least weekly.[1]

This setting provides quasi-experimental variation in youth club availability through austerity reforms, which led to the closure of nearly 30% of youth clubs. Between 2010 and 2019, spending on youth services (which include youth clubs) fell by 72% across the UK from an initial £1.4 billion. The closures were staggered and the decisions on which youth clubs would close were taken by local authorities.[2]

[1] Source: Young Londoner Survey (2009). There are no data on attendance after age 16.
[2] Local authorities are local governments, also known as councils or boroughs.

I hand-collected a dataset on the location of youth clubs using Freedom of Information (FOI) requests. I leverage longitudinal data from the Understanding Society survey, which tracks individuals aged 10 to 15, to examine changes in time use. I use administrative records from the Department for Education containing test scores in national standardised exams for all pupils in London, and crime data from the London Metropolitan Police including the universe of crimes recorded and details on offenders for detected crimes.

I employ difference-in-differences (DiD) models to compare residents in areas affected by closures with those from unaffected areas. *Treated* blocks are those where nearby youth clubs closed, while *control* blocks are those where nearby clubs remained open.[3] I estimate average treatment effects on treated areas (ATT) using two-way-fixed-effects (TWFE) and *stacked* research designs (Cengiz et al. 2019, Deshpande & Mueller-Smith 2022). The latter approach excludes already-treated observations from the comparison group, ensuring consistency in the presence of dynamic or heterogeneous effects (Callaway & Sant'Anna 2021, Roth et al. 2023).[4] I also estimate standard event studies to assess the parallel trends assumptions and characterise the dynamics of the effects.

Youth club closures reduced participation in structured after-school activities, with the likelihood of attending after-school programmes at least monthly decreasing by 16 percentage points (a decrease of about 44%). This *first-stage* estimate suggests that when youth clubs closed, children had few or no alternative structured after-school activities available to them.

The loss of youth clubs led to significant declines in educational performance, with teenagers in affected areas performing 3 to 4% worse in the national standardised exams taken at ages 15–16 (Key Stage 4 or GCSEs), measured in standard deviations (s.d.). This is comparable to the effects of Sure Start centres, which provided a comprehensive set of early childhood resources and improved test scores by 3.1% s.d., as evaluated in Carneiro et al. (2024). The effects of youth club closures were stronger for pupils from lower-income backgrounds, as proxied by their free school meals (FSM) status. For these

[3]Blocks are Lower Level Super Output Areas (LSOAs), statistical units created by the Office for National Statistics (ONS). According to the 2011 Census they had an average population of 1,690. Treatment is defined using commuting distances between each youth club and the population-weighted centroid of each *block*. *Nearby* in this context means within 40 minutes on foot – a reasonable commuting time according to survey answers and conversations with youth workers.

[4]While several estimators can recover unbiased ATTs in the presence of dynamic effects or heterogeneity across treatment time, I opt for the stacked design due to its ease of explanation, computational efficiency and flexibility.

pupils, test scores fell by 12% s.d. They were also stronger for pupils with lower prior attainment, proxied by their grades at ages 10-11.

The closures also led to increases in youth offending rates in terms of both the proportion of youths offending and the number of crimes committed by youths. The magnitudes are 14 to 15% for the proportion of youths offending and 15 to 16% for the crimes committed per youth. The crime rises occurred across all the main crime categories (drugs, violence and acquisitive crimes), both for younger and older teenagers (aged 10 to 15 and aged 16 and 17), in and out of school holidays and at all hours of the day. The rises were more pronounced for acquisitive crimes.

Analyses exploring heterogeneity by length of exposure to youth clubs suggest strong dynamics. Those who were older when the youth club closed do not see their grades as hampered or their crime participation as increased as those pupils who were younger. This might be due to youth clubs instilling civic values, promoting healthier habits or reducing opportunities to accumulate criminal capital (Arora 2023).

I also examine the impact of youth club closures on *local crime* rates. These analyses focus on *where* crimes occur rather than whether *residents* offend, which is the focus of most studies on place-based policies (Jacob & Lefgren 2003, Luallen 2006, Akee et al. 2014, Steinberg et al. 2019). Youth club closures did not alter the spatial distribution of crimes, a result that holds even when focusing on detected crimes committed by minors. These results underscore the importance of differentiating between the study of *people* and the study of *place*.

The effects do not seem to be driven by general austerity. As placebo exercises, I estimate the effects of closures on test scores at ages 10-11 – when people were presumably too young to be exposed to the benefits of the youth clubs. I find a null effect. Offending rates do not seem to have increased for people aged 18 to 34, although there might be some spillover effects to people aged 35 and over. However, triple difference-in-differences analyses using younger or older groups as an additional control group suggest that the closures differentially affected teenagers. I rule out the effects on crime being driven by changes in policing as there were no increases in detection or stop-and-search rates after closures. The estimates are also robust to several changes in assumptions and estimation. The analysis of a few youth club openings suggests the effects may be symmetric, with openings improving education outcomes and reducing youth offending.

A key channel explaining the effects is that the bundle of positive amenities provided at youth clubs is not easily substitutable for other activities. Survey data show that after closures, affected youths spend fewer hours doing homework and have more difficulty concentrating. Meanwhile, they appear to substitute time at the club for cheap leisure, such as videogames, TV and social media. Proximity to libraries, parks or sports centres does not mitigate the effects. Only proximity to operative youth clubs does. Notably, the data do not support pure incapacitation (youth clubs acting merely as *holding spaces*) being a key driver as the observed increases in youth crime are not confined to hours when youth clubs would have been open.

Despite the objectives of the closures being to cut governmental spending, these might not have been cost-effective. Combining my estimates with the available literature on the costs of crime and the returns to education (National Audit Office 2011, Heeks et al. 2018, Hodge et al. 2021) in a cost–benefit analysis suggests that, under conservative assumptions, the savings do not offset the associated costs. For every £1 saved, there are estimated losses of £2.85 in public spending and private costs. Adopting a marginal value of public funds approach shows that for each £1 saved from public funds, there are *private* losses of £5.13 (Finkelstein & Hendren 2020).

The closures might not have been cost-efficient either. Taking into account substitutability and commuting to operative youth clubs could have mitigated crime rises. I show this using a discrete choice model of after-school time use to recover the structural cross-elasticity between crime participation and distance to a youth club.[5] For a 1% increase in commuting costs, crime increases by 0.42%. To assess counterfactual closures and effects on crime, I compute average commuting costs under different closing regimes and optimal closures using p-median models (Church & ReVelle 1974). Local authorities did only marginally better than randomly closing youth clubs. Considering the problem from a spatial perspective might have fully mitigated youth crime rises. The policy implication is that youth clubs might be more effective if set up in areas with lower accessibility to operative clubs. In my context, Outer London boroughs appear to be particularly in need of more youth clubs.

This is the first paper to provide credible causal estimates on the effects of community-based after-school clubs on education and crime. Prior causal work had

[5] I assume that all youth clubs are similar and that crime and attending clubs cannot occur simultaneously.

established that after-school clubs – particularly school-based programmes – could be beneficial, but studied relatively small samples in specific settings (Gottfredson et al. 2004, Dinarte 2020). Instead, work on youth clubs (which take place mostly in community-based spaces) was only correlational. Findings tended to be mixed, with some authors finding beneficial effects (Goldschmidt et al. 2007), but others finding null effects on education (Jones & Offord 1989) or crime (Hirsch et al. 2011). In fact, a few papers found a positive association between attending clubs and worsened outcomes (Mahoney et al. 2001, Feinstein et al. 2005). The creation of a database on youth club locations, the use of administrative crime records and studying large samples through a causal lens are novel contributions.

I also contribute to the literature on the determinants of youth development by focusing on investments made during adolescence. Research has long emphasised the importance of early childhood interventions (Heckman 2006, Cunha & Heckman 2007, Ludwig & Miller 2007, Guryan et al. 2008, Berlinski et al. 2009, Heckman et al. 2010) and in-utero experiences (Currie & Almond 2011, Bharadwaj et al. 2013, Hoynes et al. 2015), but there is an increasing interest in adolescence as we learn more about the development of the brain at this stage and the risks associated with this period (Blakemore 2012, Steinberg 2014). Earlier work had highlighted the role of schooling and inputs to the education production function, showing that additional years of schooling (Card 1999), high-quality teachers (Chetty et al. 2014, Jackson 2018) and positive peer influences are critical determinants of improved outcomes (Grossman 2006, Oreopoulos & Salvanes 2011, Lochner 2011). This paper brings a new perspective by exploring the role of non-formal education.

This paper builds on the work exploring the determinants of youth crime. Most existing papers assess the role of formal schooling and highlight the crime-reducing effects of additional years of education (Machin et al. 2011, Hjalmarsson et al. 2015, Bell et al. 2022) and the fact that pupil interaction can increase some crimes (Jacob & Lefgren 2003, Luallen 2006, Akee et al. 2014). I expand our knowledge by assessing non-formal education and exploring both *offending* and *local crime*.

Last, this paper provides new evidence on the unintended effects of austerity. Several authors have highlighted that welfare reforms in the UK led to increases in local crime (Giulietti & McConnell 2021, Facchetti 2024). Research in other contexts has shown that parental losses in benefits can increase children's offending (Corman et al. 2017, Dave et al. 2021, Dustmann et al. 2024). My paper focuses on cuts that affect young people specifically.

The rest of the paper is structured as follows. Section II describes the characteristics of youth clubs in the UK and the change in spending on youth services in 2010–19. Section III describes the data sources and spatial statistics constructed. The empirical strategies are described in Section IV. Section V presents the results. Section VI discusses threats to identification and robustness checks. Section VII covers channels explaining the effects. Section VIII evaluates the austerity policy and Section IX concludes.

## II. BACKGROUND

This section describes the characteristics of youth clubs in London, presents statistics on usage and describes the related austerity policies.

### II.A Youth clubs in London

Youth clubs are community-based after-school programmes that provide young people with a safe space to engage in recreational, educational and social activities. The target age is usually 10 to 19 and some youth clubs allow individuals up to age 25 if they have special educational needs or disabilities. According to publicly available data online – detailed in Appendix B – the most common hours in which clubs operate are from 15:00 to 20:30 on school days, and many are also open on weekends and school holidays. This paper focuses on *open access* clubs, which any young person can access irrespective of attributes other than their age. These differ from *targeted* clubs, where people need to be referred to use the service by their teachers, the police or other social services.

Youth clubs provide free or heavily subsidised access to a diverse set of activities and exposure to a youth worker – who oversees the use of the amenities, organises workshops and sometimes becomes a mentor young people can confide in. The range of activities varies across clubs, but most have a space to socialise, pool or ping-pong tables, and board games. In London, 65% offer sports, at least 22% have computers or videogame rooms, 16% provide education support and 12% have music equipment. In many clubs, there is a cafe with drinks and food available to purchase, but free food is not usually provided. A few clubs have dedicated sessions for girls or LGBT users.

Local authorities are responsible for managing youth clubs and have a statutory duty to provide these services. However, the regulation does not include binding requirements and many aspects of the running of clubs are open to interpretation (Davies 2018). There is no legal requirement to fund youth clubs, nor a minimum amount of spending. Local authorities can run and manage youth clubs or coordinate clubs run by independent charities. In London, about 52% of all youth clubs are fully run by local authorities, 42% are managed by charities and the rest by churches or other organisations. Council-managed youth clubs rely almost entirely on public funds. Charities can obtain funds from other sources, but a third to two-thirds of their resources come from public sources, according to youth workers' insights.

The yearly running costs of youth clubs – covering the salaries of youth workers and trips or paid workshops – range from £32,500 to £610,523 according to FOI data and surveys among youth workers. The mean cost is £169,567 and the cost per user is about £350 per year.

Countries implementing similar programmes beyond the UK include Canada, Australia, the US (e.g. the *Boys and Girls Clubs*), Scandinavian countries, Germany (*Jugendzentren*) and France (*Maisons des jeunes et de la culture*).

### II.B Youth club attendance

Attendance at youth clubs is voluntary and is not often recorded. Gathering statistics is not a priority, and many youth workers argue that tracking visits might deter participation.

The best estimates of attendance come from survey data. According to the Greater London Authority (2009), 40% of Londoners aged 11 to 16 used youth clubs at least weekly in 2009 and 12% attended at least every two days. This survey was not repeated.

Youth club use correlates with time use and demographic attributes. People who attend hang out with friends more often, are more likely to visit parks, do sports more often, use libraries more, engage more in homework clubs and spend less time watching TV or using computers. They are more likely to be Black and to come from deprived households.[6]

[6] Source: Own analysis. See Appendix B.

## II.C Austerity and spending in youth services

As part of the 2010 Spending Review, the government announced substantial welfare cuts. These impacted many areas of public policy, including social care, public housing, policing and health services. Youth services were among the most hit aspects of public provision, likely due to relaxed laws regarding the statutory duty of local authorities and because several grants specifically ring-fenced for these services were abolished (HM Government 2010).

In London, spending on youth services dropped by 71% between 2010 and 2019, from an initial £215 million (Department for Education 2020). The trend was similar across the UK (Berry 2018, YMCA 2020, UK Youth 2021). Out of all youth clubs open in London in 2010, nearly 30% had closed by the end of the decade – a loss of 85 youth clubs across 24 boroughs. These closures were staggered and did not differently occur in areas differentially affected by other austerity cuts or with specific political inclinations. The fall in spending on youth services was the most pronounced among all the cuts affecting young people, as shown in supplementary analyses in Appendix C.

Clubs remaining open had to operate with a lower budget. Youth workers report that this often meant reducing the number of operative days, shifting from paid to volunteer staff and decreasing the activities offered. Information on how each youth club was affected by austerity is unfortunately not available.

# III. DATA

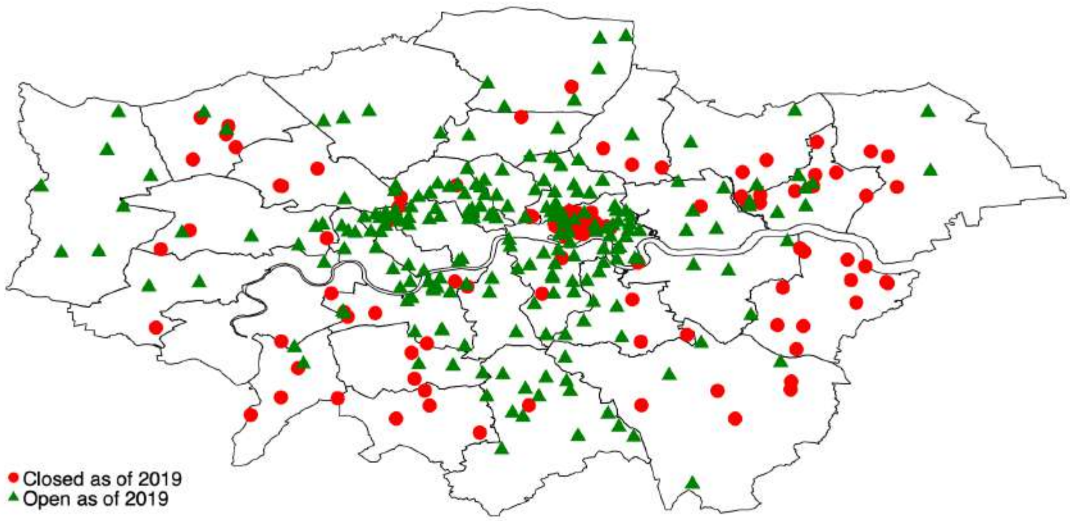
This section describes the datasets used, which include a hand-created database of youth clubs, survey data, administrative records from the Department for Education and administrative records from the London Metropolitan Police.

## III.A Youth centres and youth clubs

I create a dataset of youth club locations using Freedom of Information responses from each local authority, which reported the postcode of each youth club in their area and the year of closure if relevant. In a few cases, they also reported the year of opening. Local authorities were compliant and for most boroughs the data appear complete between 2010 and 2019. I cannot observe closures before 2010 nor which centres remained open during the COVID-19 pandemic. Two boroughs (Hackney and Hammersmith & Fulham) stated they did not hold data before 2015; I assume that the youth clubs that were open as of 2015 had been open since 2010 to create a complete panel. Three boroughs were not able to provide any dates on openings or closures (Camden, Hillingdon and Westminster); I assume that there were no openings or closures.[7]

Figure 1 shows a map of London with open and closed youth clubs. There were 298 open clubs as of 2010, and 85 closed for universal provision during the decade, with most closures happening by 2015. There were only 34 new openings during the decade, of which 17 also closed during the observation period. At the end of 2019, there were 230 open youth clubs in London.

Figure 1: Youth clubs in London and their opening status as of 2019




*Notes:* The red circles show youth clubs that closed between 2010 and 2019 and the green triangles show clubs that were operative throughout the decade. The youth club locations come from FOI data, and the map represents borough boundaries from Ordnance Survey.

I use the precise address of each youth club and the HERE API geocoder to compute distance measures from each block population-weighted centroid to all youth clubs within a 5-kilometre radius.[8] Using population-weighted centroids (instead of Euclidean centroids) allows me to capture differences in urban structures. I compute

[7]The estimates are robust to excluding the borough–year combinations where data are questionable.
[8]The population-weighted centroid represents the centre of a geographic area taking into account the distribution of its population.

the Euclidean distance and the commuting time on foot. The average commuting time is 24.90 minutes.

This data collection effort is a novel contribution to the literature, which previously could not provide an accurate count of youth clubs per area or over time. To the best of my knowledge, this is the most comprehensive and up-to-date source on youth clubs in London for the 2010–19 period.[9]

### III.B Survey on youths' time use

I use survey data from Understanding Society to understand changes in after-school time use. This source is representative of the UK population at the national level and includes a youth questionnaire with respondents aged 10 to 15. This source was created by University of Essex, Institute for Social and Economic Research and includes a unique identifier per respondent, details on the block of residence, and many questions on socio-economic attributes and habits. I focus on interviews taking place between 2010 and 2019.

The source includes a question on attendance at *organised after-school activities* which is asked in alternate years and ranges from *most days* to *never.* These activities include but are not restricted to, youth clubs (for instance, they also refer to private sports clubs, after-school paid tutoring and organisations such as the Scouts or Guides). Unfortunately, sources with information on attendance at youth clubs specifically are not available as a panel. In 2010, nearly 30% of 10–15-year-olds attended structured after-school activities at least once a week and about 8% did so almost daily. For my main estimates, I create a dummy variable equal to 1 if a youth attended at least monthly in the year preceding the year in which the question was asked. More details on this question and correlates to other attributes are available in Appendix A.

I incorporate variables measuring weekly hours spent on homework, videogames, television and social media and an index indicator of students' self-reported ability to concentrate. Data availability varies as some variables were only introduced from 2012 onwards.

[9]This includes more clubs than Berry (2018) or Ordnance Survey's Points of Interest database.

### III.C Educational outcomes

I use information on educational performance from administrative records from the National Pupil Database (NPD) for the academic years ending between 2010 and 2019, made available by the Department for Education. This source contains records for the universe of pupils residing and attending school in London, with details on their gender, ethnicity and block of residence. It also includes whether they are eligible for free school meals (FSM), which serves as an indicator of family income as students are only eligible if their family receives certain income-based government benefits. Approximately 25% of all pupils are eligible for FSM.

In the UK, it is not possible to track students' test scores year-on-year. Instead, there are national standardised exams that pupils take in Year 6 – at ages 10 or 11 – and in Year 11 – at ages 15 or 16. My key outcome of interest is the performance at ages 15 or 16, when pupils are in the target age of youth clubs (pupils aged 10 or 11 might be allowed in youth clubs but are slightly too young). Exams in Year 11 (KS4, also called GCSEs) mark the completion of compulsory education and cover a broad range of subjects, including English, mathematics, science and history. These results determine eligibility for future educational paths such as A levels, vocational training and other post-secondary pathways. I also use the exams in Year 6 (KS2, also called SATs) in a placebo analysis. These assess students' proficiency in core subjects, including English, mathematics and science. I create standardised variables for test scores with mean 0 and standard deviation 1 to ensure comparability over time, as there were some changes to the score scale in the study period. Whenever a pupil was in a school Year more than once (a very small number of pupils), I use the year they were first in it.

### III.D Offending, local crime and policing intensity

I leverage administrative records from the London Metropolitan Police Service (MPS) and data from Police UK to create measures of offending rates, local crime and policing activity by block and year.

The administrative records were obtained through a Data Sharing Agreement and contain the universe of crimes recorded within the force between April 2010 and December 2019. The data include the date of the crime (with hourly information for most records), the crime type as per the Home Office classification system, and the location of the crime anonymised to block level. There are 7,601,196 crimes in the sample and the most common crimes are thefts (29%) and violence against the person (21%). For a subset of crimes – *detected* (UK) or *cleared* crimes (US) – the database also includes a unique identifier for each offender, their home address coded on the block level and their age. Around 20% of all crimes are detected (1,426,993 observations). These crimes are connected to 952,237 unique individuals. The most common crimes with offender details are drug offences (33%), violence against the person (24%) and thefts (14%).[10]

I use the set of detected crimes to create measures of residents' propensity to commit crimes by combining the records with population estimates from Office for National Statistics, available by block, age and year.[11] This allows me to compute two measures: *offending rates* and *crime incidence rates.* Offending rates represent the proportion of residents who were accused of a crime per 1,000 population, calculated for different age groups, blocks and years. Crime incidence rates represent the number of crimes committed per 1,000 residents of different age groups, blocks and years. I also compute crime incidence by crime type and by the hour in which crimes are committed, distinguishing school days from school holidays.

An individual can be accused of several crimes in the same incident, and a single offender can commit many crimes, so the crime incidence rate will always be greater than the offending rate. Note that these statistics leverage information on where offenders *live* rather than where crime *happens.*

I also compute *local crime rates* as the sum of crimes that happen in a block per 1,000 population. This differs from the above in that the geographical information of interest is where crimes *happen*, rather than where the offender lives. I compute general local crime rates and rates for local crimes committed by specific age groups (e.g. by 10- to 17-year-olds).

The remaining crime variables measure police presence, which I use in robustness checks. I compute detection rates as crimes that occur in a block for which the identity

[10]I do not have information on the proportion of crimes in which the felon is convicted, although aggregate statistics suggest the vast proportion of crimes detected by police end in a conviction (CPS 2024).

[11]The population data are only available by detailed age and block from 2012 onwards. Prior to 2012, they are only provided by age group. To obtain estimates for 2010 and 2011, I linearly interpolate the population for each cohort and block. The results are similar when replacing the values with those of 2012 instead of linearly extrapolating.

of the offender is known over total crimes occurring in a block. I also calculate stop-and-search rates using publicly available data from Police UK as total searches per 1,000 population from April 2016 onwards (as the source is not available for earlier years). Last, I construct measures of proximity to police stations per block and year using information on police stations from FOI data from MPS. This last control is pertinent in light of recent findings in Facchetti (2024), who shows that closures of police stations in London in 2010 to 2019 affected local crime rates.

### III.E Other sources

I leverage building age data from Verisk to understand the determinants of youth club closures in Appendix C. The data are available as a cross-section as of 2015. I use the 2011 Census from Office for National Statistics to describe the socio-economic attributes of the analytical sample at baseline. I use FOI data containing each local authority's school calendar to create indicators on school days versus school holidays and I define crime categories by hour for heterogeneity analyses. I use the Points of Interest database (POI) from Ordnance Survey from 2010 to 2019 to understand the presence of amenities in small areas in London, which I use to explore mechanisms. I compute distance measures between each block centroid in London and libraries, sports centres and parks, and distinguish between blocks that are above or below median distance to these alternative amenities.

## IV. IDENTIFICATION STRATEGY

Understanding the effects of youth clubs is challenging from an empirical standpoint. Youth clubs tend to be located in more deprived areas and residents attending tend to come from more deprived backgrounds. Those features are not orthogonal to education or crime; hence regressions of such outcomes on youth club availability may be biased due to omitted variables and/or reverse causality. Without an unexpected shock to youth club availability, estimates may be biased.

I leverage the closure of youth clubs as a shock to youth club availability, which I argue is conditionally uncorrelated with other block-level determinants of educational outcomes or crime. When a youth club closes, the cost of attending youth clubs increases, either because the option has been removed entirely or because the commuting cost to attend a club has increased. A quote from a local newspaper illustrates this relationship anecdotally: *'The average distance someone has to walk [to the youth club] is half a mile. Norbiton is a deprived area. If [the youth club] moves not as many people will attend' - member, age 12* (Burford 2016).

Which youth clubs closed? In Appendix Section C.B, I explore the correlation between different club-level attributes and closures. I find that the only statistically significant predictor of closures is whether youth clubs were managed by the local authority instead of charities. Other factors such as building age, socio-economic attributes or political inclination of the council do not correlate with the likelihood of closure. Within the sample of council-managed youth clubs, I find that none of the attributes are predictors of closing. From local authorities' meeting minutes, the selection of youth clubs for closure appears to be multifaceted. Some councils shifted from *universal* to *targeted* provision of youth clubs and closed the ones that were not considered to reach the intended youths. In some cases, proximity to other youth clubs and their size were considered – favouring centres that were more isolated and/or bigger. Some authorities chose to close low-quality buildings due to their higher maintenance costs. At least one borough had a corruption scandal, leading to the closure of youth clubs involved in fraud or mismanagement (Evening Standard 2016).

I also assess whether different attributes correlate with the probability of being council-managed as opposed to run by charities at baseline. Councils appear to run youth clubs in areas that are less densely populated, but other characteristics are not statistically significant at conventional levels.

### IV.A Main results

I estimate difference-in-differences (DiD) models comparing areas affected by closures with areas unaffected. I define *treated* areas as those blocks where all nearby youth clubs closed and *control* areas as those where all nearby youth clubs remained open. In this context, *nearby* means within 40 minutes on foot – aligning with youth workers and young people's testimonies.[12] I exclude a small number of areas that experienced both openings

[12] This also seems reasonable when compared with pupils' commutes to school. In London, the average distance to their school is 1.7 miles (which takes about 40 minutes to walk).

and closures (373 blocks, or 7.7% of the sample) to facilitate interpretation. I restrict the sample to areas within 40 minutes on foot because people living very far from youth clubs are unlikely to be affected and are probably an unsuitable comparison group. The analytical sample comprises 2,243 blocks. Whenever more than one youth club nearby closed in treated areas, I adopt an intention to treat (ITT) approach and select the year of the first closure.

Summary statistics for the main outcomes of interest and selected socio-economic attributes in treated and control areas at baseline are in Table 1. I also present their difference and test whether the difference is significant using t-tests clustered on MSOA level. MSOAs are larger census areas than my treatment *blocks* and are defined by ONS. There are 982 in London. Clustering at this level allows me to account for spatial autocorrelation while maintaining a relatively large number of clusters. Treated areas have lower population density, a lower proportion of residents living in social housing, fewer people from ethnic minorities, lower crime rates, and a lower proportion of FSM-eligible children suggesting they may be less deprived than control areas. This aligns with the possibility that some councils prioritised higher-risk areas. The DiD design allows for selection into treatment as long as *trends* in the outcomes of interest would have evolved similarly in the absence of treatment. I argue that the fact that treated areas seem to be richer likely implies that my estimates could be a lower bound on the true effects of youth clubs.

To assess the effects on attendance at structured after-school activities and educational outcomes, I use individual-level data. In equation 1, $E$ is an outcome for pupil $n$ residing in block $i$ in year $t$ and $X$ is a vector of individual characteristics. In the education data, I control for gender, ethnicity, month of birth and FSM eligibility. Since statistical power is limited in the survey data, I only control for age and gender. I also include an additional control of population density at baseline interacted with year trends, to allow areas near closures (more likely to be council-run) to be on a differential trend from areas near open clubs (more likely to be charity-run). *After* $\times$ *Closure* is 1 if the individual resides in an area where all nearby youth clubs closed in years after closure. I control for yearly trends which are constant across space $\mu_t$ and for local block characteristics which are constant across time $\mu_i$. The coefficient of interest is $\beta$, which captures the average treatment effect on treated areas (ATT); $u$ is the idiosyncratic error term.

Table 1: Summary statistics at baseline, main analytical sample

| | Control (no nearby clubs closed) | Treated (all nearby clubs closed) | Diff | N |
|---|---|---|---|---|
| ***Block–level data*** | | | | |
| **Census 2011** | | | | |
| Population | 1680.03 | 1652.90 | -27.13* | 2,243 |
| | (8.23) | (11.31) | (13.95) | |
| Population density | 98.23 | 66.63 | -31.60*** | 2,243 |
| | (2.49) | (1.97) | (3.16) | |
| % aged 0-13 | 17.66 | 17.81 | 0.14 | 2,243 |
| | (0.21) | (0.22) | (0.30) | |
| % aged 14-17 | 5.58 | 6.43 | 0.85*** | 2,243 |
| | (0.08) | (0.10) | (0.13) | |
| % BAME | 40.40 | 33.50 | -6.91*** | 2,243 |
| | (0.86) | (1.68) | (1.86) | |
| % no qualifications | 9.17 | 11.51 | 2.34*** | 2,243 |
| | (0.24) | (0.31) | (0.38) | |
| % social housing | 22.80 | 15.37 | -7.43*** | 2,243 |
| | (0.80) | (1.02) | (1.28) | |
| Crime participation at ages 10–17 | 18.44 | 14.65 | -3.79*** | 2,243 |
| | (0.55) | (0.66) | (0.86) | |
| Crime incidence at ages 10–17 | 28.37 | 20.67 | -7.70*** | 2,243 |
| | (0.95) | (1.02) | (1.39) | |
| Crime participation at ages 18–24 | 43.72 | 42.27 | -1.45 | 2,243 |
| | (1.09) | (1.44) | (1.79) | |
| Crime incidence at ages 18–24 | 56.39 | 54.04 | -2.34 | 2,243 |
| | (1.50) | (2.06) | (2.53) | |
| Crime participation at ages 25–34 | 20.82 | 19.61 | -1.21 | 2,243 |
| | (0.57) | (0.77) | (0.95) | |
| Crime incidence at ages 25–34 | 26.57 | 24.66 | -1.92 | 2,243 |
| | (0.78) | (1.03) | (1.28) | |
| Crime participation at ages 35 and over | 9.48 | 6.64 | -2.84*** | 2,243 |
| | (0.27) | (0.30) | (0.40) | |
| Crime incidence at ages 35 and over | 11.81 | 8.10 | -3.70*** | 2,243 |
| | (0.35) | (0.38) | (0.51) | |
| ***Individual–level data*** | | | | |
| **Survey responses in 2010 or 2012** | | | | |
| P(attends monthly or more) 2010 | 0.45 | 0.59 | 0.14 | 199 |
| | (0.04) | (0.08) | (0.09) | |
| Attendance (standardised) (2010) | -0.05 | 0.31 | 0.35** | 199 |
| | (0.08) | (0.17) | (0.18) | |
| Hours doing homework (2010) | 2.56 | 1.80 | -0.76 | 199 |
| | (0.57) | (0.19) | (0.60) | |
| Easy to concentrate (2012) | 0.11 | 0.32 | 0.22 | 196 |
| | (0.08) | (0.11) | (0.14) | |
| Hours on social media (2012) | 2.55 | 1.88 | -0.67* | 165 |
| | (0.19) | (0.31) | (0.36) | |
| Hours playing videogames (2012) | 1.99 | 1.92 | -0.08 | 184 |
| | (0.19) | (0.30) | (0.33) | |
| Hours watching TV | 4.37 | 3.76 | -0.60* | 198 |
| | (0.15) | (0.27) | (0.32) | |
| **Education records 2010** | | | | |
| FSM at age 15–16 (dummy)(2009) | 0.21 | 0.13 | -0.08*** | 45,521 |
| | (0.00) | (0.00) | (0.00) | |
| Score at age 10–11 (2009) | 0.04 | 0.15 | 0.11 | 45,521 |
| | (0.01) | (0.01) | (0.01) | |
| Score at age 15–16 (2009) | -0.01 | 0.06 | 0.07 | 33,988 |
| | (0.01) | (0.01) | (0.01) | |

*Notes:* Mean and standard deviation of selected variables in the treated and control blocks, as well as their difference and standard error of the difference. Standard errors clustered at the MSOA level. Stars (*, **, ***) indicate significance at the 90%, 95% and 99% confidence levels, respectively. The Census is from ONS, crime rates were computed by combining administrative records from MPS with population data from ONS, survey responses are from Understanding Society and education variables are from the NPD. The sample comprises blocks (or individuals living in blocks) within 40 minutes of a youth club at baseline. Crime rates expressed per 1,000 residents.

$$E_{nit} = \mu_i + \mu_t + \eta X_n + \beta After \times \ Closure_{it} + u_{nit} \tag{1}$$

To assess the effects on crime, I use block-level data. The notation in equation 2 is as above, but $R$ is a crime rate in block $i$ in year $t$. I include population density at baseline interacted with yearly trends to allow for differential trends as above in vector $X$. I weigh the coefficients by block-level population. I also estimate event studies to assess the plausibility of the parallel trends assumption and evaluate the dynamics in the effects. I assume no spillovers.

$$R_{it} = \mu_i + \mu_t + \eta X_{it} + \beta After \times \ Closure_{it} + u_{it} \tag{2}$$

I estimate the models using TWFE and stacked designs for the main results (Cengiz et al. 2019, Deshpande & Mueller-Smith 2022). The latter allows me to obtain unbiased ATT estimates in *staggered* settings in the presence of heterogeneous or dynamic effects over time (Callaway & Sant'Anna 2021, Roth et al. 2023). The stacked estimator in my setting uses the never-treated group as the comparison and weights each cohort equally.[13]

I report clustered standard errors at the MSOA level. Additional inference estimates, changes in sample, alternative estimators and changes in the dependent variables are discussed in Section VI and do not alter the results.

### IV.B Triple difference-in-differences

I estimate triple difference-in-differences models (DiDiD) to further account for unobserved block-level shocks. These models evaluate the differential effects of youth club closures on the target age group (teenagers) relative to residents who are either too old or too young to attend.

To evaluate differential effects on education, I compare the impact on test scores at ages 15–16 with that on test scores at ages 10-11. In equation 3, $g$ denotes an age group (15–16 or 10-11), and other notation is as before. The coefficient of interest is $\delta$. The model includes age-group–year, age-group–block and year–block fixed effects.

[13] To implement the stacked design, I first create individual *stacks* by selecting each cohort of closed areas (where the centre closed in the same year $c$) and appending these to areas where centres never closed. I then use all stacks to create the final database and augment the model to include stack–area and stack–year fixed effects.

$$E_{itg} = \mu_{tg} + \mu_{ig} + \mu_{it} + \delta After_t \times Closure_i \times AgeGroup_g + \epsilon_{itg} \quad (3)$$

To evaluate differential effects on offending and crime incidence rates, I estimate effects at the block–year–age-group level. I include minors (the age group of interest), young adults who might be allowed in youth clubs if they have learning difficulties or disabilities (those aged 18 to 24), other adults (24 to 34) and adults 35 and over. In equation 4, $\delta$ captures differential effects for each group relative to those aged 24 to 34 – set as the control group. The model includes age-group–year, block–age-group and block–year fixed effects.

$$R_{itg} = \mu_{tg} + \mu_{ig} + \mu_{it} + \delta After_t \times Closure_i \times AgeGroup_g + \epsilon_{itg} \quad (4)$$

### IV.C Alternative treatment definitions

I estimate additional models redefining treatment to capture the unintended effects of the austerity policy across London. In these analyses, treated areas are those where the *nearest* youth club closed and control areas are those where the *nearest* youth club remained open. The difference from the main analysis is that treated (control) areas might have operative (closed) youth clubs nearby. These estimates are useful to understand the effects of the average closure in London, which can then be used in cost–benefit analyses to evaluate overall closures.

I also assess the effects of the small number of openings. Treated areas are those that experienced the opening of a youth club between 2010 and 2019 and control areas are those that did not have clubs and did not experience an opening. Because areas where youth clubs opened may be very different from those where they did not, I include additional analyses using propensity score matching techniques to assess the similarity between treated and control based on observable characteristics. In particular, I match areas based on their proximity to clubs at baseline, the proportion of the population living in social housing, the proportion with higher education, the proportion of age 14 to 17, the distance to schools and the distance to parks. I then use those propensity scores to weight coefficients by the similarity across areas or by the inverse propensity to be in the treated group.

## V. RESULTS

In this section, I present the estimated effects on attendance at after-school activities, educational performance and crime. In the main results, I evaluate changes in outcomes in areas where after closures there were no operative youth clubs available nearby relative to areas that kept the same centres open throughout the decade. Additional estimates with other treatment definitions follow in Section V.C.

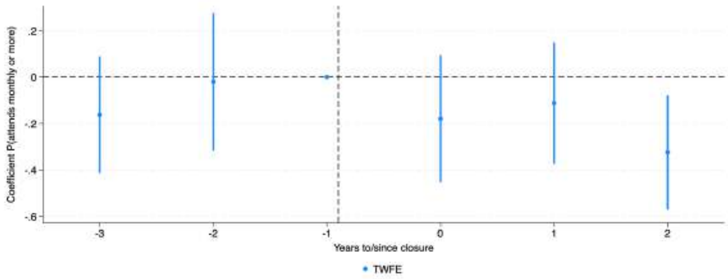
### V.A Main results

After youth clubs closed, attendance at organised activities fell, educational performance worsened and youth crime increased. Event studies for the main variables of interest are in Figure 2 and estimated ATTs are in Table 2. The estimated effects are nearly identical in TWFE or stacked designs.

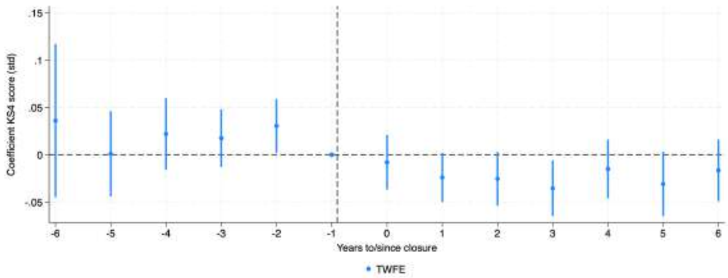
First, I estimate large falls in attendance at organised activities after closures. The likelihood of at least monthly attendance fell by 16.1 percentage points – which is a sizeable effect, at 44% of the mean. The event study does not show a violation of parallel trends, and I cannot reject that the pre-treatment coefficients are jointly zero in F-tests (with a p-value of 0.392). While the analysis is based on a small number of observations (1,132 unique individuals, of whom 190 are treated), these are very large falls, suggesting that youth clubs were not easily substitutable for other structured after-school activities. This *first-stage effect* of the availability of youth clubs on attendance allows me to infer total effects on the treated (TOT) from reduced-form estimates. The ATT coefficients will need to be multiplied by 2.27 (100/44).

Second, the closures led to large decreases in performance in the national standardised exams at ages 15–16. These exams are also called KS4, or GCSEs and take place when students are in Year 11. I estimate a drop of 3.5% standard deviations (s.d.) after closure, which implies a TOT of nearly 8% s.d. The event study shows pre-trends centred at zero (the p-value of joint F-tests is 0.323), and I estimate substantial drops up to six years following the closures. This estimate is very similar to those in Carneiro et al. (2024), who study the effect of Sure Start centres – spaces that provide a comprehensive set of resources in early childhood. In their study, the availability of Sure Start centres improved test scores at ages 15–16 by 3.1% s.d. for the average pupil.

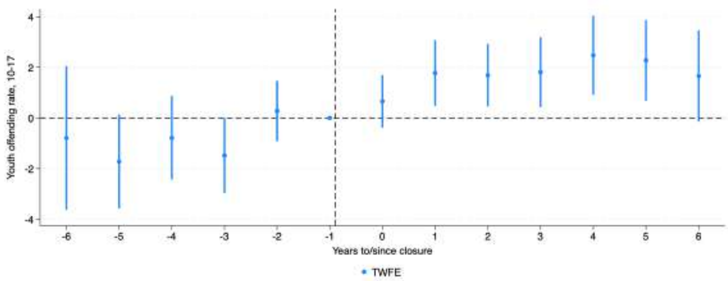
Figure 2: Event studies: effect of closures on attendance at organised activities, educational performance and youth offending



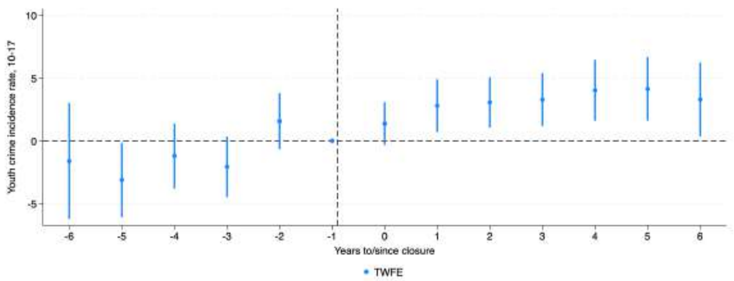

(a) P (attends organised activities)




(b) Test score in exams at ages 15–16 (standardised)




(c) Offending rate at ages 10–17




(d) Crime incidence rate at ages 10–17

*Notes:* Panel a shows the likelihood of at least monthly attendance at organised activities using individual-level survey data from Understanding Society. *P (attends organised activities)* is a binary indicator which is 1 if the individual states attending monthly or more. Panel b estimates educational performance in national exams at ages 15–16 using individual-level data from the NPD. *Test score in exams at ages 15–16 (standardised)* is the average score in the GCSEs standardised to mean 0 and standard deviation of 1. Panel c shows *youth offending rates*, defined as the residents aged 10–17 who have offended per 1,000 residents aged 10–17, and panel d shows *youth crime incidence*, defined as crimes committed by residents aged 10–17 per 1,000 residents aged 10–17. Panels c and d use administrative records from MPS combined with population estimates from ONS and are estimated using block-level data. Coefficients calculated using block-level data are weighted by population. Confidence intervals are calculated using MSOA-level clustered standard errors.

Table 2: Effect of closures on attendance at organised activities, educational performance and youth offending

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | **P (attends activ.)** | **Test score** | **Offending rate** | **Crime incidence rate** |
| | ages 10–15 | ages 15–16 | ages 10–17 | ages 10–17 |
| | First-stage | | Reduced form | |
| *Panel A: TWFE* | | | | |
| ATT | -0.161** | -0.035*** | 2.018*** | 3.176*** |
| | (0.079) | (0.009) | (0.453) | (0.776) |
| Magnitude (%) | -44.27 | | 14.09 | 14.90 |
| $\widehat{TOT}$ | | -7.88 | 31.83 | 33.66 |
| p-value | 0.042 | 0.000 | 0.000 | 0.000 |
| Mean | 0.365 | 0.014 | 14.320 | 21.311 |
| N | 976 | 352,454 | 22,430 | 22,430 |
| *Panel B: Stacked* | | | | |
| ATT | -0.143* | -0.036*** | 2.232*** | 3.581*** |
| | (0.074) | (0.010) | (0.491) | (0.833) |
| Magnitude (%) | -41.09 | | 15.08 | 16.04 |
| $\widehat{TOT}$ | | -8.76 | 36.69 | 39.03 |
| p-value | 0.053 | 0.000 | 0.000 | 0.000 |
| Mean | 0.349 | 0.014 | 14.807 | 22.334 |
| N | 6,296 | 2,232,426 | 146,590 | 146,590 |

*Notes:* Column 1 estimates the likelihood of at least monthly attendance at organised activities using individual-level survey data from Understanding Society. *P (attends activ.) ages 10–15* is a binary indicator which is 1 if the individual states attending monthly or more. Column 2 estimates test scores in national exams at ages 15–16 using individual-level data from the NPD. *Test score ages 15–16* is the average score across all subjects in the GCSEs standardised to mean 0 and standard deviation of 1. Columns 3 and 4 estimate crime rates using administrative records from MPS combined with population estimates from ONS, using data at the block level. *Offending rate ages 10–17* is defined as the residents aged 10–17 who have offended per 1,000 residents aged 10–17; and *crime incidence rate ages 10–17* is defined as crimes committed by residents aged 10–17 per 1,000 residents. Coefficients calculated using block-level data are weighted by population. Standard errors clustered at the MSOA level are in parentheses. Stars (*, **, ***) indicate significance at the 90%, 95% and 99% confidence levels, respectively.

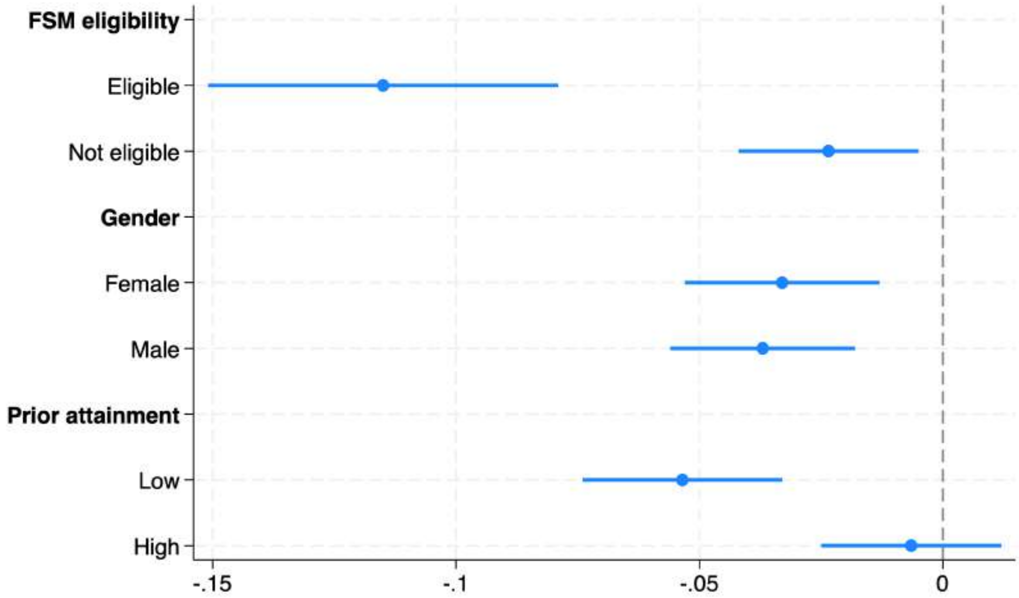
Third, the closures led to large increases in youth crime participation. The proportion of young offenders per 1,000 young population increased by 2 per block and year, a rise of 14% over mean offending rates. The implied TOT is a rise of 32% for pupils who would have attended youth clubs. The number of crimes committed by youths increased by about 3 crimes per 1,000 young population, a rise of 15% over the mean. The implied TOT is nearly 34%. These are very large increases in offending rates. As a comparison, Bell et al. (2022) find that extending the legal school dropout age in the US *reduced* arrest rates by 6% in a given year. My event studies show parallel pre-trends and higher youth offending up to 6 years following the closures. I cannot reject that the coefficients are jointly zero for offending rates (p-value of 0.110). For crime incidence rates I reject that the coefficients are jointly zero (p-value of 0.021), but additional analyses in Section VI reject that violations of the parallel trends assumptions are driving the effects.

Figure 3 presents the average treatment effects on treated areas by pupil and crime attributes. The key attribute determining effects on education performance is whether a pupil was from a lower-income family, as proxied by their free school meals status. For FSM pupils, test scores fell by a staggering 11.5% s.d (for non-FSM by only 2.4). The effects are driven by pupils with lower prior attainment, as proxied by their average test scores at ages 10-11 (in KS2). This heterogeneity is consistent with pupils from lower-income backgrounds or those facing educational challenges relying more heavily on youth clubs for a safe study environment and homework support. I do not find statistically significant differences by gender.

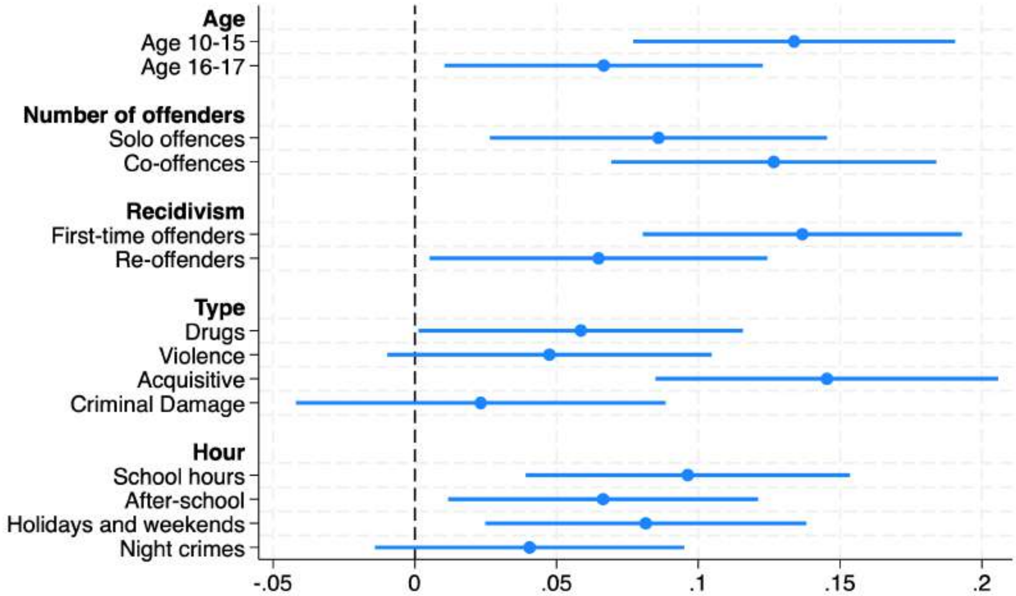
The effects on crime are more precisely estimated for pupils within compulsory schooling age (below age 16), but I cannot reject that they could be similar for pupils aged 16 to 17. The effects are slightly larger for first-time offenders than for re-offenders. There are no significant differences between crimes committed by a single individual and crimes where more than one suspect is involved. The offending increases occurred across all the main crime categories. Drug crimes rose by about 13%, violent crimes by 20 to 21% and acquisitive crimes by 28 to 29%.[14] I do not find a statistically significant increase in criminal damage and arson crimes, which include graffiti and other damage to public property.

[14] Acquisitive crimes include thefts, robberies, burglaries and shoplifting.

Figure 3: Effect of closures on educational performance and youth offending, by pupil and crime attributes



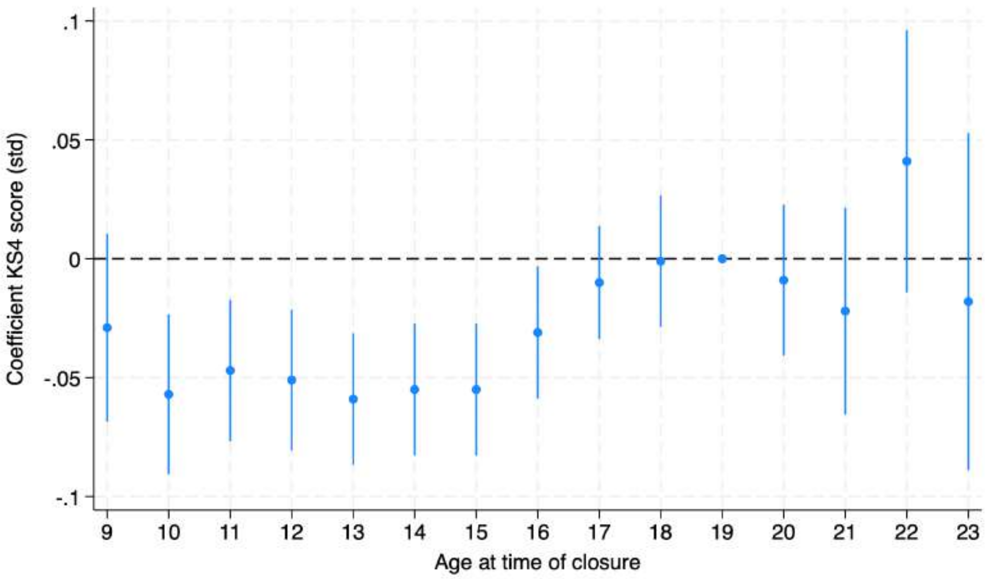

(a) Effects on test scores



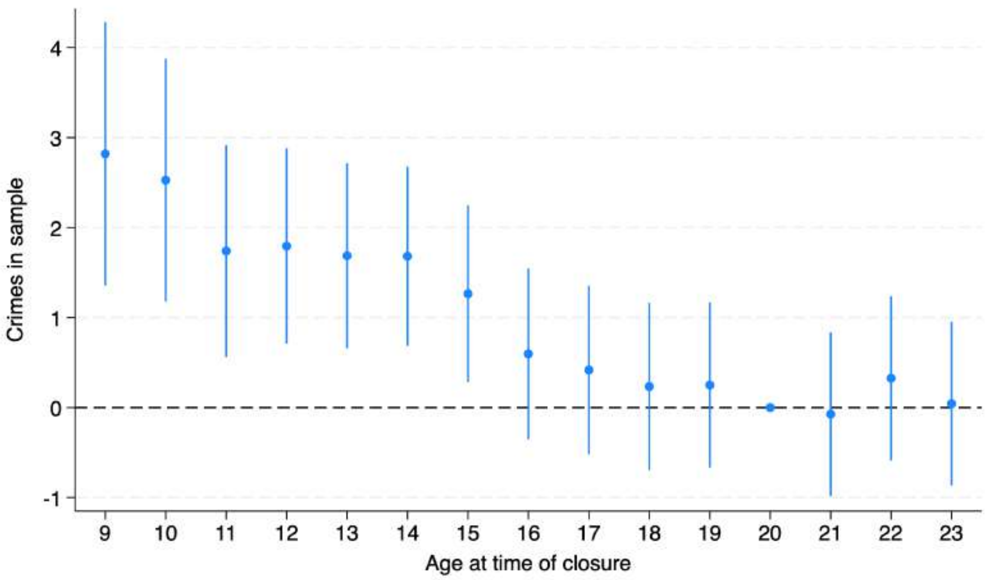

(b) Effects on youth offending (standardised variables)

*Notes:* Panel a presents the estimated effect of closures on educational performance in national exams at ages 15–16 using individual-level data from the NPD. *Test score in exams at ages 15–16 (standardised)* is the average score in the GCSEs standardised to mean 0 and standard deviation of 1. FSM stands for free school meals. Low prior attainment indicates below median grades at ages 10–11, and high prior attainment indicates median or above median grades at ages 10–11. Panel b shows estimated effects on youth crime incidence rates by crime attributes using administrative records from MPS combined with population estimates from ONS, at the block level. *Youth crime incidence* is defined as crimes committed by residents aged 10–17 per 1,000 residents. Coefficients calculated using block-level data are weighted by population. Confidence intervals are calculated using MSOA-level clustered standard errors.

Figure 4: Effect of closures on educational performance and offending, by age at closure




(a) Effects on test scores




(b) Effects on offending (count of offences in sample)

*Notes:* Panel a presents the estimated effect of closures on educational performance in national exams at ages 15–16 using individual-level data from the NPD. *Test score in exams at ages 15–16 (standardised)* is the average score in the GCSEs standardised to mean 0 and standard deviation of 1. Panel b shows estimated effects on the count of crimes committed in sample (2010 to 2019) using administrative records from MPS and the regressions are estimated at the cohort–block level. Age at time of closure is set to 20 for people for whom their nearest youth club never closes, and for whom it closes after they are 18. Confidence intervals are calculated using MSOA-level clustered standard errors.

I explore heterogeneity by the hour in which crimes are committed. Except for night crimes, the increases appear similar on school days during school hours, on school days after school and on holidays and weekends. Hence, crime rises also occurred at hours in which youth clubs would have been closed, which suggests that pure incapacitation is not the key mechanism explaining the effects.

As additional analyses, Figure 4 illustrates estimated effects on education and crime by age at closure. The younger an individual was when the youth club closed, the larger the fall in test scores or the rise in crime participation.

I also assess the effects of youth club closures on local crime rates. This analysis focuses on crimes occurring in the vicinity of youth clubs. I do not find a change in the spatial distribution of crimes following closures. This is true for the total local crime rate, which uses the universe of crimes regardless of whether a suspect was identified, and for undetected crimes, detected crimes and even crimes where the offender was a minor. The results are in Table 3 and an event study is shown in Figure 6. The results are similar even restricting to a smaller radius (areas closer to youth clubs), as shown in Table 12. The different results for residents' offending rates and for local crime rates can be explained by mobility, as people might not necessarily commit crimes in the area they live in. In the data, only 20% of all detected crimes happen in the same block as the offender's residence.

Table 3: Effect of closures on the spatial distribution of crimes

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | **Local crime rates** | | | |
| | All | Undetected | Detected | Detected, ages 10–17 |
| *Panel A: TWFE* | | | | |
| ATT | -0.312 | -0.698 | 0.386 | 0.891 |
| | (0.996) | (0.749) | (0.517) | (1.143) |
| Magnitude (%) | -0.50 | -1.44 | 2.83 | 4.00 |
| p-value | 0.754 | 0.352 | 0.456 | 0.436 |
| Mean | 62.024 | 48.377 | 13.647 | 22.308 |
| N | 22,430 | 22,430 | 22,430 | 22,430 |
| *Panel B: Stacked* | | | | |
| ATT | -0.073 | -0.356 | 0.283 | 1.858 |
| | (1.171) | (0.839) | (0.620) | (1.171) |
| Magnitude (%) | -0.11 | -0.67 | 2.00 | 8.04 |
| p-value | 0.950 | 0.672 | 0.648 | 0.113 |
| Mean | 67.036 | 52.888 | 14.148 | 23.121 |
| N | 146,590 | 146,590 | 146,590 | 146,590 |

*Notes:* Estimated effect of closures on local crime rates. *Local crime rate* is defined as the number of offences occurring per 1,000 resident population, by block and year. Undetected crime is one in which no suspect was identified. Crime data come from administrative records from MPS combined with population estimates from ONS and the regressions are estimated at block level. Coefficients weighted by resident population. Standard errors clustered at the MSOA level are in parentheses. Stars (*, **, ***) indicate significance at the 90%, 95% and 99% confidence levels, respectively.

### V.B Triple difference in differences estimates

Since the period of study was characterised by austerity reforms in many areas, one might be concerned that other local area austerity shocks could confound the estimates. To disentangle the effect of other policies from the youth-specific shock of closures, I estimate triple difference-in-differences specifications where I use younger or older residents – who should not be affected by closures – as a second control group.

For education, I estimate the effect of closures on exam scores at ages 10-11. While it is possible that some pupils of this age could go to youth clubs, in most cases they would not have been exposed for long. Table 4 shows that closures did not significantly affect test scores at ages 10-11. The differential effect at ages 15–16 in the DiDiD is 2.5% s.d. without controlling for individual-level attributes and 2.3% with controls. Although the effect is not statistically significant at conventional levels (with p-values between 0.07 and 0.1), it goes in the expected direction, with closures differentially affecting teenagers.

For offending, I use adults as a second control group. I group adults into those aged 18 to 24 (potentially allowed in youth clubs if they have special needs but above the age of majority), those aged 25 to 34 (younger adults) and those aged 35 and over. Table 5 presents DiD estimates for offending and crime incidence rates at ages 18 and above. The results show null effects for people aged 18 to 34. For people aged 35 and over, the ATT from the DiD is positive and significant, with estimated rises in offending and crime incidence rates of 7% and 8%, respectively. This suggests some spillovers across age groups from youth club closures.[15] However, the DiDiD shows that when compared with people aged 18 to 24, only minors appear to commit crimes differentially. The magnitude is 11% for offending rates and 12% for crime incidence rates. Both coefficients are significant at the 99% confidence level.

[15]Heterogeneity analyses in Figure 5 shows that the rise is driven by first-time offenders, both solo and co-offences, and crimes in all major crime categories.

Table 4: Effect of closures on educational performance at ages 10-11 and triple difference

| | (1) | (2) | (3) |
|---|---|---|---|
| | **Test score in national exam** | | |
| | age 10–11 | | |
| ATT | -0.003 | | |
| | (0.011) | | |
| After × Treated × Age 15–16 | | -0.025* | -0.023* |
| | | (0.014) | (0.014) |
| Model | DiD | DiDiD | DiDiD |
| Controls | Yes | No | Yes |
| p-value | 0.781 | 0.076 | 0.099 |
| Mean | 0.004 | -0.055 | -0.055 |
| N | 359,029 | 713,275 | 713,275 |

*Notes:* Estimated effect of closures on test scores at ages 10-11 and on differential test scores at ages 15–16. The data come from the NPD and the regressions are estimated at the individual level. Both test score variables are standardised to mean 0 and standard deviation of 1. Column 1 includes block and year fixed effects. Columns 2 and 3 include block–year, test–year and test–block fixed effects. Standard errors clustered at the MSOA level are in parentheses. Stars (*, **, ***) indicate significance at the 90%, 95% and 99% confidence levels, respectively.

### V.C Additional results

I measure the effects of all youth club closures, not just those that left areas completely unprovided for. In these alternative analyses, *treated* areas are those where the *nearest* youth club closed and *control* areas are those where the nearest youth club remained open. This approach mirrors the strategy employed in Facchetti (2024) and measures estimated effects across London.

The estimates in Table 10 show that, relative to areas in which all nearby youth clubs closed, the effects are slightly attenuated. After closures, attendance at youth clubs fell

Table 5: Effect of closures on adult offending rates and triple difference

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | **Offending rate** | | | |
| | age 18–24 | age 25–34 | age 35+ | |
| ATT | 0.079 | 0.028 | 0.562*** | |
| | (1.003) | (0.472) | (0.162) | |
| After × Treated × 10–17 | | | | 2.399*** |
| | | | | (0.571) |
| After × Treated × 18–24 | | | | -0.244 |
| | | | | (1.001) |
| After × Treated × 35 and over | | | | 0.732* |
| | | | | (0.439) |
| Model | DiD | DiD | DiD | DiDiD |
| Magnitude (%) | 0.19 | 0.14 | 7.33 | 11.10 |
| p-value | 0.937 | 0.953 | 0.001 | |
| Mean | 41.951 | 19.518 | 7.664 | 21.621 |
| N | 22,430 | 22,430 | 22,430 | 145,960 |
| | **Crime incidence rate** | | | |
| | age 18–24 | age 25–34 | age 35+ | |
| ATT | -0.372 | 0.392 | 0.764*** | |
| | (1.460) | (0.646) | (0.224) | |
| After × Treated × 10–17 | | | | 3.522*** |
| | | | | (0.873) |
| After × Treated × 18–24 | | | | -0.956 |
| | | | | (1.410) |
| After × Treated × 35 and over | | | | 0.590 |
| | | | | (0.605) |
| Model | DiD | DiD | DiD | DiDiD |
| Magnitude (%) | -0.68 | 1.57 | 7.85 | 12.26 |
| p-value | 0.799 | 0.544 | 0.001 | |
| Mean | 54.553 | 24.972 | 9.729 | 28.736 |
| N | 22,430 | 22,430 | 22,430 | 145,960 |

*Notes:* Estimated effect of closures on adult crime incidence rates and on differential offending rates. The data come from administrative records from MPS combined with population estimates from ONS. *Offending rates* are the residents who have offended per 1,000 residents (by age group). *Crime incidence rates* are the crimes committed by residents per 1,000 residents (by age group). These regressions are estimated at the block and year level. Coefficients are weighted by population. Columns 1 to 3 include block and year fixed effects, while column 4 includes age-group–year, age-group–block and block–year fixed effects. Standard errors clustered at the MSOA level are in parentheses. Stars (*, **, ***) indicate significance at the 90%, 95% and 99% confidence levels, respectively.

by 36 to 45% depending on the specification. Test scores fell by 2.6% (against 3.5% in the main results), and the rise in offending rates was 7 to 8% (as opposed to 14% in the main results). The effects on crime are driven by acquisitive offences (up by 18%) and using the stacked estimator there are some rises in violence (up by 8%). Overall, these analyses suggest that operative youth clubs might mitigate the effects of other closures and that commuting might play an important role in youths' decisions to attend organised activities.

I also evaluate the effects of the small number of openings to assess symmetry. These analyses should be interpreted with care due to limited statistical power. However, they suggest symmetric effects. After openings, test scores improved by 3 to 6% s.d. and youth crime decreased by about 30% (see Table 11). The pre-trends in the offending estimates look parallel before openings (Figure 8). For education, the pre-trends start changing in the year before the openings (Figure 7). This might be due to noise in the data or could reflect anticipation effects.

## VI. ROBUSTNESS CHECKS

I conduct several exercises to test the robustness of the estimates against various changes in assumptions and restrictions. A summary of the findings follows and tables are available in Appendix D.

**Sample restrictions**: In the main results, I restrict the sample to pupils living within 40 minutes' walk of a youth club at baseline. The findings are similar restricting the radii at different levels (20, 30 or 50 minutes). The estimated falls in test scores at ages 15–16 range from 3.1 to 6%, and the increases in crime range from 9 to 15%. The estimated effects on education are statistically significant at the 99% confidence level across all samples. For crime, power is limited in the 20 minute sample (p-value 0.094), but the effects are statistically significant at the 99% confidence level in the 30 and 50 minute radii samples. The estimates are not driven by a specific borough either, as shown in leave-one-out estimations. The results are also similar omitting Inner London from the analysis or excluding borough–year observations where data quality was questionable – Hackney and Hammersmith & Fulham before 2015, and Camden, Hillingdon and Westminster for the whole sample period.

**Other inference calculations**: In the main results, I cluster standard errors at the MSOA level. As additional estimates, I calculate standard errors using clustered wild bootstrap at the local authority level: the authorities deciding on closures, of which there are 32 in London; and by nearest youth club. The effects on education and crime remain statistically significant at the 95% confidence level or above with either inference method.

**Control variables**: In the education estimates, I control for gender, ethnicity, year and month of birth, whether a pupil is eligible for FSM, and population density at baseline interacted with year trend. Additional models excluding these controls yield larger and statistically significant coefficients within the confidence intervals of the main estimates (falls in performance of 4.4% s.d). In the offending estimates, I control for population density interacted with year and weight the coefficients by population. The findings are consistent when omitting the different trends control, controlling for proximity to police stations, or combinations of both controls. Omitting population weights does not alter the estimates either. The magnitudes for offending rate rises are between 13 and 18%.

**Different offending definitions**: The distribution of crime rates often features a very long right tail, which might represent outliers. To mitigate this issue, the main results present rates censored at the 99$^{th}$ percentile. The findings are similar censoring at the 95$^{th}$ percentile, not censoring, or using offender counts instead of rates. The estimated increases – ranging between 10 and 13% – are always statistically significant at the 99% confidence level.

**Honest bounds and synthetic difference-in-differences**: The parallel trends assumption is critical to interpreting the effects as causal. However, for youth crime incidence rates, the p-value of the joint F-test of the pre-trends is 0.02, which is below the threshold of 0.05 and hence challenges the validity of the assumption. For offending rates, the p-value of the joint F-test on the pre-trends is 0.110, which fails to reject that they might be zero at the 95% confidence level, and marginally at the 90% level.

I assess the sensitivity of these results to allowing pre-trends to differ from zero within specified bounds, following Rambachan & Roth (2023). The effects remain statistically significant at the 95% confidence level allowing deviations from linear trends 0.2 times the largest deviations observed in the pre-treatment period.

I also estimate the effects weighting the control areas to resemble the treated areas in their pre-trends using the estimator developed in Arkhangelsky et al. (2021) and generalised in Porreca (2022). The estimated rise in offending rates is 17% and the estimated rise in crime incidence is 21%. Both estimates are statistically significant at the 99% confidence level.

**Estimates of offending rates using non-linear methods**: In the main results, I estimate linear models. For offending rates, a non-negative outcome, I also calculate ATTs using Poisson quasi maximum likelihood (PQMLE). I estimate two-way-fixed-effects (TWFE) models, stacked models and saturated models as proposed by Wooldridge (2023). The estimated rise in offending using these estimators ranges from 14 to 35% and is always statistically significant at the 99% confidence level.

**Alternative explanations**: I reject that the observed effects on youth offending are driven by the diversion of police presence to affected areas. I use detection and stop-and-search rates as proxies of police activity and argue that if police were patrolling strategically, I would estimate rises in those variables. The results in Table D8 reject this alternative explanation. In fact, detection and stop-and-search rates appear to have declined in affected areas after closures. Overall detection rates fell by 4%, overall stop-and-search rates fell by nearly 21% and stop-and-search rates among minors fell by 26%.

## VII. MECHANISMS

The estimates show that youth clubs play an important role in young people's development, contributing to educational performance and averting youth crime. The effects are stronger for youths from more deprived backgrounds and for people who are younger when youth clubs closed. I argue that this might be due to youth clubs providing a safe space with unique attributes which are not easily substitutable for other activities.

First, as shown above, young people are not substituting for other structured after-school activities. But what are they doing with their time? Evidence from survey data suggests they might divert time towards cheap leisure amenities. Table 6 shows that, after closures, weekly hours on social media increase by 12%, hours playing videogames rise by 38%, and TV hours increase by 9%. Young people are not substituting time away towards more studying, as hours doing homework decline. For some pupils, this might be due to lacking a safe space and support to study (some youth clubs offer homework clubs). In fact, youths affected state that they have difficulties concentrating after closures.

I also find that other spaces are not able to mitigate the effects of youth club closures. Table 7 presents heterogeneity analyses by availability of different leisure amenities obtained from the Points of Interest database. Assessing effects across London (nearest youth club closed versus did not), I include an additional term with the interaction between being in affected areas after treatment and a dummy indicator if the area is below median distance to each type of space. Relative proximity to operative youth clubs mitigates the falls in educational performance. The fall in performance across London is 3.7%, whereas in areas that are relatively close to operative youth clubs it is only 1.1%. Proximity to operative youth clubs also mitigates rises in crime. Whereas in the average affected area the offending rate per 1,000 increases by 1.656, if an area is near operative youth clubs the rise is only 0.091. Libraries might also mitigate the effects on education slightly (the p-value on the coefficient *After* $\times$ *Near libraries* is 0.10), but not the effects on crime. Parks and sports centres do not mitigate effects in either education or crime. Some explanations are that youth clubs differ from libraries or parks in that they provide structured activities (in a safe space). They differ from sports centres in that they provide activities very cheaply.

Taken together, the results suggest that youth clubs provide a unique set of amenities. Individual testimonials often report that youth club support kept them from engaging with *the wrong people* and redirected them from potentially *harmful paths*. Other pupils reported feeling valued at the youth club, where exposure to many different activities helped them develop hobbies and new interests and improved their self-perception.

Table 6: Effect of closures on youths' time use

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | **Study time** | | **Screen time** | | |
| | Homework hours | Easy to concentrate | Social media hours | Videogames hours | TV hours |
| ATT | -1.504*** | -0.549*** | 0.413* | 0.863* | 0.373* |
| | (0.551) | (0.197) | (0.234) | (0.489) | (0.215) |
| Magnitude (%) | -32.39 | -54.87 | 12.16 | 37.63 | 8.73 |
| p-value | 0.007 | 0.006 | 0.078 | 0.079 | 0.083 |
| Mean | 4.644 | 0.004 | 3.397 | 2.293 | 4.273 |
| N | 907 | 1,071 | 1,500 | 825 | 2,079 |

*Notes:* Estimated effect of closures on time use indicators using individual-level data from Understanding Society. *Homework hours* is the sum of the number of hours a student reports doing homework on schooldays and weekends in a given week. *Easy to concentrate* is an index variable on whether a student agrees with the statement that they are easily distracted and find it difficult to concentrate: larger values mean more disagreement and the variable is standardised to have mean 0 and standard deviation of 1. The variables *social media hours*, *videogames hours* and *TV hours* refer to daily engagement with each activity on school days. Standard errors clustered at the MSOA level are in parentheses. Stars (*, **, ***) indicate significance at the 90%, 95% and 99% confidence levels, respectively.

Table 7: Effect of closures on educational performance and youth offending, by availability of other leisure amenities

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | **Test scores at ages 15–16** | | | |
| ATT | -0.037*** | -0.035*** | -0.024*** | -0.032*** |
| | (0.009) | (0.009) | (0.009) | (0.009) |
| After × Treated × Near youth club | 0.026** | | | |
| | (0.013) | | | |
| After × Treated × Near library | | 0.019 | | |
| | | (0.012) | | |
| After × Treated × Near sports centre | | | -0.006 | |
| | | | (0.011) | |
| After × Treated × Near park | | | | 0.012 |
| | | | | (0.012) |
| Mean | -0.008 | -0.008 | -0.008 | -0.008 |
| N | 590,455 | 590,455 | 590,455 | 590,455 |
| | **Offending rate at ages 10–17** | | | |
| ATT | 1.656*** | 1.021** | 0.831* | 1.070** |
| | (0.464) | (0.473) | (0.487) | (0.501) |
| After × Treated × Near youth club | -1.565** | | | |
| | (0.726) | | | |
| After × Treated × Near library | | 0.059 | | |
| | | (0.707) | | |
| After × Treated × Near sports | | | 0.545 | |
| | | | (0.721) | |
| After × Treated × Near park | | | | -0.046 |
| | | | | (0.666) |
| Mean | 15.208 | 15.208 | 15.208 | 15.208 |
| N | 36,490 | 36,490 | 36,490 | 36,490 |

*Notes:* Estimated effect of closures on test scores in national exams at ages 15–16 uses individual-level data from the NPD. *Test score in exams at ages 15–16 (standardised)* is the average score in the GCSEs standardised to mean 0 and standard deviation of 1. The regressions on offending rates use administrative records from MPS combined with population estimates from ONS, using data at the block level. *Offending rates* is defined as the residents aged 10–17 who have offended per 1,000 residents aged 10–17. The block-level estimates are weighted by resident population. The amenities come from Ordnance Survey's Points of Interest database. *Near* is defined relative to the median distance for each type of amenity across London and is 1 if distance is equal to or below the median. Standard errors clustered at the MSOA level are in parentheses. Stars (*, **, ***) indicate significance at the 90%, 95% and 99% confidence levels, respectively.

## VIII. COST–EFFECTIVENESS AND COST–EFFICIENCY OF THE CLOSURES

This section evaluates the youth club closures from a cost-effectiveness and cost-efficiency perspective. First, I provide a cost–benefit analysis. Second, I examine commuting costs and whether the closures could have been implemented in ways that minimised crime effects.

### VIII.A Cost–benefit analysis

Youth club closures led to reduced educational attainment and an increase in youth crime, which have important welfare implications by affecting individuals' future earnings, imposing additional costs on policing and the criminal justice system, and inflicting costs on victims, among others. I compare the savings from not funding youth clubs with the short-term costs of crime and the long-term costs of forgone educational returns. This analysis aims to provide a rough estimate of the magnitudes but is far from perfect. For instance, I omit important considerations such as effects on other services, social returns to education, externalities on neighbourhood value, and inflation.

I combine my estimates on the average effects of closures on education and crime across London with data from the academic and policy literature to understand the costs and benefits associated. The estimates are in Table 10 and use the identification strategy that leverages all closures (nearest closes versus nearest open, across London). The reason I do not use the estimates reported in the main results is that we are interested in the effects of all closures, not just those that left areas completely unprovided for. I base my main cost–benefit analysis in Table 8 on the ATT estimates. Additional analyses in Tables 13 and 14 use the lower and upper bounds of the 95% confidence interval, respectively, to provide plausible ranges.

**Benefits**: The short-term benefit of closures is savings in yearly running costs of youth clubs. Between 2010 and 2019, 102 youth clubs closed in London, leading to a total of 586 club–years unfunded.[16] Since the mean annual cost per club in London is approximately £170,000 according to FOI data and a survey among youth workers, the estimated savings amount to £99.62 million (586 × £170,000).[17]

[16] Out of the 102 youth clubs, 85 were open in 2010 and 17 opened after 2010 and later closed.

[17] The figure is within a reasonable ballpark. For instance, aggregate data indicate that spending on youth services fell by £150 million from 2010 to 2019 in London. The £150 million would also include

**Costs due to educational losses**: Hodge et al. (2021) estimate that a 1 s.d. increase in exam scores at ages 15–16 is associated with a £96,111 rise in the present value of lifetime earnings. I estimate that the closures resulted in a 2.6% s.d. decline in test scores, which implies an expected loss of £2,499 (rounded) in lifetime earnings per pupil (96,111 × 0.026). Since 75,608 pupils live near closed clubs in the period of analysis, the total forgone earnings are nearly £190 million (75,608 × £2,499). These forgone earnings imply long-run private costs, but also some losses to public finances. Assuming that each affected individual would have been taxed at the average rate of 23.7% on their income (OECD 2024), the forgone returns imply nearly £45 million lost to public finances in tax collection in net present value. This is almost half of the savings.

**Costs due to increased crime**: The rises in offending imply substantial costs to public funds associated with the criminal justice system, which include the costs of police, courts, offender management and custody. The average offender costs £8,000 according to National Audit Office (2011). My estimates suggest that closures resulted in an additional 1.250 young offenders per 1,000 per area, representing an 8% increase over the mean. Since there were 16,116 young offenders in areas and years affected, 1,289 additional offenders (16,116 × 0.08) are attributable to the closures. Multiplying by £8,000 yields costs of £10 million.

The rises in crime also imply costs to victims. In my analyses, I estimate increases in violence and acquisitive crimes. Heeks et al. (2018) estimate that each violent incident costs approximately £11,446 in crime prevention, property damage, health services, physical and emotional harm, lost output and victim support.[18] I estimate an 8.3% increase in violent crime rates in affected areas. Since there were 5,127 violent crimes by young offenders in treated areas, 426 can be attributed to closures (5,127 × 0.083). This means nearly £5 million in costs to violent crime victims (426 x 11,446).

For acquisitive offences, my estimates indicate an increase of 1.413 crimes per 1,000 population per area and year, representing a 16.7% rise over the mean. In affected areas, there were 7,210 acquisitive crimes; hence approximately 1,204 of those are attributable to youth club closures (7,210 × 0.167). This has an estimated cost of nearly £5 million to victims using the average cost of £4,093 from Heeks et al. (2018).

reductions to the funding of clubs that remained open and to services such as targeted provision (not only open-access youth clubs).

[18] To obtain the average cost I compute the weighted average of the costs of homicide, violence with injury and violence without injury, weighting by their prevalence.

Beyond these detected crimes, there are likely additional undetected offences attributable to closures. In the administrative records, the detection rate for violence is 23.5%. If the 5,127 detected violent crimes represent only 23.5% of violent crimes committed by youths, the actual violent crimes attributable to closures could be 4.25 times larger (100/23.5). This has substantial costs, of more than £25 million. For acquisitive crimes, detection rates are as low as 9.1%. Assuming the observed crimes are 9.1% of the total, the actual cost to victims of acquisitive crimes could be as high as £59 million.[19] The total crime costs might be more than £95 million. This excludes potential costs of future crimes.

The closures result in net losses of £184 million. For every £1 saved, the costs to affected users, crime victims and public spending in the police and the criminal justice system amount to £2.85. The majority, £1.90, is due to educational losses and £0.95 is due to crime costs. As a comparison, a policy report by Frontier Economics and UK Youth (2022) estimates that returns on investment to youth clubs are between £3.20 and £6.40, considering different data and using a different methodology. Hence, my estimates are lower than those from youth organisations, but within a reasonable ballpark.

I also present an approximation of the marginal value of public funds (MVPF) (Finkelstein & Hendren 2020), by computing societal losses over public savings. In the numerator, I include forgone returns to education after tax and costs of violent and acquisitive crimes. In the denominator, I include savings from closures, forgone tax revenue and incurred costs in the criminal justice system. For every £1 saved by the government, there are associated losses of £5.13 for the general public.

In Tables 13 and 14, I present the lower and upper bounds of these estimates. The total costs over total benefits range from 0.90 to 4.43. The MVPF ranges from -0.88 to -23.02.

[19] Summary statistics show comparable detection rates in treated and control areas at baseline. The results from Table D8 suggest that detection rates decreased following closures, which might imply that the additional undetected crimes could be higher.

Table 8: Cost–benefit analysis of youth club closures

| | | Most Likely Scenario | |
|---|---|---|---|
| | Public vs private | **Calculation** | **Value (£)** |
| **Benefits** | | | |
| Savings from not spending on clubs | Public | *586 × 170,000* | 99,620,000 |
| **Costs due to educational losses** | | | |
| NPV forgone returns | Private | *75,608 × 0.026 × 96,111* | -188,935,773 |
| NPV forgone tax revenue | Public | Forgone returns × 0.237 | -44,777,778 |
| **Costs due to increased crime** | | | |
| Public spending in criminal justice system | Public | *16,116 × 0.080 × 8,000* | -10,283,972 |
| Detected violent crime, costs to victims | Private | *5,127 × 0.083 × 11,446* | -4,870,058 |
| Potentially undetected vio. crime, costs to victims | Private | *5,127 × 0.083 × 11,446 × 4.25* | -20,697,747 |
| Total costs in violent crimes | Private | *Detected + undetected* | -25,567,805 |
| Detected acquisitive crimes, costs to victims | Private | *7,210 × 0.167 × 4,093* | -4,930,042 |
| Potentially undetected acqu. crime, costs to victims | Private | *7,210 × 0.167 × 4,093 × 10.98* | -54,131,861 |
| Total costs in acquisitive crimes | Private | *Detected + undetected* | -59,061,903 |
| **Net benefit** | | | |
| Net benefit to private individuals | Private | | -228,787,702 |
| Net benefit to public funds | Public | | 44,558,250 |
| **Cost/Benefit** | | *Total costs / Total savings* | 2.85 |
| Education costs over Savings | | | 1.90 |
| Crime costs over Savings | | | 0.95 |
| **MVPF** | | *Total private costs / Total public savings* | 5.13 |

*Notes:* Estimated costs and benefits of youth club closures. Estimates on the yearly running costs of youth clubs come from FOI data and surveying youth workers. The numbers in orange are estimated coefficients on the effects of closures across London from Table 10. Estimates on forgone returns to education are from Hodge et al. (2021), estimates on cost of each young offender are from National Audit Office (2011) and estimates on the costs of crime are from Heeks et al. (2018).

## VIII.B Commuting costs and counterfactual closures

The closure of youth clubs dampened educational performance and increased crime, but in those areas that had alternative operative youth clubs the detrimental effects were partially mitigated. This suggests that commuting costs play a role in people's decisions around participation in after-school activities and that having a core spatial provision of clubs could be beneficial.

In this section, I explore counterfactual closures and whether these might have been welfare-improving. First, I estimate a discrete choice model of after-school time use to recover the structural cross-price elasticity between proximity to youth clubs and alternatives (such as engaging in crime). Second, I compute p-median models to find which youth clubs minimise commuting time across space, conditional on only being able to finance a limited number of youth clubs. Third, I use the cross-price elasticity to assess how different closing regimes would have affected crime rises.

Suppose the underlying behavioural model determining after-school time use is as follows. Each individual $n$ chooses the option $j$ that maximises utility $V$ in each period $t$. They can choose to attend youth clubs (option 1) or to do something else (option 0). Utility is linear. The value of attending youth clubs is determined by individual attributes (observed in vector $X$ and unobserved in $\mu_n$), distance to the nearest youth club $d$, other block-level unobserved attributes $\mu_i$ and other unobserved yearly characteristics $\mu_t$. The value of doing something else (the alternative to youth clubs) is normalised to 0.

$$U_{n1t} = V_{n1t} + \varepsilon_{n1t}$$

$$U_{n0t} = \varepsilon_{n0t} \quad j = 0$$

Assuming the error term $\varepsilon$ is i.i.d. extreme value distributed over individuals $n$, alternatives $j$ and time $t$, the conditional choice probability of choosing option $j$ is given by the closed-form expression below (McFadden 1989). This corresponds to a logistic regression which can be estimated using longitudinal survey data on attendance at organised activities from Understanding Society. The variation of interest is given by individuals who appear more than once in the data and were exposed to some closures.

The probability of choosing option 1 is given by

$$P_{n1t} = \frac{\exp(V_{n1t})}{1 + \exp(V_{n1t})}$$

where:

$$V_{n1t} = \mu_n + \mu_i + \mu_t + \xi X_{nt} + \alpha d_{nt}$$

Table 15 estimates the elasticity between attendance and distance to the youth club. The elasticity is 1.01 using an indicator for yearly attendance and 0.69 using an indicator for weekly attendance, suggesting that heavy users have a lower elasticity (are willing to travel more).

The probability of attending activities at least yearly was 0.412. This implies that the probability of not attending (choosing the alternative) yearly was 0.588 (1–0.412). As the cost of attending youth clubs increases, alternative time-use options become more attractive. Suppose this alternative is either to commit a crime or to do something else. Assuming these alternatives are independent and act as substitutes (i.e. the IIA assumption holds), the cross-price elasticity with respect to youth club attributes can be inferred with information on the proportions choosing each activity at baseline and the estimated own-elasticity of youth clubs.

Let us assume that of those who did not attend youth clubs in a given year, 1 per 1,000 committed crimes (the observed rate in the population). The rest neither did crime nor attended youth clubs. The cross-price elasticity with respect to crime (option $c$) is given by

$$E_{c,1} = -1.01 \times 0.01 \times 0.412 \approx 0.42\%$$

A 1% increase in distance to the youth club would result in a 0.42% increase in crime participation rates. Given that the average distance to youth clubs increased by 3.58 minutes over the decade – a 14% rise from the mean distance of 24.90 minutes – crime participation rates in London are estimated to have increased by 5.98% due to closures (calculated as the cross-price elasticity $\times$ 14).

Next, I evaluate how commuting increased under different closing regimes and portray estimated changes in crime in Table 9. By the end of 2019, there were 230 youth clubs open in London and 85 youth clubs had been closed since 2010 (102 closures in total counting those which had opened since 2010 and also closed). First, I calculate the average commute after closing youth clubs randomly (1,000 repetitions), which leads to an increase in the average commuting time of 4.29 minutes (or 17% more relative to the baseline before austerity). The random closures are only slightly better than the observed closures in terms of their effects on accessibility. Under random closures, crime would have increased by an estimated 7.17%.

Second, I compare these with the closures that would have minimised average commuting across London. These are computed using the p-median model (Church & ReVelle 1974) and linear programming. A detailed explanation is available in Appendix E, where I also include maps with the optimal spatial location of youth clubs as identified in these exercises. Optimal closures would have mitigated crime rises almost entirely, with youth crime increasing by less than 1%.

Third, I hypothesise how crime might change if many more youth clubs were to close, such that the average travel time would be 40 minutes on foot. Under this hypothetical scenario, youth crime would rise by a notable 25%.

The closure of youth clubs was not cost-efficient. Assuming that closed clubs cost the same as the clubs that remained open implies that local authorities could have closed the same number of clubs without bearing the costs.

Table 9: Closing regimes – change in commute times and youth offending rates

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **Scenario** | **Avg. commute** | **$\Delta$ mins** | **%$\Delta$ mins** | **$\Delta$ offending 10–17** |
| | | | | %$\Delta$ mins $\times$ 0.42 |
| Baseline | 24.90 | | | |
| Austerity | 28.48 | 3.58 | 14% | 5.98% |
| Random | 29.19 | 4.29 | 17% | 7.17% |
| Optimal | 25.36 | 0.46 | 2% | 0.77% |
| Close more | 40.00 | 15.10 | 61% | 25.24% |

*Notes:* Column 1 is the average commuting time across London blocks under different closing regimes. Column 2 shows the relative change in commute with respect to the baseline. Column 3 shows the proportional change in commute with respect to the baseline. Column 4 infers the proportional change in crime across London under different regimes using the inferred cross-price elasticity. Austerity represents the real observed policy, where 230 youth clubs were open by the end of 2019. Random shows the average of 1,000 random closing regimes which would have maintained open 230 centres as of December 2019. The optimal exercise was computed using the p-median model, which aims at minimising commuting time between demand nodes (block centroids) and youth clubs maintaining the real youth club network, but limiting the maximum facilities to only 230. The last row extrapolates what might happen if youth clubs closed such that the average minimum commute across London would become 40 minutes.

# IX. CONCLUSIONS

Youth clubs are after-school programmes which provide free leisure activities to individuals aged 10 to 19 years, typically in community-based spaces. This study presents the first causal estimates of their effects on education and crime, using evidence from London from 2010 to 2019.

I leverage a hand-constructed spatial database of youth clubs in London, survey data and administrative records, among other data sources. The setting allows me to exploit variation in youth club availability induced by austerity cuts to youth service funding, which resulted in the closure of 30% of youth clubs. I compare residents affected by closures with those unaffected in difference-in-differences models and assess the welfare effects of the closures.

After closures, residents are much less likely to attend organised after-school activities, by 44%. The closures lead to worse educational performance in exams at ages 15–16, by 4% of a standard deviation. The effects are larger for lower-income pupils as proxied by their free school meals status. In affected areas, minors became 14% more likely to commit crimes and committed 15% more crimes. The results were driven by the most common youth crime categories: acquisitive, violent and drug offences. I find crime rises across all hours of the day, suggesting that the effects of youth clubs on crime are not limited to pure incapacitation.

These estimates are not driven by general austerity. Triple difference-in-differences models using younger or older individuals as a second control group show that teenagers were differentially affected by the youth club closures. The effects are not driven by changes in crime reporting and are robust to several changes in estimation and assumptions.

These effects might be explained by youth clubs supporting young people in ways that are not easily substitutable, combining mentorship, a safe space to study and multiple activities. After closures, youths spend more time on screens. Proximity to parks, sports centres or libraries does not mitigate the effects.

Under reasonable assumptions, the savings from not funding youth clubs are outweighed by the costs of increased crime and forgone educational returns. For every £1 saved by closing youth clubs, there are societal costs of £2.85. The closures were also inefficient and considering commuting costs to ensure some areas were not entirely deprived of youth clubs could have mitigated the negative impacts.

A limitation of this study is that London may have unique characteristics. For example, lower-income households are spread across the city and not restricted to a few areas. This may limit the external validity of the findings. However, youth clubs in London are not substantially different from those in the rest of the UK and they resemble similar initiatives worldwide. The views of youth workers on the sector's evolution are similar across the UK. Thus, I believe the results are relevant to UK policy and offer a useful starting point for understanding effects in other regions.

Given the lasting impact of crime on various outcomes, such as labour market prospects and future violence, youth club closures are likely to exacerbate inequality in the long run. Public investments in after-school activities support education and reduce crime in deprived communities and hence contribute to a more equitable society.

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## Additional tables and figures

Table 10: Effect of closures on attendance at organised activities, educational performance, and youth offending – all closures

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | **P (attends activ.)** | **Test score** | **Offending rate** | **Drug offences** | **Violence inc. rate** | **Acquisitive rate** |
| | ages 10–15 | ages 15–16 | ages 10–17 | ages 10–17 | ages 10–17 | ages 10–17 |
| *Panel A: TWFE* | | | | | | |
| ATT | -0.153** | -0.026*** | 1.048*** | 0.248 | 0.150 | 1.483*** |
| | (0.063) | (0.007) | (0.395) | (0.185) | (0.183) | (0.374) |
| Magnitude (%) | -44.82 | | 6.89 | 5.43 | 3.51 | 18.45 |
| $\widehat{TOT}$ | | -5.80 | 15.37 | 12.12 | 7.83 | 41.16 |
| p-value | 0.015 | 0.000 | 0.008 | 0.179 | 0.412 | 0.000 |
| Mean | 0.341 | -0.008 | 15.208 | 4.567 | 4.286 | 8.040 |
| N | 1,794 | 590,455 | 36,490 | 36,490 | 36,490 | 36,490 |
| *Panel B: Stacked* | | | | | | |
| ATT | -0.119** | -0.024*** | 1.250*** | 0.174 | 0.360* | 1.413*** |
| | (0.057) | (0.008) | (0.411) | (0.177) | (0.186) | (0.372) |
| Magnitude (%) | -36.45 | | 7.98 | 3.65 | 8.30 | 16.71 |
| $\widehat{TOT}$ | | -6.58 | 21.88 | 10.02 | 22.77 | 45.83 |
| p-value | 0.038 | 0.002 | 0.002 | 0.326 | 0.054 | 0.000 |
| Mean | 0.327 | -0.008 | 15.670 | 4.768 | 4.342 | 8.458 |
| N | 11,891 | 3,618,957 | 236,960 | 236,960 | 236,960 | 236,960 |

*Notes:* Column 1 estimates effects on attendance using survey data from Understanding Society, available from ages 10 to 15. *P (attends organised activ.)* is a binary indicator which is 1 if an individual states attending monthly or more. Column 2 estimates test scores using the NPD. *Test score ages 15–16* is the average score across all subjects in the GCSEs standardised to mean 0 and standard deviation of 1. Columns 3 to 6 estimate crime rates using administrative records from MPS combined with population estimates from ONS. *Offending rate* is defined as the residents aged 10–17 who have offended per 1,000 residents aged 10–17. *Drug offences, violence inc. rate* and *acquisitive rate* refer to crimes committed by residents aged 10–17 per 1,000 residents aged 10–17 (by type). Columns 1 and 2 are estimated at the individual level; columns 3 to 6 are estimated at the block level. The block-level estimates are weighted by resident population. All columns include controls, and block and year fixed effects. Panel A presents estimates using TWFE and panel B reports the estimated effects using stacked designs. Treated areas are those where the nearest youth club closed and control those where the nearest youth club remained open. Standard errors clustered at the MSOA level are in parentheses. Stars (*, **, ***) indicate significance at the 90%, 95% and 99% confidence levels, respectively.

Table 11: Effect of openings on educational performance and youth offending rates

| | (1) | (2) | (3) |
|---|---|---|---|
| | | **Test scores at ages 15–16** | |
| ATT | 0.033* | 0.056** | -0.031 |
| | (0.018) | (0.025) | (0.032) |
| Weight | - | Prop. score | Inverse prop. score |
| p-value | 0.073 | 0.026 | 0.080 |
| Mean | 0.089 | 0.089 | 0.328 |
| N | 127,503 | 127,503 | 127,503 |
| | | **Offending rates at ages 10–17** | |
| ATT | -3.250** | -3.505** | -6.770 |
| | (1.271) | (1.479) | (6.969) |
| Magnitude (%) | -29.77 | -32.11 | -62.02 |
| Weight | Population | Prop. score | Inverse prop. score |
| p-value | 0.011 | 0.018 | 0.332 |
| Mean | 10.916 | 10.916 | 10.916 |
| N | 9,000 | 9,000 | 9,000 |

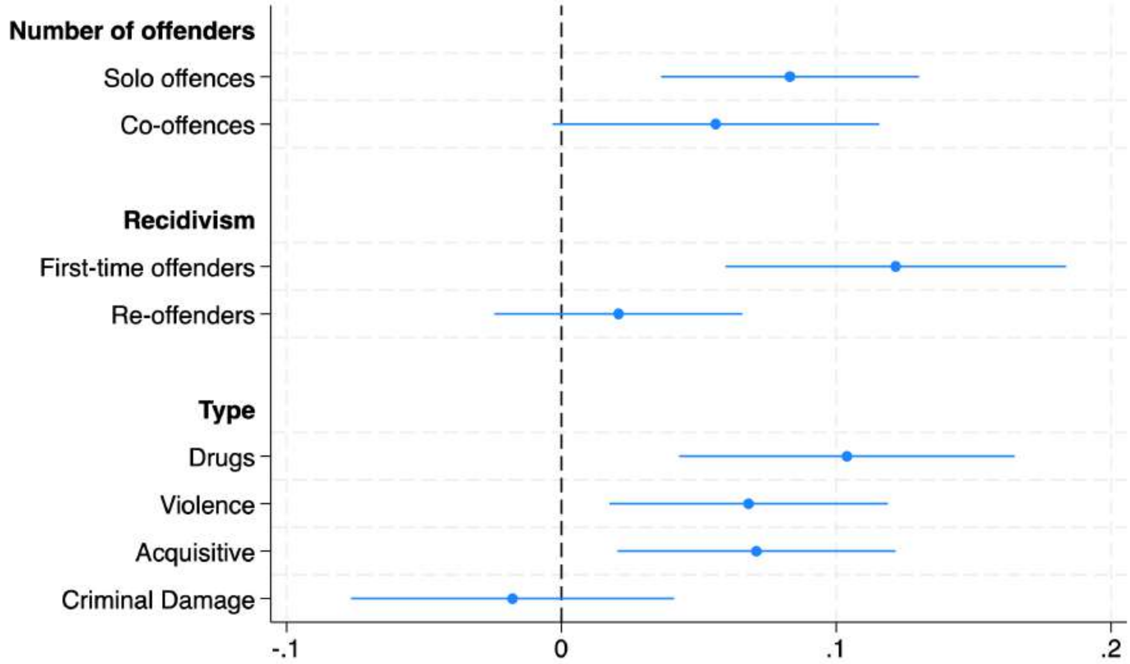
*Notes:* Estimated effect of openings on areas with new youth clubs against areas with no youth clubs. Education data from NPD. *Test scores at ages 15–16* is the average score across all subjects in the GCSEs standardised to mean 0 and standard deviation of 1. *Offending rate* comes from MPS combined with population estimates from ONS and is defined as the residents aged 10–17 who have offended per 1,000 residents aged 10–17. Column 1 weights areas by population in the offending estimates. Column 2 weights areas by the propensity similarity between treated and control. Column 3 weights areas by their inverse propensity score. Standard errors clustered at the MSOA level are in parentheses. Stars (*, **, ***) indicate significance at the 90%, 95% and 99% confidence levels, respectively.

Table 12: Effect of closures on the spatial distribution of crimes – changes in sample radii

| | (1) | (2) | (3) |
|---|---|---|---|
| | | Local crime rates | |
| ATT | -0.281 | 0.418 | -0.226 |
| | (1.783) | (1.303) | (0.994) |
| Sample | 20 minutes | 30 minutes | 50 minutes |
| Magnitude (%) | -1.76 | 2.74 | -1.58 |
| p-value | 0.875 | 0.749 | 0.820 |
| Mean | 15.949 | 15.246 | 14.320 |
| N | 8,520 | 16,070 | 22,430 |

*Notes:* Estimated effect of closures on local crime rates. *Local crime* is defined as the number of offences occurring per 1,000 resident population. Crime data come from administrative records from MPS combined with population estimates from ONS and the regressions are estimated at block level. Different radii relative to block centroid. The block-level estimates are weighted by resident population. Standard errors clustered at the MSOA level are in parentheses. Stars (*, **, ***) indicate significance at the 90%, 95% and 99% confidence levels, respectively.

Figure 5: Effect of closures on adult offending, by crime attributes




*Notes:* Estimated effects on offending rates of adults aged 35 and over by crime attributes using administrative records from MPS combined with population estimates from ONS, at the block level. *Offending rates* are defined as the crimes by type committed by residents aged 35 and above per 1,000 residents aged 35 and above. The variables are standardised to mean 0 and standard deviation 1. Confidence intervals are calculated using MSOA-level clustered standard errors.

Table 13: Cost–benefit analysis of youth club closures – lower bound

| | Public vs private | Optimistic Scenario<br>**Calculation** | **Value (£)** |
|---|---|---|---|
| **Benefits** | | | |
| Savings from not spending on clubs | Public | *586 × 170,000* | 99,620,000 |
| **Costs due to educational losses** | | | |
| NPV forgone returns | Private | *75,608 × 0.008 × 96,111* | -58,134,084 |
| NPV forgone tax revenue | Public | Forgone returns × 0.237 | -13,777,778 |
| **Costs due to increased crime** | | | |
| Public spending in criminal justice system | Public | *16,116 × 0.028 × 8,000* | -3,640,859 |
| Detected violent crime, costs to victims | Private | *5,127 × -0.001 × 11,446* | 76,536 |
| Potentially undetected vio. crime, costs to victims | Private | *5,127 × -0.001 × 11,446 × 4.25* | 325,279 |
| Total costs in violent crimes | Private | *Detected + undetected* | 401,815 |
| Detected acquisitive crimes | Private | *7,210 × 0.081 × 4,093* | -2,383,117 |
| Potentially undetected acqu. crime | Private | *7,210 × 0.081 × 4,093 × 10.98* | -26,166,623 |
| Total costs in acquisitive crimes | Private | *Detected + undetected* | -28,549,739 |
| **Net benefit** | | | |
| Net benefit to private individuals | Private | | -72,504,230 |
| Net benefit to public funds | Public | | 82,201,363 |
| **Cost/Benefit** | | *Total costs / Total savings* | 0.90 |
| Education costs over Savings | | | 0.58 |
| Crime costs over Savings | | | 0.32 |
| **MVPF** | | *Total private costs / Total public savings* | 0.88 |

*Notes:* Estimated costs and benefits of youth club closures. Estimates on the yearly running costs of youth clubs come from FOI data and surveying youth workers. The numbers in orange represent the lower bounds (in absolute value) of the 95% confidence intervals for the coefficients on the effects of closures across London, as shown in Table 10. Estimates on forgone returns to education are from Hodge et al. (2021), estimates on the cost of each young offender are from National Audit Office (2011) and estimates on the costs of crime are from Heeks et al. (2018).

Table 14: Cost–benefit analysis of youth club closures – upper bound

| | Public vs private | Pessimistic Scenario<br>**Calculation** | **Value (£)** |
|---|---|---|---|
| **Benefits** | | | |
| Savings from not spending on clubs | Public | *586 × 170,000* | 99,620,000 |
| **Costs due to educational losses** | | | |
| NPV forgone returns | Private | *75,608 × 0.039 × 96,111* | -283,403,659 |
| NPV forgone tax revenue | Public | Forgone returns × 0.237 | -67,166,667 |
| **Costs due to increased crime** | | | |
| Public spending in criminal justice system | Public | *16,116 × 0.131 × 8,000* | -16,927,085 |
| Detected violent crime, costs to victims | Private | *5,127 × 0.167 × 11,446* | -9,816,652 |
| Potentially undetected vio. crime, costs to victims | Private | *5,127 × 0.167 × 11,446 × 4.25* | -41,720,773 |
| Total costs in violent crimes | Private | *Detected + undetected* | -51,537,425 |
| Detected acquisitive crimes | Private | *7,210 × 0.253 × 4,093* | -7,476,967 |
| Potentially undetected acqu. crime | Private | *7,210 × 0.253 × 4,093 × 10.98* | -82,097,099 |
| Total costs in acquisitive crimes | Private | *Detected + undetected* | -89,574,066 |
| **Net benefit** | | | |
| Net benefit to private individuals | Private | | -357,348,483 |
| Net benefit to public funds | Public | | 15,526,247 |
| **Cost/Benefit** | | *Total costs / Total savings* | 4.43 |
| Education costs over Savings | | | 2.84 |
| Crime costs over Savings | | | 1.59 |
| **MVPF** | | *Total private costs / Total public savings* | 23.02 |

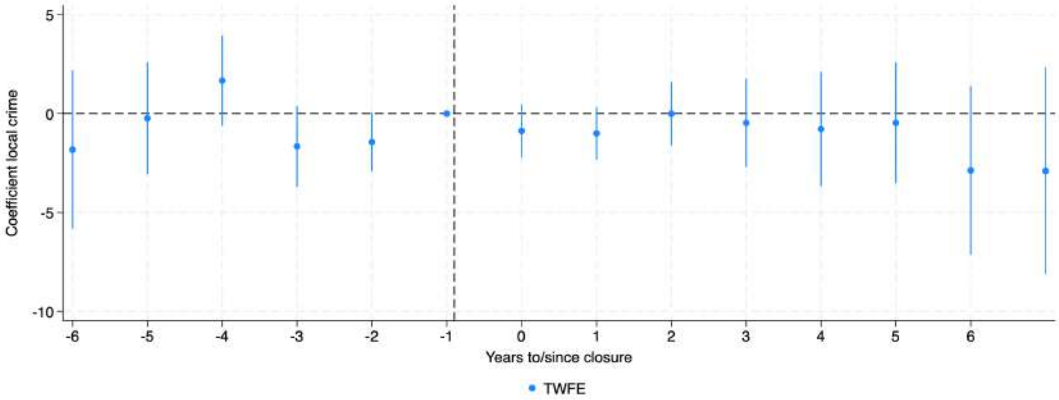
*Notes:* Estimated costs and benefits of youth club closures. Estimates on the yearly running costs of youth clubs come from FOI data and surveying youth workers. The numbers in orange represent the upper bounds (in absolute value) of the 95% confidence intervals for the coefficients on the effects of closures across London, as shown in Table 10. Estimates on forgone returns to education are from Hodge et al. (2021), estimates on the cost of each young offender are from National Audit Office (2011) and estimates on the costs of crime are from Heeks et al. (2018).

Table 15: Proximity to youth clubs and attendance at organised activities

| | (1) | (2) | (3) |
|---|---|---|---|
| | P(attends yearly or more) | P (attends monthly or more) | P (attends weekly or more) |
| Commuting time (mins) | -0.067*** | -0.057** | -0.044* |
| | (0.025) | (0.024) | (0.025) |
| Elasticity (%) | -1.01 | -0.91 | -0.69 |
| p-value | 0.008 | 0.018 | 0.079 |
| Mean in population | 0.412 | 0.351 | 0.307 |
| N | 181 | 207 | 179 |

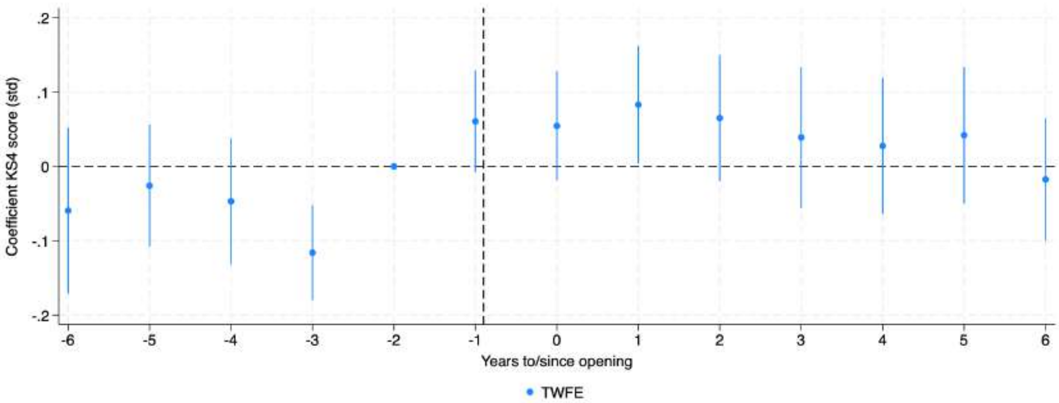
*Notes:* Relationship between commuting time to centres and attendance at organised activities estimated from logit regressions. The dependent variables are binary indicators which are 1 if an individual states attending (at least) yearly, monthly or weekly, respectively. The regressions include age, individual fixed effects and year fixed effects. The mean is across population, not in the N on which regressions are estimated. Standard errors are heteroskedasticity-robust. Stars (*, **, ***) indicate significance at the 90%, 95% and 99% confidence levels, respectively.

Figure 6: Event study – effect of closures on the spatial distribution of crime



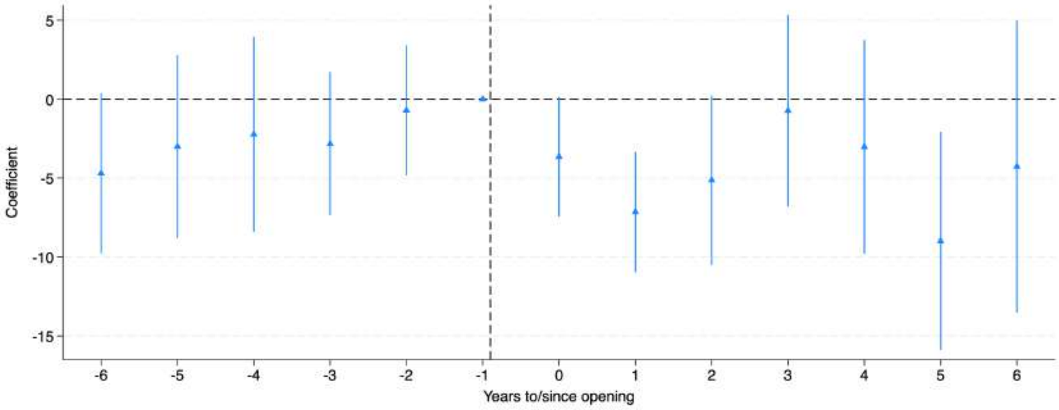

*Notes:* Crime data from administrative records from MPS combined with population estimates from ONS. *Local crime* is defined as the number of offences occurring per 1,000 resident population. Confidence intervals are calculated using MSOA-level clustered standard errors.

Figure 7: Event study – effect of openings on education performance at ages 15–16




*Notes:* Education data from NPD. *Test scores at ages 15–16* is the average score across all subjects in the GCSEs standardised to mean 0 and standard deviation of 1. Coefficients weighted by propensity scores assessing similarity between areas with openings and areas without youth clubs and without openings. Confidence intervals are calculated using MSOA-level clustered standard errors.

Figure 8: Event study – effect of openings on offending rates at ages 10–17



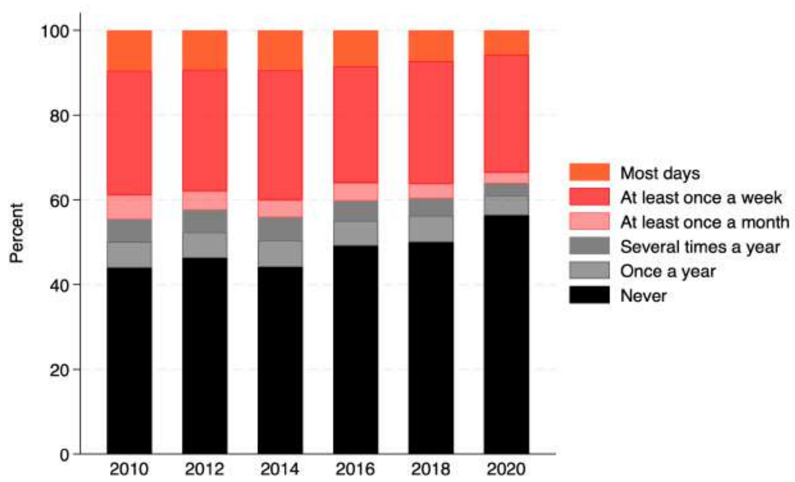

*Notes:* Crime data from administrative records from MPS combined with population estimates from ONS. *Offending rate* is defined as the residents aged 10–17 who have offended per 1,000 residents aged 10–17. Coefficients weighted by propensity scores assessing similarity between areas with openings and areas without youth clubs and without openings. Confidence intervals are calculated using MSOA-level clustered standard errors.

## Supplementary appendices

### A. Attendance at organised activities and youths' attributes

This appendix presents the correlation between attendance at after-school activities and various time-use and socio-economic indicators, using survey data from Understanding Society. I leverage waves a to l in the youth questionnaire. The distribution of the answers on attendance at organised activities over time is in Figure A1.[20]

Figure A1: Participation in organised after-school activities




*Notes:* Distribution of responses to the question: *How often do you go to youth clubs, scouts, girl guides or other organised activities?* in waves a to l in Understanding Society. The survey includes youths aged 10 to 15 living in the Greater London area, from 2010 to 2020.

Table A1 shows the difference in selected variables between youths who attend organised activities at least yearly and youths who do not. This variable captures a larger group than the variable in the main estimates and aims to provide insights on the entire population of youth club attendees, not just regular users. People responding that they attend spend less time on screens, do similar amounts of homework, have more friends and report being more satisfied with their lives. They are also slightly less likely to report having consumed alcohol in their lives and do sports more often. Youths who attend organised activities tend to come from slightly richer families (their parents earn an additional £235 per month) and are more likely to have working mums.

[20]Organised activities include but are not restricted to youth clubs, and hence can include paid-for after-school activities or other clubs such as the Scouts or Guides.

Table A1: Differences in socio-economic and behavioural attributes between youths who attend organised activities and those who do not

| | **Attends organised activities** | | |
|---|---|---|---|
| | No | Yes | Diff |
| Hours watching TV schoolday | 3.90 | 3.73 | -0.17*** |
| | (0.02) | (0.02) | (0.02) |
| Hours social media schoolday | 3.65 | 3.38 | -0.27*** |
| | (0.02) | (0.02) | (0.03) |
| Hours videogames schoolday | 2.78 | 2.35 | -0.43*** |
| | (0.07) | (0.08) | (0.11) |
| Hours homework schoolday | 2.34 | 2.34 | -0.00 |
| | (0.03) | (0.04) | (0.05) |
| Ever alcohol | 0.28 | 0.22 | -0.06*** |
| | (0.00) | (0.00) | (0.00) |
| Days sports per week | 3.24 | 3.92 | 0.68*** |
| | (0.02) | (0.02) | (0.04) |
| Number of friends | 7.33 | 8.17 | 0.85*** |
| | (0.20) | (0.26) | (0.33) |
| Feel about life (std) | -0.07 | 0.08 | 0.15*** |
| | (0.01) | (0.01) | (0.01) |
| N | 16,257 | 14,685 | 30,942 |
| Parental take home monthly pay | 1750.13 | 1984.73 | 234.59*** |
| | (48.89) | (50.30) | (70.68) |
| Working mum (dummy) | 0.59 | 0.67 | 0.08*** |
| | (0.00) | (0.00) | (0.01) |
| N | 7,608 | 8,817 | 16,425 |

*Notes:* Mean and standard deviation of selected variables for youths who attend organised activities and youths who do not, as well as their difference and standard error of the difference. Stars (*, **, ***) indicate significance at the 90%, 95% and 99% confidence levels, respectively. The data come from Understanding Society, waves a to l. The column 'No' shows mean values for people who replied *never* or *almost never* to the question *How often do you go to youth clubs, scouts, girl guides, or other organised activities?*. The column 'Yes' shows mean values for those who responded *several times a year*, *several times a month*, *at least once a week* or *most days*.

## B. Youth clubs and youth club users in London

This appendix includes additional information on the characteristics of youth clubs in London and the attributes of youth club users. The information was gathered from Freedom of Information requests, my own survey among youth workers, and available details online.

Table B1 summarises the attributes of London's youth clubs. Figure B1 presents a word cloud of the activities advertised by youth clubs on their websites. The sample for which online information was available was mostly youth clubs that were open by the end of 2019.

Youth club users tend to come from more deprived backgrounds, and have healthier time-use habits. Figure B2 provides log odds ratios computed using the Young Londoners' Survey, which included a question that specifically asked about youth club usage, and which was conducted in 2009 (before the closures explored in this study). Youth club usage correlates with more homework time, more time in libraries, more time outdoors and more time on sports. It correlates negatively with time watching TV or using a computer. Youths attending clubs are more likely to self-identify as Black and more likely to come from households with higher indices of deprivation.

Table B1: Attributes of London's youth clubs

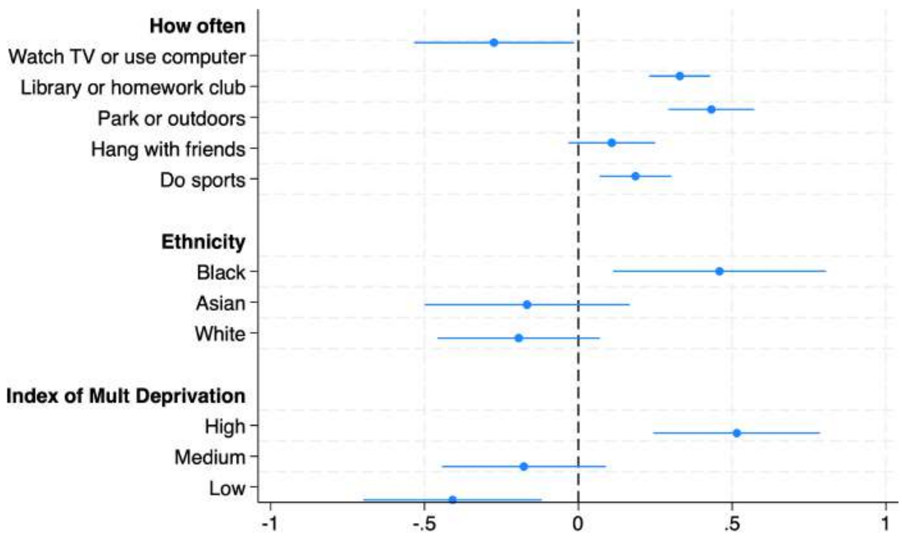
| | Mean | SD | Range | N |
|---|---|---|---|---|
| **Management** | | | | |
| Charity managed (dummy) | 0.42 | 0.49 | 0-1 | 335 |
| Yearly spending (GBP) | 169,567 | 187,992 | 32,500-610,523 | 18 |
| **Opening** | | | | |
| Year club opened | 1997 | 19.84 | 1929-2019 | 75 |
| Year club closed | 2014 | 2.20 | 2009-2019 | 105 |
| Opening hour | 13:00 | 6 hours | 10:00 - 19:00 | 238 |
| Closing hour | 19:30 | 4 hours | 19:00-23:00 | 238 |
| **Visitors** | | | | |
| Minimum age | 10 | 2.33 | 4-16 | 232 |
| Maximum age | 19 | 2.27 | 11-25 | 233 |
| Number of yearly visits | 5,031 | 6,943 | 2,046 - 25,681 | 25 |
| **Activities** | | | | |
| Sports activities (dummy) | 0.65 | 0.48 | 0 - 1 | 235 |
| Videogames/IT activities (dummy) | 0.22 | 0.42 | 0-1 | 240 |
| Music workshops (dummy) | 0.12 | 0.32 | 0-1 | 240 |
| Homework support (dummy) | 0.16 | 0.36 | 0-1 | 339 |
| **Building attributes** | | | | |
| Building post-1979 (dummy) | 0.13 | 0.34 | 0-1 | 285 |
| Building 1960 – 1979 (dummy) | 0.09 | 0.28 | 0-1 | 285 |
| Building 1945 – 1959 (dummy) | 0.24 | 0.43 | 0-1 | 285 |
| Building 1918 – 1944 (dummy) | 0.13 | 0.34 | 0-1 | 285 |
| Building pre-1918 (dummy) | 0.41 | 0.49 | 0-1 | 285 |

*Notes:* Characteristics of youth clubs gathered through FOI requests and an online search of each youth club in London. Total number of youth clubs in sample is 339. Information was more readily available for clubs that were still open as of 2019. The median opening hour was 15:00 and the median closing hour was 20:30.

Figure B1: Word cloud of advertised activities in youth clubs


*Notes:* Activities and amenities in youth clubs from online information available for 277 youth clubs.

Figure B2: Log odds ratios of youth club usage and individual characteristics




*Notes:* Correlates of youth club usage and youth attributes from logit regressions, expressed in log odds ratios. The data come from the Young Londoners' Survey, which took place in 2009. The dependent variable is an index from 0 to 4 indicating frequency of attendance (higher values indicate more frequent attendance). The variables on other behavioural aspects are also indexes from 0 to 4. The variables on ethnicity and index of multiple deprivation are dummy indicators.

### B.A Insights from surveying youth workers

I conducted an online survey using Qualtrics, which was disseminated with the help of various youth organisations across the UK among adults. The survey ran during September 2024 and 40 people completed the survey, of whom 33 were youth workers and 7 were people who used to attend youth clubs. Respondents lived in all areas of the UK.

According to youth workers, the average cost of running clubs per year across the UK was £87,333. However, the range was very wide, and several respondents pointed out that costs varied with the type of activities offered and the number of days the club was open. The survey also asked about total users per year for each youth club. Dividing the reported cost per youth club by the number of users yielded an average cost of £354 per user per year.

Nearly all youth workers stated that funding had been affected in recent years. Only one said it had not because their organisation did not rely on public funds from local authorities or grants. Free-text answers consistently reported funding drops leading to lower numbers of youth clubs and effects through labour markets as there is more unemployment among youth workers and difficulty attracting talent due to the low prospects in the profession. Several respondents also pointed out that, due to the cuts, they could no longer provide one-to-one support or fund trips. The majority (66%) responded that they never had to turn away people due to capacity issues.

Most youth club workers reported that their club offered a chill-out area, mentorship programmes, sports activities and board games. According to youth workers, the most important activities were having a chill-out space and mentorship.

The survey also allowed youth workers to share any additional insights they considered. Many reported that the value of youth work was not well understood and that they feel unsupported by government, mostly due to a lack of financing.

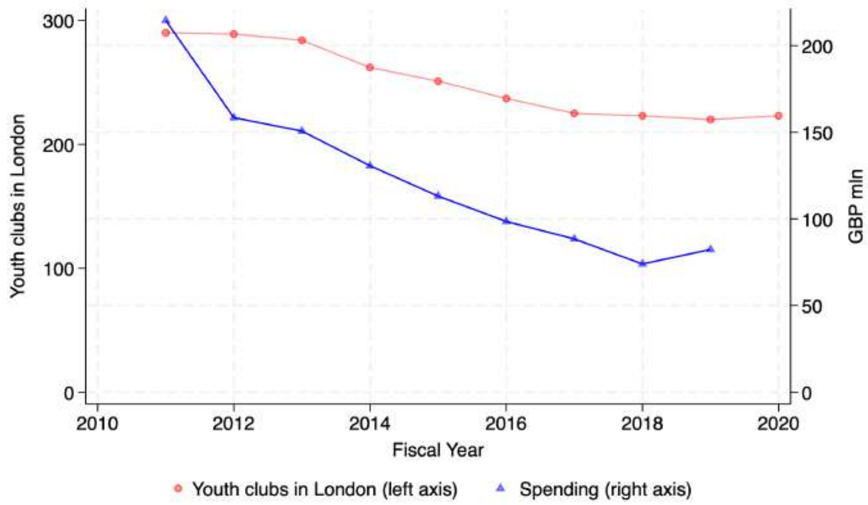
I obtained very few answers for youth club users (only 7). The majority (60%) reported that they used to access the youth club on foot and that the commute took less than 30 minutes. The most important activities to users were sports, mentorship programmes and a chill-out area.

## C. Determinants of youth club closures

### C.A Correlates of borough-level closures and other measures

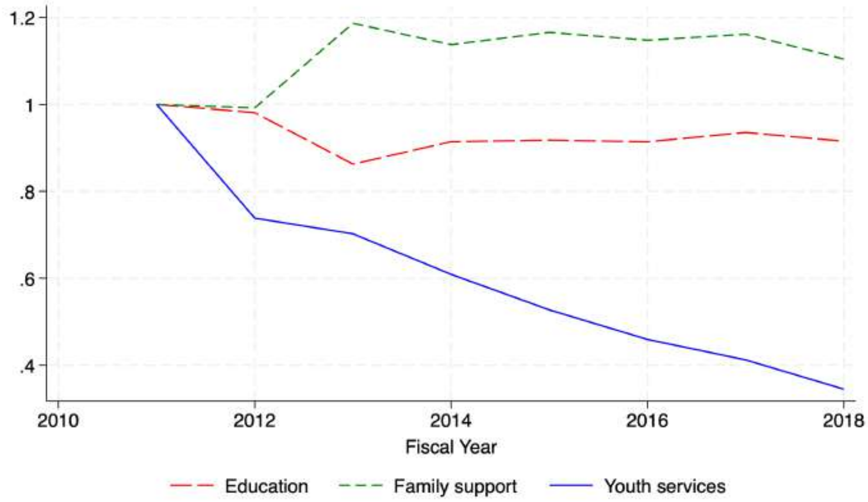
The decade from 2010 to 2019 was marked by austerity across many public services. In this section, I dig deeper into cuts to other youth services and demonstrate that the closures of youth clubs do not correlate with austerity shocks at the local area level. This is crucial to assert that the observed effects of youth clubs on outcomes are not confounded by other policies. Figure C1 shows the fall in spending on youth clubs and the number of operative clubs in London, and Figure C2 shows the trends in spending on various children's and young people's policies.

Figure C1: Spending on youth services and operative youth clubs in London




*Notes:* Trends in youth club spending, and operative youth clubs in 2011 to 2020. The blue line shows spending on *youth services* from the Department for Education in London boroughs. The red line shows the number of operative youth clubs in London, created from FOI data. Youth club closures began in 2011 and continued up to 2018, with most clubs closing by 2015.

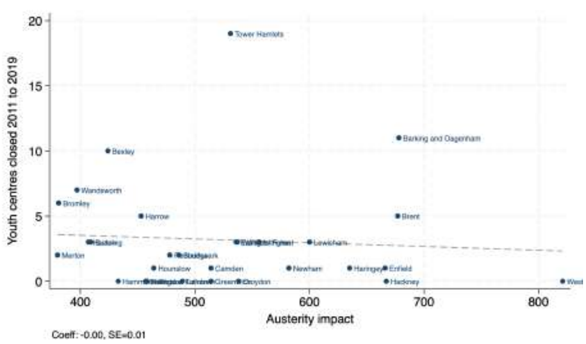
Figure C2: Spending on education, children's and young people's services in London



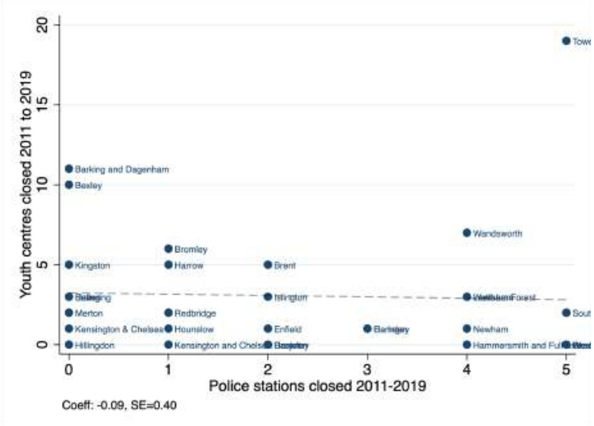

*Notes:* Trends in spending on various youth-related policies in London boroughs. The data are from the Department for Education.

First, there is no correlation between the closures and the reduction in individual welfare transfers, as measured by Beatty & Fothergill (2014). This metric, previously used by Fetzer (2019) to examine the impact of austerity on the Brexit vote, and by Giulietti & McConnell (2021) to explore local area austerity effects on crime, does not show a statistically significant relationship with youth club closures. A local-authority-level OLS regression of the number of youth centre closures on total welfare cuts yields a coefficient of -0.003, with a p-value of 0.704 (N=32). Second, I examine the correlation between youth club closures and police station closures. During the study period, 70% of police stations in London closed, with significant effects on crime and detection, as shown in Facchetti (2024). When comparing the numbers of closures of police stations and of youth centres across London from 2011 to 2019, the two shocks appear orthogonal. An OLS regression relating the number of youth centres closed in a borough to the number of police stations closed yields a coefficient of 0.086, which is not statistically significant, with a p-value of 0.831 (N=32). Additionally, police station closures did not typically coincide with youth club closures in the same areas; both closures occurred in the same borough and year in only 7% of all possible borough–year combinations. Third, youth club closures are uncorrelated with changes in unemployment. Finally, the closures are uncorrelated with youth offending rates at baseline.

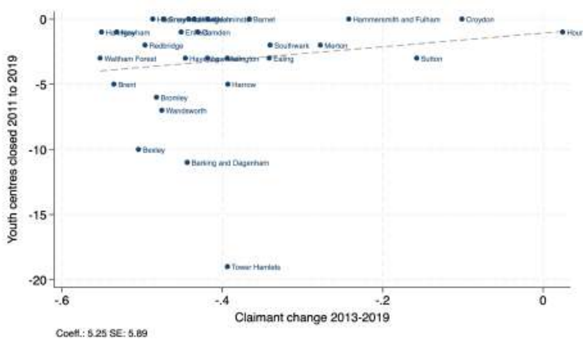
Figure C3: Youth club closures and borough-level attributes



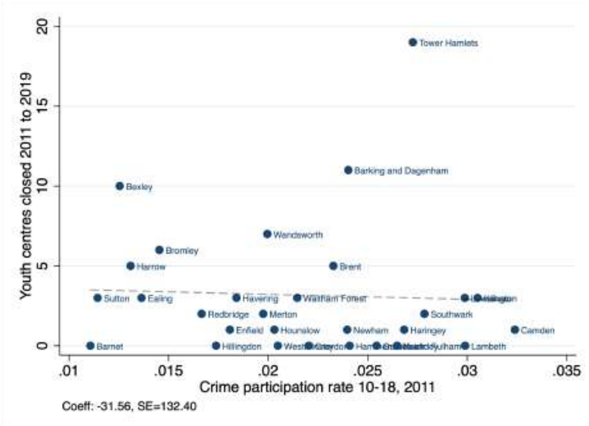

(a) Loss of individual welfare transfers




(b) Closure of police stations




(c) Change in unemployment rates




(d) Youth crime participation in 2011

*Notes:* Correlation between various borough-level measures and closures of youth clubs. The loss in individual welfare transfers is per working-age individual in GBP and comes from Beatty & Fothergill (2014). Police station closures are from FOI data from MPS. The change in unemployment is calculated between 2013 and 2019 using data from ONS (data prior to 2013 are not available). Youth offending rates are from administrative records from MPS.

### C.B Correlates of closures and youth club characteristics

Selection into treatment does not undermine the causal interpretation of difference-in-differences estimates, provided the parallel trends assumption holds. However, it is valuable to explore the factors that led to youth club closures to uncover potential confounders.

I estimate linear probability models at the youth club level, utilising youth club attributes obtained from FOI requests and external databases. Not all youth clubs have available data for all variables. In the equation below, $X$ includes a dummy variable indicating whether the centre was council-led (versus run by other institutions such as charities or churches), the age of the building housing the centre, the proportion of the population aged 0–13, the proportion of the population renting from the council, population density, political control in the borough in 2011, and youth crime rates in the block in 2011. I estimate these effects for the full sample of youth clubs and the sub-sample of council-managed clubs.

$$P(Close = 1)_i = \alpha + \xi X_i + \mu_{i(j)}$$

The estimates in columns 1 to 4 of Table C1 indicate that council-managed centres were significantly more likely to close than those operated by charities. This characteristic almost entirely explains the likelihood of closure. Indeed, the raw data show that the vast majority of youth club closures involved council-managed centres. No other factors emerge as significant predictors of closures across all youth clubs in London, strongly suggesting that austerity was the primary driver behind these closures. For instance, charities, unlike councils, have access to private funding, which may have insulated them from the impact of austerity. Moreover, closures do not appear to be correlated with local youth crime rates. Within the sample of council-funded centres (columns 3 and 4), I do not find that different attributes predict closures.

I also explore whether different attributes correlate with the likelihood of being council-run as opposed to led by a charity (columns 5 and 6). Youth clubs in less densely populated areas are more likely to be council-managed, but other attributes do not play a role. This could be due to charities setting up in areas where perhaps density facilitates fundraising.

Table C1: Relationship between youth club attributes and likelihood of closure

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | | | P (closure) > 0 | | P (council managed) > 0 | |
| Council managed | 0.265*** | 0.184** | | | | |
| | (0.053) | (0.061) | | | | |
| Building pre–1945 | 0.054 | 0.099 | 0.092 | 0.103 | 0.027 | 0.072 |
| | (0.083) | (0.076) | (0.131) | (0.127) | (0.093) | (0.143) |
| Building 1945–1959 | -0.009 | 0.035 | -0.060 | -0.028 | 0.029 | -0.006 |
| | (0.092) | (0.085) | (0.141) | (0.134) | (0.102) | (0.163) |
| Building 1960–1979 | 0.049 | 0.016 | 0.125 | 0.046 | 0.137 | 0.151 |
| | (0.115) | (0.104) | (0.171) | (0.148) | (0.132) | (0.179) |
| % social housing (2011) | -0.179 | 0.014 | -0.225 | -0.014 | -0.345* | -0.370 |
| | (0.164) | (0.149) | (0.234) | (0.233) | (0.169) | (0.215) |
| % population 0-13 (2011) | 0.154 | -0.458 | 0.613 | -0.272 | 1.325* | 2.250* |
| | (0.519) | (0.564) | (0.903) | (0.984) | (0.668) | (0.997) |
| Pop. density (log) | -0.082 | -0.004 | -0.121 | -0.027 | -0.193*** | -0.108 |
| | (0.050) | (0.052) | (0.071) | (0.071) | (0.046) | (0.061) |
| Conservative council | -0.079 | | -0.028 | | 0.020 | 0.180** |
| | (0.063) | | (0.098) | | (0.066) | (0.063) |
| Offenders aged 10–17 | 0.010 | 0.013 | 0.013 | 0.013 | 0.003 | 0.004 |
| | (0.009) | (0.009) | (0.014) | (0.015) | (0.010) | (0.012) |
| Sample | All | All | Council managed | Council managed | All | Closed |
| Mean | 0.323 | 0.323 | 0.451 | 0.451 | 0.323 | 1.000 |
| Borough FE | No | Yes | No | Yes | No | No |
| N | 285 | 285 | 164 | 164 | 285 | 92 |
| R-squared | 0.129 | 0.389 | 0.064 | 0.446 | 0.114 | 0.216 |
| F-stat | 5.28 | 1.83 | 1.33 | 0.56 | 5.39 | 3.02 |

*Notes:* Linear probability models of youth club closures. The variable *Council managed* was derived from FOI requests and online data. Building age comes from Geomini. The proportion of population living in social housing and the proportion aged 0-13 come from the 2011 Census. Robust standard errors are in parentheses. Columns 2 and 4 include borough-level fixed effects. Columns 3 and 4 only include the subsample of youth clubs that were council-managed. Stars (*, **, ***) indicate significance at the 90%, 95% and 99% confidence levels, respectively.

## D. Robustness of the main results

Table D1: Effect of closures on educational performance and youth offending – changes in sample radii

| | (1) | (2) | (3) |
|---|---|---|---|
| | Test score at ages 15–16 | | |
| ATT | -0.060*** | -0.044*** | -0.031*** |
| | (0.014) | (0.011) | (0.009) |
| Sample | 20 minutes | 30 minutes | 50 minutes |
| p-value | 0.000 | 0.000 | 0.000 |
| Mean | -0.031 | -0.016 | 0.031 |
| N | 145,175 | 257,967 | 414,023 |
| | Offending rate at ages 10-17 | | |
| ATT | 1.476* | 2.253*** | 1.672*** |
| | (0.878) | (0.564) | (0.428) |
| Sample | 20 minutes | 30 minutes | 50 minutes |
| Magnitude (%) | 9.26 | 14.78 | 12.13 |
| p-value | 0.094 | 0.000 | 0.000 |
| Mean | 15.949 | 15.246 | 13.781 |
| N | 8,520 | 16,070 | 26,530 |

*Notes:* Estimates on test scores in national exams at ages 15–16 use individual-level data from the NPD. *Test score at ages 15–16* is the average score across all subjects in the GCSEs standardised to mean 0 and standard deviation of 1. Estimates on offending rates use administrative records from MPS combined with population estimates from ONS, using data at the block level. *Offending rate at ages 10-17* is defined as the residents aged 10–17 who have offended per 1,000 residents aged 10–17. The block-level estimates are weighted by resident population. Different radii are relative to block centroid. Standard errors clustered at the MSOA level are in parentheses. Stars (*, **, ***) indicate significance at the 90%, 95% and 99% confidence levels, respectively.

Table D2: Effect of closures on educational performance and youth offending – additional sample restrictions

| | (1) | (2) |
|---|---|---|
| | Test score at ages 15–16 | |
| ATT | -0.031*** | -0.027*** |
| | (0.009) | (0.009) |
| Sample | Exclude Inner London | Exclude Low Quality |
| p-value | 0.001 | 0.003 |
| Mean | 0.049 | 0.033 |
| N | 324,865 | 378,270 |
| | Offending rate at ages 10–17 | |
| ATT | 1.984*** | 1.837*** |
| | (0.460) | (0.461) |
| Sample | Exclude Inner London | Exclude Low Quality |
| Magnitude (%) | 14.85 | 12.78 |
| p-value | 0.000 | 0.000 |
| Mean | 13.358 | 14.368 |
| N | 15,560 | 20,090 |

*Notes:* Estimates on test scores in national exams at ages 15–16 use individual-level data from the NPD. *Test score at ages 15–16* is the average score across all subjects in the GCSEs standardised to mean 0 and standard deviation of 1. Estimates on offending rates use administrative records from MPS combined with population estimates from ONS, using data at the block level. *Offending rate at ages 10–17* is defined as the residents aged 10–17 who have offended per 1,000 residents aged 10–17. The block-level estimates are weighted by resident population. Outer London boroughs include Barking & Dagenham, Barnet, Bexley, Brent, Bromley, Croydon, Ealing, Enfield, Harrow, Havering, Hillingdon, Hounslow, Kingston upon Thames, Merton, Newham, Redbridge, Richmond upon Thames, Sutton, and Waltham Forest. The low data-quality observations are Hackney and Hammersmith & Fulham before 2015, and Camden, Hillingdon and Westminster for the whole sample period. Standard errors clustered at the MSOA level are in parenthesis. Stars (*, **, ***) indicate significance at the 90%, 95%, and 99% confidence levels, respectively.

Table D3: Effect of closures on educational performance and youth offending – alternative inference calculations

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Test score | | Offending rate | |
| | ages 15–16 | | ages 10–17 | |
| ATT | -0.035** | -0.035*** | 2.018*** | 2.018*** |
| | [-0.072, 0.003] | [-0.059, -0.011] | [0.572, 3.174] | [0.927, 3.108] |
| Clustering | Borough-wildbootstrap | Youth club | Borough-wildbootstrap | Youth club |
| Magnitude (%) | | | 14.09 | 14.09 |
| p-value | 0.041 | 0.004 | 0.008 | 0.000 |
| Mean | 0.014 | 0.014 | 14.320 | 14.320 |
| N | 352,454 | 352,454 | 22,040 | 22,040 |

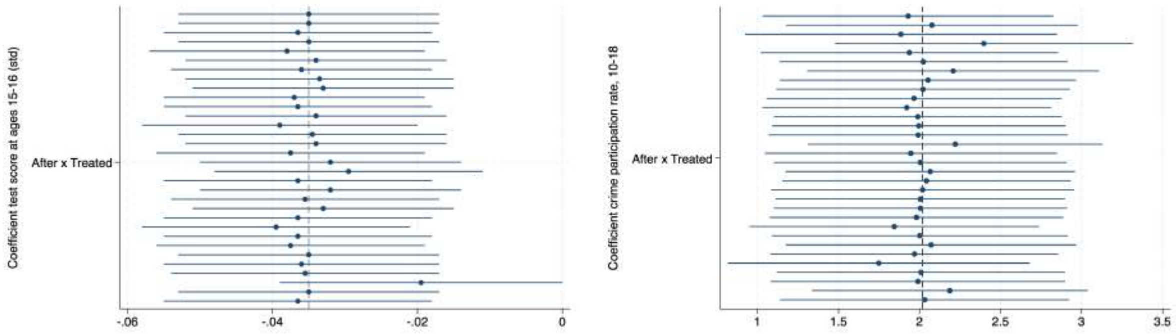
*Notes:* Estimates on test scores in national exams at ages 15–16 use individual-level data from the NPD. *Test score at ages 15–16* is the average score across all subjects in the GCSEs standardised to mean 0 and standard deviation of 1. Estimates on offending rates use administrative records from MPS combined with population estimates from ONS, using data at the block level. *Offending rate at ages 10–17* is defined as the residents aged 10–17 who have offended per 1,000 residents aged 10–17. The block-level estimates are weighted by resident population. Columns 1 and 3 show confidence intervals from wild bootstrap clustered standard errors at the borough level. Columns 2 and 4 cluster standard errors at the youth club level (by nearest youth club to each block). Stars (*, **, ***) indicate significance at the 90%, 95% and 99% confidence levels, respectively.

Table D4: Effect of closures on educational performance and youth offending – different sets of controls

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Test score | | Offending rate | | |
| | ages 15–16 | | ages 10–17 | | |
| ATT | -0.044*** | 2.601*** | 2.609*** | 2.047*** | 1.889*** |
| | (0.009) | (0.449) | (0.457) | (0.463) | (0.457) |
| Spec | None | None | Dist to police stations | All | No weights |
| Magnitude (%) | | 18.16 | 18.15 | 14.24 | 13.19 |
| p-value | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Mean | 0.014 | 14.32 | 14.37 | 14.37 | 14.32 |
| N | 352,454 | 22,430 | 21,730 | 21,730 | 22,430 |

*Notes:* Estimates on test scores in national exams at ages 15–16 use individual-level data from the NPD. *Test score at ages 15–16* is the average score across all subjects in the GCSEs standardised to mean 0 and standard deviation of 1. Estimates on offending rates use administrative records from MPS combined with population estimates from ONS, using data at the block level. *Offending rate at ages 10–17* is defined as the residents aged 10–17 who have offended per 1,000 residents aged 10–17. The block-level estimates are weighted by resident population. Columns 1 and 2 do not include controls. Column 3 controls for proximity to operative police stations. Column 4 controls for proximity to police stations and interacts population density with year. Column 5 includes population density interacted with year but does not weight the coefficients by population. Standard errors clustered at the MSOA level are in parentheses. Stars (*, **, ***) indicate significance at the 90%, 95% and 99% confidence levels, respectively.

Figure D1: Effect of closures on educational performance and youth offending, excluding one borough at a time




(a) Test score at ages 15–16 (b) Offending rate at ages 10–17

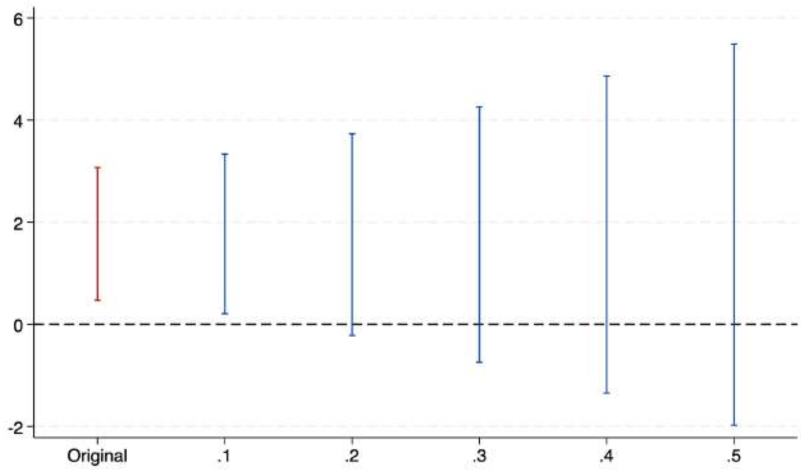
*Notes:* Estimates on test scores in national exams at ages 15–16 use individual-level data from the NPD. *Test score at ages 15–16* is the average score across all subjects in the GCSEs standardised to mean 0 and standard deviation of 1. Estimates on offending rates use administrative records from MPS combined with population estimates from ONS, using data at the block level. *Offending rate at ages 10–17* is defined as the residents aged 10–17 who have offended per 1,000 residents aged 10–17. The block-level estimates are weighted by resident population. Each coefficient and confidence interval is estimated excluding one of 32 boroughs. Standard errors clustered at the MSOA level are in parentheses. Stars (*, **, ***) indicate significance at the 90%, 95% and 99% confidence levels, respectively.

Table D5: Effect of closures on youth offending – different dependent variables

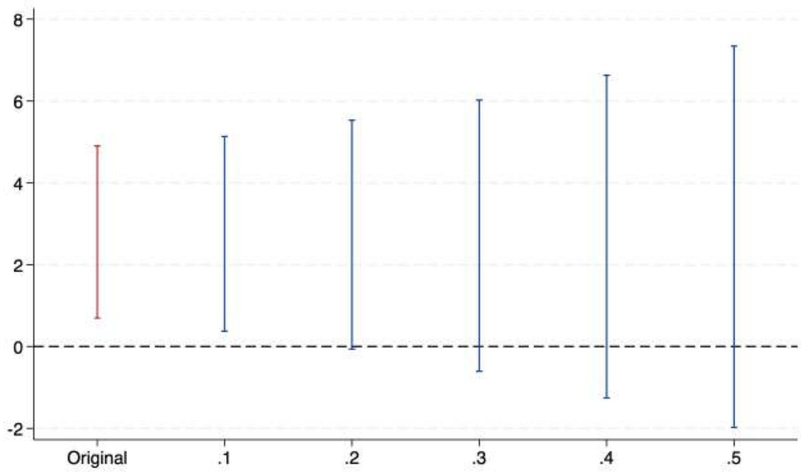
| | (1) | (2) | (3) |
|---|---|---|---|
| | Offending rate, uncensored | Offending rate, censored at p95 | Offenders (count) |
| | | ages 10–17 | |
| ATT | 1.842*** | 1.683*** | 0.220*** |
| | (0.437) | (0.428) | (0.071) |
| Magnitude (%) | 12.75 | 11.77 | 9.60 |
| p-value | 0.000 | 0.000 | 0.002 |
| Mean | 14.449 | 14.298 | 2.295 |
| N | 22,040 | 22,040 | 22,040 |

*Notes:* The data were created with administrative records from MPS and population estimates from ONS, and the regressions are estimated at the block–year level. *Offending rate at ages 10–17* is defined as the residents aged 10–17 who have offended per 1,000 residents aged 10–17. Column 1 uses offending rates uncensored. Column 2 winsorises the offending rate at the 95th percentile. Column 3 uses the count of offenders rather than the rate of offending. Coefficients weighted by resident population. Standard errors clustered at the MSOA level are in parentheses. Stars (*, **, ***) indicate significance at the 90%, 95% and 99% confidence levels, respectively.

Figure D2: Effect of closures on youth crime rates, allowing pre-trends to differ from 0 up to different $\bar{M}$ levels



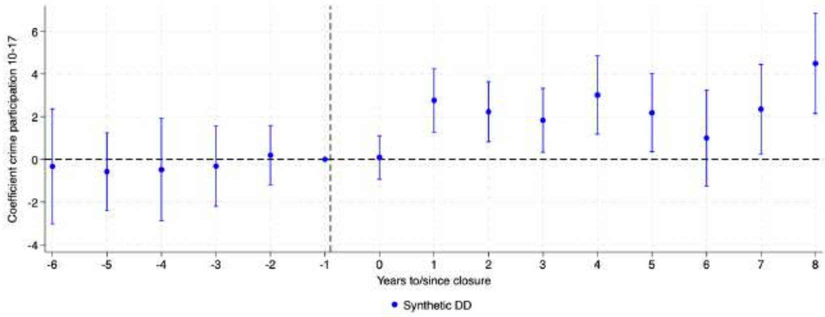

(a) Offending rate at ages 10–17



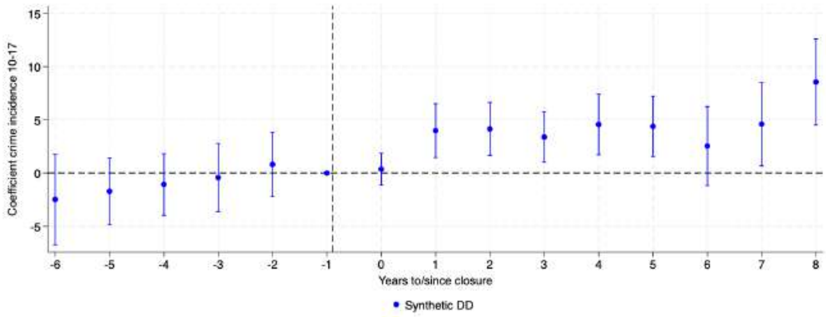

(b) Crime incidence rate at ages 10–17

*Notes:* Sensitivity of effects to pre-trends differing from zero by different $\bar{M}$ following Rambachan & Roth (2023). The data were created with administrative records from MPS and population estimates from ONS, and the regressions are estimated at the block–year level. *Offending rate at ages 10–17* is defined as the residents aged 10–17 who have offended per 1,000 residents aged 10–17. *Crime incidence rate at ages 10–17* is defined as crimes committed by residents aged 10–17 per 1,000 residents. Confidence intervals are calculated using MSOA-level clustered standard errors.

Figure D3: Event studies – effect of closures on youth crime rates from synthetic difference-in-differences




(a) Offending rate at ages 10–17




(b) Crime incidence rate at ages 10–17

*Notes:* Synthetic estimates as in Arkhangelsky et al. (2021). The data were created by combining administrative records from MPS and population estimates from ONS, and the regressions are estimated at the block–year level. *Offending rate at ages 10–17* is defined as the residents aged 10–17 who have offended per 1,000 residents aged 10–17. *Crime incidence rate at ages 10–17* is defined as crimes committed by residents aged 10–17 per 1,000 residents. To produce the event studies, I first obtain the weights using the `sdid` package. Then, I match the weights on a stacked database and weight the control observations by the resulting values and the treated observations equally (each by 1). The base year is the year before centres close. Confidence intervals are calculated using MSOA-level clustered standard errors.

Table D6: Effect of closures on youth crime rates – synthetic difference-in-differences

| | (1) | (2) |
|---|---|---|
| | Offending rate | Crime incidence rate |
| | ages 10–17 | |
| ATT | 2.501*** | 4.410*** |
| | (0.608) | (1.327) |
| Magnitude (%) | 17.46 | 20.70 |
| p-value | 0.000 | 0.001 |
| Mean | 14.32 | 21.31 |
| N | 22,430 | 22,430 |

*Notes:* Synthetic estimates as in Arkhangelsky et al. (2021). The data were created by combining administrative records from MPS and population estimates from ONS, and the regressions are estimated at the block–year level. *Offending rate at ages 10–17* is defined as the residents aged 10–17 who have offended per 1,000 residents aged 10–17. *Crime incidence rate at ages 10–17* is defined as crimes committed by residents aged 10–17 per 1,000 residents. Standard errors clustered at the MSOA level are in parentheses. Stars (*, **, ***) indicate significance at the 90%, 95% and 99% confidence levels, respectively.

Table D7: Effect of closures on youth offending rates – non-linear estimators

| | (1) | (2) | (3) |
|---|---|---|---|
| | | Offending rate at ages 10–17 | |
| ATT | 0.137*** | 0.303*** | 0.180*** |
| | (0.028) | (0.078) | (0.035) |
| Estimation | Poisson - TWFE | Poisson - Wooldridge | Poisson - stacked |
| p-value | 0.000 | 0.000 | 0.000 |
| Mean | 13.578 | 13.578 | 15.019 |
| N | 28,890 | 28,890 | 144,520 |

*Notes:* The data were created by combining administrative records from MPS and population estimates from ONS, and the regressions are estimated at the block–year level. *Offending rate at ages 10–17* is defined as the residents aged 10–17 who have offended per 1,000 residents aged 10–17. Columns 1 to 3 estimate non-linear models. Coefficients weighted by resident population. Standard errors clustered at the MSOA level are in parentheses. Stars (*, **, ***) indicate significance at the 90%, 95% and 99% confidence levels, respectively.

Table D8: Effect of closures on crime detection and stop-and-search rates

| | (1) | (2) | (3) |
|---|---|---|---|
| | % detected | Stop & search rate all ages | Stop and search rate ages 10–17 |
| ATT | -0.010** | -0.334*** | -2.859*** |
| | (0.004) | (0.125) | (0.756) |
| Magnitude (%) | -4.31 | -20.83 | -25.58 |
| p-value | 0.014 | 0.007 | 0.000 |
| Mean | 0.228 | 1.604 | 11.178 |
| N | 36,490 | 14,476 | 14,476 |

*Notes:* Column 1 presents estimated effects on the *proportion of crimes detected*, from administrative records from MPS combined with population estimates from ONS. Columns 2 and 3 estimate effects on *stop and search rates*, defined as stops conducted by police in an area over resident population. It uses stop-and-search records from Police UK and population estimates from ONS. All regressions are estimated at the block–year level. Stop-and-search data come from Police UK and are available from April 2016 onwards. Standard errors clustered at the MSOA level are in parentheses. Stars (*, **, ***) indicate significance at the 90%, 95% and 99% confidence levels, respectively.

## E. The p-median model and counterfactual closures

This appendix describes in detail the p-median model used to determine the optimal closures of youth clubs discussed in Section VIII.B.

The p-median model formalises a location problem where a fixed number of facilities must be strategically placed to minimise the distance between facilities and demand nodes. A detailed review of the model and its properties can be found in Mirchandani & Francis (1990). This model is commonly used in operations research and various other fields.

Let $i$ denote potential facility locations, $j$ represent demand points, and $d_{ij}$ denote the distance between facility $i$ and demand point $j$. The objective is to select $p$ facilities from the set of potential locations to minimise the total cost or distance. In my context, the demand nodes are population-weighted centroids and the facilities are youth centres. The objective function for the p-median model is typically expressed as

$$\min \sum_i \sum_j d_i \cdot x_{ij}$$

where $x_{ij}$ is a binary variable indicating whether facility $i$ serves demand point $j$. The constraints to the optimisation problem are:

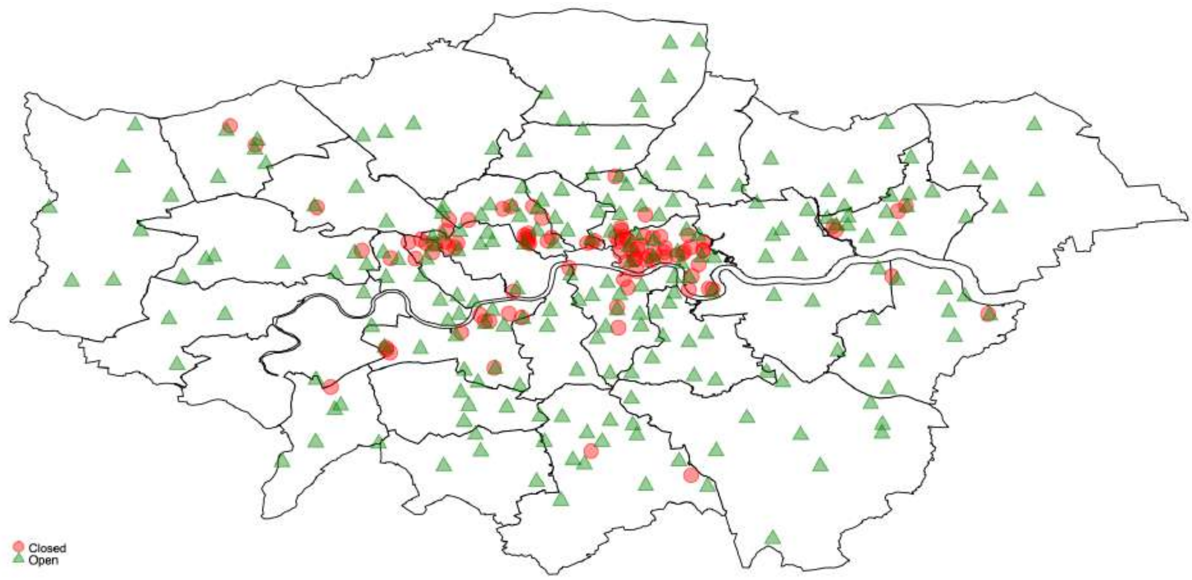
- Each demand point must be served by at most one facility.
- Exactly $p$ facilities must be selected from the set of potential locations.
- There are binary constraints on $x_{ij}$ variables.

The set of possible facilities or youth centres is given by the location of youth clubs (at any point) between 2010 and 2019. The maximum set of facilities that can be selected is 230, which is the number of youth clubs that were open in 2020 (after the austerity policies). I weight each centroid equally.[21]

A map of the resulting optimal locations is included in Figure E1, and Figure E2 compares the optimal allocation with the true observed closures. Fewer centres should have closed in Outer London and more should have closed in Inner London, according to this minimisation in commuting time exercise. This map also shows the clubs that were correctly left open (grey triangles) and those that were correctly closed (grey circles – many of those are in Tower Hamlets and not clearly visible on the map). This is consistent with other considerations of the city's socio-economic attributes, such as more children living in Outer London areas, and with gentrification dynamics pushing families away.

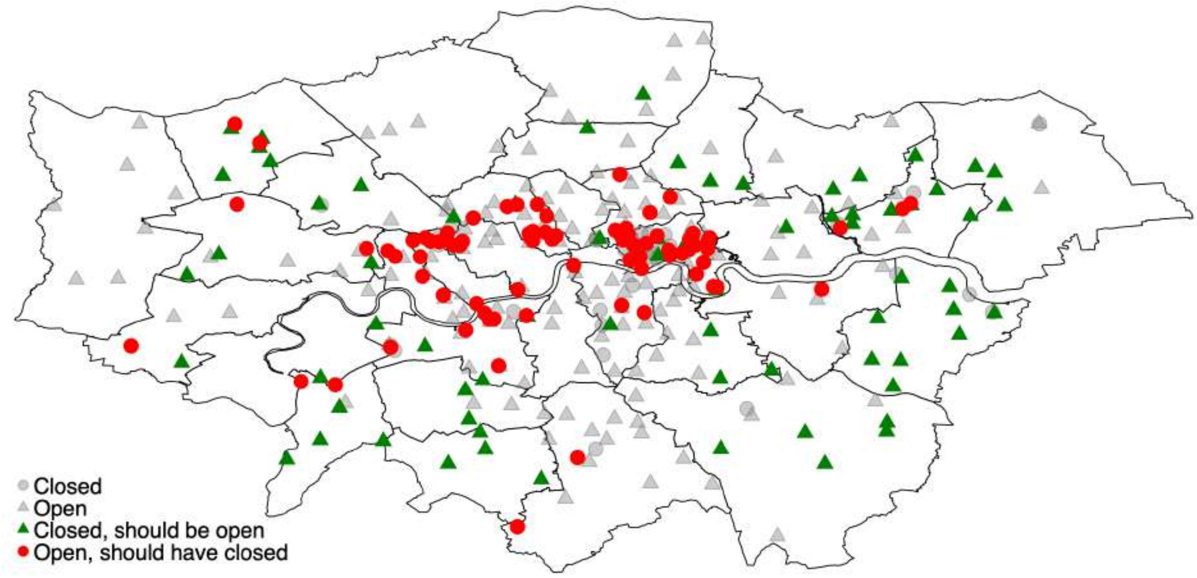
[21] Additional analyses weighting centroids by young population or interacting youth population with deprivation yielded similar estimates. The results can be provided upon request.

Figure E1: Optimal youth club locations according to the p-median model solution




*Notes:* The triangles are youth clubs that should have been kept open according to the p-median model solution, and the circles are youth clubs that should have closed – many of the latter are located in Tower Hamlets and are very close to one other.

Figure E2: Comparison of effective closures and optimal closures as per p-median model solution




*Notes:* Comparison between observed closures and the optimal locations of youth clubs to close from p-median model solution.