AFTER SCHOOL ACTIVITIES AND CRIME: EVIDENCE FROM LONDON'S YOUTH CLUBS

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Abstract

Youth clubs are after school programs, often offered free of charge in underprivileged neighbourhoods. While they might impact people's lives in many aspects, the public debate around their effectiveness often focuses on crime prevention. I provide the first causal estimates of their effects on crime leveraging quasi-experimental variation from austerity-related cuts which led to the closure of 30% of the youth clubs that were open as of 2010 in London. Closures increase crime participation rates for people aged 10-15 living near the closed centres by 9.6%. The effects are not explained by changes in policing intensity, nor by general austerity. Instead, these type of programs might have a crime reducing effect on teenagers beyond short-term incapacitation.

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

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I. INTRODUCTION

After-school activities such as developing hobbies, or learning new skills play an important role in children's development. Survey data shows that teenagers partake in after-school organised activities spend less time playing video-games, watching TV, or on social media, and have more friends. In the UK, parents spend an average of £2,000 per year on their schoolchildren's after-school activities, and teenagers devote nearly three hours per day to hobbies and sports (GPK 2019, Chatzitheochari et al. 2015). Given the potential positive impact of structured after-school programs, the public financing of after-school clubs could yield considerable benefits, particularly in disadvantaged areas where parents may not afford paid after-school leisure.

In the UK, open-access after-school programs for voluntary attendance, known as youth centres or youth clubs, are provided free of charge to local residents aged 10 to 18.[1] Similar models are found in Canada, Australia and New Zealand, the U.S., (*Boys and Girls Clubs*, or local organisations like the *Chicago Area Project*), Sweden (*Ungdomsgårdar*), Finland (*Nuorisotalot*), Norway (*Ungdomshus*), Germany (*Jugendzentren*), and France (*Maisons des jeunes et de la culture*), among others.

The policy discussion around their financing and effectiveness is often centred around safeguarding teenagers from engaging in criminal activities, considering their higher propensity to risk taking behavior. By providing a cheap leisure alternative to spend after-school hours, the time available to commit crimes might be reduced, and the preferences steering some towards crime could also be affected. Studies exploring the effect of formal education (schooling) on crime, have overall found very large and positive effects of longer days of schooling, and more years of schooling, which might suggest that non-formal programs like youth clubs could play a similar role (Lochner & Moretti 2004, Machin et al. 2011, Bell et al. 2022). Besides, specific programs that enable the formation of skills, and encourage forward-looking behaviour through mentoring have also been shown to be crime-reducing Heller et al. (2017), Bhatt et al. (2023), Cattan et al. (2022), and it is possible that youth clubs could encompass several of these channels.

However, there are also theoretical foundations that might suggest some costs of funding youth clubs. Some argue that these spaces could create hubs for crime, and facilitate gang formation by concentrating deprived youths, serving as 'schools-of-crime'. The literature exploring peer effects among teenagers has shown substantial peer effects in crime, at least when studied in a school setting and in prison (Dinarte 2017), and hence one might hypothesize that some negative interactions could be prompted in publicly financed after-school spaces.

The available empirical evidence on the relationship between youth clubs and crime is very scant, and is reduced to descriptive correlations obtained from studying longitudinal data (Mahoney et al. 2001, Feinstein et al. 2005). Those studies find a positive association between attending to youth clubs and committing crimes, but they cannot disentangle causal effects. In fact, such positive association might mask spurious relationships, for there could be many unobserved reasons that determine crime decisions which could

[1] Throughout this paper, 'youth club' refers to open-access venues dedicated to youth activities outside school settings. After-school activities provided in schools which are exclusive to school students are not considered youth clubs. Some programs extend access to individuals up to 25 years old with learning difficulties or disabilities. Occasionally, especially when youth clubs are run by charities, there may be a nominal fee, such as a 5-pound annual subscription and a 50p fee per visit.

also determine youth centre attendance which makes 'non-attendants' a non-suitable comparison group.

The challenge in gathering data on youth work in part explains the scarcity of causal evidence. In the UK, there's no centralised census of youth clubs, and the coordination of these services is delegated to local governments (boroughs, councils, local authorities). In London alone, this equates to 32 distinct institutions. Besides, the provision of these services falls under a 'statutory duty', implying a responsibility to offer these services, but no stringent legal mandates. Furthermore, youth clubs are largely autonomous and data collection has not been a priority in the sector. Youth organisations do not systematically record attendance, and the best estimates on usage come from survey data. Alongside these issues, there are substantial empirical challenges when trying to assert the causal impact of youth clubs on crime. Since these services tend to locate in areas with higher levels of deprivation, and given that people who attend youth clubs are more likely to come from deprived backgrounds a regression of crime incidence on youth club availability, might suffer from omitted variable bias. Going to youth clubs is not mandatory, and the characteristics that determine attendance may not be orthogonal to those determining crime decisions.

In this paper I combine several data sources with credible variation in youth centre availability to provide the first causal estimates of the effects of youth centres on crime. I study London (UK) between 2010 and 2019, a context in which youth clubs were popular, with approximately 41% of children aged 10-15 frequenting these spaces at least monthly in 2010, and 10% doing so almost daily. In this setting, from an initial 290 open youth clubs as of 2010, 30% had closed by 2019 due to austerity reforms which were implemented in 2010 and which reduced the national budget for youth services by 74% by 2019 (from an initial £1.4 billion across the UK).

I create a new hand-collected spatial dataset of the location of youth clubs open at any point between 2010 to 2019 using from Freedom of Information Requests. The database includes information on the year of closure of each youth club, when relevant. Using this new source and a geo-routing API, I create measures of proximity to youth clubs for all small 'blocks' in London.[2] I define whether a block is affected by closures or not assessing proximity to each centres as measured in commuting minutes on foot. If a block is closer to a youth club that closed I deem it was 'treated'. If it is closer to a youth club that is open I deem it 'control'.

This new dataset is then matched spatially to other data sources. First, I assert whether the treatment definition captures changes in attendance to youth clubs using survey data from Understanding Society containing details on attendance to organised after-school activities from respondents aged 10 to 15 living in London in my sample period. My research design is a difference-in-differences (DD) model where I compare individuals whose nearest youth center - relative to their home address - closed, to individuals whose nearest center remained open. Given that the closures of youth centres took place in difference years (the intervention is staggered), and that Two-Way-Fixed-Effects (TWFE) estimators might not accurately estimate the Average Treatment Effects on the Treated (ATE) (Roth et al. 2023, Callaway & Sant'Anna 2021) I estimate the models using a stacked-difference-in-differences approach in the

[2]These blocks are defined as Lower Lever Super Output (LSOAs) areas, which are statistical units designed by the Office for National Statistics (ONS). These blocks have an average population of 1700 people, or 650 households.

style of Cengiz et al. (2019) and Deshpande & Mueller-Smith (2022).[3] This strategy allows to compare different treatment cohorts (residents of areas where the nearest centre closes in a given year), to all never treated cohorts (areas where the nearest centre remains open throughout the sample), and weights each cohort equally.

I estimate a drop of 24 percentage points in the probability of attending organised activities for people aged 10-15 living near youth clubs that close, which represents a fall of 51% over mean attendance. While the estimates are more imprecise for the intensive margin of attendance, they also go in the expected direction, with the frequency of attendance falling by 39% standard deviations. The effects are concentrated in areas within 40 minutes of a youth club as of 2010 (people living further than 40 minutes away are not as affected).

I then look at the effects on crime leveraging novel administrative crime records from the London Metropolitan Police which contain detailed information on the location of crimes, and the home address of offenders for cleared (detected) crimes to create measures of crime participation for blocks in London. To the best of my knowledge this is the first paper to study crime participation at such small level of granularity. I estimate DD models using Poisson regression techniques on a stacked database as above-described. Given that my effects on attendance are concentrated within 40 minutes of a youth club at baseline my preferred analytical crime sample focuses on these areas. In my preferred specifications I weight the regressions by the similarity between treated and control in key socio-economic indicators.

I find that young residents aged 10-18 become more likely to commit crimes after closures in areas affected, by 8.3%. Exploring heterogeneity across age groups reveals that the effects are concentrated in people within the compulsory schooling age (aged 10-15), for whom crime participation increases by 9.6%. I find no statistically significant increases for people above 16 years old. Estimates on crime incidence show closures lead to 12% more crimes committed by those aged 10-18, and the effect begin driven by those aged 10-15, for whom it rises by 8.9%. Residents aged 16 and 17 might also commit more crimes, although the estimate is more imprecise (with a p-value of 0.103).

These estimates are not driven by general austerity. Youth club closures are uncorrelated with other well-studied austerity shocks such as the loss in individual welfare transfers, and the closure of police stations. Besides, I find similar estimated rises in crime when assessing differential crime rates in a triple difference-in-differences (DDD) strategy where I use people who are too old to attend youth clubs as a second control group (aged 26 to 35). This allows to isolate the effect of youth clubs - which should only affect young people - from 'common shocks' affecting people across age groups. People under 18 are more likely to commit crimes than their older neighbours post-closure in areas affected. The effects on the younger group aged 10-15 are driven by drug crimes, up by 14.2%. Residents aged 16 and 17 commit differentially more drug offences (rates increase by 11.5%) and more violent crimes (rates increase by 15.7%) than their older neighbours.

Beyond the effects on offending one might be interested in the effects of closures on

[3] In a stacked setting with areas and years, first each 'experiment' is separated by matching areas treated in the same year (a cohort) to all non-treated areas. Then, all 'experiments' or 'stacks' are appended. A regression which resembles TWFE, but which is augmented with stack-year and stack-area fixed effects is then ran. This allows to eliminate concerns around 'forbidden comparisons', as the never treated group are always the comparison group. Besides, each wave is given the same weight in the regression, which reduces concerns around differences in weights providing a biased estimate.

the overall distribution of crime. The channels through which youth clubs might affect crime need not go only through their impact on people's lives, but might instead be related to an area having higher or lower footfall, or more activity in certain days. I show that the closures do not affect substantially the spatial distribution of overall crimes, crimes committed by young individuals, or crimes undetected by computing the number of crimes happening in a block divided by total residents of that block. In this instance the relevant spatial measure is where crimes take place, and not where offenders live. This highlights the importance of using precise residents' data to assess place-based policies, for blindly assuming higher crime necessarily takes place in the immediate vicinity of a person's address might yield different estimated effects (Kirchmaier et al. 2021).

I then explore the channels driving the effects, focusing on whether the effects are short-lived, or rather permanent. Youth centres might only decrease crime in the short-term by mechanically limiting time available to engage in crime, or might instead affect people's preferences (or taste) for crime in the medium and long run. The largest rises in crime are found in areas where, following the closures, there are no other youth clubs within 40 minutes on foot, which suggests substitutability to other centres within a reasonable commuting time. I don't find strong evidence of the rises in crimes differing by proximity to other leisure amenities, such as schools or libraries, which suggests that youth clubs serve a unique purpose. Analyses of heterogeneity in effects by the hour in which crimes are committed shows the effects are more precisely estimated for crimes happening on school days after-school but I cannot reject that crime rises in other hours (in which centres might have been closed) might be similar. This suggests that pure incapacitation might play a role, but is unlikely to be the main channel.

I use schools exclusions and suspensions data from the Department for Education as a proxy for anti-social behaviour incidents that would only happen in the classroom and hence hours in which centres would not have been open. After closure, local residents are 25.8% more likely to be permanently excluded from schools, and the length of temporary suspensions more than doubles. In addition, the age at which one is affected by closures also matters, and people for whom the youth club was open at some point when they were 10-18 have lower crime rises than people who were never exposed to youth clubs (younger than 10 at the time of closure). This suggests that dynamic incapacitation effects, where centres alter people's taste for crime in the longer run might dominate. Qualitative evidence from conversations with youth workers, youth organisations, and young people suggests that the specific ways in which youth centers deter crime relate to identity-formation, and aspirations.

I also reject alternative explanations, such as the effect being driven simultaneous changes in police patrolling. Detection rates, total stop and search rates, and stop and search rates to young individuals remain constant after closure in areas affected. Besides, the effects appear to be symmetric as suggested by analysing a small subset of openings of youth clubs, which show that openings leading to falls in crime participation for young people. This suggests that the effects are not explained by other factors related to a 'sense of loss' following austerity in the area, or other drivers.

An important limitation of my analysis is the lack of availability of quality or capacity estimates of youth clubs. Besides, I do not have detailed information on the composition of attendants, which might also affect crime differentially (Dinarte 2017). However, the substitutability between centres and crime, and the role of commuting distance motivate exploring counter-factual closing regimes, and asking whether the effects on crime could have been mitigated, even given necessary cutting back on available youth clubs. I estimate the relationship between commuting distance and youth club participation and crime continuously and find that that for every additional 10 minutes of commuting time on foot, young people become 17% less likely to partake in youth clubs, and 2.5% more likely to commit crimes. I use the results from the above-estimates to compare the austerity related closures to random closing regimes, and to optimal closures as computed using p-median models that minimise commuting distance across space. These analysis suggest that actual closures were only marginally better than random closing regimes, and that crime rises could have been mitigated by taking into account commuting decisions.

This paper contributes to the literature on place-based policies and their effects on youth crime. My paper is the first to provide credible causal estimates on the relationship between youth clubs and crime. It also contributes to the literature on incapacitation and youth crime most of which has focused on the impact of compulsory schooling, but has remained silent around the effect of after-school activities (or non-formal education. My paper is also linked to works linking skill development and crime, and the unintended effects of austerity, which I describe in Section II.

The rest of the paper is structured as follows. Section III describes the characteristics of youth centres and youth clubs in the United Kingdom, and the austerity cuts to youth services in the 2010 to 2020 period. Section IV describes the various data sources and spatial statistics constructed. This provides grounds for introducing the empirical strategies in section V. Section VI presents the estimated effect of closures on crime. Section VII shows that these results are robust to changes in assumptions and sample restrictions. Section VIII presents additional analysis on the channels explaining the effects. Section IX discusses counterfactual closing regimes, and section X concludes.

II. LITERATURE

II.A Incapacitation, skill development, and crime

My paper contributes to a strand of the literature on the economics of crime assessing the effects of 'incapacitation' mechanisms.

I am the first to explore the impact of youth clubs on young people from a causal standpoint. To the best of my knowledge, the only papers that assess the effect of youth clubs on crime are Feinstein et al. (2005) and Mahoney et al. (2001), who provide descriptive insights from a longitudinal survey from the UK and Sweden, respectively. The authors compare criminal prospects of people who reported attending youth clubs to people who reported not attending conditioning on a number of socioeconomic characteristics. These papers find that people who attend are more likely to engage in criminal activities, but recognise that there are individual level characteristics that determine youth club attendance which might simultaneously affect crime that cannot be controlled for.

Other works exploring place-based policies have focused on the role of schools. The work most directly connected to this paper is Dinarte (2017), who study after-school clubs in El Salvador and their effects on self-reported violence. The study is an experimental evaluation of programs which take place in the schools (and not in other specific places as are the youth clubs of study), and finds that the direction of effects depends on the composition of peers, with more diverse groups leading to lower violent behaviours. Most of the literature has however focused on understanding how changes in mandatory school provision affect children's probabilities to engage in crime. For instance, Jacob & Lefgren (2003), Akee et al. (2014) compare days in which school is on to days in which it is not, leveraging variation from teacher 'in-service' days across cities and years, and across furloughed days respectively. Luallen (2006) assesses this effect using variation from teacher strikes instead, highlighting that there is heterogeneity in effects by crime type, with property crimes decreasing on school-days, but violent crimes increasing. A paper by Berthelon & Kruger (2011) explores the effects of a reform extending the school day in Chile, and finds that longer schooling hours not only decreases youth crime, but also the probability of other risky behaviours and in particular of teenage pregnancies. Other papers have found that some schools - poorly performing ones - can instead be crime generators and that closing them (permanently) reduces local violence (Steinberg et al. 2019).

My paper contributes to this literature by exploring the incapacitation effect of non-compulsory after school programs. Ex-ante we might think that the effects could be similar to those found in papers assessing longer schooling days, for the youth clubs take place in the after school hours (which in the UK means as early as 3PM). However, studying youth clubs could be more complex than studying schooling due to self-selection of people into youth clubs (i.e. how often to attend, or for how long...). Furthermore, while schooling should affect nearly all of the young population, youth clubs are more often attended by young people from lower-income backgrounds, and as such likelier to be at risk of offending.

This paper also connects to works exploring the effect of informal education on crime. Several empirical papers have shown the positive effects of different programs, such as cognitive behavioural therapy and tutoring in crime prevention (Garces et al. 2002, Schochet et al. 2008, Heller et al. 2017, Bhatt et al. 2023), albeit these effects are sometimes only concentrated in the short term. Resnjanskij et al. (2021) and Alfonsi et al. (2022) abstract from crime, but find positive effects of mentoring programs on labour market outcomes. One might hypothesize that youth clubs could impact crime by providing a range of opportunities for self-development which have been shown to decrease crime.

II.B Austerity and crime

My work connects with the literature exploring the impact of austerity reforms on crime, and the potential lasting effects of austerity reforms. First, I contribute to a set of papers analysing the context of the U.K. following the Great Recession and their potential long term effects. For instance, Giulietti & McConnell (2020) show that areas which were more exposed to welfare reforms experienced an increase in the levels and concentration of crime, studying regional-level variation, at the local authority level. Bray et al. (2022) document that hate-crimes appeared to increase disproportionately in areas with higher welfare cuts. Other work has linked austerity to other outcomes, such as Brexit (Fetzer 2019).

The paper in this strand of literature which is most closely related to my work is Facchetti (2021), who uses very detailed spatial data to explore how austerity-induced police station closures in 2010 to 2019 affected the spatial distribution of crimes in London. This work shows that areas which were exposed to police station closures became more likely to concentrate crimes. My paper allows to disentangle effect of place-based policies not only on the spatial distribution of crimes but on residents' outcomes more specifically.

Besides, it focuses on the sub-population which was affected by this particular austerity measure (young people).

In terms of the broader literature on austerity and crime, my paper relates to works assessing the impact of welfare cuts in young people's outcomes in isolation. Overall, these projects have shown that benefit loses leads to increases in crime, namely studying the U.S. context. For instance Corman et al. (2017) and Dave et al. (2021) have analysed drops in benefits for mothers in the 90s and also find increases in crime participation for the children of those mothers, and on some schooling outcomes. I study a younger population than these papers (as young as 10 years old), and focus on policy shocks that cut funding specifically to young people's services.

III. BACKGROUND

III.A Youth centres and youth clubs in London

Youth centres and youth clubs providing universal services in the UK are also called 'open access youth-work'. This means that all local residents within a given age range are allowed to enter the space or to become 'members', and differs from 'targeted' youth work - where young people need to be referred to use the service. Youth centres and youth clubs are in most cases free of charge for their members. Whenever fees are present these are largely symbolic. The objective of these centres is provide a space for young people to make friendships, learn new skills, and safely spend the after school hours. In some cases, the centres were open with the specific intention of preventing youth crime.

In London the most common hours in which these centres are open are between 16:00 and 20:00, with some opening during mornings and up to 22:00. Often, the youth clubs are only open on specific days, and many are also open on weekends and school holidays.[4] In some cases the centres have dedicated days or hours of the week for different age groups, and it is common to offer some girls-only sessions. The target audience are residents between 10 and 18 years old, occasionally allowing the maximum age to be 25 for people with special educational needs, or disabilities. The daily organisation of youth clubs and the provision of activities is administered by accredited youth workers or accredited volunteers. Figure 1 shows a word cloud of the type of activities and amenities that are offered in youth clubs from a sample of 277 youth clubs in London for which I could find available information online. All youth clubs have a space to spend time socialising and most have either a pool table, board games, or offer arts and crafts. In my database, 64% of centres offer sport activities, 36% of centres advertise musical activities, and at least 22% of all centres have IT suites or other video game consoles.

Local authorities have a 'statutory duty' to provide these type of services for young people. However, the regulation is very loose, and open to interpretation (Davies 2018). There is no legal requirement to fund youth clubs, nor a minimum amount of spending recommended for their functioning. The decision as to how to provide for these services falls entirely upon local communities, who can either run and manage their own youth clubs, commission youth clubs to charities, or coordinate/oversee the running of completely independent charities. In my sample 52% of all youth clubs are fully ran by local authorities or commission, 43% are managed by charities with some degree of

[4] Unfortunately I was not able to obtain historic variation in the number of days each youth club is open. Local authorities or youth clubs rarely keep track of this data, and there is low institutional memory due to people changing jobs.

financial support from the council, and the rest are ran by churches or other organisations. Council managed youth clubs rely almost entirely on public funds; and in the case of charities 33 to 66% would come from public sources as I learnt in conversations with youth workers.

Youth centres and youth clubs are notoriously difficult to study from an accounting perspective. Unfortunately local authorities seldomly record financial for each individual youth activity or youth club, and charity organisations are reluctant to sharing their financial records. According to limited information obtained through Freedom of Information requests the yearly running costs of youth clubs oscillates between 30,000 and 300,000 GBP. The mean yearly spending is 174,119 GBP, and the median is 74,167 GBP.

These type of spaces are a relatively common policy in many developed countries, such as Canada, Australia and New Zealand, the U.S., (*Boys and Girls Clubs*, or local organisations like the *Chicago Area Project*), Sweden (*Ungdomsgårdar*), Finland (*Nuorisotalot*), Norway (*Ungdomshus*), Germany (*Jugendzentren*), and France (*Maisons des jeunes et de la culture*), among others. While the external validity of my analysis is contestable, for London may have unique characteristics that affect young people's relationship with crime and youth clubs, I believe they can be a useful starting point when analysing the relationship between youth services and youth crime beyond Great Britain.

III.B Youth club attendance

There is no single database containing information on all youth club users across the UK. Attendance to individual youth clubs is not tracked as youth workers report this would make it be *"too much like school"*. Some local authorities have estimates on number of visitors, but that is often anonymous and hence not possible to link to other sources.

As such, we rely on survey data to understand the extent of youth club use. One of those is the Young Londoner Survey, which contains responses for above 1,000 children aged 10-15 years old and residing in London as of 2010. This survey is particularly useful due to the precision of the question on youth clubs, which states: ´how often do you attend youth clubs´. About 40% of young people reported attending, but those that attend more often are more likely to come deprived backgrounds and Black. Table 2 displays the differences in socioeconomic characteristics.

As a second resource I use data from the British Household Panel Survey and Understanding Society, which contains information for 16,114 people residing in London during their childhood between 1997 and 2020. It has the advantage of tracking people over time, often throughout adulthood, but the question on youth clubs and clubs is slightly less precise as it includes other organised activities, like paid-for after-school activities or boy-scouts and girl guides. Nonetheless the proportions reporting attendance is very similar to those from the Young Londoner Survey. From 1997 to 2010 the proportion of users reporting attending at least monthly grew, to reach a peak of 56% of respondents. It then declined to reach an all-time-low of only 31% attending occasionally in the survey of 2020, likely due to the lockdown policies associated with the Covid-19 pandemic when many youth clubs had to close temporarily. Comparing attendance in 2010 to 2018 shows a drop in attendance of 6 percentage points (a 12% decline). The more recent waves of the survey include more detailed categories on attendance and show that, as of 2010 10% of respondents reported attending organised activities nearly every day, and 41% reported attending monthly.

These details on attendance are only available up to age 15, even if people up to 18 (and sometimes 25) are allowed in youth clubs. Youth workers' perception is that as teenagers get older attendance falls. This might be due to educational options changing as people turn 16, new interests being developed, or perhaps a lower need to develop new friendships as people might have already formed their peer groups.

III.C Austerity as a natural experiment

As part of the 2010 Spending Review, several grants that specifically targeted the funding of youth services were abolished or reduced. In particular, a ring-fence around a Dedicated Schools Grant, which local authorities could previously use to fund out-of-school activities for young people, was fully removed. Reports estimate that the cuts have resulted in the loss of 4,500 jobs in youth services and in 750 youth centre and youth club closures across England - see for instance Berry (2018), YMCA (2020), UK Youth (2021). In London, funding for youth services in the fiscal year 2010/2011 was £215,051,000. By 2019/2020, it had fallen by 71%.[5]

This shift led local authorities to reassess their provision of youth services. They ways in which they adapted to the new budget appear to be multifaceted from qualitative evidence available in council minute meetings and popular press. Some local authorities shifted from 'universal' provision (open-access youth work) to 'targeted' provision, and decided to close centres that were in principle nor reaching their intended target audience (more risk-prone individuals). In some cases, proximity to other youth centers and the size of centres was considered, apparently favouring centers which were more isolated, and bigger. Some authorities argued closing low-quality buildings due to their higher maintenance costs, and in at least one borough a corruption scandal uncovered many youth centers being implicated in fraudulent activity or mismanagement.

I attempt at seeking what factors were most important in the selection of which centres to close gathering youth club characteristics, block characteristics, and borough characteristics, and running a linear probability model of the likelihood of closure on those characteristics in Table 3. In the equation below X encapsules a vector of controls. This vector includes a dummy which is 1 if the centre was council-led (0 if ran by other institutions like charities or churches), proximity from the centroid of the LSOA in which a centre is located to another youth club in walking minutes, the age of the building in which the centre is located, the proportion of population aged 0-13, the proportion of population that rents from the council, population density, and political control in the borough in 2011.

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

Council-ran and managed centres were more likely to close than those charity-led, and centres in blocks with higher population density were less likely to close. I do not find other factors to be strong predictors when examining the type of building, the political party running the local authority, or other socioeconomic variables from the Census. Overall this suggests that the main reason for closure were the austerity cuts, which affected more directly council-managed boroughs.

[5]Figures from the Department for Education Children and Young People's Spending by Local Authority. The 2010 figure comes from the column 'Services for Young People' for 'London'; the 2019 figure comes from the column Net Current Expenditure and it is added for Outer and Inner London.

Beyond closures, there were other effects of austerity on the provision of youth services. The youth clubs that remained open had to readjust, which often meant being open for fewer days, changing from paid youth work to volunteer youth work, or decreasing the number of activities that they offered. While these are interesting margins to study, at present the data is in-existent.

IV. DATA

The analytical database includes all blocks in London from 2010 to 2019 and detailed information on youth club availability, residents' crime records (cleared crimes), local crime records (all crimes), school exclusions, and survey data. A detailed description of each resource follows, and summary statistics are available in Table 5.

IV.A Youth centres and youth clubs

The information on youth clubs was created using Freedom of Information responses from each local authority, who reported on youth centres and youth clubs in their area together with the year of closure or opening, when available. The data collection is a novel contribution to the literature, which previously couldn't provide an accurate count of the youth clubs per area or track youth club closures over time. While it is possible that some small centres are not present (if the local authority is not aware of them), to the best of my knowledge this is the most comprehensive and up-to-date source of data on youth clubs in London for the 2010-2019 period.[6]

According to my database, there were 290 open youth centres and youth clubs in London as of 2010. Of the youth clubs that were open at baseline, 87 closed for universal provision. Figure 2 displays the cumulative distribution of closures over time. I observe 33 new openings during the decade, of which 13 also had to close during the observation period. In 2020, the number of open youth centres was 223.

To create spatial statistics on centre availability by block, I use the precise address of each youth club and the HERE API to calculate the commuting time on foot from each block centroid to all centres within a 5-kilometre radius. With this information I can distinguish blocks where the nearest centre closes from blocks where the nearest centre remains open, and also evaluate differences in exposure to other centres. Whenever an area is 'treated' more than once I adopt an Intention-to-treat strategy and take the earliest closure.[7] In my main analysis I exclude a small number of areas which experience both openings and closures for ease of interpretation.

IV.B Youth surveys

I use survey data from the youth questionnaire in Understanding Society (previously, the British Household Panel Survey) to understand attendance to youth clubs over time. The survey tracks respondents aged 10-15 over time between 2010 and 2020 and contains some information on young people's life. It includes a question on attendance to organised activities such as youth club, with information on frequency (as shown in Figure 3). I use a controlled version of the data which includes the block of residence of

[6]For example, my database includes more youth centres and clubs than those in the reports by Berry (2018), and it also surpasses the number of youth centres listed in Ordnance Survey's Points of Interest.

[7]About 17% of the total sample is affected by more than one closure. There are 5% of areas affected by an opening.

the respondents. There are 771 individuals reporting attendance and living in London during my observation period.

Table ?? shows differences in reported habits for people who state attending organised activities and people who don't. Those who report attending spend less time on other leisure alternatives on schooldays, like watching TV, social media, or playing videogames. Instead, they spend more days per week doing sports, and report having more friends.

IV.C Crime records

I leverage administrative records from the London Metropolitan police (MPS) which contain the universe of crimes recorded within the force between January 2010 and December 2019. MPS is responsible for policing an estimated population of around 8 million people in Greater London.[8] There are a total of 7,601,196 crimes in the database with details on the crime type, precise geographic location of the crime coded up to block level. The most common crimes in London are thefts (29%), followed by violence against the person (21%).

A subset of this database includes detailed information on the suspect of each crime, such as the age at time of offence, and the home address of the offender coded on block level.[9] This data is naturally only available for crimes which an offender was identified at the time of extracting the data - June of 2023. These are 'cleared' crimes (US) or 'detected' crimes (UK). The total number of crimes with offender information is 1,426,993 - that is around 20% of all crimes. These crimes are connected to 952,237 unique individuals. The most common crimes including suspect data are drug offenses (32% of all detected crimes), followed by violence against the person (25%). The sum of acquisitive crimes (including theft, robbery, and burglary) was the third most common offense.

I focus on the impact on residents' crime participation and crime incidence. Crime participation is defined as the number of unique individuals who are identified as suspects in any given year divided by the total number of residents in that block (the extensive margin of crime). Crime incidence is defined as the number of crimes committed by residents of a given block in a given year (the intensive margin of crime). The second measure is noisier, and the distribution by block and year has a very long tail. The reason is, in the same incident an individual can be accused of multiple crimes. For instance, an offender who is stopped by police and found in possession of cannabis and a knife might be accused of Drug Possession and Possession of Weapons. In order for our results not to be driven or biased by outliers I censor the crime incidence variables at the value of the 99^{th} percentile.

I create outcome measures per block and year, first, for all age groups potentially allowed in youth clubs (people between 10-18 years old) and for different age groups in each block and year. I use the following age bands: 10-15, 16-17, 18-25, 26-35, and 36 and above. In the UK, 16 is the compulsory schooling age which might affect incentives for crime; and 18 is the age of majority, after which punishment for crime is likely to be very different (Arora 2023).[10]

[8] The City of London has its own police force and is excluded from the analysis.

[9] I do not have information on the proportion of crimes in which the felon is convicted, although aggregate data suggests the vast proportion of crimes which are detected by police end in a conviction.

[10] Since 2013 people aged 16 and 17 need to be in either full-time education, enrol in an apprenticeship or traineeship, or spend 20 hours or more a week working or volunteering if in part-time education or working.

IV.D Other sources

I include population counts by block from ONS for 2011-2019 to present my results in terms of rates. I also use publicly available data on stop and search downloaded through Police UK containing detailed information on searches conducted, as well as outcomes between April 2016 and December 2019. The Department for Education was able to provide residents' school exclusions through a Freedom of Information request. The database includes the number of residents who were permanently excluded (expelled from school), as well as the number of residents who were suspended (temporary restriction to attend school). For this second measure I also obtain the number of days residents' are suspended. Suspension can happen for a variety of reasons, such as repeated disruptive behaviour, bullying, or physical violence. These variables are aggregated on block and year level and not matched to other records.

As additional sources, I leverage the Points of Interest Database from Ordnance Survey from 2010 to 2019 to explore how the effects differ depending on the local amenities available to residents of different areas. I select libraries, sports centres, employment agencies, and art venues from the full sample of points of interests as potential leisure alternatives to youth clubs, and compute distance measures to each block in London.[11] Building age, used to assert probabilities of closing youth clubs in a previous section come from Geomini and are a cross-section as of 2015.

V. IDENTIFICATION STRATEGY

Understanding the relationship between youth clubs and crime is challenging from an empirical standpoint due to endogeneity concerns. Youth clubs tend to locate in areas with higher indices of deprivation, not orthogonal to crime outcomes. As such regressions of crime on youth club availability may be biased due to omitted variables, and/or reverse causality. Without an unexpected shock to youth club availability, estimates on the relationship between youth clubs and crime may be spurious. I leverage the closure of youth clubs in 2010-2019 as a shock to youth club availability, and which I argue is uncorrelated with other block-level crime determinants of crime.[12]

Conceptually, when a youth club closes the cost of attending public youth activities increases, either because the option has been removed entirely, or because the commuting cost to attend a centre has increased. In conversations with young people, they often highlight barriers to access as one of the reasons why they believe fewer young people engage in youth work. A quote from a local newspaper reporting on youth club closures during austerity illustrates the link between commuting costs and attendance anecdotally: *"The average distance someone has to walk [to the youth club] is half a mile. Norbiton is a deprived area. If [the youth club] moves not as many people will attend" - member, age 12, (*Burford *2016).* As such, this lack of time in the youth club might have impacts on criminal behaviour if youth clubs have incapacitation elements, if they affect taste for crime, or if instead they decrease time exposed to peers who might negatively influence each other.

[11]Unfortunately, while the data is available over time, the information is not very reliable to assess closures, as not all points as surveyed in each year. As such I make an assumption that those amenities did not close during my study period.

[12]Unfortunately I cannot observe closures before 2010, and while I have information on which centres were open in 2020 I cannot ensure which centres remained open during 2020 and the years of the Covid-19 pandemic. For these reasons I focus on the period 2010-2019.

I use difference-in-differences research designs to assess the average treatment effects of closures and disentangle whether positive or negative effects dominate. I compare residents of areas where the nearest youth club closed to residents of areas where the nearest youth club stayed open, before and after closures. I implement this in a stacked difference-in-differences manner where each cohort of closed areas (areas in which the centre closed in the same year c) is compared to areas where centres never closed, to create an experiment or 'stack'. I then append each 'stack', and run the regressions below in the style of Cengiz et al. (2019) and Deshpande & Mueller-Smith (2022).

First, I assert whether this approach indeed capture changes in attendance to organised activities including youth clubs. To do so I estimate the equation below using individual level survey data, where Y indicates attendance to organised activities (including youth clubs) for individual n living in block i in year t. I include stack-year fixed effects, and stack-area fixed effects, and estimate using OLS. The stack-year fixed effects capture any change implemented in a given year at regional level, that is, affecting the whole of London (e.g. changes in regulation).

$$Y_{nitc} = \mu_{nc} + \mu_{tc} + \gamma \text{After x Closure}_{itc} + \varepsilon_{nitc}$$

I use survey data from Understanding Society, which contains panel-level data for 1,071 Londoners aged 10-15 from 2010 onwards. Even if the question includes activities other than youth clubs, some of these specifics might be captured by the individual fixed effects. I estimate a drop of 24 percentage points in the intensive margin of attendance (the proportion of respondents who report attending), and of 38% standard deviations in frequency of attendance (the intensive margin). The results are available in Table 6.

I also assess heterogeneity in effects by distance at baseline. A graph of the coefficients estimated in Figure 5 shows that declines in attendance are concentrated within 40 minutes of the centres. This motivates a 40 minute radius around centres being my preferred sample to assert crime effects, which is consistent with youth workers' testaments of attendants being very local.[13] The map in Figure 6 denotes which areas are 'treated', which are 'control', and which are excluded.

Treated and control areas are similar in many characteristics at baseline, in 2011. They have similar total population, ethnic composition, and crime participation rates for people aged 10-15 years old. Treated areas have, however, slightly higher proportions of population living in social housing. Other variables are statistically significantly different but not economically significantly different as shown in Table 4. When compared to areas excluded from the analysis (outside the 40 minute boundary) they are more likely to host minorities (Black and Asian population), and have higher proportions of population living in social housing. I take into account the potential differences in composition by running a propensity matching algorithm that predicts treatment based on the variables that are statistically significantly different: the proportion of population younger than 18, the proportion with no qualifications, the proportion renting from the council, and the proportion of lone parent families. In my preferred specifications I use the propensity scores obtained to weight observations based on their similarity to the treatment areas.

[13]For comparison, the most common commute to school in London is on foot according to figures from Understanding Society (41%) and most primary school children in the UK have a commute to school of about 13 minutes either on foot or by car. A 40 minute commute on foot would typically correspond to about a 10 minute commute by bike, a 15 to 25 commute by public transport, or a 15 minute car ride in most areas in London (depending on the hour of the day). As such, this radius captures several reasonable commuting means to youth clubs.

To assess the effects on crime, I estimate the models in the equation below, which is modified to adapt to the fact that the crime data needs to be aggregated on block level, and expressed in terms of rates.[14] The notation is as follows: λ is the rate of an outcome (such as crime) in block i in year t. I include stack-area and stack-year fixed effects to control for yearly trends and local block characteristics which are constant across time. I estimate the models using Poisson quasi-maximum likelihood (PQMLE).

$$log(\lambda_{itc}) = \mu_{ic} + \mu_{tc} + \delta \text{After x Closure}_{itc} + u_{itc}$$

I also present event study plots including leads and lags to closures to assess pre (and post)-trends. In the equation below g is the year in which the nearest youth club to a block closed (it indicates each treatment cohort, and it is coded as 0 if the nearest youth club remains open). The coefficients $\xi\tau$ over the interactions of years-to-closure with treatment dummies reflect pre-and post differences in treated and comparison blocks.

$$log(\lambda_{itc}) = \mu_{ic} + \mu_{tc} + \sum_{\tau=-6}^{4} \xi_\tau 1(t - g_i = \tau) + v_{itc}$$

Given that the period of analysis is characterised by welfare cuts in many public policy areas one might be concerned that the shock to youth clubs in fact masks other austerity cuts and hence that the DD design doesn't in fact captured the relation of interest. To address these concerns I show that youth club closures are orthogonal to the loss in individual welfare benefits as measured by Beatty & Fothergill (2014), and to police station closures in Figure A.1 and Figure A.2, respectively. Nonetheless, and given that I cannot measure and observe loss in funding in every aspect of public policy at the local level I estimate a triple difference-in-differences (DDD) using people who are young, but too old to go to youth clubs, as a second comparison group. I thus evaluate the differential change in outcomes for children to older people in areas affected to such differential in areas unaffected, and the coefficients of interest are ϕ. The variable notation is as before, but now the observation is at the age-group s, block and year level. The model further includes fixed effects on the interaction between age group-stack-year, stack-year-area, and age group-stack-area.

$$\log(\lambda_{itsc}) = \mu_{tsc} + \mu_{isc} + \phi \times \text{After}_t \times \text{Closure}_i \times \text{AgeGroup}_s + \epsilon_{itsc}$$

While crimes committed by children and adults people might be very different, the underlying assumption is that any differences in types and composition are constant across space (in treated and comparison areas). Any shock that is unobservable but that affects all cohorts within a block would be captured by additional fixed effects. Event study plots can be expressed as:

$$log(\lambda_{itsc}) = \mu_{tsc} + \mu_{isc} + \sum_{s} \sum_{\tau=-6}^{4} \kappa_{\tau,s} 1(\underbrace{t - g_i = \tau}_{\text{Years to closure}} , \underbrace{s = 1)}_{\text{Age group}} + w_{itsc}$$

Regarding inference, in my main results I present standard errors clustering on individual level, for individual regressions, and on MSOA level for block-level

[14]The reason is that I do not have individual level data for people who do not commit crimes, but only aggregate population estimates by age.

regressions.[15]. This allows to capture spatial auto-correlation, while maintaining a relative large number of clusters for unbiased estimation of standard errors.

The validity of these research designs hinges on the parallel trends assumption. I assume that trends in residents' outcomes would have followed a similar trajectory in both treated and control areas had there been no closures. The event studies suggest that this assumption is plausible, as the pre-treatment dummies are centred at zero up to 4 years prior to the centres closing. In addition, I require that there is no anticipation leading to changes in behaviour prior to the closures. Often the decisions on closing centres where taking relatively fast, not allowing much time for adjustment. Once again, the event study plots suggest that, rather than anticipation, if anything the effects build up over time.

In Appendix A.B I discuss potential biases of the estimates. I argue that my estimates might be a lower bound of the full effects of youth clubs on crime, rather than the opposite.

VI. RESULTS

I use administrative crime records from the Metropolitan Police to create measures of residents' crime participation and crime incidence by block, for different age groups. Naturally, the crimes for which I have information on age and home location are those for which an offender has been identified. This represents 20% of all crimes being reported by the police in London.

The results in Table 7 show an estimated increase in the crime participation rate for people aged 10-18 of 8.3% (this proportional magnitude is obtained by calculating the exponent of the coefficient estimated). This is driven by younger individuals, aged 10-15, for whom the crime rate rises by 9.6%. I do not observe significant effects for people above 16 years old, not on adults. Table 10 shows estimates for changes in crime incidence, defined as total crimes committed by residents, divided by total residents. I estimate a rise in crime incidence rates for those aged 10-18 of 12%. This is driven by younger individuals, for whom the estimated increase is 8.9%. There also appear to be some rises in crime for people aged 16 and 17, although the effects are nor significant at conventional levels (p value of 0.103). Event study plots showing the crime participation rate for young people shows no pre-trends up to 4 years before closures, and consistently positive results after two years since closure. This suggests that positive effects of youth clubs (such as keeping youths off the street, and perhaps teaching them new skills) offset any potential negative effects (for instance around negative peer effects).

Looking at the differential change in crimes for younger people with respect to older individual shows that crime participation and incidence rates for people aged 10-18 differently rise. I use the Home Office crime classification to categorise crime and assess differences by crime type in Table 9. For the younger group this is driven by drug crimes (up by 14.2%). People aged 16 and 17 appear to commit more drug and violence crimes than their neighbours aged 26 to 35, by 11.5% and 15.7%, respectively. These results are large increases in crime rates. As a comparison to estimates from other incapacitation channels Bell et al. (2022) find that extending the legal dropout age in the US decreased crime rates by 11% and reduced the arrest rate by 6% in a given year. While the context of study is of course very different, closing youth clubs might offset the achievements of longer periods of formal schooling. Interestingly, drug crimes is also

[15] MSOAs are census blocks larger than those on which I have the level of variation (LSOAs), of which there are 968 in London

the area where Bell et al. (2022) find larger results in the context of exploring formal schooling and incapacitation.

As mentioned above, the crime data for which there is a suspect identified (cleared crimes) represent about 20% of all crimes in London, with some variation across crime categories. It is hence also of interest to understand whether undetected crimes increase near youth clubs after closure using the location of crimes, particularly given that some evidence suggests crime being very local.[16]

In this case the channels through which we might observe an effect could be related to their effects on young people, but also on the general urban structure and change in footfall resulting from the change in use of a building, or potentially the building being left vacant (which is what conversations with youth workers suggest). The results, in Table 10 show that total crimes happening in the area do not increase on the aggregate after closure.

VII. ROBUSTNESS CHECKS

Sample restriction

My main results restrict the sample to areas within 40 minutes of commuting time on foot. Since this distance is calculated from the centroid of the LSOA there could be some measurement error in capturing a reasonable commuting distance. However there is a trade-off between restricting the sample to smaller radius around the youth clubs (which arguably may make the assumption on attendance more plausible) and including more observations (which increases statistical power). Table A2 shows coefficients for the main results making the radius smaller at 20 minutes, and 30 minutes, which point at very similar magnitudes in the rises in crime, ranging from 9% to 11% for people aged 10 to 15. These distances in minutes are measured on foot given that informal conversations suggest most individuals would either walk or bike to a youth club. However, these robustness are useful as a proxy for distance using alternative methods.

I also run additional checks restricting the sample to exclude one borough at a time in 'leave-one-out' estimations. The results are very robust to these decisions, and hence not driven by a singular area of London, as shown in Figures **??** in the Appendix.

As additional analyses I re-run the main results dropping Inner London boroughs. If one studies the map of treatment and control, many areas within Inner London are in the control group and hence were not exposed to closures. This allows to ensure that any effects are not driven by fundamental differences in areas where many centres remained open (Inner London). Dropping Inner London shows larger estimated effects. The results are in the Appendix.

Estimating the effect of closures on crime counts

I present additional results estimating the main effects on the number of criminal residents instead of the rates. The results in Table A3 show similar estimated magnitudes. This is unsurprising, since blocks are designed by the statistical authority to have similar sizes in population. While different areas might have different age distributions these are

[16]In detected crimes data around 20% of all crimes happen in the block of residence of the offender, and half occur within 4 kilometers of their residence (less than 1 hour commute on foot, or less than 20 minutes on a bicycle).

unlikely to change suddenly year-on-year and most of the demographic variation would be captured by the block fixed effects.

Other inference calculations

I run additional specifications computing standard errors in different ways using clustered bootstrap on borough level, of which there are 32 clusters, and using randomisation inference. The first approach has the advantage of capturing large spatial auto-correlation, which are valid given that it the decision maker with regards to which centres to close would often have been the local authority government. Bootstrapping reduces concerns around bias when the clusters are relatively low (Bertrand et al. 2004). The results in Table A4 shows that the estimated effects on crime participation remain significant at the 95% confidence level.

I also compute p-values using randomisation inference, which implies randomly closing centres many times (1,000), and re-estimating the effects based on these 'false' treatments. I then compare the estimated crime participation under the true austerity regime against the distribution of effects obtained from re-samplings. This shows that it is very unlikely that the estimated effects under the true regime are the product of 'luck'. The p-value over the coefficient for crime participation for people aged 10-18, and for those aged 10-15 specifically are significant at the 99% confidence level.

Alternative estimators

As additional analyses, I calculate ATTs using the methods proposed in Wooldridge (2023) which allow for consistent estimation of treatment effects in non-linear settings where the staggered introduction of a treatment might induce dynamic or heterogeneous effects across treatment cohorts. Instead of a stacked database, I estimate the effects on the classic panel data including cohort-year fixed effects to allow different treatment cohorts to have different treatment effects or potentially dynamic effects, and estimate using pooled QMLE. The results show very similar magnitudes.

I also run additional models using a linear specification rather than a Poisson. These include traditional TWFE specifications, and estimators from synthetic differences-in-differences, as described in Arkhangelsky et al. (2021). The results are provided in the Appendix.

VIII. MECHANISMS

My estimates show that youth clubs can mitigate youth crime, with closures leading to crime rises. In this section I explore the reasons underpinning this effect, with a focus on understanding whether the effects are mechanical (pure incapacitation, where youth clubs simply limit time available for crime), or instead whether youth clubs have dynamic incapacitation elements (where changes in crime are due perhaps to the acquisition of values that make crime less attractive). I also show rule out alternative explanations.

VIII.A Pure incapacitation

An explanation of the effects is that youth clubs might mechanically restrict the amount of time available for crime, as have other leisure amenities discussed in the literature (Dahl & DellaVigna 2009). To explore this channel I assess heterogeneity by availability of other youth clubs and leisure amenities, and by hour in which crimes are committed. If youth clubs only incapacitate people in the very short term we should expect crime rises to be concentrated in hours in which youth centres would have been open, and in areas where there are no other leisure alternatives nearby.

There was substantial heterogeneity in alternatives to the closed youth clubs in different areas, with a few neighbourhoods becoming completely unprovided for, but others retaining a core service by having other centres nearby. The results in Figure X show that the largest effects are concentrated in areas where the second nearest centre is more than 40 minutes away. We can also assess this in heterogeneity analyses, and include other leisure amenities using the Points of Interest database from Ordnance Survey. I create a dummy stating whether the area is above or below the median distance to the second nearest open centre, and also dummy variables indicating whether an area is above or below median distance to other leisure amenities, including sports complexes, schools, libraries, employment agencies, music venues, and parks. When looking at other leisure amenities in Table 11 the results are unclear. I cannot reject that areas closer or further to other leisure spaces, but overall the coefficients are more precise in estimating rises in crime in areas further from schools and libraries.

As another way to assess short-term incapacitation elements I present an analysis by hour in which crimes are committed in the Appendix. This analysis is limited by statistical power, and hence the results call for caution in interpretation. The strongest effects on crimes committed by 10-15 year olds are found in after school hours. However, I cannot reject that the rise in crime in other hours to be similar.

VIII.B Dynamic incapacitation

Beyond providing immediate alternatives to partaking in crime, youth clubs might affect people's taste for crime in the short, medium, and long run. This could happen, for instance, if time spent at youth clubs means higher civic values, or the development of skill that help young people in their education, or their prospects in legal sectors. For instance, youth clubs might develop young people's social skills, teach them about team-work, improve their self-perception etc.

To assess the effects of youth clubs beyond hours in which centres are open I use school exclusions data from the as a proxy for anti-social behaviour beyond crime, and cohort-studies to assess longer term trends. School exclusions happen due to wrongdoing in the classroom, and hence in hours in which centres would not have been open. As such, observing effects on exclusions would suggest that the incapacitation effect of youth clubs are not restricted to pure, mechanical, time spent inside the centre. Table 12 shows that the rate of permanent exclusions goes up by 25.8%. In terms of temporary suspensions the results are positive and significant for the number of days suspended per pupil (the intensive margin of school suspensions) but not for the rate of students suspended. This implies that a few students might behave badly more often.

I also use the detailed data on the age of individuals to evaluate the effects on distinct cohorts. People for whom the youth club closes once they are 15 might not become as likely to commit crimes as people for whom the centre closes when they are 10 if there are dynamic incapacitation elements. One can think of such elements in different lights, for instance, in terms of accumulated criminal capital Arora (2023), accumulated benefits of non-crime alternatives (Grogger 1998, Machin et al. 2011) etc. To do so I compute the number of crimes committed by people born in each cohort and living in a giving block over my sample period. Naturally, for those people who have committed crimes before 2010 I cannot observe prior crimes committed, but this wouldn't differ depending on when the youth club closed. I then compute, for each cohort, how old they were at the time of closure.

This database has 409,666 cohort c -block i observations. I use only cohorts born between 1994 and 2007. In the period 2010-2019 I observe crimes committed by those born in 1994 between ages 16 to 26, and by those born in 2007 from ages 3 to 13. In the regression below A is how old people were at the time of closure, and it is set at 18 for people who always have a centre (who in fact, do not experience a closure).

$$Y_{ic} = \mu_i + \mu_c + \sum_{a=4}^{25} \zeta_a 1(A_{ic} = a) + w_{ic}$$

The results of this event-study, where the event is the age at time of closure suggests strong dynamic elements of youth clubs. Figure X shows that cohorts who are relatively younger when the centre closes commit many more crimes than people born in the same year for whom the centre closes later, and than people born earlier than them living in the same area. This finding is consistent cumulative effects of investments in skill development, whether those are crime-focused, or focused on other things. For instance, a person who starts investing on criminal capital at a young age, will derive higher returns from crime than someone who starts investing on those skills older. Given this finding and the previous literature, which shows that people who offend at younger ages are more likely to re-offend, this suggests potential long-run effects of the closure of youth clubs as younger cohorts might commit crimes over the life-cycle.

Conversations with youth workers, young people attending youth clubs, and with youth scholars studying these spaces from a sociological and anthropological perspective highlight several channels through which centres affect people's development, hence likely affecting their vulnerability to engaging in crime. Participants consistently describe these centers as spaces where they can explore their interests, connect with peers, and develop essential life skills. The mechanism of social bonding emerges as a central element, as youth centers serve as catalysts for building positive relationships and a sense of community among attendees. This is particularly important in these early years, in which young people are learning how to behave, and developing their interests. Below I include some quotes from interviews from reports by YMCA (2020), UK Youth (2021) and Spunout.

"They help you meet new people and learn new things" - Emma, 15, interview from YMCA England & Wales

"They get me out the house and being social" - Craig, 12, interview from YMCA England & Wales

Youth workers also often highlight the mechanism of skill-building and personal development. Participants often recount their experiences of acquiring practical skills, such as teamwork, leadership, and problem-solving, through engaging in a variety of activities offered at youth centers. These centers are viewed as nurturing grounds for personal growth and empowerment, equipping young people with the tools they need to navigate life's challenges. Youth centers might which contribute not only to skill acquisition but also to enhanced self-esteem and self-confidence among attendees. For instance, a young person reported not feel particularly talented in school, but feeling much better about themselves upon discovering new talents through the exposure to various activities within youth clubs.

"They ran loads of fun activities each week and there was something different on special occasions like ice skating, Halloween parties and more (...) I finally felt like I found purpose" - Joseph, user since age 12, interview from Spunout.

"I feel safe in the youth club because all the youth workers are really nice, they have a lot of fun, a lot of laughs." - Scott, 14, interview from YMCA England & Wales.

Another crucial mechanism underlying the positive impact of youth centers is the role of trusted adults that youth workers have within these spaces. Youth workers offer support, guidance, and a listening ear to young people who perhaps do not feel understood by teachers or family. Some experts highlight that these relationships can be particularly transformative when the youth worker is someone from the community who young people can relate to, perhaps due to their origin, gender or ethnicity.

VIII.C Alternative explanations

In this section I present evidence rejecting other plausible explanations, such as the effects being driven by changes in policing, or by feelings of loss associated to losing a service rather than the youth centres serving particular needs.

First, one might be concerned that patrolling has intensified strategically after youth clubs closed in areas near youth clubs. These critics might argue that, in areas where centres are open crime participation is just as high, but perhaps more difficult to detect. This is a particular concern given that we observe a sharp rise in drug offences, which are crimes that are typically identified through proactive policing, such as the use of stop and search.

To this purpose, I calculate detection rates (clearance rates) by block and year using the administrative data above-mentioned. I also calculate the number of stop and searches from a second database, which is publicly available in Police UK - and which includes detailed spatial data since 2016. If police are patrolling strategically, we might observe an increase in crime detection for police would be more frequently in the area. Similarly, we might observe an increase in the rate of stop and search.[17] The results, in Tables 13 and 14 reject this hypothesis.

An important consideration for policy design relates to the symmetry in the effects. One might hypothesize that the rises in crime could be due to discontentment upon losing a service (a youth club), and that perhaps an opening needn't lead to a similar drop in crime. There are very few openings suitable for study in the 2010-2019 decade, and for that the results need be interpreted with care. Of 33 new youth club openings during the decade, 13 also closed and 5 only opened in 2020 - hence outside our crime analytical sample. In the Appendix I present estimated effects of those 15 openings. To find a suitable comparison group I use a propensity score matching algorithm based on distance to youth clubs ex-ante, distance to other amenities, and socioeconomic characteristics.

[17] It is important to note that I do not observe Court outcomes, only whether a crime record has been linked to a particular suspect.

I run the same specification is as before, but now the treatment is 1 after an opening instead of after a closure. I restrict the sample to areas which were within 40 minutes of the new youth clubs at the end of the sample (after opening) and cluster standard errors on MSOA level as before. The results show that, after opening, crime participation for people aged 10-15 falls by 16.4%, and the effect is significant at the 94.6% confidence level (p-value 0.054).

IX. COUNTERFACTUAL CLOSURES

The results show that the closure of youth clubs increases crime, and hence that positive effects of youth clubs dominate negative channels on average. I find that the rises on crime participation for youths occur due to closures within 40 minutes on foot from youth's home address. Proximity to other centres matters, and having some areas very isolated from youth clubs appears to be particularly damaging. This suggests that having a core spatial provision is more important than other considerations - perhaps capacity - which I sadly cannot observe. Informal discussions with youth workers suggest that the presence of youth clubs facilitates partnerships with other agencies and the organisation of borough-wide initiatives by the centres. Besides, it is possible that, due to peer effects, serving some children has positive spill-overs on others.

In this section I estimate the relationship between the likelihood of attending youth clubs and continuous measures of proximity to youth clubs. The equation to estimate is a logit and is presented below, where n indicates a given individual, and t is a given year. X includes age and block of residence controls, and the distance to the nearest youth club is presented as d, which is a cost. These regressions are identified from variation in youth club availability induced by closures, which change continuously the distance to nearest centres year-on-year for individuals affected.

$$P(j = attends)_{n,t} = \frac{\exp(\mu_i + \mu_t + \xi X_{nt} + \alpha d)}{1 + \exp(\mu_i + \mu_t + \xi X_{nt} + \alpha d)}$$

This equation can be micro-founded in a discrete choice framework where individuals aged 10-18 with limited time need to decide how to use their after-school hours. Their choice set includes attending youth clubs or not.[18] To incorporate crime decisions, the discrete choice framework could be extended into a nested discrete choice, where first youths decide to attend centres or not, and if 'not' is chosen they they face a second set of decisions which include committing crimes or not - see Figure A.E in the Appendix. Unfortunately, individual level data allowing to estimate the nested model jointly (the first and second nest) is not available. The best we can estimate is a simplified version where we connect the relationship between attendance and distance to centres, and the relationship between shares choosing crime and distance to centres.

[18] The logit equation is derived from the utility equation below where individuals choose among two options j: attending centres or not.

$$U_{n,t,j=attend} = \mu_n + \mu_t + \xi X_{nt} + \alpha d_{n,t} + \epsilon_{nt}$$

If the error term ϵ is assumed to be Gumbel I.I.D distributed, the relationship can be expressed as the logit above-mentioned (Train 2009).

The results of estimating the logit regressions in Table 15 show that, for every 10 additional minutes of commuting cost, individuals are 17% less likely to attend youth clubs. This is driven by high-frequency attendants (people who attend at least monthly). For every 10 additional minutes, the high-attendance group are 28% less likely to attend. Poisson QMLE estimations of crime participation rates for individuals aged 10-15 on continuous commuting time show that crime participation increases by 2.5% for every 10 minutes of commuting distance.

Using the 2.5% estimated I compare different closing regimes. As of 2023 there were 223 youth clubs open, and 100 total centres of the possible centres that could be open were closed. I now compare the observed austerity closures to a regime were closures are chosen randomly, and to optimal closures obtained from computing p-median models (Church & ReVelle 1974). These models optimise the location of youth clubs based on stated parameters, such as weights depending on different factors, and proximity. The objective is to minimise the average commuting time between demand nodes (the areas where individuals might demand youth clubs), and supply points (spaces where youth clubs existed as of 2010). A detailed explanation of the problem is available in the Appendix.

The results in Table 16 show that local authorities only did marginally better than random closures (1000 random iterations). When looking at the various solutions to the optimisation exercises, these suggest that taking into account the optimal spatial distribution of centres and people's commuting costs might have mitigated the rise in observed crime to nearly zero. I run several models first only attempting to minimise average commuting cost, second weighting by population aged 10-15, and third weighting by the number of young deprived residents.

X. CONCLUSIONS

In this paper, I present the first causal estimates on the impact of open-access youth work on residents' crime participation. I use evidence from London (UK), where these spaces are commonly called youth centres and youth clubs, and which are characterised by being largely publicly financed, open to anyone between 10 and 18 years old, and free to use.

To this end I create a novel spatial database of youth clubs in London, which I match to survey data, administrative crime records from the London Metropolitan Police, and other data sources. My research design leverages variation in youth club availability induced by austerity cuts to youth service funding, which resulted in the closure of 30% of the youth clubs which were open as of 2010 in London. I compare residents whose nearest centre remained open, to residents whose nearest centre closed in difference-in-differences models.

I first use individual level data to assert whether the closures affected attendance to this type of activities and find falls of 24 percentage points in the probability of attending after closure (a drop of 51% with respect to the mean). These effects are concentrated in areas within 40 minutes of a youth centre on foot at baseline. Turning to crime effects, I find that young residents' crime participation rates increases in areas affected by closures. People aged 10 to 18 years old are more likely to commit crimes, and the effects are driven by individuals aged 10 to 15, for whom crime participation rates rise by 9.6%. The effects are not driven by general austerity, and are robust to using older individuals as a second control group (individuals aged 26 to 35 who are not allowed in centres, and hence not affected by closures). Relative to their older neighbours, individuals aged 10-15 are more likely to commit drug crimes after closures (by 14.2%), and those aged 16 and 17 are more likely to commit more drug and violent crimes (by 11.5% for drugs, and by 15.7 for violence).

Exploring the mechanisms, the most convincing analysis suggest a strong role of dynamic incapacitation. Crime rises in areas affected might not be restricted to hours in which youth clubs would have been open. Besides, I observe rises in anti-social behaviour during school hours and residents' of areas affected being more likely to be excluded from school and suspended for longer periods. On top of that, it is those cohorts who spend fewer time exposed to youth clubs who suffer the largest crime rises. Qualitative evidence suggests that enabling peer relations, improved self-perception and aspirations, and positive relations with trusted adults are key channels explaining lasting drops in taste for crime.

I also reject alternative explanations, such as the rise in crimes being driven by changes in policing, or by the sense of loss in services alone.

Given the effects, which suggest that having a core provision of youth clubs across space is important, I run counterfactual policy exercises comparing closing regimes. According to my results, crime rises could have been mitigated to nearly zero by taking into account commuting costs, and by prioritising maintaining a core provision, particularly in outer London areas.

These findings suggest that social policies that provide after school activities - especially in areas where other leisure alternatives are unavailable - might be particularly effective in crime prevention at a very young age, while people are in full-time education. Given the lasting effects of crime on many different outcomes, from labour markets, to family formation and future violence, it is likely that closures of these services exacerbate lasting inequality, as these youth clubs typically locate in more deprived areas.

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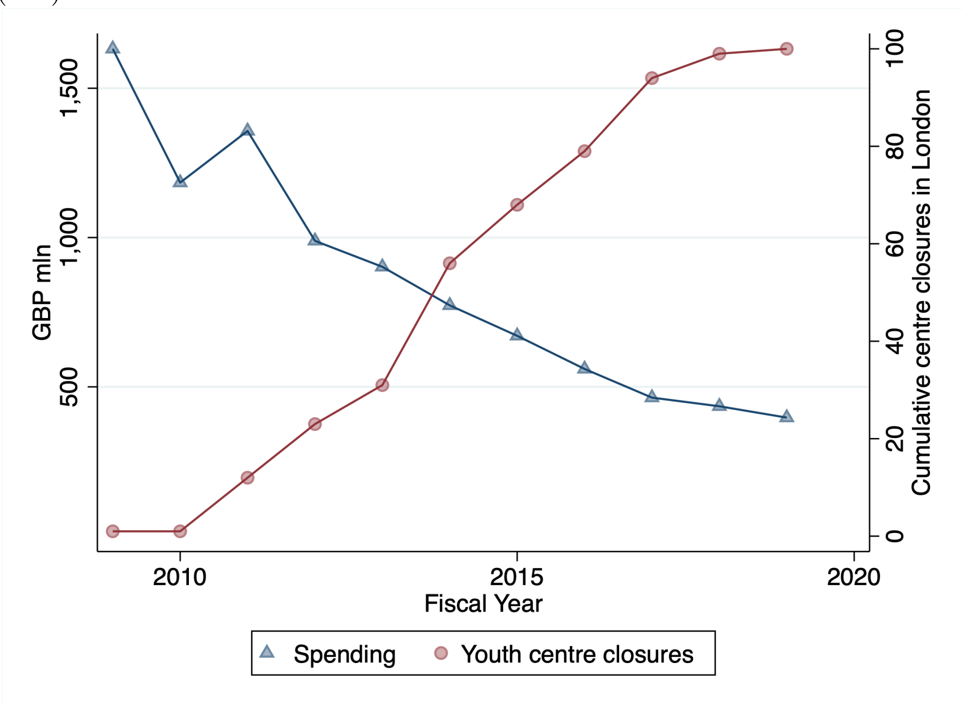
Figure 1: Wordcloud of advertised activities in youth clubs

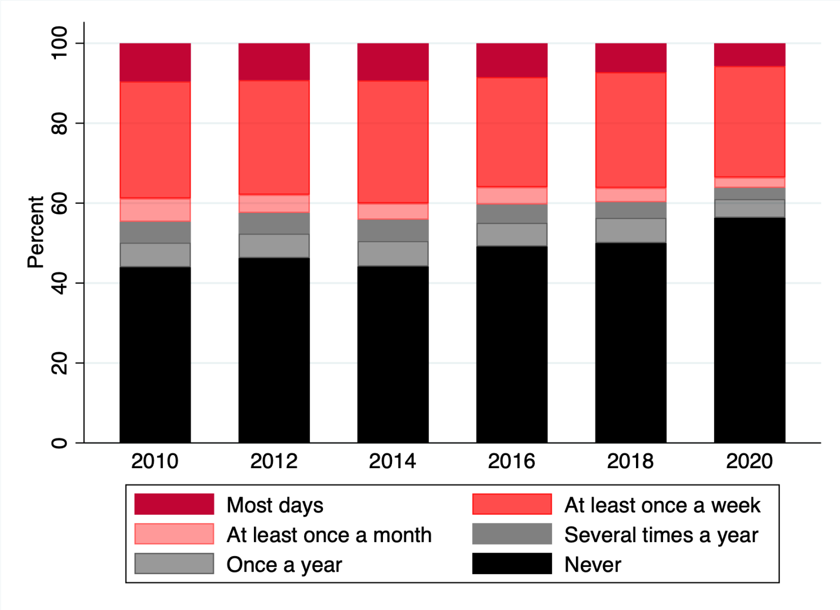
Notes: Wordcloud of activities and amenities mentioned on 277 youth clubs' websites.

Figure 2: Youth services' funding in England and Wales (blue), and centre closures in London (red)



Notes: The blue line shows funding to 'youth services' according to figures from the Department for Education, 2010-2019. The red line shows cumulative youth club closures in London from FOI data.

Figure 3: youth club usage



Notes: Responses to the question: 'how often do you go to youth clubs, scouts, girl guides or other organised activities' from waves 'a' to 'l' in Understanding Society. Youths aged 10 to 15 living in the Greater London area.

Table 1: Differences between youths attending centres and not, 2010

	Attends youth clubs		
	No	Yes	Diff
Index Mult Depriv High	0.37	0.42	0.058*
	(0.02)	(0.02)	(0.031)
Index Mult Depriv Medium	0.36	0.36	-0.004
	(0.02)	(0.02)	(0.031)
Index Mult Depriv Low	0.27	0.22	-0.054*
	(0.02)	(0.02)	(0.028)
White	0.62	0.57	-0.053*
	(0.02)	(0.02)	(0.031)
Black	0.16	0.24	0.084***
	(0.01)	(0.02)	(0.025)
Asian	0.19	0.15	-0.041*
	(0.02)	(0.02)	(0.024)
Observations	612	413	1,025

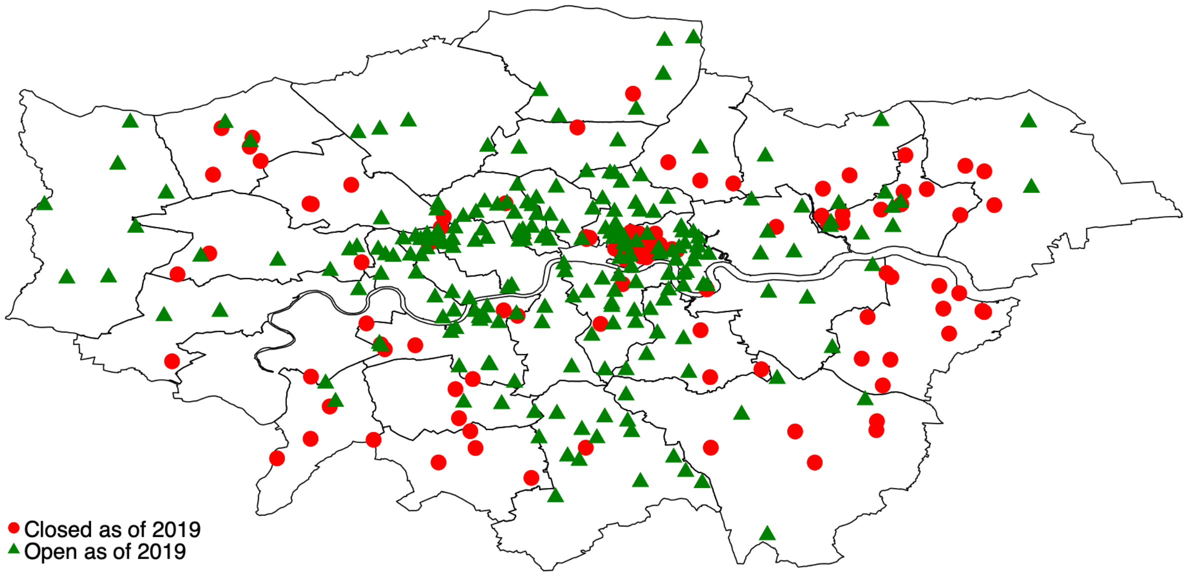
Notes: The table shows the mean values of selected socioeconomic characteristics comparing respondents from the London Youth Survey (2009). Column 'No' shows mean values for people who replied 'never' to the question 'How often do you go to youth clubs or other youth projects such as youth councils'. Column 'Yes' for respondents who state 'rarely', 'sometimes', 'quite often' or 'very often'. The third column shows the difference in means and estimated standard error between the two groups. Stars (*, **, ***) indicate significance at the 90%, 95%, and 99% confidence levels, respectively.

Table 2: Differences between youths attending centres and not, 2010-2019

	Attends organised activities		
	No	Yes	Diff
Hours watching TV schoolday	3.9	3.73	-0.173***
	(0.02)	(0.02)	(0.023)
Hours social media schoolday	3.65	3.38	-0.269***
	(0.02)	(0.02)	(0.033)
Hours videogames schoolday	2.78	2.35	-0.432***
	(0.07)	(0.08)	(0.108)
Hours homework schoolday	2.34	2.34	-0.003
	(0.03)	(0.04)	(0.051)
Days sports per week	3.24	3.92	0.684***
	(0.02)	(0.02)	(0.04)
Number of friends	7.33	8.17	0.846***
	(0.20)	(0.26)	(0.328)
N	16257	14685	30942

Notes: The table shows the mean values of selected socioeconomic characteristics comparing respondents from Understanding Society. Column 'No' shows mean values for people who replied 'never' or 'almost never' to the question 'How often do you go to youth clubs or other youth projects such as youth councils'. Column 'Yes' for respondents who state 'several times a year', 'several times a month', 'at least once a week' or 'most days'. The third column shows the difference in means and estimated standard error between the two groups. Stars (*, **, ***) indicate significance at the 90%, 95%, and 99% confidence levels, respectively.

Figure 4: youth clubs open (green) and closed (red) in London



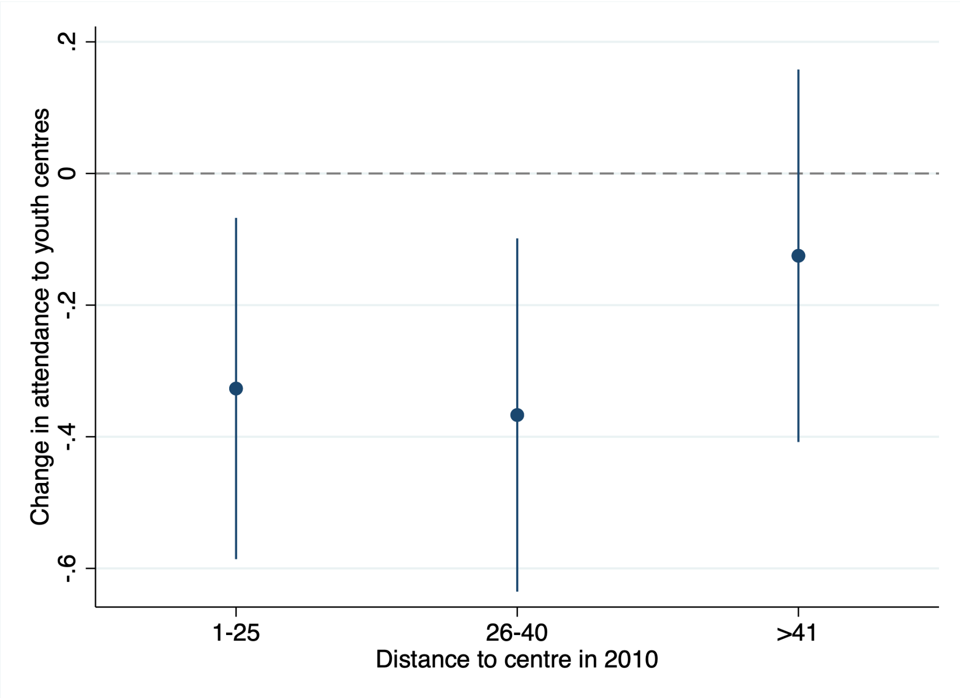
Notes: Youth centres and youth clubs in London from FOI requests. The dots in red show youth centres and youth clubs which have closed between 2010 and 2019. The triangles in green represent youth centres and youth clubs which were open throughout.

Table 3: Correlates of closures

	P (closure) > 0			
Council managed	0.280***	0.179**		
	(0.053)	(0.062)		
Dist. to nearest alternative centre	-0.083	-0.074	-0.099	-0.135*
	(0.045)	(0.047)	(0.058)	(0.067)
Building pre 1945	0.051	0.089	0.076	0.098
	(0.083)	(0.077)	(0.131)	(0.128)
Building 1945-1959	-0.008	0.021	-0.063	-0.020
	(0.090)	(0.087)	(0.140)	(0.137)
Building 1960-1979	0.055	0.020	0.150	0.081
	(0.116)	(0.103)	(0.178)	(0.153)
% social housing (2011)	-0.207	0.051	-0.310	0.010
	(0.165)	(0.148)	(0.245)	(0.233)
% population 0-13 (2011)	0.367	-0.328	0.966	-0.610
	(0.532)	(0.560)	(0.879)	(1.017)
Pop. density (log)	-0.116*	-0.020	-0.159*	-0.033
	(0.051)	(0.051)	(0.073)	(0.071)
Conservative council	-0.073		-0.045	
	(0.060)		(0.091)	
Sample	All	All	Council funded	Council funded
Mean	0.323	0.323	0.451	0.451
Borough FE	No	Yes	No	Yes
N	285	285	164	164
R-squared	0.142	0.389	0.080	0.459

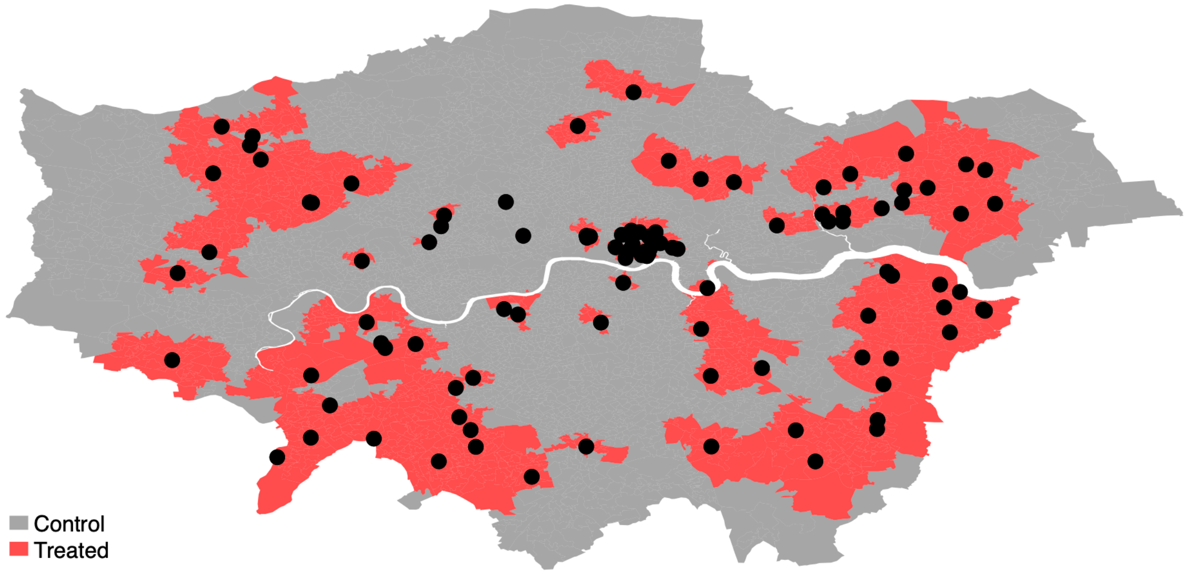
Notes: The table shows the results of estimating linear probability models of the probability of a youth club closing on various characteristics. The variables on s comes FOI requests. The building data comes from Verisk, downloaded via Digimap. The proportion of population living in social housing, and the proportion aged 0-13 come from the 2011 Census. Standard errors clustered at level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure 5: Effects on probability of attending by distance



Notes: The table shows the impacts of closures on the probability of a respondent reporting attending s in a given year. The effects are presented by distance between the respondents home and the nearest at baseline (in 2011), expressed in commuting minutes on foot. Confidence intervals are calculated clustering standard errors at the MSOA level (by respondents' address).

Figure 6: Treatment areas (red) and untreated areas (gray)



Notes: The areas in red show blocks (LSOAs) where the nearest closed between 2010 and 2019. The areas in gray are blocks in which the nearest remained open since 2010. The dots show s that closed at some point between 2010 and 2019.

Table 4: Balance tests: treatment and control blocks

	Control mean/sd	Treated mean/sd	Diff diff/sd	> 40 mins mean/sd	Diff diff/sd
Population	1697.99	1708.44	10.44	1655.52	-45.436***
	(273.12)	(257.43)	(9.75)	(243.21)	(9.08)
% aged 0-13	0.174	0.177	0.003*	0.176	0.001
	(0.048)	(0.042)	(0.002)	(0.036)	(0.002)
% aged 14-18	0.055	0.058	0.003***	0.061	0.005***
	(0.020)	(0.022)	(0.001)	(0.017)	(0.001)
% ethnic minorities	0.410	0.412	0.002	0.334	-0.077***
	(0.193)	(0.226)	(0.007)	(0.194)	(0.007)
% no qualifications	0.094	0.103	0.009***	0.105	0.009***
	(0.052)	(0.051)	(0.002)	(0.052)	(0.002)
% social housing	0.262	0.221	-0.041***	0.153	-0.098***
	(0.210)	(0.194)	(0.007)	(0.159)	(0.007)
% lone parents	0.089	0.084	0.005***	0.077	-0.011***
	(0.052)	(0.048)	0.002	(0.046)	(0.002)
Crime participation rate 10-15	0.013	0.012	-0.00	0.009	0.003***
	(0.025)	(0.034)	(0.00)	(0.013)	(0.001)
Observations	2,684	1,060		1,085	

Notes: The table shows the mean values of selected socioeconomic characteristics from the 2011 Census in areas affected by closures, areas unaffected, and areas which are further than 40 minutes from a at baseline (labeled 'rest of London'). The values in red highlight economically meaningful differences in selected indicators.

Table 5: Summary statistics

	mean	sd	min	max
Crime participation rate 10-15	0.009	0.012	0	0.055
Crime participation rate 16-17	0.035	0.047	0	0.250
Crime participation rate 18-25	0.043	0.031	0	0.143
Crime participation rate 26-35	0.022	0.017	0	0.080
Crime incidence rate 10-15	0.012	0.016	0	0.055
Crime incidence rate 16-17	0.049	0.065	0	0.250
Crime incidence rate 18-25	0.055	0.040	0	0.143
Crime incidence rate 26-35	0.027	0.022	0	0.080
Observations	46779			

Notes: Summary statistics for 4,615 block-year observations. Crime participation is calculated as the number of residents committing crimes over the population, and is computed by block, age, and year. Crime incidence is calculated as the total number of crimes committed by residents over total residents, again by block, age and year. Crime data comes from administrative records from the London Metropolitan Police. Population estimates at the block, year and age level come from ONS.

Table 6: Effect on attendance to s

	Frequency (std)	P (attends yearly or more > 0)
After x Treated	-0.389	-0.237**
	(0.239)	(0.112)
Sample	Full	Full
P-value	0.104	0.035
Mean	-0.066	0.472
N	8,912	8,912

Notes: The table shows the results of OLS regressions of attendance to s as reported by respondents born after 2000 in Understanding Society between 2010 and 2019. Frequency of attendance is a standardised variable of responses originally categorised into six values, ranging from 'Never/Almost never' to 'Most days'. The probability of attending is 1 if a person reports attending centres at least once a year. All columns include individual-stack and year-stack fixed effects. Standard errors are clustered at the MSOA level in parenthesis. Stars (*, **, ***) indicate significance at the 90%, 95%, and 99% confidence levels, respectively.

Table 7: Impact of closures on crime participation

	Crime participation rates				
	10-18	10-15	16-17	18-25	26-35
After x Treated	0.084***	0.092***	0.040	-0.013	0.015
	(0.025)	(0.035)	(0.033)	(0.016)	(0.015)
Marginal effect	0.174	0.094	0.044	-0.067	0.075
P-value	0.001	0.008	0.226	0.427	0.337
Mean	0.017	0.011	0.052	0.048	0.023
N	250,940	242,840	247,071	254,401	254,320

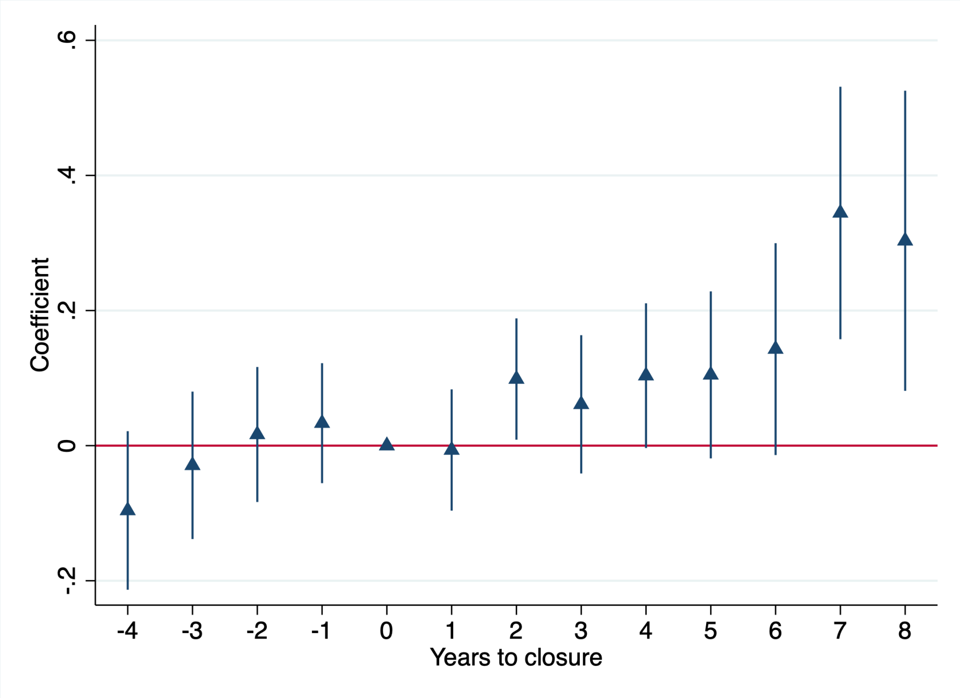
Notes: Crime rates expressed with respect to each age group block and year. The regressions show estimated coefficients from a stacked difference-in-differences research design on block and year level. To create each 'stack', each cohort of areas which suffer closures in the same year are matched to all never treated areas. The coefficients are estimated using Poisson QMLE. All columns include block-stack and year-stack fixed effects. Propensity weights applied. Standard errors clustered at the MSOA level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Impact of closures on crime incidence

	Crime incidence rates				
	10-18	10-15	16-17	18-25	26-35
After xTreated	0.114***	0.086**	0.062	-0.026	0.021
	(0.032)	(0.040)	(0.038)	(0.018)	(0.018)
Marginal effect	0.348	0.124	0.098	-0.175	0.134
P-value	0.000	0.031	0.103	0.153	0.244
Mean	0.027	0.016	0.079	0.063	0.030
N	251,220	243,410	247,541	254,401	254,320

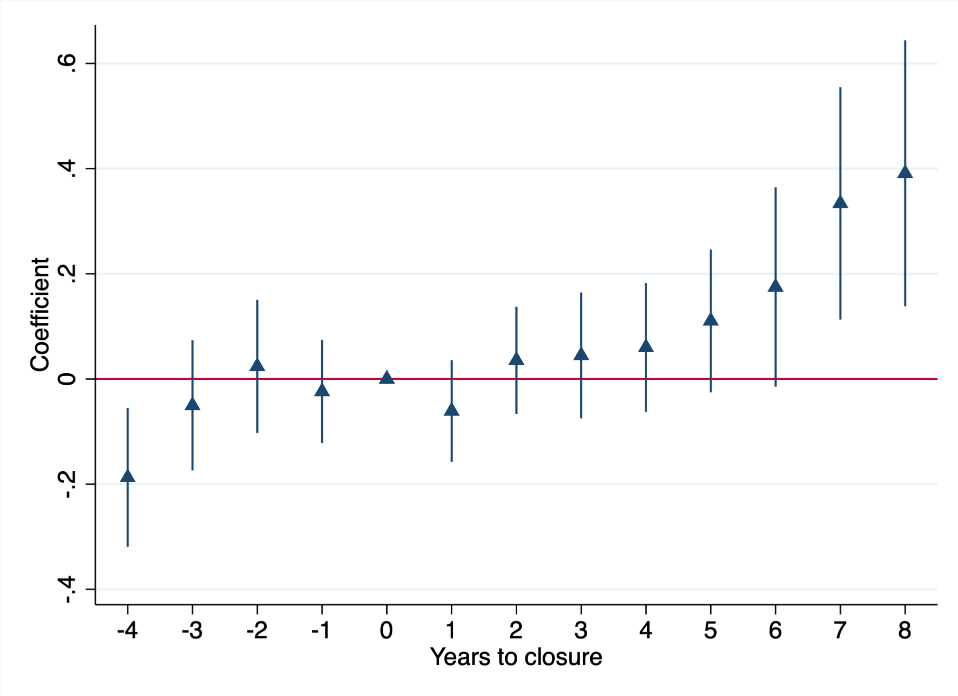
Notes: Crime rates expressed with respect to each age group block and year. The regressions show estimated coefficients from a stacked difference-in-differences research design on block and year level. To create each 'stack', each cohort of areas which suffer closures in the same year are matched to all never treated areas. The coefficients are estimated using Poisson QMLE. All columns include block-stack and year-stack fixed effects. Propensity weights applied. Standard errors clustered at the MSOA level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure 7: Event study, effect of closures on crime participation age 10-15



Notes: Crime rates expressed with respect to each age group block and year. Estimated coefficients from a Poisson regression of crime participation rates in a block and year over time. The base year is the year centres close. Propensity weights applied. Confidence intervals from MSOA-level clustered standard errors.

Figure 8: Event study, effect of closures on crime incidence age 10-15



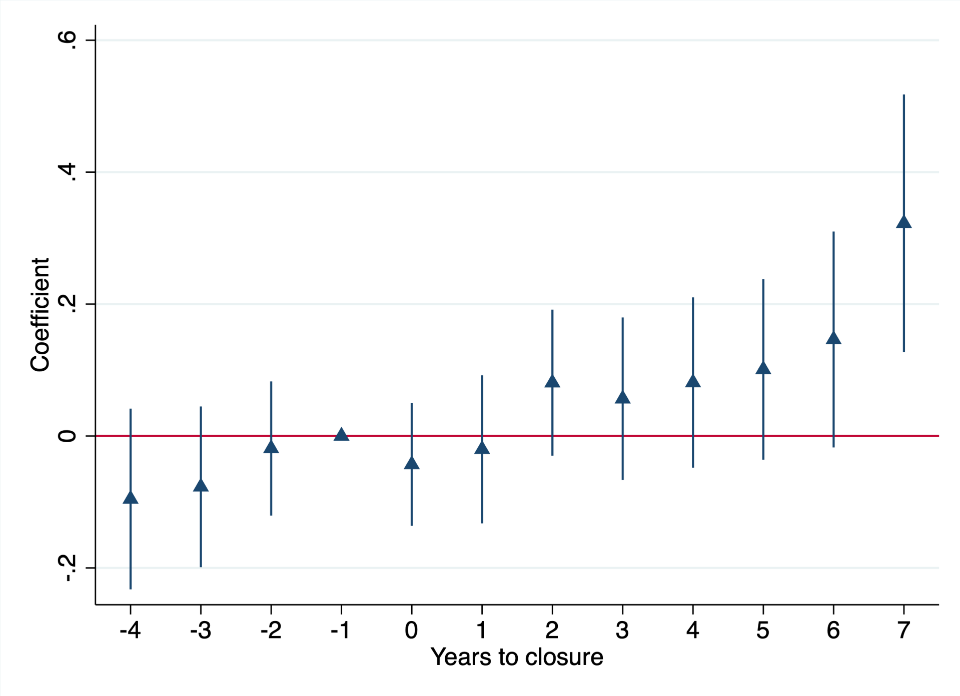
Notes: Crime rates expressed with respect to each age group block and year. Estimated coefficients from a Poisson regression of crime incidence rates in a block and year over time. The base year is the year centres close. Propensity weights applied. Confidence intervals from MSOA-level clustered standard errors.

Table 9: Impact on differential crime rates - base group aged 26-35

	Participation	Incidence			
	All	All	Drugs	Violence	Robbery
After x Treated x 10-15	0.120***	0.111***	0.133*	0.096	0.260
	(0.034)	(0.039)	(0.076)	(0.062)	(0.173)
After x Treated x 16-17	0.083**	0.101***	0.109**	0.146**	0.003
	(0.034)	(0.039)	(0.050)	(0.068)	(0.182)
After x Treated x 18-25	-0.016	-0.032	0.026	-0.050	0.136
	(0.020)	(0.022)	(0.030)	(0.040)	(0.163)
Marginal effect 10-15	0.370	0.444	0.200	0.090	0.030
Marginal effect 16-17	0.257	0.406	0.164	0.136	0.000
Mean	0.033	0.047	0.015	0.008	0.003
N	1,017,367	1,017,367	1,017,367	1,017,367	1,017,367

Notes: The regressions show estimated coefficients from a stacked triple difference-in-differences research design on block-age group and year level. Each stack are areas which are treated in a given year matched to all never treated areas. The omitted age group are individuals aged 26 to 35 years old. Crime rates expressed with respect to each age group block and year. The coefficients are estimated using Poisson QMLE. All columns include block-stack-year, age group-stack-year, and block-age group-stack fixed effects. Propensity weights applied. Standard errors clustered at the MSOA level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure 9: Event study, effect of closures on crime participation age 10-15 vs 26-35



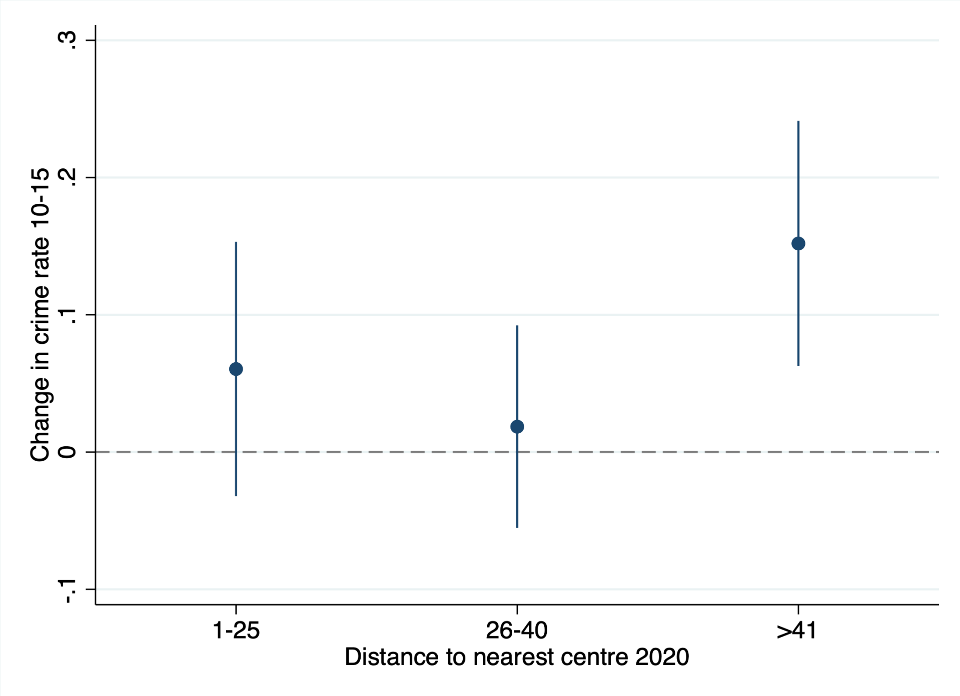
Notes: Estimated impact of closures on differential crime participation for residents aged 10-15 against residents aged 26 to 35 in areas affected by closures vs areas unaffected. Crime rates expressed with respect to each age group block and year. The base year is the year centres close. Confidence intervals from MSOA-level clustered standard errors.

Table 10: Effect on the spatial distribution crimes

	All	Crimes by 10-18	Undetected crime
After x Treated	-0.509	0.027	1.052
	(0.987)	(0.108)	(0.784)
P-value	0.606	0.803	0.180
Mean	0.078	0.002	0.062
N	216,254	215,922	216,254

Notes: Crime rates expressed over total population residing by block and year. The regressions show estimated coefficients from a stacked difference-in-differences research design on block and year level. To create each 'stack', each cohort of areas which suffer closures in the same year are matched to all never treated areas. The coefficients are estimated using Poisson QMLE. All columns include block-stack and year-stack fixed effects. Propensity weights applied. Standard errors clustered at the MSOA level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure 10: Effect of closures by proximity to other centres



Notes: The plot shows the estimated effects of closures on crime by distance to the nearest centre after closure (second nearest centre at baseline - in 2011). Confidence intervals from MSOA-level clustered standard errors.

Table 11: Effect on crime participation by availability of other leisure amenities

	Crime participation rate, 10-15				
	Any	Schools	Libraries	Sports	Parks
After x Treated x Far	0.118***	0.121***	0.092**	0.086**	0.077*
	(0.044)	(0.042)	(0.043)	(0.042)	(0.045)
After x Treated x Near	0.038	0.037	0.093*	0.107**	0.108**
	(0.051)	(0.051)	(0.053)	(0.051)	(0.047)
Mean	0.011	0.011	0.011	0.011	0.011
N	242,840	242,840	242,840	242,840	242,840

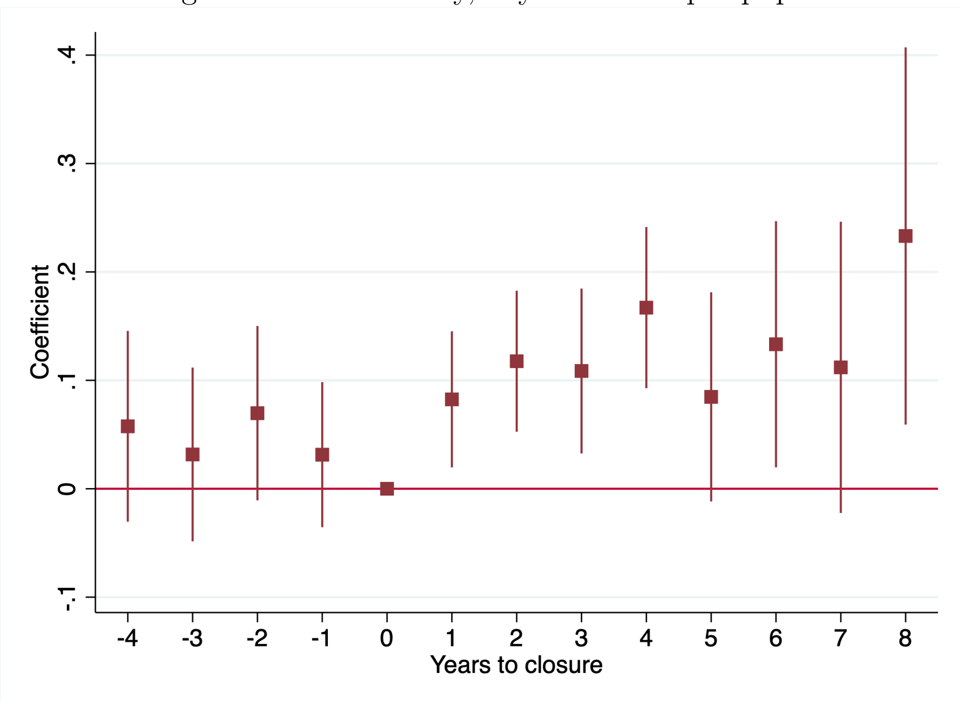
Notes: Estimated the impact of closures by availability of other leisure amenities. ‘Far’ or ‘Near’ are defined relative to the median distance to each type of amenities from the Points of Interest database in Ordnance Survey. The regressions show estimated coefficients from a stacked difference-in-differences research design on block and year level. To create each ‘stack’, each cohort of areas which suffer closures in the same year are matched to all never treated areas. The coefficients are estimated using Poisson QMLE. All columns include block-stack and year-stack fixed effects. Propensity weights applied. Standard errors clustered at the MSOA level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 12: Effect on the rate of school exclusions

	Perm. exclusion rates	Suspension rates	
	headcount	days	headcount
After x Treated	0.231***	0.069**	0.024
	(0.067)	(0.029)	(0.023)
Marginal effect	0.065	1.166	0.123
P-value	0.001	0.016	0.309
Mean	0.002	0.176	0.048
N	177,470	253,060	253,060

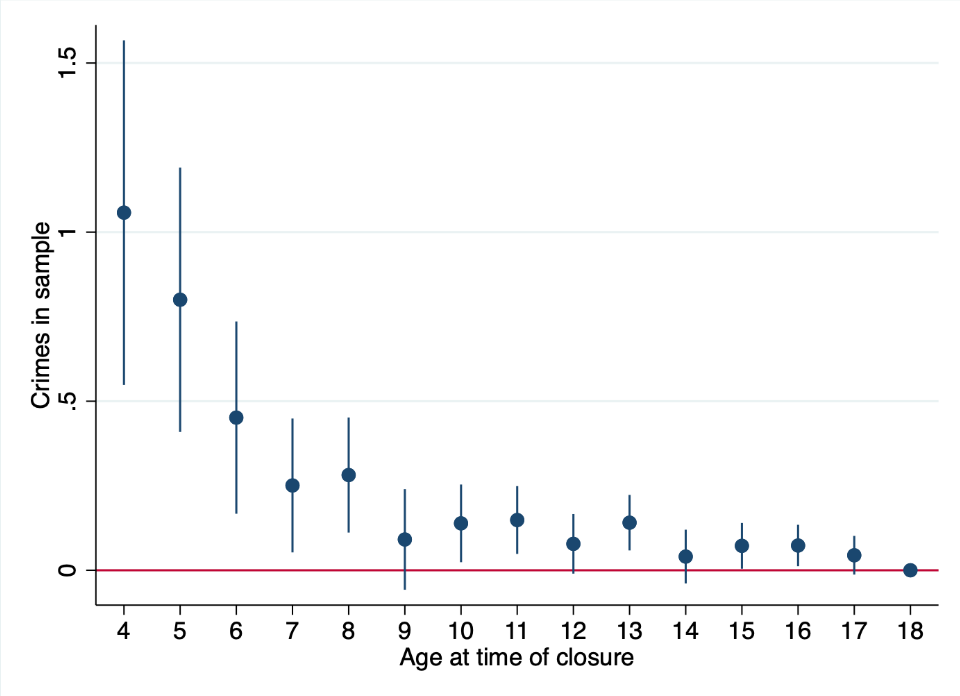
Notes: Estimated effects of closures on school suspensions and exclusions, from the Department of Education. While we do not have precise data on the age of the excluded pupils 98% of exclusions relate to people under the age of 16, as such rates are calculated using the population aged 10-15. The regressions show estimated coefficients from a stacked difference-in-differences research design on block and year level. To create each 'stack', each cohort of areas which suffer closures in the same year are matched to all never treated areas. The coefficients are estimated using Poisson QMLE. All columns include block-stack and year-stack fixed effects. Propensity weights applied. Standard errors clustered at the MSOA level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure 11: Event study, days excluded per pupil



Notes: Estimated impact of closures on school suspension length rate. The variable is defined as the total number of days residents of a given block are temporarily suspended over total residents aged 10-15. While we cannot assert the precise age of those suspended, aggregate data from the Department of Education shows that 98% of school exclusions happen within that age range. The base year is the year centres close. The bars indicate confidence intervals at the 95 % confidence level. Standard errors clustered at the MSOA level.

Figure 12: Event study, cohort crimes, by age at time of closure



Notes: Estimated impact of closures on crimes at cohort and block level for individuals born between 1993 (aged 17 to 27 in sample) and 2007 (aged 3 to 13 in sample). The dependent variable is the total crimes committed by residents born in the same year in a given block. The running variable is the age in which a cohort in a given block experiences the nearest centre closure. The variable is set to 18 for people for whom the centre never closes, or for whom it closes after they are 18. Cohort and block fixed effects included. Confidence intervals from MSOA-level clustered standard errors.

Table 13: Effect on crime detection

	Detection rates			
	All	Drugs	Violence	Acquisitive
After x Treated	-0.011	-0.008	-0.002	-0.040
	(0.014)	(0.005)	(0.013)	(0.032)
Marginal effect	-0.200	-0.046	-0.008	-0.135
P-value	0.422	0.139	0.908	0.222
Mean	0.218	0.845	0.260	0.080
N	253,618	239,298	252,798	253,502

Notes: Crime detection rates are defined as the number of crimes happening in a block and year for which a suspect is identified over total crimes recorded in a given block and year. The regressions show estimated coefficients from a stacked difference-in-differences research design on block and year level. To create each 'stack', each cohort of areas which suffer closures in the same year are matched to all never treated areas. The coefficients are estimated using Poisson QMLE. All columns include block-stack and year-stack fixed effects. Propensity weights applied. Standard errors clustered at the MSOA level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 14: Effect on stop and search

	Stop and search rate		
	All	10-15	16-17
After x Treated	0.071	-0.187	-0.634
	(0.955)	(0.343)	(0.411)
P-value	0.941	0.586	0.123
Mean	0.019	0.061	0.428
N	100,326	94,298	98,360

Notes: Stop and search data comes from Police UK and is available from April 2016 onwards. Stop rates expressed with respect to all residents by block and year in column 1, and by age group block and year in columns 2 and 3. The regressions show estimated coefficients from a stacked difference-in-differences research design on block and year level. To create each 'stack', each cohort of areas which suffer closures in the same year are matched to all never treated areas. The coefficients are estimated using Poisson QMLE. All columns include block-stack and year-stack fixed effects. Propensity weights applied. Standard errors clustered at the MSOA level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 15: Continuous treatment estimates

	P(attends yearly or more)	P (attends monthly or more)	Crime participation 10-15 years old
Commuting time (10s mins)	-0.459*	-0.600***	0.025**
	(0.277)	(0.215)	(0.011)
Estimation	Logit	Logit	Poisson
P-value	0.098	0.005	0.048
Magnitudes from linear spec	-17%	-28%	2.5%
Mean dep. var	0.452	0.429	0.01
N	686	686	38,920

Notes: The table shows the results of Logit regressions of the relationship between commuting time to centres, and attendance, and Poisson regressions of the commuting time to centres, and crime participation rates. The regression includes block and year fixed effects. Standard errors are heteroskedasticity-robust in the Logit specifications, and clustered on block level in the Poisson regression. The sample is restricted to areas within 40 minutes of a centre at baseline for the crime regression. Stars (*, **, ***) indicate significance at the 90%, 95%, and 99% confidence levels, respectively.

Table 16: Comparison of counterfactual closures - taking into account change in commute

Scenario	**Description**	**Avg. commute**	**Δ mins**	**Δ crime 10-15**
Baseline		24.97		
Austerity	Closure of 100 youth clubs	29.18	4.21	1.05%
Random	Random closures of 100 centres	29.19	4.22	1.06%
Optimal I	Minimise distance, close 100	25.36	0.39	0.10%
Optimal II	Minimise + weight pop, close 100	25.37	0.40	0.10%
Optimal III	Minimise + weight depriv, close 100	25.39	0.42	0.10%

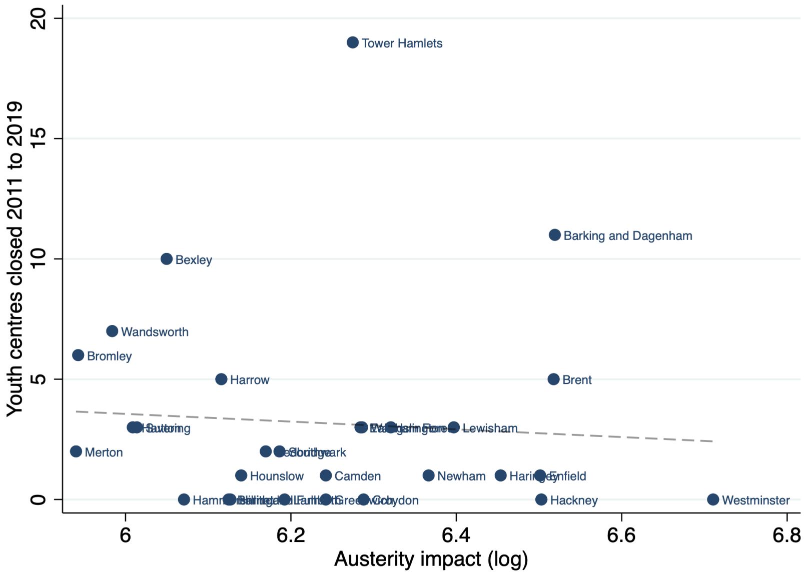
Notes: The table shows the estimated change in crime of different closing regimes. Austerity represents the real observed policy, where 223 youth clubs were open in 2020. Random shows the average of 1,000 random closing regimes which would have maintained open 223 random centres as of 2020. The various optimal exercises were 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 centres to only 223 open. The different optimality exercises weight centroids by different attributes, taken from the 2011 census.

A. Appendix

A.A Youth club closures and other austerity shocks

The decade 2010 to 2019 was marked by austerity in many public services. In this section I show, descriptively, that the closure of youth centres and youth clubs does not correlate with other well studied austerity cuts. First, I use the measure developed by Beatty & Fothergill (2014), and used in other work around the unintended consequences of austerity (Fetzer 2019) to proxy the loss in individual transfers at the local authority level. The number of youth centre closures and the austerity shock in this area don't correlate strongly. The relationship is, if anything negative (areas with higher welfare losses have lower youth centre closures), but it is not statistically significant. A local authority level OLS regression of the number of youth centre closures on the total welfare cuts yields a coefficient of -0.003, with a p-value of 0.704 (N=32).

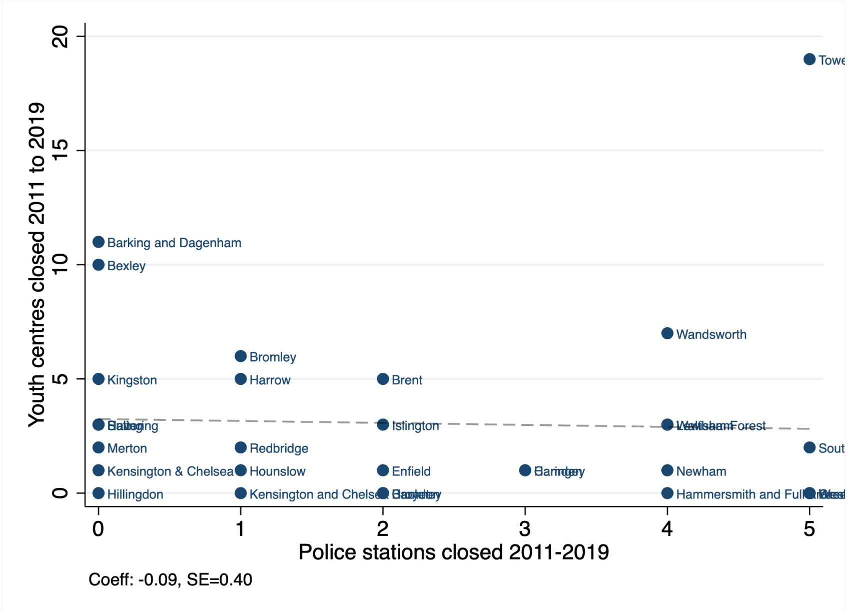
Figure A.1: Correlation youth centre closures and total austerity



Notes: Correlation between the number of youth clubs closing in each local authority between 2011 and 2019, and the expected impact of the welfare reforms per working age individual in GBP from Beatty & Fothergill (2014).

Second, I look at another well studied shock and obtain data on the number of police stations that closed across London from publicly available FOI data, as in Facchetti (2021). First, I only observe both a closure of a police station and a closure of a youth centre in the same borough and year in 7% of all possible borough-year combinations. Looking at the total number of closures of police stations and youth centres across London between 2011 and 2019 also shows the two shocks to be orthogonal. An OLS regression relating the number of youth centres closed in a borough to the number of police stations closed shows a coefficient of 0.086, which is not statistically significant, with a p-value of 0.831 (N=32).

Figure A.2: Correlation youth club closures and police station closures



Notes: Correlation between the number of youth clubs closing in each local authority between 2011 and 2019, and the number of police stations closed in 2010-2019.

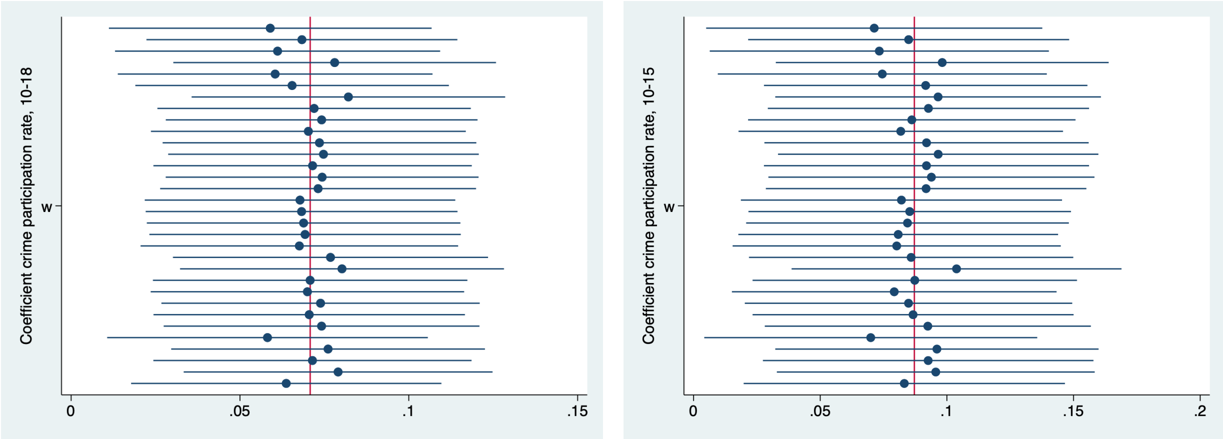
A.B Potential biases

I argue that any concerns around data limitations, or around potential threats to identification would imply that the coefficients reported in the paper could be a lower bound on the total effects of youth clubs on crime, rather than the opposite.

First, a discussion on the parallel trends assumption is due to appease any concerns around local authorities strategically closing centres based on crime trends. While - in my view - the event study plots provide suggestive evidence in support of the validity of the assumption, I argue that if that is a concern the only logical strategic choice for local authorities to close is to do so in areas where crime trends would have been decreasing, or where youth clubs were not deemed as necessary (perhaps due to demand). In fact the data shows that areas with higher proportions of population renting from the council where less likely to suffer closures, suggesting some prioritisation. Hence, and given that I observe rises in crime post-closures, this suggests that in the absence of these potential 'selection' based on trends the effects might have been larger.

With regards to data limitations I can only study youth centres as a neighbourhood amenity which is either open or not. However, centres in the comparison group were also affected by austerity, having to perhaps open fewer days, or decreasing payments to their youth workers as discussed above. Hence, the quality of the service is likely to also be affected in control areas. My estimates assess the difference between areas near centres that close and centres near areas that remain open, but not the difference with respect to areas where operation of youth clubs remained 'as usual'. I argue that, had those centres remained open without any cuts to their services the rises in crime measured would have been even larger.

Figure A.3: Coefficients from leave-one-out robustness checks



Notes: Poisson regressions of offenders in a block and year, by age group. Each dot and confidence interval shows the result excluding a borough in London at a time. Unweighted sample. Standard errors clustered at the MSOA level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

A.C Robustness of the main results

I present estimated coefficients for the main results, in which I find rises in crime participation for young people in areas affected by closures when compared to areas unaffected by closures (where centres remain open). The results are robust to changes in assumptions, sample selection, functional form, and alternative inference calculations.

Table A1: Crime participation rate, changes in sample

	Crime participation rate			
	10-18	10-15	10-18	10-15
After x Treated	0.179***	0.110**	0.170***	0.099***
	(0.065)	(0.045)	(0.050)	(0.035)
Sample	30 minutes	30 minutes	50 minutes	50 minutes
Marginal effect	0.179	0.110	0.170	0.099
P-value	0.006	0.013	0.001	0.004
Mean	0.018	0.011	0.017	0.011
N	209,610	203,300	274,760	265,150

Notes: Poisson regressions of offenders in a block and year, by age group. All columns include block-stack, and year-stack fixed effects. Unweighted sample. Standard errors clustered at the MSOA level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A2: Crime participation rate, Outer London boroughs

	Crime participation rate	
	10-18	10-15
After x Treated	0.090***	0.134***
	(0.028)	(0.037)
Marginal effect	0.192	0.141
P-value	0.001	0.000
Sample	Outer	Outer
Mean	0.015	0.009
N	123,240	120,010

Notes: Poisson regressions of offenders in a block and year, by age group. Outer London boroughs include Barking and 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. All columns include block-stack, and year-stack fixed effects. Unweighted sample. Standard errors clustered at the MSOA level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A3: Estimating the effect on counts instead of rates

	Crime participation (residents)	
	10-18	10-15
After x Treated	0.055**	0.087***
	(0.024)	(0.032)
Marginal effect	0.119	0.093
P-value	0.019	0.007
Mean	2.538	1.171
N	250,940	242,840

Notes: Poisson regressions of offenders in a block and year, by age group. All columns include block-stack, and year-stack fixed effects. Unweighted sample. Standard errors clustered at the MSOA level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A4: Effect on crime, by age group - alternative inference methods

	Crime participation rate			
	cluster on borough		randomisation inference	
	10-18	10-15	10-18	10-15
After x Treated	0.071**	0.087**	0.087***	0.087***
	[0.023,0.010]	[0.042,0.003]	[0.000,0.018]	[0.000,0.028]
Marginal effect	0.152	0.093	0.152	0.093
P-value	0.023	0.042	0.000	0.005
Mean	0.017	0.011	0.017	0.010
N	250,940	242,840	254,410	254,410

Notes: Poisson regressions of offenders in a block and year. All columns include block-stack, and year-stack fixed effects. Unweighted sample. Confidence intervals in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table A5: Effect on crime, by age group - alternative estimators

	Crime participation rate					
	10-18	10-15	10-18	10-15	10-18	10-15
ATT	0.072***	0.106***	0.001***	0.002***	0.002***	0.002**
	(0.026)	(0.035)	(0.023)	(0.001)	(0.001)	(0.001)
Estimation	PQMLE - W	PQMLE - W	TWFE	TWFE	SDID	SDID
Marginal effect magnitude	0.16	0.11	0.12	0.24	0.15	0.16
Mean	0.02	0.01	0.02	0.01	0.02	0.01.
N	38,380	37,160	38,810	38,810	38,810	38,810

Notes: Regressions of offenders in a block and year. All columns include block and year fixed effects. Columns 3 and 4 include year and cohort of treatment fixed effects. Unweighted sample. Standard errors clustered at the MSOA level in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

A.D Exploring mechanisms

I present additional figures exploring the potential channels explaining the effects observed on crime.

First, I assess heterogeneity in effects by hour in which crimes are committed, although this severely limits statistical power in the analysis. To do so I group crimes into those committed during school hours (from 8AM to 3PM), those committed after-school (from 3PM to 9PM), and those committed during school holidays and weekends (any hour). There are very few crimes committed at night by 10 to 15 year olds, and is hence excluded from the analysis. The results show slightly more precise estimates for crimes committed after-school, but I cannot reject that crime rises in other hour groups might be similar.

Table A6: Impact by hour - crimes by 10-15 year old residents

	Crime incidence rates		
	School hours	After-school	Holidays and weekend
After x Treated	0.044	0.090*	0.058
	(0.051)	(0.053)	(0.054)
P-value	0.382	0.087	0.281
Mean	0.004	0.004	0.005
N	210,100	207,200	221,810

Notes: Poisson regressions of crime rates in a block and year. All columns include block-stack and year-stack fixed effects. Propensity weights applied. Standard errors clustered at the MSOA level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Second I present evidence of symmetric effects, which help rule out alternative explanations, such as the effects on crime being driven by frustration upon losing services, rather than by the benefits of partaking in youth clubs. Unfortunately, there are very few openings in the decade, and only 15 that we can evaluate.[19] and only 227 lower super output areas for whom these centres are the nearest one. As such the analysis is limited by statistical power.

To find a suitable comparison group to those I use a propensity score matching algorithm that finds the most similar blocks based on on their distance to youth centres at baseline (before openings), the proportion of council housing renters, the proportion of college graduates, the proportion without qualifications, the proportion aged 0-14 years old, and the distance to schools and parks. Once determined the comparative sample, the specification is as before, but now the treatment is 1 after an opening instead of after a closure. I restrict the sample to areas which were within 40 minutes of the new youth centres at the end of the sample (after opening) and cluster standard errors on MSOA level.

The results in Table A7 show that, after opening, crime participation for people aged 10-15 falls by 27.9%, significantly. An event study plot is in Figure A.4, and shows slightly noisy post-opening results, but going in the direction of falls in crime.

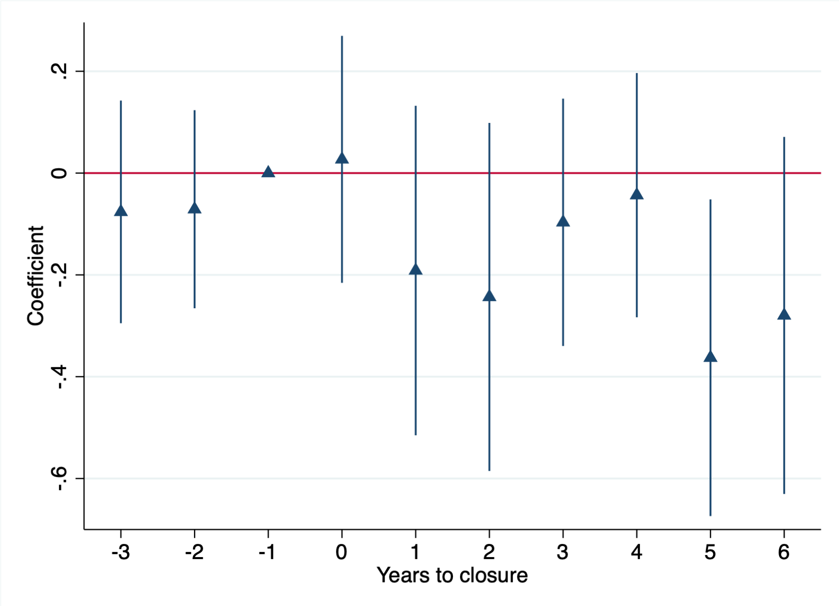
[19]There are 28 new youth clubs opening between 2010 and 2019. Of those, 13 also closed during the decade.

Table A7: Impact of openings on crime participation

	Crime participation rates		
	10-18	10-15	16-17
After x Treated	-0.092	-0.171**	-0.054
	(0.074)	(0.086)	(0.082)
Marginal effect	-0.311	-0.279	-0.089
P-value	0.211	0.046	0.512
Mean	0.017	0.011	0.052
N	61,380	59,400	60,532

Notes: Poisson regressions of crime rates in a block and year. All columns include block-stack and year-stack fixed effects. Propensity weights applied. Standard errors clustered at the MSOA level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure A.4: Event study, effect of openings on crime participation age 10-15

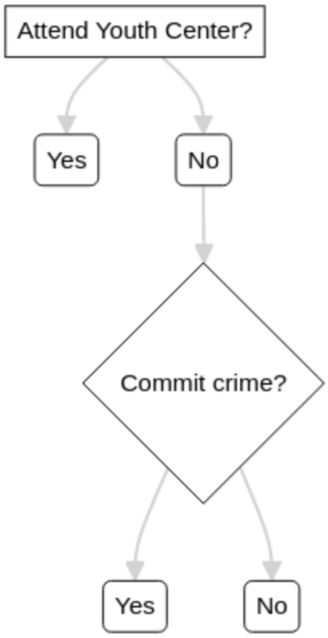


Notes: Estimated impact of youth centre openings on crime participation rates for 10-15 year old. The base year is the year before youth centres opened. The bars indicate confidence intervals at the 95% confidence level. Standard errors clustered at the MSOA level.

A.E Counterfactual closures

In Section IX I discuss counterfactual closing regimes using estimates from estimating the relationship between youth centre attendance, or crime participation on a continuous measure of distance. This is conceptualised from a discrete choice nested framework, for which a tree diagram is included below.

Figure A.5: Tree diagram for after-school leisure activity choice.



I now describe in detail the p-median model, used to calculate optimal closures. The P-median model is formalisation of a location model where a limited number of facilities must be placed over space so as to minimise the distance between facilities and demand nodes. A review of the model and its properties in a more formal framework can be found in Mirchandani & Francis (1990).

Let i denote potential facility locations, j represent demand points, 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 minimize the total cost or distance. In my context of study the demand nodes are given by population weighted centroids, and 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.

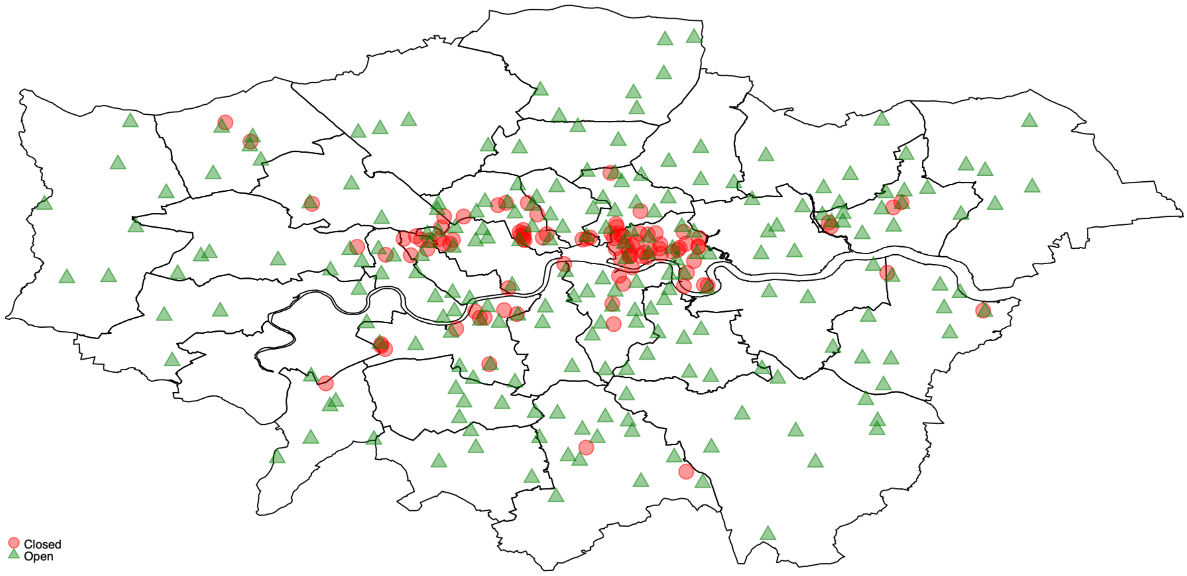
Constraints to the optimisation problem include:

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

In my context I assume that governments can at most finance 223 youth centres (which is the observed number of open youth centres as of 2020). I then calculate which centres are most optimal to maintain open based on these distance decisions. I compute the problem three times. The first weights each population centroid equally. The second gives more weights to those areas with more children as of 2011 (more population aged 0-13). The third augments the latter point by interacting children population with the number of households who are deprived in at least one dimension.

A map of the optimal locations according to the first optimisation exercise (all locations weighted equally) is included in Figure A.6 below. Additional results can be provided upon request. Overall, in all results fewer centres should have closed in Outer London, and more in Inner London.

Figure A.6: Optimal locations according to p-median model solution



Comparing this to the true closure regime shows that more youth centres in Inner London should have closed as opposed to in Outer London. While the model only takes into account commuting decisions, this is consistent with younger populations being more prevalent in outer London, and with gentrification dynamics which push families outside the city centre.

Figure A.7: Comparison of real closures and optimal locations according to p-median model solution