The Effects of Youth Clubs on Education and Crime

Carmen Villa*

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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 in the 2010s. Teenagers affected performed nearly 4% worse in national high-school exams and became 14% more likely to commit crimes. The effects are not explained by incapacitation alone; youth clubs provide lasting support by fostering positive behaviours. Closing them was not cost-effective, for every £1 saved there are costs of nearly £3. (JEL H72, I38, J13, J24, K14, K42)

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). 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-Diaz & Egana del Sol 2023). 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

*University of Zurich, Schonberggasse 1, 8001 Zurich, Switzerland; carmen.villa@econ.uzh.ch. I thank Manuel Bagues, Mirko Draca, Jeff Grogger and Marta Santamaria for their generous guidance in this project. This paper has benefited from suggestions from Ashna Arora, Elena Casanovas, SeeYu Chan, Mira Chaskes, Nikhil Datta, Magdalena Dominguez, Gaia Dossi, Elisa Facchetti, Immanuel Feld, Ludovica Gazze, James Heckman, Daniela Horta Saenz, Amrita Kulka, Yuchen Lin, John MacDonald, Angelica Martinez-Leyva, Clara Martinez-Toledano, Jose Montalban Castilla, Akash Raja, Evan Rose, Joseph Sabia, Bruno Santos de Souza, Ellen Stuart, Edoardo Tolva, Ao Wang, Ben Zaranko, Natalia Zinovyeva and seminar participants at various conferences and workshops. I am grateful for support from the Nuffield Foundation through the research grant *Transforming Justice: The Interplay of Social Change and Policy Reforms (SFS /FR-000024397).* Special thanks to the London Metropolitan Police for allowing the use of their data, making this project possible. Some of this work was undertaken in the Office for National Statistics (ONS) Secure Research Service. The conclusions in this paper do not reflect the opinions or endorsement of the Nuffield Foundation, the Metropolitan Police, the ONS or any other data owners. All errors are my own.

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 children 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 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]

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 administrative records from the London Metropolitan Police with the universe of crimes, and offenders' details for detected crimes. The crime data allows me to create person-based measures of crime (e.g. the proportion of residents offending) and place-based measures of crime (e.g. local crime rates).[3] It also allows me to distinguish youth crimes from crimes committed by adults.[4]

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.[5]

I estimate average treatment effects on treated areas (ATT) using two-way-fixed-effects

[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. Youth services include youth clubs and targeted youth support. Data on spending on youth clubs specifically are inexistent. Conversations with youth workers suggest at least 50% of spending on youth services in the 2010s related to youth clubs.

[3]Research disentangling people and place effects is scarce. See Autor et al. (2025) for an excellent example in the context of differences in effects of labour market adjustments.

[4]In England and Wales the age of criminal majority is 10 years old.

[5]Blocks are Lower Level Super Output Areas (LSOAs), statistical units created by the Office for National Statistics. In 2011 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 is defined as within 40 minutes on foot.

(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).[6] 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 residing 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.). The effects were larger for pupils from lower-income backgrounds, as proxied by their free school meals (FSM) status, for whom test scores fell by 12% s.d. They were also larger for pupils with lower prior attainment, proxied using their performance at ages 10–11 (KS2, or SATs). These magnitudes are 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), suggesting interventions during adolescence can be as important as interventions in early childhood.

The closures also led to increases in youth offending rates (person-based measures of crime) in terms of both the proportion of resident youths offending and the number of crimes committed by resident youths. The magnitudes are 13 to 14% in both margins. The largest crime rises were in acquisitive offences, with drugs and violence also increasing. I find rises in crime both for younger and older teenagers, and both during school holidays and on school days. These effect sizes are comparable in magnitude, although of opposite sign, to the estimated drop in crime after extending the compulsory schooling age (explored in the US context Bell et al. 2022). Using the result on effects on attendance to structured after school activity as a first-stage effect and under some assumptions, the crime increase is also within similar bounds – although of opposite sign – of crime drops estimated in targeted youth interventions fostering participation in summer jobs (Heller 2014, Modestino 2019).[7]

Analyses exploring heterogeneity by length of exposure to youth clubs suggest strong

[6] 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.

[7] These effects are consistent with operative youth clubs decreasing the returns to crime, or increasing the utility of non-crime options as per Becker (1968) models of crime engagement.

dynamics. People 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 focus on where crimes occur – rather than whether residents offend – and is traditionally the focus of most studies on place-based policies, in part because data on offenders and their addresses has not been readily available in the past (Jacob & Lefgren 2003, Luallen 2006, Akee et al. 2014, Steinberg et al. 2019). Youth club closures did not alter the spatial distribution of crimes or the commuting to crime patterns of youth. There are more youths offending than their would have been, but they are committing offences in the same places which were hubs of crime before. Crime attractors (and deterrents) do not appear to be altered by youth club closures.

The effects are not confounded by general austerity. Controlling for local unemployment and for local spending in other youth policies (schools or family support services) does not alter the coefficients. They are not confounded by other unobserved changes at the local authority and year level either. In addition, as placebo exercises, I show that the closures had null effects on test scores at ages 10–11 – when people were presumably too young to be exposed to the benefits of the youth clubs – and that offending rates didn't increase for adults – when people were too old to attend. Triple difference-in-differences analyses using younger or older groups as an additional control group – which allow to control for unobservable attributes at the block-year, cohort-year, and cohort-block level – suggest that the closures differentially affected teenagers. I rule out the effects being driven by improvements in police recording as there were no increases in detection or stop-and-search rates after closures in areas affected. I show that any deterrence effects are not confounded by the closure of police stations studied in Facchetti (2024) either. The effects are not due to changes in resident composition, as households in affected areas are not differentially likely to have recently moved into the neighbourhood. The estimates are also robust to several changes in assumptions and estimation. The analysis of a few youth club openings suggests the effects might be symmetric.

A key channel explaining the effects is that the bundle of positive amenities provided at youth clubs is not easily substitutable. Survey data suggest that after closures, affected youths substitute time at the club for cheap leisure, such as videogames, TV and social media. Youth clubs might also have a directly positive effect on education by providing a safe space to study; after closures young people spend less time doing homework and report greater difficulty concentrating. The closures likely affected how youth perceive their local area, making them

less likely to report liking their neighbourhood. 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 being the sole explanation for the effects as the observed increases in youth crime are not confined to hours when youth clubs might have been open.

Despite the objective of the closures being to cut governmental spending, these might not have been cost-effective. Combining my estimates with 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, for every £1 saved, there are estimated losses of £2.65. Adopting a marginal value of public funds approach shows that if the government had been able to raise funds to keep youth clubs open, there would have been societal returns of £4.63 for every £1 spent (Finkelstein & Hendren 2020).

The closures might not have been cost-efficient either. Taking into account substitutability and commuting to operative youth clubs may have mitigated crime rises. I argue 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.[8] For a 1% increase in commuting costs, youth 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.

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 established that after-school clubs delivered in schools to targeted populations could be beneficial, but studied small samples in specific settings (Gottfredson et al. 2004, Dinarte-Diaz & Egana del Sol 2023). Work on youth clubs specifically was only correlational. Findings tended to be mixed, with some authors finding beneficial effects of youth clubs (Goldschmidt et al. 2007), others estimating null effects (Hirsch et al. 2011), and yet others finding an association between attendance to clubs and worsened outcomes (Mahoney et al. 2001, Feinstein et al. 2005). The focus on youth clubs, the creation of a database with youth club locations, the use of administrative records and studying large samples through a causal lens are novel contributions.

I also contribute to the literature on community-based programs targeted toward adolescents. Research has long emphasised the importance of early childhood interventions

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

(Heckman 2006, Cunha & Heckman 2007, Ludwig & Miller 2007, Berlinski et al. 2009, Heckman et al. 2010) but there is an increasing interest in adolescence as we learn more about brain development and associated risks at this stage (Blakemore 2012, Steinberg 2014). Prior work, often targeting disadvantaged students, has shown that summer jobs can decrease reoffending (Heller 2014, Modestino 2019, Davis & Heller 2020), improve educational outcomes (Modestino & Paulsen 2023) and that programs targeting socio-emotional skills can achieve falls in arrests (Heller et al. 2017). Other recent literature has focused on the effects of tutoring programs, showing benefits on educational performance (Guryan et al. 2023). A separate literature highlights the educational role of public libraries (Gilpin et al. 2024). These papers have suggested that those interventions' effects go beyond pure incapacitation and that adolescence can be a critical period of intervention. My paper explores an intervention open to all pupils – not just some identified as high risk – which encompasses a broad range of activities, and where youths actively choose to attend, placing youth clubs at the intersection of place-based policies and youth-targeted interventions. While most of the existing evidence on youth interventions focuses on American cities, my paper explores London, which differs in its institutional setting, policing practices, and youth service provision.

My work also builds on a strand of the literature exploring the role of education, time use and youth crime. Some papers in this realm assess the role of formal schooling and highlight crime-reducing effects of additional years of education (Machin et al. 2011, Hjalmarsson et al. 2015, Bell et al. 2022). These papers typically assess person-based measures of offending. Others have shown that pupil interaction can increase local crimes (Jacob & Lefgren 2003, Luallen 2006, Akee et al. 2014) – particularly violence and drugs. I explore the role of after-school activities and hence the role of education and pupil interaction beyond the classroom. I am able to explore both person-based and place-based measures of crime at a very fine geographical level.

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 (Dustmann et al. 2024). My paper focuses on cuts that affect young people specifically.

I. Background

I.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 C – 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.[9]

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, from available online data, 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). Local authorities can run and manage youth clubs or coordinate those managed by charities. In London, about 52% of youth clubs are run by local authorities, 42% by charities and the rest by religious associations. 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 funds, according to youth workers' insights.

The mean yearly running costs of youth clubs in London is £169,567, according to Freedom of Information data and surveys among youth workers. The mean cost per user is about £350 per year. Attendance is voluntary and is seldomly recorded. Gathering statistics is not a priority, and youth workers often 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

[9]Countries implementing similar programmes include Canada, Australia, the US (e.g. the *Boys and Girls Clubs*), Scandinavian countries, Germany (*Jugendzentren*) and France.

every two days. Youth club use correlates with time use and demographic attributes. Those attending 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 from deprived households.[10]

I.B Austerity and spending in youth services

As part of the 2010 Spending Review, the UK government announced substantial welfare cuts, impacting many areas of public policy. Local authorities, responsible for many local area services, received fewer funds to spread among their various needs. Youth services were among the most hit areas of social policy due to relaxed laws regarding the statutory duty of local authorities. In addition, several grants specifically ring-fenced for these services were abolished.

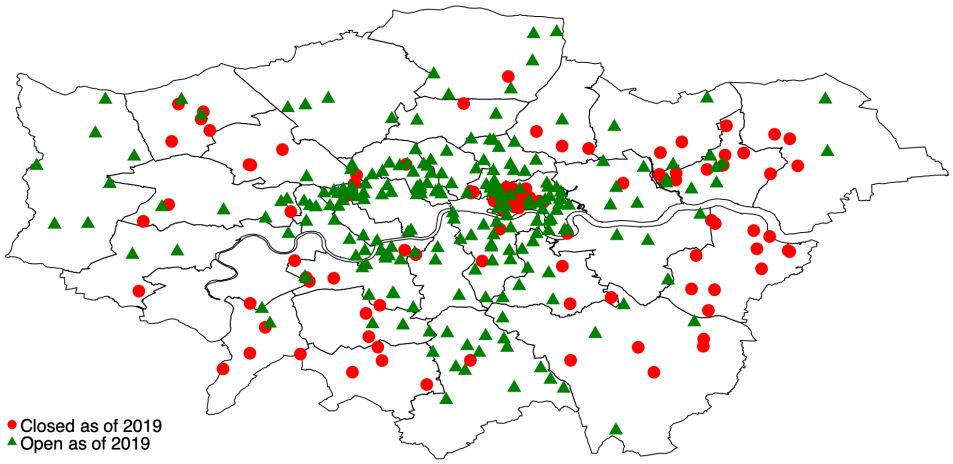
In London, spending on youth services dropped by 71% between 2010 and 2019, from an initial £215 million (Department for Education 2020). Out of all youth clubs open in London in 2010, nearly 30% had closed by the end of the decade – a loss of 85 youth clubs across 24 boroughs. These closures were staggered and did not 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. For instance, spending on schools or on family support was stable. At the local authority level, the closure of youth clubs is uncorrelated with falls in individual welfare transfers, the closure of police stations, the change in unemployment and youth crime at baseline. At the small area level (census block), the closures are also uncorrelated with police station closures. Details on these analyses are in Appendix D. 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.

II. Data

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

[10]Source: Own analysis. See Appendix C.

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.

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.[11] Quality or capacity indicators were not provided.

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

I use the precise address of each youth club and the HERE API geocoder to compute the distance between each block population-weighted centroid and all youth clubs within a 5-kilometre radius. Using population-weighted centroids (instead of Euclidean centroids) allows me to capture differences in urban structures. I compute 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 2010–19.[12]

Survey on youths' time use: I use survey data from Understanding Society to understand youth's changes in time use and neighbourhood perception. I also use it to assess changes in neighbourhood composition. This source, created by University of Essex, Institute for Social

[11] The estimates are robust to excluding the borough–year combinations where data are questionable.
[12] This includes more clubs than Berry (2018) or Ordnance Survey's Points of Interest database.

and Economic Research, is representative of the UK population at the national level. The data include 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 in 2010–2019.

The youth questionnaire, which focuses on respondents aged 10–15 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 include private sports clubs, after-school paid tutoring and organisations such as the Scouts or Guides).[13] 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 throughout the year before the question was asked. Details on this question and correlates to other attributes are available in Appendix B.

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. I also study how much youths like their neighbourhood, an index variable. Data availability varies as some variables were only introduced from 2012.

I use the adults' questionnaire (asking those aged 16 onwards) to assess whether youth club closures led to migration in the area. For each adult I calculate whether they moved to the neighbourhood in recent years, and whether they are planning to move. I distinguish parents, and non-parents.

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. The NPD contains records for the universe of pupils residing and attending school in London, details on their gender, ethnicity and block of residence. It also includes whether they were eligible for free school meals (FSM), an indicator of family income as eligibility is conditional on receiving other income-based government benefits. Approximately 25% of all pupils are eligible for FSM in London.[14]

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 (ages 10–11) and in Year 11 (ages 15–16). My key outcome of interest is the performance in Year 11, when pupils are in the target age of youth clubs.[15] Exams in Year 11 (KS4, also called GCSEs) mark the completion of compulsory

[13]To the best of my knowledge, the only source with information on attendance at youth clubs specifically is Greater London Authority (2009), which is not available as a panel.

[14]I could not systematically obtain attributes of the school, or family links across pupils.

[15]Pupils aged 10 or 11 might be allowed in some youth clubs, but they are too young to have been exposed to their services for long.

education and cover a broad range of subjects, including English, mathematics, science and history. These results determine eligibility for future educational paths. I use the exams in Year 6 (KS2, also called SATs) in a placebo analysis, and also as a proxy for lower attainment in heterogeneity analyses. These exams 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 within year to ensure comparability over time, as there were changes to the score scale over the study period. Whenever a pupil sat an exam more than once (a very small number of pupils), I use the year they first sat it.

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, and the location of the crime anonymised to block level. There are nearly 7.5 million crimes in the sample and the most common crimes are thefts (29%) and violence against the person (21%). For detected crimes – called cleared crimes in the U.S – the data also include a unique identifier for each offender, their home address coded at the block level and their age. Around 20% of all crimes are detected (slightly over 1.4 million observations). These crimes are connected to 952,237 unique individuals.[16] The most common crimes with offender details are drug offences (33%), violence against the person (24%) and thefts (14%).[17] The data do not include anti-social behaviour incidents.

I use the set of detected crimes to measure residents' propensity to commit crimes by combining the records with population estimates from Office for National Statistics, available by block, age and year.[18] This allows me to compute person-based measures of crime: offending rates and crime incidence rates.[19] Offending rates represent the proportion of residents who were accused of a crime per 1,000 population, calculated for each age group, and block-year. Crime incidence rates represent the number of crimes committed per 1,000 residents of each age group, and block-year. These analyses use all incidents in which a given resident is involved, regardless where the incident occurs. I also compute crime incidence by

[16]The unique identifiers are created using first names, last names and date of birth. There could be error in name recording and hence in the identifiers. The number of unique offenders is likely lower.

[17]I do not have information on court outcomes. Aggregate statistics suggest the vast proportion of crimes detected by police end in a conviction (CPS 2024).

[18]The population data are only available by detailed age from 2012 onwards. To obtain estimates for 2010 and 2011, I linearly interpolate the population for each cohort and block.

[19]The numerator is winsorised at the 99th percentile at the block year level.

crime type and by the hour in which crimes are committed, distinguishing school days from school holidays. I create measures of recidivism using offender identifiers. An individual can be accused of several crimes in the same incident. A single offender can commit many crimes.

I also compute local crime rates as the sum of crimes that occur in a block per 1,000 population. This place-based measure of crime differs from the above in that the geography of interest is where crimes happen, rather than where the offender lives. I compute local crime rates overall, and committed by specific age groups when the crime was detected (e.g. total crimes committed by 10–17-year-olds over resident population). I also compute the average distance between the home address and the location of the crime for the subset of detected crimes involving youth for additional analyses on crime deterrents and attractors.

The remaining crime variables measure police presence, which I use in robustness checks. I compute detection rates as crimes recorded in a block for which the identity of the offender is known, over total crimes recorded in a block. I also calculate stop-and-search rates using publicly available data from Police UK as total searches per 1,000 population. These data are only available from April 2016 onwards. 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 2011 to 2016 affected local crime rates.

Other sources: I leverage building age data from Verisk to understand the determinants of youth club closures in Appendix D. 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 (before most youth clubs closed). I use FOI data containing each local authority's school calendar to create indicators on school days versus school holidays 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 and explore mechanisms.[20] I compute the distance between each block centroid in London and libraries, sports centres and parks, and distinguish blocks that are above or below median distance to each of these amenities.[21] I use data on the number of claimants for Job Seekers Allowance and Universal Credit from ONS, which is publicly available. Last, I use publicly available data on local authority spending on educational and social policies from the Department for Education to control for spending on schools and family support services in robustness checks.[22]

[20]The data can be publicly downloaded from 2014 onwards, and secured through a data sharing agreement for earlier years.

[21]Some amenities are much more prevalent than others, which implies average distances to each type of amenity differ substantially.

[22]These two spending categories are some of the largest in total GBP relating to youth.

III. Identification Strategy

Understanding the effects of youth clubs is challenging from an empirical standpoint. Youth clubs tend to locate in disadvantaged areas and those attending often come from 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.

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'* (Burford 2016). In Becker (1968) models, where individuals compare crime and non-crime options making rational decisions, the effects of youth club closures on crime depend on the relationship between youth clubs and returns to criminal activity.

Which youth clubs closed? In Appendix D.A, I explore the correlation between different club-level attributes and closures. The only statistically significant predictor of closures is whether youth clubs were managed by the local authority instead of charities. 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, none of the selected attributes are predictors of closing.

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.[23]

III.A Methods

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

[23]It is possible that closing youth clubs differed in unobservable ways – such as quality – from those remaining open. If closing clubs were similar, estimates in this paper would be unbiased. If closing clubs were of lower quality, the effects would be a lower-bound. If, improbably, closing clubs were higher quality – e.g., if councils had closed the best clubs – the effects would be overstated.

and control areas as those where all nearby youth clubs remained open. In this context, nearby means within 40 minutes on foot from a person's home – aligning with youth workers and young people's testimonies.[24] I exclude a small number of areas that experienced both openings 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 closed within treated areas, I adopt an intention to treat (ITT) approach and select the year of the first closure.[25]

Summary statistics for the main outcomes of interest and selected socio-economic attributes in treated and control areas at baseline (before the austerity closures) are in Table A1. I also test whether any differences are significant using t-tests clustered on MSOA level. MSOAs are larger census areas than blocks and are defined by ONS. There are 982 in London. Clustering at this level allows to account for spatial autocorrelation while maintaining a 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. A plot of means over time in treated and control areas in Figure A1 highlights that youth offending was declining along the decade. Trends were evolving in parallel before 2012 and then began diverging in treated areas.

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, FSM eligibility and 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, and characterised by higher population density). In the survey data – where I have very limited statistical power – I control for age and gender at the individual level as well as the population-density interacted with year trends. $After \times Closure$ is 1 if the individual resides in an area where all nearby youth

[24] This seems reasonable in light of pupils' commutes to school, which is 1.7 miles on average (about 40 minutes on foot). Any noise in treatment definition – for instance, if the relevant spatial unit was the school and not the home address, or if youths living in boroughs in the limit between London and other regions attend youth clubs beyond Greater London – would attenuate the estimates.

[25] Among the 691 treated blocks, 290 experienced just one youth club closure, another 290 suffered two, 82 had three, and 29 experienced four.

clubs closed in years after closure. I control for yearly trends which are constant across space μ_t and for local block characteristics which are constant across time μ_i. The coefficient β captures the average treatment effect on treated areas (ATT); u is the idiosyncratic error term.

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

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 further control for local-area attributes changing over time such as the proportion of claimants for unemployment benefits – to control for changes in economic hardship in the area – and the distance to the nearest police stations – to control for changes in police deterrence.[26] I weight the coefficients by block-level population.

I also estimate event studies to assess the plausibility of the parallel trends assumption and evaluate the dynamics of the effects. I assume no spillovers, which in this context requires, for instance, that families with adolescent children are not moving homes from areas where youth clubs close into areas where youth clubs remain open, or that youth living near open youth clubs are not indirectly affected through their friends. I show that the assumption that people are not moving due to closures seems plausible, and argue that any spillovers to youth living in control areas would mitigate the observed coefficients. I discuss the robustness of the results to changes in sample restrictions, inference methods, the inclusion of controls, functional form, and assumptions of parallel trends in section V. The strategy also requires a no anticipation assumption. While there is evidence that in some areas the closure was very unexpected (announced within weeks), in others it is likely that residents knew with some time. Unfortunately, it is impossible to obtain accurate data on the timings of announcements or other margins such as a decline in quality preceding closures. I discuss implications of this assumption being violated in the results section.

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

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). I use the never-treated group as the

[26] Additional analyses in the Appendix omitting some of these controls or further controlling for other local-area changes in funding do not affect the coefficients.

comparison and weight each cohort equally.[27]

I report clustered standard errors at the MSOA level. Additional inference estimates, changes in sample, alternative estimators, additional control variables – including controlling for police station closures, and changes in local spending in other youth policies – as well as changes in the dependent variables are discussed in section V.

III.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, c denotes a cohort (15–16 or 10-11), and other notation is as before. The coefficient of interest is δ. The model includes cohort-year, cohort-block and year-block fixed effects.

$$E_{itc} = \mu_{tc} + \mu_{ic} + \mu_{it} + \delta After_t \times Closure_i \times Cohort_c + \epsilon_{itc} \tag{3}$$

To evaluate differential effects on offending and crime incidence rates, I estimate effects at the block-year-cohort level, where cohorts are defined as broad age groups. I focus on 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), and adults who are never allowed in youth clubs (25 and over). In equation 4, δ captures differential effects for each group relative to those aged 25 and above.

$$R_{itc} = \mu_{tc} + \mu_{ic} + \mu_{it} + \delta After_t \times Closure_i \times Cohort_c + \epsilon_{itc} \tag{4}$$

III.C Alternative treatment definitions

I estimate additional models redefining treatment to evaluate closures across London. In these analyses, treated areas are those where the nearest youth club closed and control areas are those where the nearest remained open. The difference with respect to the main analysis

[27]To implement this design, I first create individual stacks by selecting each cohort of closures (areas experiencing the first closure in the same year c) and areas where centres never closed. I select cohorts for which I can observe at least three years before and after closures and focus on that window. I append all stacks to create the final database. In estimation, I augment the model to include stack-area and stack-year fixed effects. The estimates from these regressions thus leverage variation from closures occurring between 2013 and 2016, both included.

is that treated (control) areas might have operative (closed) youth clubs. 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 the austerity policy.

I also assess the effects of a small number of openings. In those analyses, treated areas are those that experienced the opening of a youth club between 2010–2019 and control areas 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 use propensity score matching techniques to assess the similarity between treated and plausible control areas based on observables. I match areas based on proximity to clubs at baseline, proportion of the population living in social housing, proportion with higher education, proportion of population aged 14–17, distance to schools and distance to parks.

IV. Results

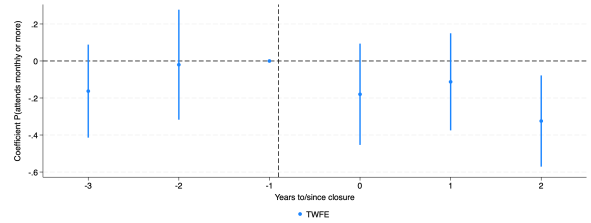
IV.A Main results

In areas where all nearby youth clubs closed attendance at organised activities fell, educational performance worsened and youth crime increased, on average. Event studies for the main variables of interest are in Figure 2 and estimated ATTs are in Table 1. The estimated effects are nearly identical in TWFE or stacked designs.

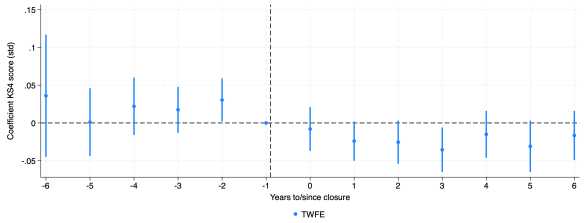
First, I estimate large falls in attendance at organised activities after closures. The likelihood of at least monthly attendance fell by 16.1 percentage points – 44% over the mean. The event study does not show a violation of parallel trends, and I cannot reject that the pre-treatment coefficients are jointly zero in F-tests (p-value of 0.392). While the analysis is based on few observations, these are very large falls, suggesting that youth clubs were not easily substitutable. This first-stage effect of the availability of youth clubs on attendance could allow to infer total effects on the treated (TOT) from reduced-form estimates, under the assumption that only those individuals attending the club would be affected by closures. The ATT coefficients would need to be multiplied by 2.27 (100/44).

Second, the closures led to decreases in performance in the national standardised exams at ages 15–16. I estimate a drop of 3.5 to 4.2% standard deviations (s.d.) after closure. The implied TOT is 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. These estimates control for individual and local area attributes. The magnitude is similar when controlling for local area attributes, such as distance to police stations, the proportion of

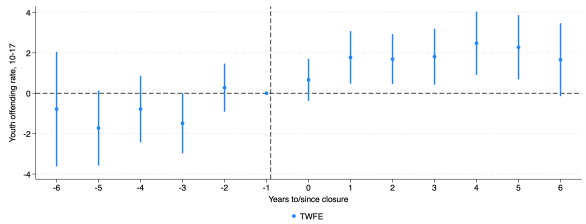
Figure 2: Event studies: effect of closures on attendance at organised activities, educational performance and youth offending



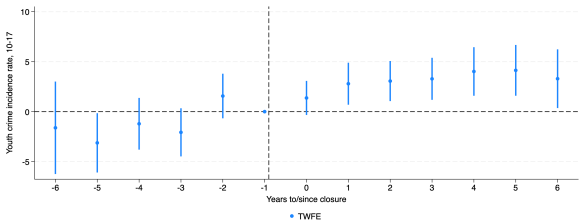
(a) P (attends organised activities)



(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.

claimants, and for local spending on formal education, or on family services (Table E4). They are also robust to changes in sample and estimation (see section V).[28] The event study could suggest that trends started to change already one year prior to the closures – perhaps due to a drop in quality or anticipation to closure in some areas – if the base year is set to t-1 of the closure, the implied effect would be larger, around 7%.

The size of the effects on test scores is substantial, being roughly equivalent to a decline of half a grade in one subject. It is similar to the estimated effects of Sure Start centres – spaces that provide a comprehensive set of resources in early childhood – in Carneiro et al. (2024), who find improvements in test scores of 3.1% s.d. at age 15–16 for the average pupil. The magnitude also within the same bounds as the work on Gilpin et al. (2024), who studies the effects of public libraries on test scores in the US.

Third, the closures led to large increases in youth crime participation, measured as the proportion of under-18s who commit crimes.[29] The proportion of young offenders per 1,000

[28]The effects do not seem to be driven by changes in the proportion of youth who attempt the exam. The estimated coefficient for the effect of youth club closures on a dummy indicator if a pupil sat the KS4 exam in Year 11 is -0.001 with a standard error of 0.001.

[29]I define such proportion year-on-year. This means that some treated individuals – who may be under 18 at

young population increased by 2 per block and year, a rise of 13% over mean offending rates. The number of crimes committed by youths increased by about 3 crimes per 1,000 youth (13%). The effect size is similar when augmenting the models to account for local area spending on education or family services (Table E4), using non-linear models and using other estimators, as discussed in section V. The event studies show parallel pre-trends and higher youth offending up to 6 years following the closures.[30] 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 V reject that violations of the parallel trends assumption are driving the effects. If the base year is set to t-1 of the observed closure (allowing for some anticipation), the estimates are increases of about 10%.

To contextualise the size of these estimates, Bell et al. (2022) find that extending the legal school dropout age in the US reduced arrest rates for the average pupil by 6% in a given year. My estimate might be larger due to youth clubs being more likely used by lower income pupils. The implied TOT is a rise of 32% for pupils who would have attended youth clubs. While these are very large increases in offending rates they are comparable to those estimated in youth interventions targeting disadvantaged pupils. In particular, causal studies on the effect of summer jobs in Heller (2014), Davis & Heller (2020) or Modestino (2019) find falls in crime between 30 and 50% due to these programs; and causal studies of after-school programs delivered in schools to youth at risk oscillate between 14 and 22% (Dinarte-Diaz & Egana del Sol 2023).

Figure 3 presents the estimated ATT by pupil and crime attributes.[31] The key attribute determining effects on educational 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). These effects are not too far from the magnitudes estimated for high-tutoring interventions on deprived youth; for instance, Guryan et al. (2023) find improvements in maths scores by 18 to 40% standard deviations. The effects are driven by pupils with lower prior attainment, as proxied by their average test scores at ages 10–11 (in KS2 or SATs). This result 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. The data also allow to assess effects for pupils who attend their nearest school and pupils who don't. For those who attend their nearest school – for whom treatment definition is less likely to be noisy – the falls in performance are

the beginning of the sample – age are ageing out of the group of interest in later years.

[30] In each year, the estimate shows the effect on youth offending. The population of interest changes each year as new children enter the group when they turn 10, or exit the group when they turn 18.

[31] Heterogeneity is assessed with individual-level regressions on subsamples by pupil attributes for educational outcomes. For offending, I estimate block-level regressions where the left-hand side captures crime rates by sub-type.

Table 1: 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	1.858	2.833
	(0.079)	(0.009)	(0.456)	(0.783)
Magnitude (%)	-44.27		12.99	13.31
p-value	0.042	0.000	0.000	0.000
Mean	0.365	0.014	14.310	21.291
N	976	352,454	22,040	22,040
Panel B: Stacked				
ATT	-0.168	-0.042	2.112	3.083
	(0.086)	(0.011)	(0.537)	(0.901)
Magnitude (%)	-48.07	-4.20	14.08	13.70
p-value	0.051	0.000	0.000	0.001
Mean	0.349	0.000	15.000	22.505
N	2,942	858,106	45,248	45,248

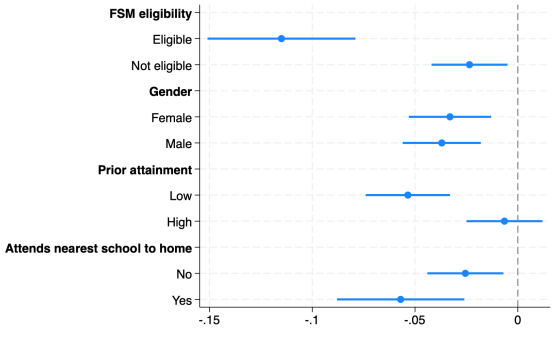
Notes: Column 1 estimates the effects of youth club closures on the likelihood of at least monthly attendance at organised activities using individual-level survey data from Understanding Society. *P (attends activ.) ages 10–15* is a binary indicator which is 1 if the individual states attending monthly or more and 0 otherwise. Controls include age, gender, and population density interacted with year. Column 2 estimates the effects on 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 (KS4) standardised in each year to mean 0 and standard deviation of 1. Controls include gender, ethnicity, month of birth, FSM eligibility and population density interacted with year. Columns 3 and 4 estimate the effects on offending rates using administrative records from MPS combined with population estimates from ONS, using block-level data and weighting coefficients by block level population. *Offending rate ages 10–17* is the proportion of residents aged 10–17 who have offended per 1,000 per block and year; and *crime incidence rate ages 10–17* is crimes committed by residents aged 10–17 per 1,000, per block and year. Controls include population density interacted with year, distance to police stations and proportion of claimants for unemployment benefits. Panel A estimates effects using TWFE. Panel B uses the stacked estimator. All columns include block and year fixed effects. In the stacked estimates, all controls and fixed effects are interacted with stack. Standard errors clustered at the MSOA level are in parentheses.

5.7% compared to 2.6% for those who do not, although the two estimates are not statistically different from each other. I do not find statistically significant differences by gender.

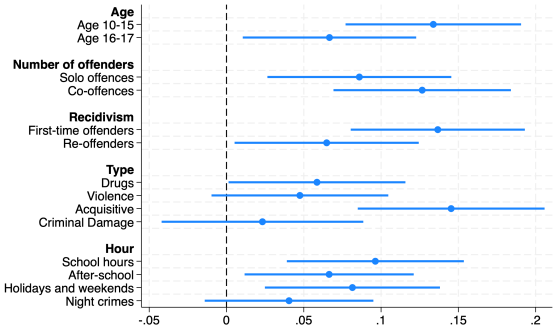
The effects on crime are more precisely estimated for pupils within compulsory schooling age (ages 10 to 15), but I cannot reject that they could be similar for pupils aged 16 to 17. The effects are slightly larger for first-time offenders than for re-offenders. There are no significant differences between 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%.[32]

[32]I assess heterogeneity in effects for the largest crime categories. Statistical power does not allow to assess narrower categories at the block, age group and year level, as crime is a reasonably rare event. 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. For each attribute, the sample is split by the attributes of individuals at baseline. *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. The dependent variables are defined at the block level, for different crime types. The sample (of blocks) is always the same. *Youth crime incidence* is defined as crimes committed by residents aged 10–17 per 1,000 residents. Block and year fixed effects included in all estimates. Coefficients calculated using block-level data are weighted by population. Confidence intervals are calculated using MSOA-level clustered standard errors.

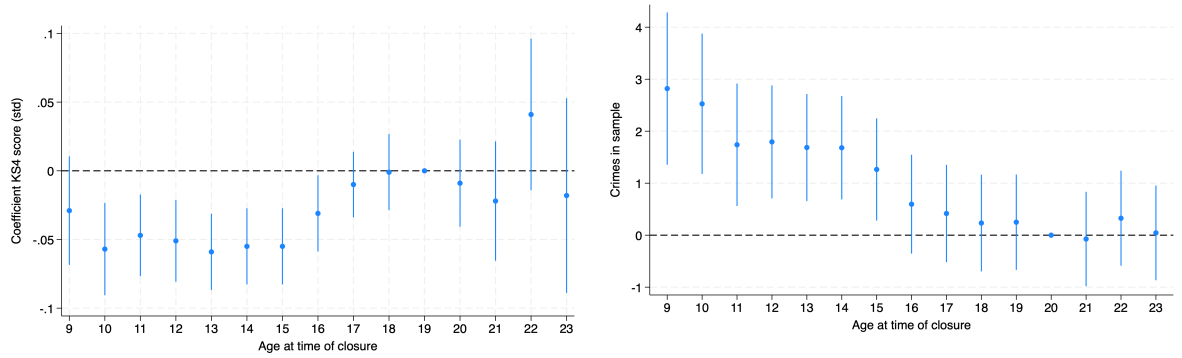
I do not find a statistically significant increase in criminal damage and arson crimes, which include graffiti and other damage to public property. They are broadly consistent with prior literature. When studying vulnerable youths, Heller (2014) and Davis & Heller (2020) find that summer jobs reduce violence-related arrests by 33 to 43%, and Modestino (2019) finds that they also reduce property crime arrests, by 29%. The effects in these papers might be larger than my estimates first, because I study the entire population and not only vulnerable youths and second, because youth clubs are spaces for universal provision.

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 –consistent with existing evidence (e.g. Heller 2014, Davis & Heller 2020).

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. I do not find a change in the spatial distribution of crimes in areas affected by closures (see Table 2). This is true for

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. Block and year fixed effects included. 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. Cohort and block fixed effects are included. 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.

local crime rates, which uses the universe of recorded crimes regardless of whether a suspect was identified, for undetected crimes, detected crimes and even crimes where the detected offender was a minor. Commuting distances to crime are not affected by the closures.[33]

How to reconcile the person-based results on rises in resident youth offending with null effects on local crime? This might be due to youth crime attractors being unchanged by youth club closures. In Table A5 in the Appendix, I present the correlation between youth offending (regardless of where those minors live) and local area attributes in blocks affected by closures, in years either before or after closures. The correlates are statistically indistinguishable across the two time samples: youth commit crimes in areas that are closer to pubs and schools (perhaps town centres with higher opportunities for crime), and areas with higher proportions of social housing. I also present estimates on the correlates across London, which are broadly similar, except that the proportions of Black residents and residents with low qualifications are also predictors of youth crime.

[33]For youths, the average distance between their homes and the location where they offend is low (0.27 kilometres or less than 10 minutes on foot). This is in line with data from Greater Manchester in Kirchmaier et al. (2024). However, the correlation between the number of crimes committed by youth in a block and the count of youth offenders in a block is only 0.23.

Table 2: Effect of closures on the spatial distribution of crimes

	(1)	(2)	(3)	(4)	(5)
		Local crime rates			**Dist. to crime**
	All	Undetected	Detected	Detec. ages 10–17	Detec. ages 10–17
Panel A: TWFE					
ATT	-0.514	-0.774	0.259	0.627	0.004
	(0.989)	(0.740)	(0.524)	(1.152)	(0.019)
Magnitude (%)	-0.83	-1.61	1.90	2.82	2.10
p-value	0.603	0.296	0.621	0.587	0.817
Mean	61.788	48.160	13.627	22.202	0.214
N	22,040	22,040	22,040	22,040	22,040
Panel B: Stacked					
After x Treated	0.816	0.544	0.272	1.941	0.010
	(1.157)	(0.873)	(0.579)	(1.255)	(0.022)
Magnitude (%)	1.22	1.04	1.88	8.33	4.63
p-value	0.481	0.533	0.638	0.122	0.668
Mean	66.826	52.322	14.504	23.309	0.207
N	45,248	45,248	45,248	45,248	45,248

Notes: Columns 1 to 4 estimate the effects of youth club closures on local crime rates. *Local crime rate* is the number of offences occurring in a block and year per 1,000 resident population. Column 1 focuses on all local crime. Column 2 on the subset of crimes that are undetected (where no suspect was identified). Column 3 assesses effects on crimes that are detected overall, and column 4 on crimes that are detected and where the suspect was between 10 and 17 at the time of the offence. Column 5 estimates the effects of closures on *Distance to crime*, the commuting distance between the home residence of the suspect and the location of the crime. Crime data come from administrative records from MPS combined with population estimates from ONS. Controls include population density interacted with year, distance to police stations and proportion of claimants for unemployment benefits in a block. All the coefficients are weighted by block population. Panel A estimates effects using TWFE. Panel B uses the stacked estimator. All columns include block and year fixed effects. In the stacked estimates, all controls and fixed effects are interacted with stack. Standard errors clustered at the MSOA level are in parentheses.

IV.B Triple difference in differences estimates

Since the period of study was characterised by austerity reforms in many areas of public policy, one might be concerned that other austerity shocks could confound the estimates. Robustness of the main results to controlling for local authority attributes, both observed (including spending in other policies) and unobserved are in the Appendix, and reject that the effects are explained by general austerity.

In addition, and to account for potentially unobserved block-level shocks, I also 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. These models control for block-year effects, cohort-year effects, and cohort-block effects. Therefore, they allow to disentangle effects on teenagers at the time of the youth club closure from any other shocks affecting all cohorts, even in very small areas (as blocks are very small).

For education, I estimate the effect of closures on exam scores at ages 10–11, which is the

Table 3: 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
N	359,029	713,275	713,275

Notes: Column 1 estimates effects of youth club closures on test scores at ages 10–11 using a difference in differences design. The dependent variable is the average score in subjects in the SAT exam (KS2). Test scores are standardised in each year to mean 0 and standard deviation of 1. Estimates include for block and year fixed effects. Columns 2 and 3 estimate effects of closures on test scores at ages 15–16 in a triple difference-in-differences setting. Estimates include block-year, cohort-year and cohort-block fixed effects. The data come from the NPD and the regressions are estimated at the individual level. Columns 1 and 3 include individual controls for gender, ethnicity, month of birth, FSM eligibility and population density interacted with year. Standard errors clustered at the MSOA level are in parentheses.

only data that exists given that there are no standardised exams year-on-year. Table 3 shows that closures did not significantly affect test scores at ages 10–11. The differential effect at ages 15–16 in the DiDiD is between 2.3 and 2.5% s.d. Although the effect is not statistically significant at conventional levels (with p-values between 0.07 and 0.1 depending on the controls), it goes in the expected direction, with closures differentially affecting teenagers.[34]

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 are above the age of majority), and those aged 25 and over (never allowed in youth clubs). Table 4 presents DiD estimates for offending and crime incidence rates at ages 18 and above.

The results show null effects for people aged 18 and above, reinforcing the validity of these groups as a plausible second control group. The DiDiD shows that when compared with people aged 25 and above, only those under 18 commit crimes differentially after closures. The magnitude is 11% for offending rates and 13% for crime incidence rates. Both coefficients are significant at the 99% confidence level.

[34]It is worth mentioning that youths aged 10 to 11 are an imperfect control group as some pupils might have gone to youth clubs or could have siblings attending youth clubs. Unfortunately, I could not systematically obtain information on the number of siblings for each pupil in the sample.

Table 4: Effect of closures on adult offending rates and triple difference

	(1)	(2)	(3)	(4)	(5)	(6)
	Offending rate			**Crime incidence rate**		
	age 18–24	age 25+		age 18–24	age 25+	
ATT	0.062	-0.336		-0.422	-0.050	
	(1.009)	(0.469)		(1.468)	(0.649)	
After x Treated x 10–17			1.697			2.827
			(0.428)			(0.731)
After x Treated x 18–24			-0.917			-1.558
			(0.962)			(1.388)
Model	DiD	DiD	DiDiD	DiD	DiD	DiDiD
Magnitude (%)	0.15	-1.71	11.86	-0.77	-0.20	13.28
p-value	0.951	0.475		0.774	0.939	
Mean	42.043	19.613	14.310	54.657	25.096	21.291
N	22,040	22,040	66,120	22,040	22,040	66,120

Notes: Columns 1, 2, 4 and 5 estimate effects of youth club closures on adult offending rates using difference-in-differences models. They include block and year fixed effects. Columns 3 and 6 estimate the differential effect of closures on youth using a triple difference in differences models. They accounts for cohort-year, cohort-block and block-year fixed effects, where cohort is defined broadly as age groups. 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). All columns control for population density interacted with year, distance to police stations and proportion of claimants for unemployment benefits. Coefficients are weighted by population. Standard errors clustered at the MSOA level are in parentheses.

IV.C Additional results

I also measure the effects of all youth club closures, not just those that left areas completely unprovided for, to assess the policy across London. In these analyses treated areas are those where the nearest youth club closed and control those where the nearest remained open.

The estimates in Table A2 (particularly, those estimated in in panel A) show that, relative to areas in which all nearby youth clubs closed, the effects are slightly attenuated. After closures, attendance at youth clubs fell by 39 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 6 to 13%. The effects on crime are driven by acquisitive offences (up by 17%) and using the stacked estimator there are some rises in violence (up by 10%). These analyses suggest that operative youth clubs might mitigate the effects of other closures and that commuting could play an important role in youths' decisions to attend organised activities.

I also evaluate the effects of a small number of openings of youth clubs 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 A3).

IV.D The role of police

I now discuss the potential role of the police in confounding or mediating the effects.

First, the rise in youth crime could be explained by increased crime recording if police chose to proactively monitor youths hanging out near closed youth clubs. I argue that this is implausible, in part, because local (recorded) crime does not increase in the area (the place-based measure of crime). I also test this potential confounder more directly using detection (clearance) and stop-and-search rates as proxies of police activity. If police were patrolling strategically, I would estimate rises in those variables. The results in Table E8 reject this explanation. Instead, detection and stop-and-search rates declined in affected areas after closures.

Second, higher offending but lower police presence would be consistent with the results being the product of lower deterrence. Lower police presence would raise concerns about the results being attributable to youth club closures if the youth club closures were confounded with other police-related policies. In particular, Facchetti (2024) studies the closure of 70% of police stations in 2011–2016 in London and finds that areas affected by closures experienced lower deterrence.

I reject this hypothesis. In Appendix D, I show that police station closures do not correlate with youth club closures either at the local authority or block levels. In addition, all the offending and local crime estimates presented control for distance to police stations, which changes year-on-year at the block level as stations close. The effects on education are also robust to this control (Table E4). Estimates separately assessing effects of police station closures on offending and local crime in Table E9 show null effects of police station closures.[35]

Last, it is still possible that the effects of youth clubs are *mediated* by lower police presence associated with youth club closures. An area with youth clubs may have higher social cohesion, where there might be wider cooperation with authorities (and police) to keep youth out of trouble. This is one way to interpret the behavioural channels through which the closure of youth clubs affects education and crime; other mechanisms are discussed in section VI.

IV.E Alternative explanations

A potential confounder of the effects is changes in neighbourhood composition. The findings could be consistent with more mobile families with adolescent children moving out of areas

[35]Facchetti uses monthly local crime data to assess effects of police station closures across London in 2011 to 2016, finding modest increases in local crime. In my analyses, I study both local crime and resident's offending and I focus on areas where all youth clubs closed and areas where all youth clubs remained open. I only leverage yearly level variation, as local authorities were not able to provide month of youth club closure systematically. The null effect of police station closures in my estimates could reflect differences in effects in this subset of areas, and/or differences in effects when extending the sample to 2010 to 2019.

where youth clubs closed and into areas where they remained open. This would potentially leave behind lower-performing students and those with a higher propensity to offend. I argue that this is unlikely, first because I observe effects immediately after closures, when families may face immediate constraints on moving. Second, I test for these changes directly using the adults' questionnaire in Understanding Society, which includes information on when the household arrived in the neighbourhood and whether they intend to move.[36] The results in Table E10 in the Appendix, indicate null effects. Households in areas affected by closures are no more likely to have recently moved in or to express a stronger preference to leave. This is true both for respondents with or without children.

V. Robustness

The estimates on education and crime reported Table 1 are robust to several changes in assumptions and estimation (see Appendix E).

Sample: In the main results, I restrict the sample to areas within 40 minutes on foot of a youth club at baseline. The findings are similar restricting to 20, 30 or 50 minutes (Table E1). The estimates are not driven by a specific borough, as shown in leave-one-out estimations (Figure E1). The results are similar omitting Inner London or excluding observations where the FOI data has lower quality (Table E2).

Inference: In the main results, I cluster standard errors at the MSOA level. Using clustered wild bootstrap at the local authority level or clustering by nearest youth club yield statistically significant effects at the 95% confidence level or above.

Controls: In the education estimates (estimated using individual level data), I control for gender, ethnicity, year of birth, month of birth, FSM eligibility, and population density at baseline interacted with year dummies. Models excluding these controls yield larger though statistically indistinguishable estimates. Controlling for observable area attributes such as distance to police stations, local unemployment, or spending on other youth-related policies does not alter the estimates (Table E4).

In the offending estimates (estimated using block level data), I control for population density interacted with year, local claimants, and distance by police stations. I also weight the coefficients by population. The findings are consistent when omitting controls, accounting for local area spending on other youth-related policies, or omitting weights.

Controlling for unobservable attributes at the borough-year level, which would account for other local area shocks, yield smaller though statistically indistinguishable estimates.

[36] Data on official population inflows and outflows is not available at such level of granularity.

Offending definitions: The distribution of crime rates often features a long right tail, which might represent outliers. The main results present rates censored at the 99$^{\text{th}}$ percentile, but the findings are similar when censoring at the 95$^{\text{th}}$ percentile, without censoring, or using offender counts instead of rates.

Parallel trends: The parallel trends assumption is critical to interpreting the effects as causal. 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). However, in the event study of youth crime incidence rates, the p-value of the joint F-test is 0.02, challenging the validity of the assumption.

I assess the sensitivity of these results to allowing pre-trends to differ from zero within specified bounds (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 re-estimate the effects weighting the control areas to resemble the treated in their pre-trends as per Arkhangelsky et al. (2021) and Porreca (2022). The estimated rise in offending rates is 17% and the estimated rise in crime incidence is 21%. Both are significant at the 99% confidence level.

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 use TWFE, stacked models, and saturated models as in Wooldridge (2023). The estimated rise in offending ranges from 14 to 35% and is always statistically significant at the 99% confidence level (Table E6).

VI. Mechanisms

Youth clubs improve young people's educational performance and avert youth crime. The effects are stronger for youths from more deprived backgrounds and those who were younger when youth clubs closed. The estimates reject pure incapacitation as a key channel, and I argue that instead, a key mechanism might that youth clubs are not easily substitutable. In addition, they might play a direct role on human capital acquisition by facilitating a space to study, and enable a happier environment in the community.

First, young people are not substituting to other structured after-school activities after closures. Instead, they seem to divert time towards cheap leisure. While statistical power is limited in analyses of survey data, self-reported weekly hours on social media increase by 12% after closures, hours playing videogames rise by 38%, and TV hours increase by 9% (Table 5).

Table 5: Effect of closures on youths' study time, neighbourhood perceptions, and screen time

	(1)	(2)	(3)	(4)	(5)	(6)
	Study time			**Screen time**		
	Homework hours	Easy to concentrate	Likes neighbourhood	Social media hours	Videogames hours	TV hours
ATT	-1.504	-0.528	-0.151	0.413	0.863	0.373
	(0.551)	(0.189)	(0.073)	(0.234)	(0.489)	(0.215)
Magnitude (%)	-32.39	-52.76	-17.22	12.16	37.63	8.73
p-value	0.007	0.006	0.041	0.078	0.079	0.083
Mean	4.644	0.107	0.875	3.397	2.293	4.273
N	907	1,071	799	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 school days 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. Likes neighbourhood is a dummy indicator which is 1 if the individual responds 'yes' to the question "Do you like your neighbourhood?" The variables *social media hours*, *videogames hours* and *TV hours* refer to daily engagement with each activity on school days. Controls include age, gender, and population density interacted with year. All columns include block and year fixed effects. Standard errors clustered at the MSOA level are in parentheses.

Amenities such as libraries, parks, or sports centres do not seem to mitigate the effects of youth clubs closing. I argue this based on analyses of effects across London (nearest youth club closed versus did not) where I distinguish affected areas by whether they are below or above median distance to other leisure spaces (Table 6).[37] Proximity to operative youth clubs mitigates the falls in educational performance and the rises in crime. In areas that are relatively close to operative youth clubs test scores only fall by 1.1% (as opposed to 3.7%) and offending rises by 0.091 per 1,000 residents (as opposed to 1.656). Compared to this, for other amenities the coefficient on the interaction is not statistically significant at conventional levels. Hence, youth clubs seem to be unique in their effects on youth. This might be due to their attributes: they differ from libraries or parks in that they provide structured activities and exposure to a trusted adult, and from sports centres in that their activities are free or very cheap.[38]

Second, the clubs might have direct effects on education by providing a safe space to study. After closures, pupils report spending fewer hours on homework, and finding it more difficult to concentrate. In addition, while statistically insignificant, the p-value of the coefficient of the interaction between being in an affected area after closures but near libraries is 0.10 for the estimand on education (although not for crime). Hence, part of the explanation on test scores might be due to youth clubs providing a safe space to study (as do libraries).

Third, youth affected are less likely to state liking their neighbourhood, which might suggest

[37]Median distance is defined relative to each amenity. Libraries, parks and sports centres are much more prevalent than youth clubs. For instance, there is no single block in London further than 1 kilometre from parks, nor further from a sports centre. Meanwhile, the average distance to a youth club at baseline was 1.8 kilometres. The interactions over being treated and close to libraries, parks, or sports centres, denote affected areas which are very close to (below median distance from) these amenities.

[38]When estimating jointly all the interaction coefficients only proximity to operative youth clubs is significant.

Table 6: 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	1.446	0.812	0.741	0.933
	(0.456)	(0.467)	(0.485)	(0.487)
After x Treated x Near youth club	-1.307			
	(0.685)			
After x Treated x Near library		0.243		
		(0.708)		
After x Treated x Near sports			0.454	
			(0.701)	
After x Treated x Near park				-0.028
				(0.650)
Mean	15.19	15.19	15.19	15.19
N	35,760	35,760	35,760	35,760

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. Controls include gender, ethnicity, month of birth, FSM eligibility and population density interacted with year. The regressions on offending rates use administrative records from MPS combined with population estimates from ONS, using block-level data. *Offending rates* is defined as the residents aged 10–17 who have offended per 1,000 residents aged 10–17. Controls include population density interacted with year. 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 the distance is equal to or below the median and 0 otherwise. Treated areas are those where the nearest youth club closed and control areas are those where the nearest youth club remained open (across London, not restricting to areas where all youth clubs closed or remained open). All columns include block and year fixed effects. Coefficients calculated using block-level data are weighted by population. Standard errors clustered at the MSOA level are in parentheses.

anger, or a lack of social cohesion in the area as another channel. If after youth club closures youth in the area feel left behind, it is plausible that some of that anger manifests through unwanted behaviours (like offending).

The results suggest that youth clubs provide a unique bundle of specialised resources which is difficult to substitute for, and which has directly beneficial effects on education (and perhaps on social cohesion). Individual testimonials report that youth clubs kept them from engaging with the wrong people, redirected them from harmful paths, that exposure to

different activities helped them develop hobbies and new interests, and that youth clubs improved their self-perception.

VII. Cost–effectiveness and cost–efficiency of the closures

VII.A Cost–benefit analysis

I estimate the cost-effectiveness of the closures to understand if keeping youth clubs open across London might have been effective. These calculations intend to provide a rough estimate using the effects measured in this paper and omit important considerations, such as social returns to education, externalities on neighbourhood value, inflation etc. I provide a cost over benefit ratio and also decompose costs and benefits by whether they affect private individuals, or the government to infer the marginal value of public funds in this context.

To that end I combine my estimates in Table A2 with existing literature on returns to education and costs of crime. The detailed calculations behind Table 7 are in Appendix F.[39]

The benefits of closing youth clubs are savings in yearly running costs borne by local authorities. I multiply the estimate of average cost per youth club and year, from FOI information and a survey among youth workers, and the number of youth clubs affected.

Against these benefits, I consider forgone returns to education. Privately, individuals affected have lower lifetime earnings – Hodge et al. (2021) connects 1 s.d increase in test scores to increased lifetime earnings of 96,111 in net present value. In terms of public finances, these forgone returns also imply forgone taxed revenue for government; I use the average tax for the UK from OECD (2024).

In terms of crime, the closures imply additional victims, and associated private damage. I consider both detected and undetected offences in violent and acquisitive crimes and obtain costs from Heeks et al. (2018). The additional offenders also mean associated costs to government through the criminal justice system (police spending, custody and courts), estimated at 8,000 per offender from National Audit Office (2011).

Under conservative assumptions, the closures resulted in net losses of £204 million. For every £1 saved, the costs to affected users, crime victims and public spending in the police and the criminal justice system amounted to £2.65.[40] The majority, £1.90, is due to educational losses and £0.75 is due to crime costs. As a comparison, a policy report by Frontier Economics and UK Youth estimates that returns on investment to youth clubs are between £3.20 and

[39]Table 7 uses the ATT Table A2. Tables A6 and A7 provide plausible ranges using the lower and upper bounds of the 95% confidence interval reported in Table A2.

[40]Using the upper and lower bounds in Tables A6 and A7, the costs over benefits are between 0.86 and 4.01.

Table 7: Cost–benefit analysis of youth club closures

	Public vs private	Most Likely Scenario **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	*14,925 × 0.080 × 8,000*	-9,524,267
Detected violent crime, costs to victims	Private	*4,734 × 0.083 × 11,446*	-4,496,870
Potentially undetected vio. crime, costs to victims	Private	*4,734 × 0.083 × 11,446 × 3.25*	-14,614,829
Total costs in violent crimes	Private	*Detected + undetected*	-19,111,699
Detected acquisitive crimes, costs to victims	Private	*6,178 × 0.167 × 4,093*	-4,224,324
Potentially undetected acqu. crime, costs to victims	Private	*6,178 × 0.167 × 4,093 × 9.98*	-42,158,758
Total costs in acquisitive crimes	Private	*Detected + undetected*	-46,383,083
Net benefit			
Net benefit to private individuals	Private		-209,652,776
Net benefit to public funds	Public		45,317,955
Cost/Benefit		*Total costs / Total savings*	2.65
Education costs over Savings			1.90
Crime costs over Savings			0.75
MVPF		*Total private costs / Total public savings*	4.63

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 A2. 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).

£6.40, considering different data and using a different methodology. Hence, my estimates are close to the lower bound of those provided by youth organisations.

I also approximate the marginal value of public funds (MVPF) (Finkelstein & Hendren 2020), by comparing the private societal returns to public spending had the government been able to raise revenue to keep youth clubs open. In the numerator – the societal returns – I include the returns to education after tax and the (avoided) costs of violent and acquisitive crimes by victims. In the denominator – the public savings – I include savings from closures, forgone tax revenue and incurred costs in the criminal justice system processing criminals. I estimate that keeping the youth clubs open would have generated returns of £4.63 for society.

VII.B Commuting costs and counterfactual closures

In areas that had operative youth clubs after closures the detrimental effects were partially mitigated, which suggests that commuting costs play a role in people's decisions to participate in after-school activities and that having a core spatial provision of clubs could be beneficial. Here, I explore counterfactual closures and whether these might have been welfare-improving.

Table 8: Closing regimes – change in commute times and youth offending rates

Scenario	(1) Avg. commute	(2) Δ mins	(3) %Δ mins	(4) Δ offending 10–17 %Δ mins × 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, estimated in Appendix G. *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.

This is pertinent as the cost-benefit analysis above does not consider opportunity costs. Hence this section asks whether, conditional on having to close, other patterns 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 the likelihood of youth offending. This model assumes that distance is the key attribute determining attendance and that crime is an independent alternative. Details on the model and computations are in Appendix G and suggest that a 1% increase in distance to the youth club would result in a 0.42% increase in crime participation rates.[41]

Second, I attempt to determine which combination of youth clubs minimise commuting time across space, conditional on only being able to finance a limited number of youth clubs. These are computed using the p-median model (Church & ReVelle 1974) and linear programming. A detailed explanation is available in Appendix G, where I also include maps with the optimal spatial location of youth clubs as identified in these exercises. These analyses assume that all youth clubs provide similar services and no capacity constraints.[42]

Third, I use the estimated cross-price elasticity to assess how different closing regimes would have affected offending in Table 8.

Under observed closures, the average distance to youth clubs increased by 3.58 minutes across London – a 14% rise relative to baseline – and hence offending is estimated to have increased by 5.98% (the cross-price elasticity × 14, and relative to what would have happened

[41] Estimates regressing the log of crime on the log of distance yield nearly identical elasticities.
[42] This assumption is contestable, but quality data at the youth club level is lacking.

had those areas remained on trend).

Did local authorities do better or worse than random? The average change in commute in 1,000 random closures is 4.29 minutes (or 17% relative to baseline). Hence, offending would have increased by about 7.17% and random closures are only slightly worse than the real ones.[43]

Under my assumptions, optimal closures would have mitigated crime rises almost entirely, with offending increasing by less than 1%. One may also 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 and my assumptions, youth crime would rise by 25%.

While these calculations need to be interpreted carefully, they might be a useful illustration on how the allocation of place-based policies can affect social interactions and the value of economics in helping such allocation designs.

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.

VIII. Conclusions

Youth clubs are after-school programmes which provide free leisure activities and exposure to a youth worker to teenagers, 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, survey data and administrative records, among other sources. The setting allows me to exploit variation in youth club availability induced by austerity cuts, which resulted in the closure of nearly 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 less likely to attend organised after-school activities, by 44%. The closures lead to worse educational performance in exams at ages 15–16, by nearly 4% of a standard deviation. The effects are larger for lower-income pupils. In affected areas, minors became 14% more likely to commit crimes and committed 15% more crimes. The results were driven by acquisitive crimes, violence and drug offences. I find crime rises across all hours of the day, suggesting that the effects are not limited to pure incapacitation. The results add to growing evidence on adolescence as a key period for intervention and the importance of education beyond the classroom.

[43]The estimates are very similar when closing 27% of youth clubs at random within each borough.

These estimates are not driven by general austerity. Controlling for observable and unobservable local attributes, including spending in other policies, do not alter the estimates. 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, not confounded by police station closures or changes in neighbourhood composition, and are robust to several changes in estimation and assumptions.

These effects might be explained by youth clubs not being easily substitutable, directly providing a safe space to study, and perhaps by other channels related to social cohesion. After closures, youths spend more time on screens and less time doing homework. 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 £2.65. The closures were also inefficient and considering commuting costs could have mitigated the negative impacts.

A limitation of this study is that London may have unique characteristics, and that I have no information on youth club quality. This may limit the external validity of the findings. However, anecdotal evidence suggests that youth clubs in other parts of the UK operate similarly to those in London, as do youth clubs in other parts of the world.

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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ONLINE SUPPLEMENTARY APPENDICES

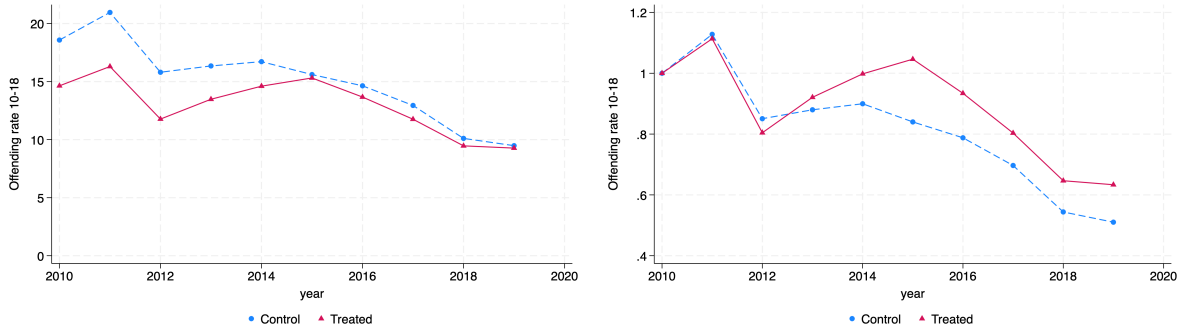
A. Additional Tables and Figures

Table A1: 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 at baseline (before the closures), as well as their difference and standard error of the difference. Standard errors clustered at the MSOA level.The Census is from ONS as of 2011, crime rates were computed by combining administrative records from MPS with population data from ONS and are shown for April-December 2010, survey responses are from Understanding Society as of 2010 and education variables are from the NPD as of 2010. 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.

Figure A1: Offending rates over time, descriptive trends



(a) Raw means

(b) Raw means indexed to 2010

Notes: The figure shows average youth offending rates per 1,000 by year and area. Control areas are those where all youth clubs within 40 minutes remained operative by 2019. Treated areas are those where all youth clubs within 40 minutes closed.

Table A2: 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	0.920	0.185	0.145	1.339
	(0.063)	(0.007)	(0.390)	(0.184)	(0.186)	(0.367)
Magnitude (%)	-44.82	-2.60	6.06	4.04	3.39	16.67
p-value	0.015	0.000	0.018	0.316	0.434	0.000
Mean	0.341	-0.008	15.192	4.565	4.283	8.032
N	1,794	590,455	35,760	35,760	35,760	35,760
Panel B: Stacked						
ATT	-0.125	-0.042	2.029	0.428	0.716	1.232
	(0.064)	(0.009)	(0.511)	(0.283)	(0.293)	(0.469)
Magnitude (%)	-38.64	-4.20	12.76	8.77	15.58	14.89
p-value	0.052	0.000	0.000	0.131	0.015	0.009
Mean	0.324	-0.016	15.904	4.879	4.593	8.274
N	6,171	2,104,447	71,260	71,260	71,260	71,260

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 and 0 otherwise. Controls include age, gender, and population density interacted with year. 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. Controls include gender, ethnicity, month of birth, FSM eligibility and population density interacted with year. 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. Controls include population density interacted with year, distance to police stations and proportion of claimants for unemployment benefits. All columns include block and year fixed effects. In the stacked regressions, all controls and fixed effects are interacted with stack. Treated areas are those where the nearest youth club closed and control those where the nearest youth club remained open (across London, not restricting to areas where all youth clubs closed or remained open). Coefficients calculated using block-level data are weighted by population.

Table A3: 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	-4.210	-3.595	-5.828
	(1.223)	(1.442)	(3.187)
Magnitude (%)	-38.56	-32.93	-53.39
Weight	Population	Prop. score	Inverse prop. score
p-value	0.001	0.013	0.068
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. All columns include block and year fixed effects. Standard errors clustered at the MSOA level are in parentheses.

Table A4: Effect of closures on the spatial distribution of crimes – changes in sample radii

	(1)	(2)	(3)
	Local crime rates		
ATT	-0.561	0.038	-0.514
	(1.747)	(1.280)	(0.989)
Sample	20 minutes	30 minutes	50 minutes
Magnitude (%)	-3.52	0.25	-3.59
p-value	0.748	0.977	0.603
Mean	15.946	15.239	14.310
N	8,370	15,770	22,040

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. Sample radii relative to block centroid. Controls include population density interacted with year, distance to police stations and proportion of claimants for unemployment benefits. All columns include block and year fixed effects. Coefficients calculated are weighted by population. Standard errors clustered at the MSOA level are in parentheses.

Table A5: Correlation between local attributes and the spatial distribution of crimes committed by minors

	(1)	(2)	(3)	(4)	(5)	(6)
	Local crime by minors					
Dist. to nearest youth club	-0.493	-0.503	-0.841	-0.928	-0.678	-0.490
	(0.104)	(0.113)	(0.381)	(0.435)	(0.324)	(0.301)
Dist. to nearest pub	-2.254	-2.736	-3.799	-4.052	-2.623	-3.228
	(0.306)	(0.335)	(1.197)	(1.364)	(1.192)	(1.208)
Dist. to nearest school	-1.856	-2.333	-2.555	-2.905	-2.960	-3.440
	(0.240)	(0.305)	(0.785)	(0.920)	(0.648)	(0.791)
Dist. to nearest park	-0.000	-0.001	-0.002	-0.003	-0.002	-0.003
	(0.001)	(0.001)	(0.002)	(0.003)	(0.002)	(0.002)
Dist. to police station	-0.088	-0.153	-0.285	-0.226	-0.506	-0.642
	(0.066)	(0.082)	(0.300)	(0.289)	(0.242)	(0.390)
% Asian pop	0.826	2.077	0.006	2.460	-0.039	3.195
	(0.480)	(0.658)	(1.250)	(2.591)	(0.859)	(3.021)
% Black pop	2.658	2.434	3.120	6.037	-4.126	4.321
	(0.905)	(0.951)	(4.939)	(4.881)	(4.207)	(3.242)
% social housing	1.208	3.443	9.588	9.771	7.681	9.661
	(0.511)	(0.778)	(3.373)	(3.661)	(2.859)	(3.975)
% no qualifications	5.737	-2.418	-8.006	-10.339	-0.733	-13.308
	(1.498)	(2.675)	(9.455)	(12.229)	(8.088)	(12.098)
Area sample	London	London	Areas affected	Areas affected	Areas affected	Areas affected
Time sample	full	full	before closures	before closures	after closures	after closures
Borough FE	No	Yes	No	Yes	No	Yes
Mean	3.541	3.541	3.875	3.875	3.254	3.254
N	48,290	48,290	2,416	2,416	4,494	4,494

Notes: Correlation between the local crime rates where the detected offender was between 10 and 18 years old and local area attributes. *Local crime* is defined as the number of offences committed by minors (residing in any area) per 1,000 resident population. Distances expressed at baseline (e.g. distance to youth clubs is regardless of whether open or closed). Crime data come from administrative records from MPS combined with population estimates from ONS and the regressions are estimated at block level. All columns include year fixed effects. Columns 2, 4, and 6 further control for local area fixed effects. Columns 1 and 2 use the full sample, columns 3 and 4 focus on areas where all youth clubs within 40 minutes closed in years before closures, and columns 5 and 6 focus on areas where all youth clubs within 40 minutes closed, in years after closures. Standard errors clustered at the MSOA level are in parentheses.

Table A6: Cost–benefit analysis of youth club closures – lower bound

	Public vs private	Optimistic Scenario **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	*15,673 × 0.028 × 8,000*	-3,540,867
Detected violent crime, costs to victims	Private	*5,134 × -0.001 × 11,446*	76,636
Potentially undetected vio. crime, costs to victims	Private	*5,134 × -0.001 × 11,446 × 3.25*	249,067
Total costs in violent crimes	Private	*Detected + undetected*	325,704
Detected acquisitive crimes, costs to victims	Private	*6,671 × 0.081 × 4,093*	-2,205,049
Potentially undetected acqu. crime, costs to victims	Private	*6,671 × 0.081 × 4,093 × 9.98*	-22,006,385
Total costs in acquisitive crimes	Private	*Detected + undetected*	-24,211,433
Net benefit			
Net benefit to private individuals	Private		-68,242,036
Net benefit to public funds	Public		82,301,355
Cost/Benefit		*Total costs / Total savings*	0.86
Education costs over Savings			0.58
Crime costs over Savings			0.28
MVPF		*Total private costs / Total public savings*	0.83

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 A2. 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 A7: Cost–benefit analysis of youth club closures – upper bound

	Public vs private	Pessimistic Scenario **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	*14,246 × 0.131 × 8,000*	-14,962,627
Detected violent crime, costs to victims	Private	*4,392 × 0.167 × 11,446*	-8,409,846
Potentially undetected vio. crime, costs to victims	Private	*4,392 × 0.167 × 11,446 × 3.25*	-27,331,999
Total costs in violent crimes	Private	*Detected + undetected*	-35,741,845
Detected acquisitive crimes, costs to victims	Private	*5,753 × 0.253 × 4,093*	-5,965,509
Potentially undetected acqu. crime, costs to victims	Private	*5,753 × 0.253 × 4,093 × 9.98*	-59,535,784
Total costs in acquisitive crimes	Private	*Detected + undetected*	-65,501,293
Net benefit			
Net benefit to private individuals	Private		-317,480,130
Net benefit to public funds	Public		17,490,706
Cost/Benefit		*Total costs / Total savings*	4.01
Education costs over Savings			2.84
Crime costs over Savings			1.17
MVPF		*Total private costs / Total public savings*	18.15

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 A2. 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 A8: Proximity to youth clubs and attendance at organised activities

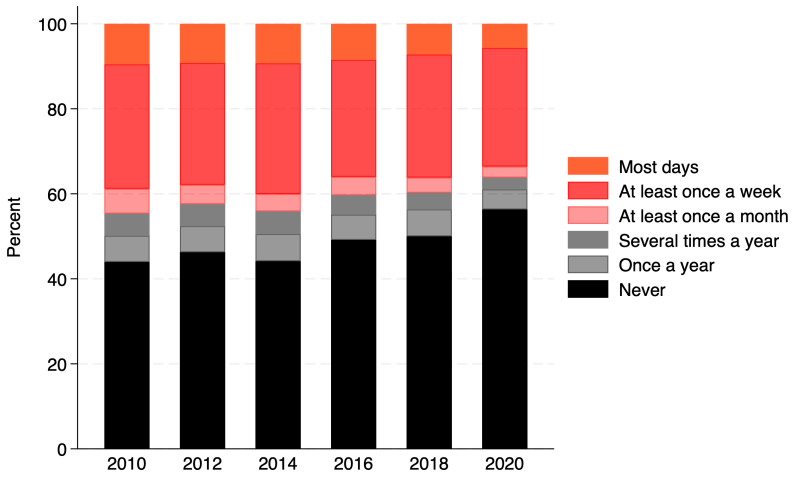
	(1) P(attends yearly or more)	(2) P (attends monthly or more)	(3) P (attends weekly or more)
Commuting time (mins)	-0.067	-0.057	-0.044
	(0.025)	(0.024)	(0.025)
Elasticity (%)	-1.01	-0.91	-0.69
p-value	0.008	0.018	0.079
Mean in population	0.412	0.351	0.307
N	181	207	179

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.

B. Attendance at organised activities and youths' attributes

This appendix presents the correlation between attendance at structured after-school activities and various time-use and socio-economic indicators, using survey data from Understanding Society. These activities include youth clubs, paid after-school initiatives, and participation in organisations like the Scouts or Guides. 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 B1.

Figure B1: 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 B1 shows the difference in selected variables between youths who attend organised activities at least yearly and those 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.

Table B1: 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.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*.

C. Youth clubs and youth club users in London

This appendix provides 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 publicly available websites.

Table C1 summarises the attributes of London's youth clubs. Figure C1 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 C2 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 C1: 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

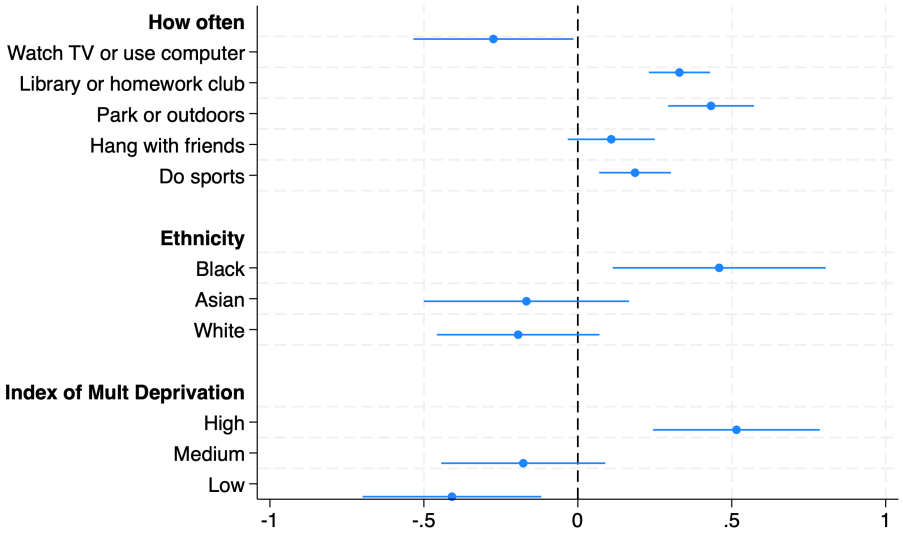
Notes: Characteristics of youth clubs gathered through FOI requests and an online search of each youth club in London. Total number of youth clubs in sample is 339. Information was more readily available for clubs that were still open as of 2019. The median opening hour was 15:00 and the median closing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

Figure C1: Word cloud of advertised activities in youth clubs



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

Figure C2: 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.

C.A Insights from surveying youth workers

I conducted an online survey using Qualtrics. The survey was disseminated among adults with the help of various youth organisations across the UK and ran during September 2024. A total of 40 people completed the survey; 33 were youth workers and 7 were people who used to attend youth clubs.

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. The activities perceived as most important 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 felt 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 activities perceived as most important to users were sports, mentorship programmes and a chill-out area.

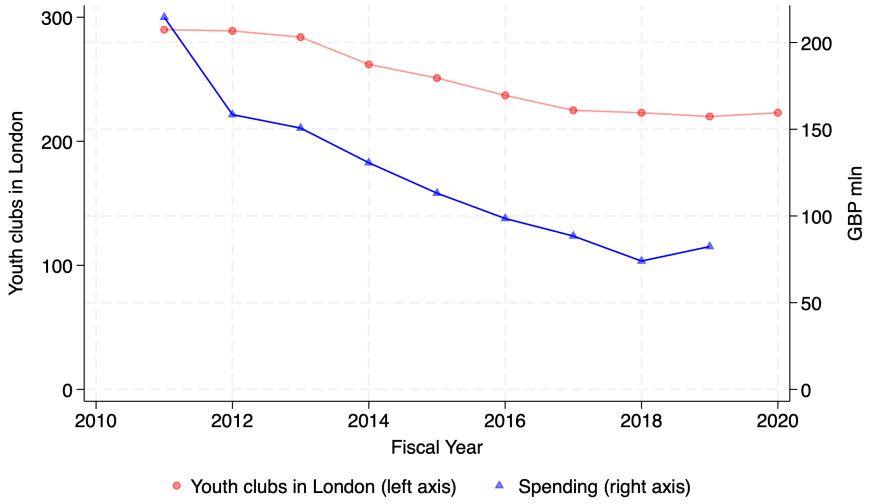
D. Youth club closures and austerity

The decade from 2010 to 2019 was marked by austerity policies affecting various areas of public life. This appendix explores whether youth club closures are correlated with other shocks to assert whether the observed effects of youth clubs on outcomes are confounded by

other policies.

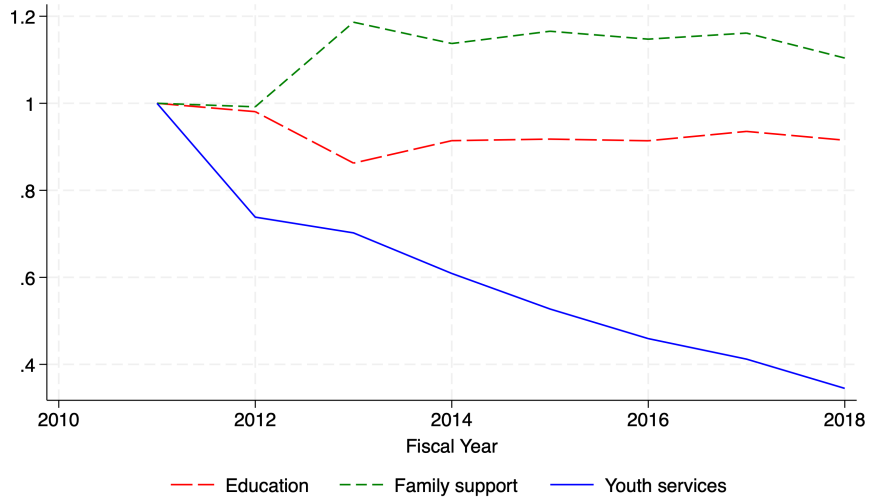
Figures D1 and D2 provide a high-level overview of the youth club closures shock compared to other policies. Figure D1 presents the fall in spending on youth services and the number of operative clubs in London boroughs in absolute terms. Figure D2 presents the trends in spending on various children and young people's policies indexed to the fiscal year 2010/2011. The fall in spending in youth services (approximately half of which relates to youth clubs specifically) is dramatic. Spending in youth services by the end of the decade was 38% of the budget in 2010/2011. In contrast, other youth-related policies, like spending in formal education (which includes schools) remained relatively constant, or in the case of family services, decreased only slightly.

Figure D1: 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 D2: 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.

Next, I explore the relationship between youth club closures and other proxies for the incidence of austerity at the local authority level; which is the administrative level at which several social policies are designed and implemented.

First, there is no correlation between youth club closures and the reduction in individual welfare transfers as measured by Beatty & Fothergill (2014).[44] A linear regression of the number of youth centre closures on individual welfare cuts at the local authority level yields a coefficient of -0.003, with a p-value of 0.704 (N=32).

Second, I study whether the youth club closures are correlated with the closure of 70% of police stations in London in 2010-2019 as studied in Facchetti (2024) – who finds short term effects in local crime and detections. At the local authority level, the shocks are uncorrelated. Only 7% of all possible borough–year combinations have both a police station closure and a youth club closure in the same year, and a linear regression relating the youth centres closed to the police stations closed yields a coefficient of 0.086, with associated p-value of 0.831 (N=32). Perhaps more convincingly, my treatment variable (all nearby youth clubs closed vs all nearby remained open) and Facchetti's treatment (nearest police station closed vs nearest remained open) are uncorrelated at the block-year level ($\widehat{\gamma}$ of 0.025 with a SE of 0.019). Hence, I reject that the two shocks are confounded at the local authority or block level.

Third, youth club closures are uncorrelated with changes in unemployment, as proxied by the proportion of claimants for unemployment benefits for Job Seekers Allowance and Universal credit, and obtained from ONS. They are also uncorrelated with youth offending

[44] This metric has been 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.

rates at baseline. Therefore, I argue that my estimates are unlikely to reflect changes in other policies.

D.A 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.

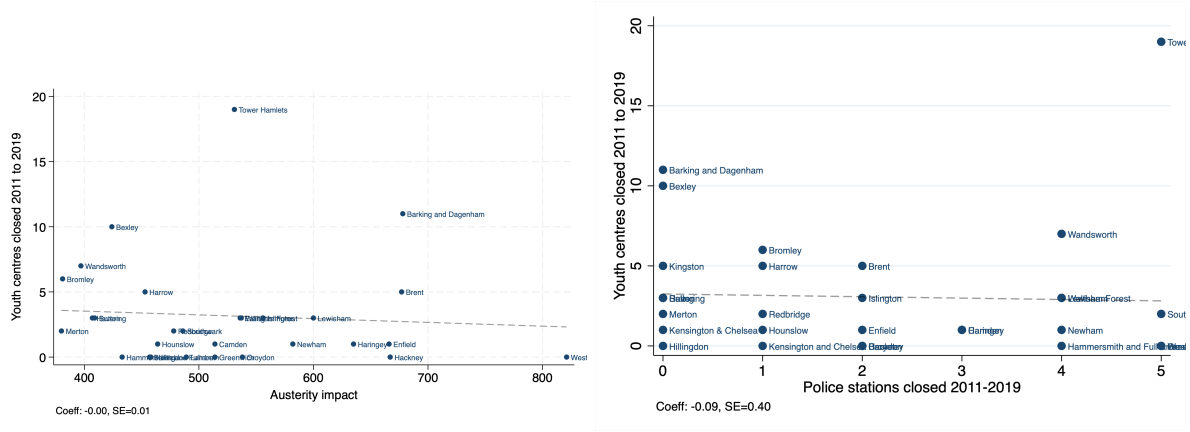
Qualitatively, from local authorities' meeting minutes, the selection of youth clubs for closure appears to be multifaceted. Some councils shifted from *universal* to *targeted* provision of youth clubs and closed the ones that were not considered to reach the intended youths. In some cases, proximity 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.

Quantitatively, 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 D1 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

Figure D3: 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.

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. In my main estimates, described in the main section of the paper I allow areas to differ in trends by population density, and also perform additional robustness checks omitting areas where the nearest youth club is a charity from the analysis.

Table D1: 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.

E. Robustness of the main results

Table E1: 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	1.412	2.152	1.508
	(0.870)	(0.571)	(0.427)
Sample	20 minutes	30 minutes	50 minutes
Magnitude (%)	8.86	14.12	10.95
p-value	0.105	0.000	0.000
Mean	15.946	15.239	13.766
N	8,370	15,770	26,120

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. Controls include gender, ethnicity, month of birth, FSM eligibility and population density interacted with year. 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. Different radii are relative to block centroid. Controls include population density interacted with year, distance to police stations and proportion of claimants for unemployment benefits. All columns include block and year fixed effects. Coefficients calculated using block-level data are weighted by population. Standard errors clustered at the MSOA level are in parenthesis.

Table E2: 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. Controls include gender, ethnicity, month of birth, FSM eligibility and population density interacted with year. 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. Controls include population density interacted with year, distance to police stations and proportion of claimants for unemployment benefits. 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. All columns include block and year fixed effects. Coefficients calculated using block-level data are weighted by population. Standard errors clustered at the MSOA level are in parenthesis.

Table E3: 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

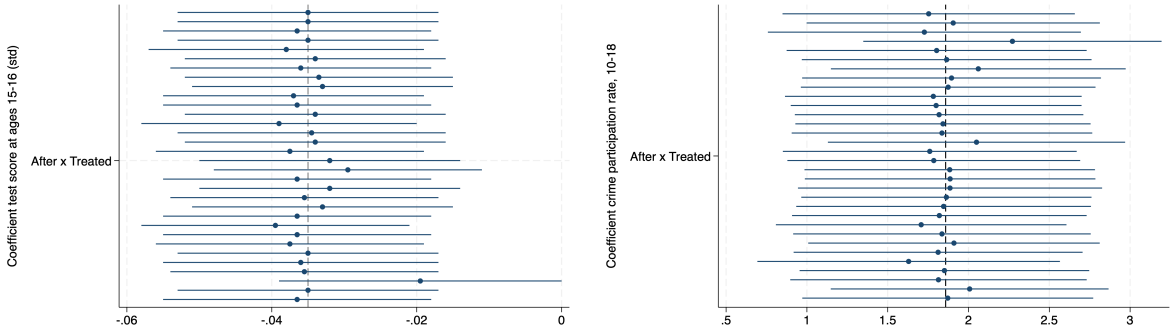
Notes: Estimates on test scores in national exams at ages 15–16 use individual-level data from the NPD. *Test score at ages 15–16* is the average score across all subjects in the GCSEs standardised to mean 0 and standard deviation of 1. Controls include gender, ethnicity, month of birth, FSM eligibility and population density interacted with year. 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. Controls include population density interacted with year, distance to police stations and proportion of claimants for unemployment benefits. All columns include block and year fixed effects. Coefficients calculated using block-level data are weighted by 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).

Table E4: Effect of closures on educational performance and youth offending – different sets of controls

	(1)	(2)	(3)	(4)	(5)
			Test score		
			ages 15–16		
ATT	-0.044	-0.035	-0.035	-0.035	-0.029
	(0.009)	(0.009)	(0.010)	(0.010)	(0.013)
Dist. police stations		-0.001	-0.002	-0.002	0.002
		(0.002)	(0.003)	(0.003)	(0.003)
LA spend on Education			0.000	0.000	
			(0.001)	(0.001)	
LA spend on family support				0.000	
				(0.001)	
Ind. Controls	Yes	Yes	Yes	Yes	Yes
Borough-Year FE	No	No	No	No	Yes
p-value	0.000	0.000	0.001	0.001	0.023
N	352,454	352,454	352,454	352,454	352,454
			Offending		
			ages 10–17		
ATT	1.880	1.858	1.896	1.888	1.272
	(0.454)	(0.456)	(0.459)	(0.458)	(0.581)
Dist. police station		-0.084	-0.079	-0.088	-0.016
		(0.456)	(0.459)	(0.458)	(0.581)
LA spend on education			0.000	0.000	
			(0.459)	(0.458)	
LA spend on family support				-0.000	
				(0.458)	
Area controls	Yes	Yes	Yes	Yes	Yes
Borough-year FE	No	No	No	No	Yes
Magnitude (%)	13.14	12.99	13.25	13.20	8.89
p-value	0.000	0.000	0.000	0.000	0.029
Mean	14.31	14.31	14.31	14.31	14.31
N	22,040	22,040	22,040	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. Controls include gender, ethnicity, month of birth, FSM eligibility and population density interacted with year. 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. Controls include the proportion of claimants for unemployment benefits and population density interacted with year. All columns include block and year fixed effects. Column 5 further controls for borough-year fixed effects. Distance to police stations obtained from MPS. LA spending obtained from the Department for Education. Coefficients calculated using block-level data are weighted by population. Standard errors clustered at the MSOA level are in parenthesis.

Figure E1: 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

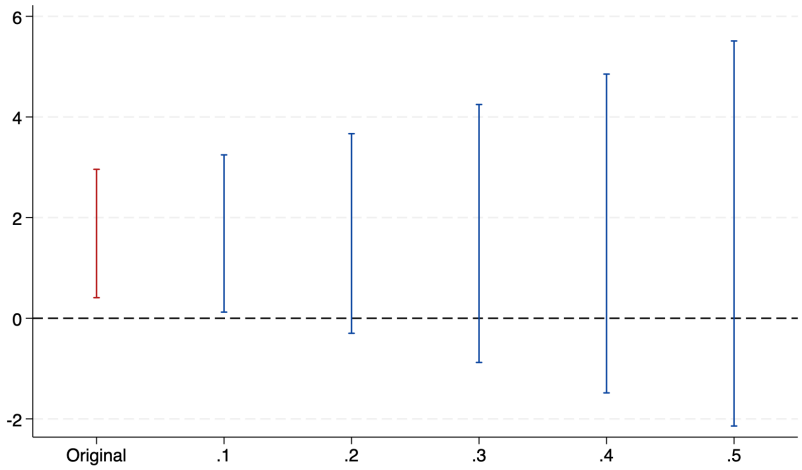
Notes: Estimates on test scores in national exams at ages 15–16 use individual-level data from the NPD. *Test score at ages 15–16* is the average score across all subjects in the GCSEs standardised to mean 0 and standard deviation of 1. Controls include gender, ethnicity, month of birth, FSM eligibility and population density interacted with year. 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. Each coefficient and confidence interval is estimated excluding one of 32 boroughs. Controls include population density interacted with year, distance to police stations and proportion of claimants for unemployment benefits. All estimates include block and year fixed effects. Coefficients calculated using block-level data are weighted by population. Confidence intervals are calculated using MSOA-level clustered standard errors.

Table E5: Effect of closures on youth offending – different dependent variables

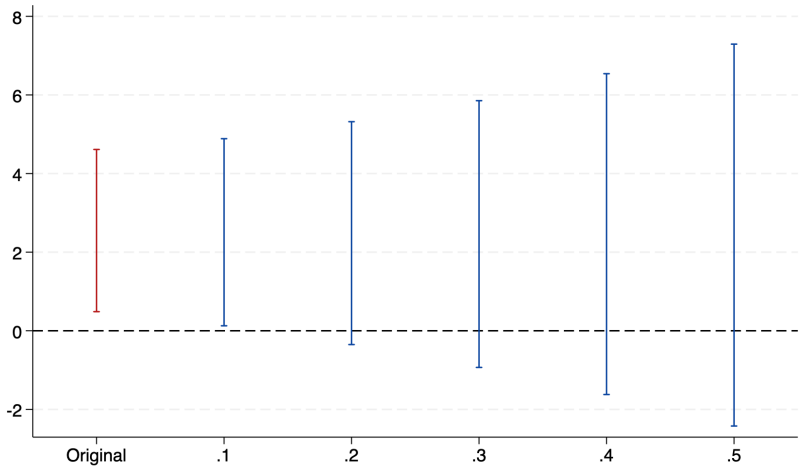
	(1)	(2)	(3)
	Offending rate, uncensored	Offending rate, censored at p95	Offenders (count)
	ages 10–17		
ATT	1.876	1.774	0.268
	(0.461)	(0.454)	(0.076)
Magnitude (%)	12.98	12.41	11.67
p-value	0.000	0.000	0.000
Mean	14.449	14.298	2.295
N	22,040	22,040	22,040

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 population. Controls include population density interacted with year, distance to police stations and proportion of claimants for unemployment benefits. All columns include block and year fixed effects. Standard errors clustered at the MSOA level are in parentheses.

Figure E2: Effect of closures on youth crime rates, allowing pre-trends to differ from 0 up to different $\bar{M}$ levels



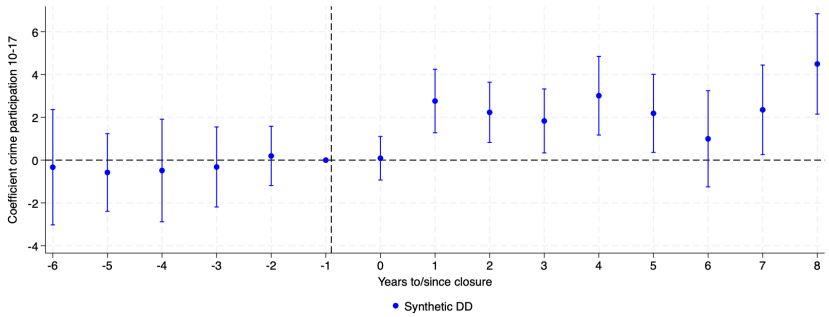
(a) Offending rate at ages 10–17



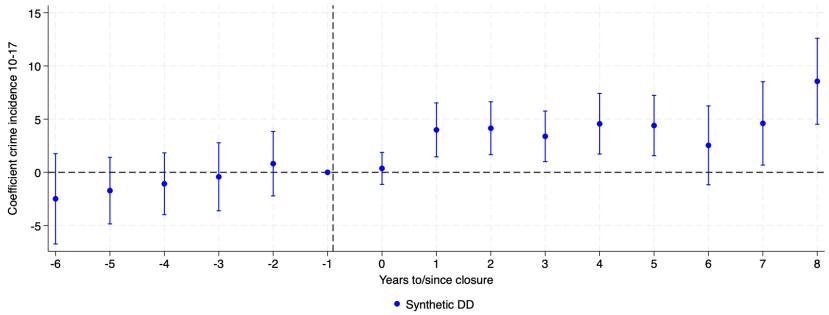
(b) Crime incidence rate at ages 10–17

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. Controls include population density interacted with year, distance to police stations and proportion of claimants for unemployment benefits. Confidence intervals are calculated using MSOA-level clustered standard errors.

Figure E3: 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. Block and year fixed effects included. Confidence intervals are calculated using MSOA-level clustered standard errors.

Table E6: 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. All columns include block and year fixed effects. Standard errors clustered at the MSOA level are in parentheses.

Table E7: Effect of closures on youth offending rates – non-linear estimators

	(1)	(2)	(3)
	Offending rate at ages 10–17		
ATT	0.132	0.304	0.172
	(0.028)	(0.077)	(0.036)
Estimation	Poisson - TWFE	Poisson - Wooldridge	Poisson - stacked
Magnitude (%)	14.12	35.55	18.80
p-value	0.000	0.000	0.000
Mean	13.556	13.556	15.005
Cohort-year FE	28,470	28,470	141,580

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. Controls include population density interacted with year, distance to police stations and proportion of claimants for unemployment benefits. All columns include block and year fixed effects. Coefficients weighted by population. Standard errors clustered at the MSOA level are in parentheses.

Table E8: 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	-0.008	-0.332	-2.670
	(0.004)	(0.126)	(0.759)
Magnitude (%)	-3.59	-20.71	-23.88
P-value	0.039	0.008	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. Coefficients are weighted by population. Stop-and-search data come from Police UK and are available from April 2016 onwards. Controls include population density interacted with year, distance to police stations and proportion of claimants for unemployment benefits. Standard errors clustered at the MSOA level are in parentheses.

Table E9: Effect of closures on offending and local crime - separately estimating effects of police station closures

	(1) Offending ages 10–17	(2) Offending ages 18 and above	(3) Local crime all ages
After youth club closes	1.869	0.000	-0.507
	(0.461)	(0.000)	(0.998)
After police station closes	-0.135	-0.000	-0.089
	(0.463)	(0.000)	(0.844)
Magnitude (%) youth club closes	13.06	2.60	-0.82
Magnitude (%) police station closes	-0.95	-0.60	-0.14
Mean	14.310	0.016	61.790
N	22,040	22,040	22,039

Notes: Each column separately estimates effects of youth club closures and police station closures. Columns 1 and 2 estimate person-based measures of crime; and column 3 estimates place-based measures of crime. All estimates come from administrative records from MPS combined with population estimates from ONS. All regressions are estimated at the block–year level. Coefficients are weighted by population. Controls include population density interacted with year, distance to police stations and proportion of claimants for unemployment benefits. Standard errors clustered at the MSOA level are in parentheses.

Table E10: Effect of closures on changes in neighbourhood composition

	(1)	(2)	(3)
	% moved since 2010	% moved since 2014	Plans to move
ATT x not parent	0.017	-0.005	-0.019
	(0.018)	(0.013)	(0.028)
ATT x parent	0.007	-0.011	-0.018
	(0.015)	(0.010)	(0.030)
Mean	0.068	0.027	0.161
N	6,261	6,261	6,594

Notes: Column 1 presents estimated effects on the proportion of population who have moved to the neighbourhood since 2010, Columns 2 on the proportion who have moved since 2014, and column 3 on a dummy variable indicating whether the respondent aims to move from the neighbourhood; all indicators from Understanding Society. All regressions are estimated at the individual level. Controls include population density interacted with year, age and gender. Standard errors clustered at the MSOA level are in parentheses.

F. Detailed cost and benefit calculations

Benefits: The short-term benefit of youth club closures is savings in yearly running costs. In 2010–2019, 102 youth clubs closed in London, leading to a total of 586 club-years unfunded.[45] The mean annual cost per club in London is approximately £170,000 according to FOI data and my own survey among youth workers, hence the estimated savings amount to £99.62 million (586 × £170,000). These savings would have been borne by local authorities.[46]

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. fall in test scores, which implies a loss of £2,499 (rounded) in lifetime earnings per pupil (96,111 × 0.026). Since 75,608 pupils lived near closed clubs in the period of analysis, the forgone earnings before taxes are nearly £190 million. These imply long-run private costs and some losses to public finances. Assuming that each affected individual would have been taxed at the average rate of 23.7% (OECD 2024), there are forgone collection of taxes of £45 million. This represents half of the savings.

Costs due to increased crime: The rises in offending imply costs in public spending

[45] Out of the closing youth clubs, 85 were open as of 2010 and 17 opened after 2010 and later closed.

[46] The figure is within a reasonable ballpark. Aggregate data indicate that spending on youth services fell by £132 million from 2010 to 2019 in London, which would include reductions to the funding of clubs that remained open and to targeted youth services.

associated with the criminal justice system, which includes police, courts, offender management and custody. The average young offender costs £8,000 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 (rounded). Since there were 16,116 young offenders in areas and years affected, the estimated number of offenders without closures would have been approximately 14,925. Thus, the offenders attributable to the closures are almost 1,194 (14,925 $\times$ 0.08). Multiplying by £8,000 yields costs above £9 million.

The rises in crime also imply private costs to victims. 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.[47] I estimate an 8.3% increase in violent crime rates. There were 5,127 violent crimes in affected areas and given the estimates, there would have been about 393 less. This means more than £4 million in costs to victims of violent crimes (393 x 11,446).

For acquisitive offences, I estimate an increase of 1.413 crimes per 1,000 per area and year, representing a 16.7% rise. In affected areas, there were 7,210 acquisitive crimes; hence approximately 1,032 are attributable the closures. This costs about £4 million to victims using the average cost of £4,093 from Heeks et al. (2018).

Beyond these detected crimes, there are likely 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 nearly £20 million.[48] For acquisitive crimes, detection rates are 9.1%. Assuming the observed crimes are 9.1% of the total, the actual cost of acquisitive crimes could be as high as £46 million.[49] The total crime costs might be more than £75 million. This excludes potential costs of future crimes.

[47]The average cost are calculated as the weighted average of the costs of homicide, violence with injury and violence without injury, weighting by their prevalence.

[48]While it is not possible to separately assess the effects by specific violent crime type due to low statistical power (crime is a rare event and my estimates leverage block, year and age group variation), it is possible to bound them. According to Heeks et al. (2018), the least costly violent crimes (violence without injury) cost £5,930. If all the rises in offending due to youth club closures are of this type, then the associated costs of violence – including detected and undetected – would have been £9 million.

[49]Summary statistics show comparable detection rates in treated and control areas at baseline. The results from Table E8 suggest that detection rates decreased following closures, which might imply that the additional undetected crimes could be higher.

G. The elasticity of proximity to youth clubs and crime, the p-median model and counterfactual closures

This appendix describes a simple discrete choice model and how it's used to recover the cross-price elasticity of distance to the youth club and crime and the p-median model used to determine the optimal closures of youth clubs discussed in Section VII.B.

G.A A discrete choice model of after-school time use

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 μ_n), distance to the nearest youth club d, other block-level unobserved attributes μ_i and other unobserved yearly characteristics μ_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 ε 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 A8 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

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

G.B The p-median model

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 and is commonly used in operations research and 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.

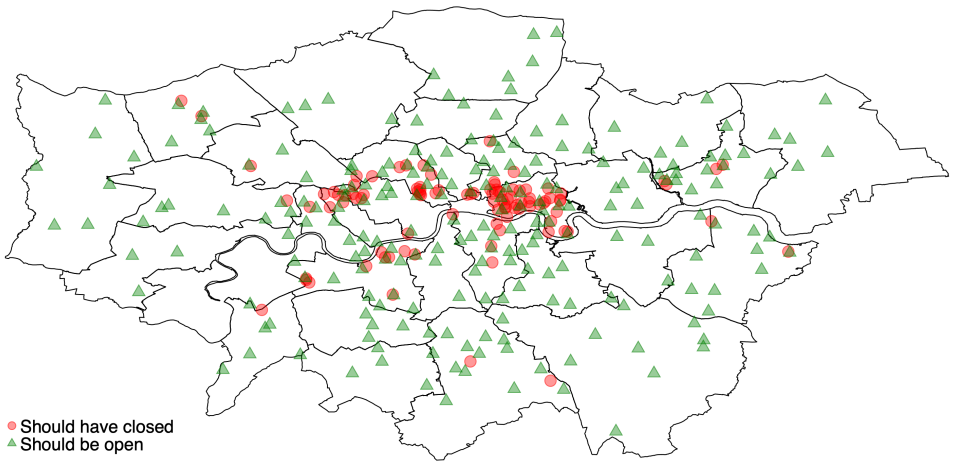
The constraints to the optimisation problem are:

- 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.[50]

A map of the resulting optimal locations is included in Figure F1, and Figure F2 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 gentrification driving families further from the centre.

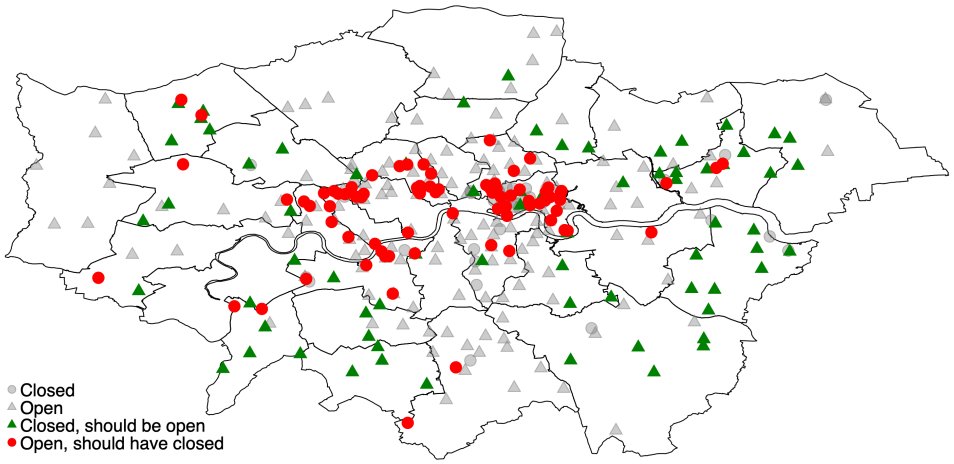
Figure F1: Optimal youth club locations from the p-median model solution



Notes: The triangles are youth clubs that should have been kept open in the 2010–2019 period according to the p-median model solution, and the circles are youth clubs that should have closed, conditional on only being able to finance 230 youth clubs. Many of the youth clubs that would have been preferred to close are located in Tower Hamlets and are very close to one other in the map.

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

Figure F2: Comparison of effective closures and optimal closures as per p-median model solution



Notes: Comparison between observed closures in 2010–2019 and the optimal locations of youth clubs from p-median model solution.