Local Social Projects and Neighbourhood Crime

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Abstract

We estimate the impact of youth-targeted interventions on crimes in the neighbourhood using exogenous variation in the timing of projects funded by the UK National Lottery. Employing a difference-in-differences approach, we find that communities receiving concurrent interventions totalling £30,000 or more experience reductions in anti-social behaviour of up to 8.1 percent over a 24-month period, with effects intensifying to 10.1 percent in urban areas. These findings suggest that modest, targeted youth programs can generate meaningful reductions in community-level antisocial behaviour. Interestingly, our results indicate that larger infrastructure projects, such as sports centres, may actually increase crime rates by providing target points for youth crime. Overall, our results have important implications for the role of community projects in crime prevention strategies.

Keywords: Public Funding, Youth Crime, Place-based policy
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1 Introduction

In most countries, criminal offending starts in adolescence, resulting in a hump-shaped offender age distribution that peaks just below age 20. At this

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age, crime incurs particularly large social costs with recent estimates for the UK putting the price of youth violence at approximately £11 billion between 2009 and 2020 Irwin-Rogers et al. (2020). Coupled with the high chance of recidivism, policies reducing youth offending should be particularly effective at mitigating these costs (see Levitt and Lochner (1999) for a review of the determinants of youth crime). Yet, public policy has often focused on increasing the expected costs of crime for the offender, even if the evidence on the effectiveness of sanction threat is ambiguous.[1] In addition, these policies often carry an additional private cost by curtailing future labour market opportunities for the perpetrator.[2] While progress towards reducing youth crime rates has been made during the last decade, incarceration rates are still stretching the UK's prison capacities and incurring a high public cost that is roughly equivalent to the cost of the averted crime (Owens, 2009).

Instead, many have advocated for interventions to keep youth and young adults out of the streets by providing opportunities for juveniles to stay school or participate in school-related activities for longer periods of time (Beck, 2024). By keeping youth busy, crime is reduced through a short-term incapacitation effect. Moreover, there is considerable evidence that additional schooling leads to crime reduction over the life-time Bell et al. (2022). Nevertheless, the overall effect is ambiguous and differs by type of crime. This is because bringing more youths together, particularly those with a higher propensity for anti-social behaviour, can lead to a higher number of potentially violent encounters: a concentration effect (Steinberg et al., 2019). There is, therefore, still limited evidence on the effectiveness of community-based interventions, such as local sport (Hartmann and Depro, 2006; Kelly, 2013) and youth centre activities at reducing crime. A notable exception is Villa (2024) who uses a difference-in-difference strategy to estimate that youth in London affected by the closure of a local youth centre became 15% more likely to commit crime, but that this had no effect on overall crime at the neighbourhood level. Interestingly, the effects were not constrained to youth centre opening time, suggesting that they were not driven solely by incapacitation effects.

[1] Relying on variations or discontinuity in sentencing between youth and adults, Piil Damm et al. (2025); Hjalmarsson (2009); Lee and McCrary (2017); Loeffler and Chalfin (2017) find not difference in crime rate while Arora (2023); Levitt (1998); Lovett and Xue (2018); Oka (2009) estimate reductions in offences committed.

[2] The literature on the private costs of incarceration is large, but with mixed estimates on the causal effect of incarceration on labour market outcomes, ranging from no long-run effects (Garin et al., 2024; Kling, 2006; Loeffler, 2013) to substantial costs (Harding et al., 2018; Mueller-Smith, 2015).

This paper contributes to this literature by providing evidence on the impact of small grass-root-led interventions, mostly targeting young people, on local crime. We examine the impact of community projects funded by the UK National Lottery Community Fund and Sport England (NLCF/SE). We exploit quasi-random variation in the timing of funding to estimate causal estimates of local community-led interventions on reported crimes in the recipients' neighbourhoods. Moreover, we differentiate between micro-interventions and larger scale projects to investigate potential differences in the effects of these place-based policies.

Both the NLCF and SE are bodies that distribute funds raised by a levy on the National Lottery. The funds are allocated to community-led organisations to help improve their communities or improve participation in sporting activities. While the funds are not solely focused on youth, or specifically targeting crime, we concentrate our investigation on projects more closely related to youth and sport, which are the most likely to result in incapacitation effects of youth. The NLCF/SE funds about 10,000 small projects (less than $\leq £10,000$) and larger projects (up to several million pounds) per year, which we investigate separately, as the mechanisms by which they might prevent crimes are likely to be different. Specifically, we consider the extensive margin of receiving a small project and the dose response to concurrent funding of several small projects on neighbourhood-level (ward) reported crime. Separately, we also estimate the impact of larger investments.

In a first step, we use a two-stage difference-in-difference approach that compares the evolution of crime rates in areas exposed to at least one small project with those that remain unaffected (Gardner, 2022). We find that at the extensive margin a small project leads no detectable effect on overall crime and anti-social behaviour, though property crime increases by roughly 1.6% relative to the monthly median. Since wards can be recipients of funding for different projects contemporaneously, in a second step, we estimate the intensive margin of funding. Again, we find no effect on overall crime, but when a ward sees £30,000 or more in small projects, there is a large negative reduction in anti-social behaviour (ASB) of 8.1%, and an increase in counts of non-ASB crimes by 5.1%. By classifying projects according to their descriptions, we pinpoint that these reductions in ASB are primarily driven by one-off projects and mental-health related interventions.

Lastly, we consider the impact of larger projects. Due to greater scrutiny in the application process and a more detailed review of the anticipated bene-

fits of these projects, we are no longer able to leverage variation in treatment timing. Hence, we use a synthetic difference-in-difference (SDID) approach where areas that do not receive a large project in our sample period are used as controls. Considering the previous evidence on the dose response to small projects, it is perhaps surprising that we find no evidence that these larger investments reduce crime, and on the contrary increase property crime (+4.1%) and violent crime (+4.6%) significantly. One possible explanation of these unexpected results is the aforementioned concentration effect: many larger projects are capital investments that bring more youths into contact with one another leading to more opportunities for potentially-violent encounters, as well as proving new targets for criminal activity.

By providing the first causal estimates of community-led projects, we advance the understanding of the efficacy of place-based crime prevention policies (Neumark and Simpson, 2015). We focus especially on small-scale projects, for which little evidence exists. Nichols (2010) suggests that many such programmes form an integral part of national crime rate reduction strategies, but empirical evidence on their effectiveness, particularly among youth, has been mixed. Our work complements the evidence of Faggio (2022) on the effectiveness of Business Improvement District (BID), a voluntary tax paid by local businesses in England and Wales to pay for neighbourhood improvements, on crime, who reports that the introduction of a BID reduces crime by 10%, similar effects are found for the US (Cook and MacDonald, 2011). The closest work to ours is Villa (2024), who estimate the effect of youth-specific community programs in London. While youth centres do not specifically target reducing criminal activities, they do so by incapacitating juveniles. Using plausibly exogenous reduction in public funding driven by austerity measures, they estimate that young people became 15% more likely to be engaged in criminal activities after the closure of youth centres. Our estimates are in line with these findings that small community projects targeting young people can reduce local crime.

The rest of the paper is structured as follows. In Section 2, we describe our data and provide some background information about what funded projects are, how they are organised as well as the application process. Section 3 introduces our identification strategy, and discusses our baseline difference-in-difference approach for small projects, and synthetic difference-in-difference approach for large projects. We describe tests for the validity of our design. Sections 4 and 5 discusses our results for small and large projects, respectively, and include several robustness checks. Section 6 concludes and provides a discussion of the findings.

2 The National Lottery Community Fund

The National Lottery is one of the largest funders of community projects and charitable causes in the UK. Since its launch in 1994, the franchise has invested £45.5 billion in funding to over 672,000 projects country-wide.[3] According to the franchise contract with the UK government, 25% of lottery revenues have to fund 'good causes', administered by the public National Lottery Distribution Fund.[4] Currently, 40% of this money funds health, education, environment and charitable causes, 20% sports projects and investments, 20% goes to the arts and 20% to heritage preservation.
We focus on the *National Lottery Community Fund*, which receives about 40% of these funds, and *Sports England*, the main distributor of money to fund sports (herein, NLCF/SE).[5] They act as funding bodies to charitable organisations, clubs and societies aiming to improve their local communities across England and Wales.

Charitable organisations apply directly to the NLCF/SE for a grant to support a local project.[6] NLCF/SE offers two separate funding streams, i) small projects worth up to £10,000, or ii) larger projects, where most funding programmes are worth up to £5m. Importantly, both the application process and regulations are different for large and small projects. In particular, smaller projects have a fixed duration of 24 months, and are not anticipated to have long-run effects. Larger projects, in contrast, may be active for 5-10 years. Accordingly, this funding is typically obtained by larger charities and voluntary organisations, while the more common smaller funding is most suited for clubs, schools and charities that pursue flexible and short-term opportunities. Applications need to be made at least 16 weeks before the anticipated start date, and are evaluated by a regional funding officer, who usually oversees one or more local authority districts in a given region. Overall, approximately 48% of applications are successful. The total funding and number of projects awarded over the period 2005 to 2019 are illustrated in Figure 1. After an initial peak when the trust first became active, the number of

[3]For more information see the latest annual report for 2023-24.

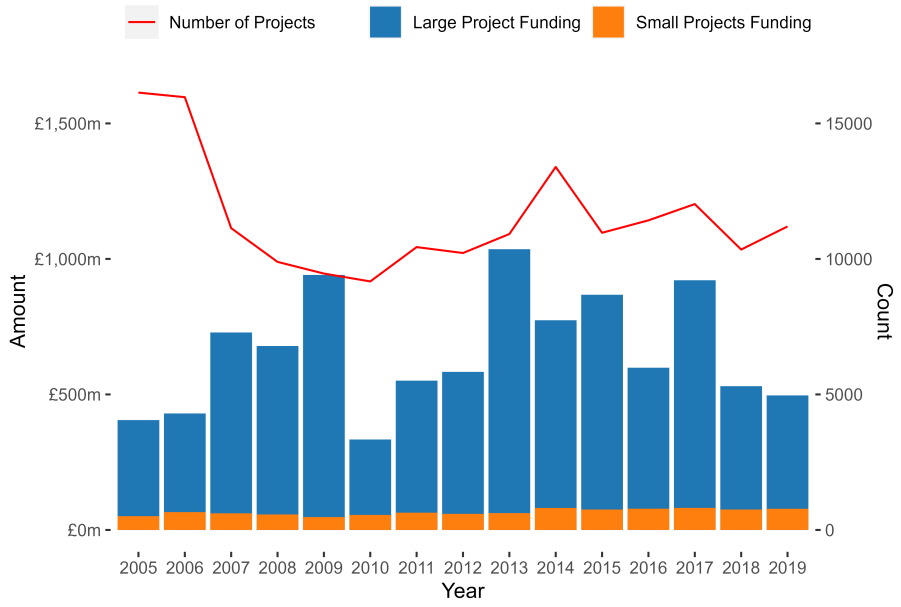
[4]The fund operates through 12 independent distributors that differ in their objectives or geographic coverage, such as the National Lottery Heritage fund which protects and restores UK heritage sites, Sports Northern Ireland which supports sporting activities around Northern Ireland, the British Film Institute, and others.

[5]A few smaller 'sub-distributors' such as Big Local Trust and Power to Change Trust receive funds by the NLCF/SE and subsequently fund projects in their area. To avoid double accounting, we exclude the direct funding to these sub-distributors and use project data provided by the sub-distributors instead.

[6]For more information on eligibility rules for the funds, see the 'Who can apply?' page.

funded projects stabilised at around 10,000 per year, at a value of just over £500 million in 2019. This breaks down into 80-90%, of small projects that are worth £10,000 or less. Yet, the small number of large projects account for a sizeable part of the overall budget, with a median value of £110,212.

Figure 1: Annual Total Funding and Project Counts (2005-2019)



Notes: Year is based on the start-date of project (not award date). For multi-year projects, the funds and number of projects is only reported for the first year.

The funded projects vary significantly by type, size and objective. They have a common emphasis on ameliorative investments aiming to "bring people together", or to "improve the places and spaces that matter to communities". Typically, projects are oriented towards deepening ties among the local community, supporting group activities, and improving the well-being of young people in the local area. Funds are also often used to organise sporting events and competitions, or to purchase equipment for future use.
The following two examples may help illustrate the difference. In April 2019, the community in Wilmslow were granted £10,000 for 12 months to "organise a Redeeming Our Community (ROC) Cafe for young people between the ages of 11-18. The aim of the project was to provide opportunities through various youth club activities for vulnerable teenagers, to make friends, socialise, and find necessary mental health support." The funding covered the operating costs of the cafe for 12 months. Meanwhile, in January 2017, Blackpool

Council were awarded £10,379,790 for a 5-year project under the "HeadStart" funding programme, that aims to "explore and test new ways to improve the mental health and wellbeing of young people aged 10 to 16 and prevent serious mental health issues from developing". Both projects offer opportunities for a cohort of youth to engage in meaningful activities and improve their mental health; and while both have "incapacitation effects", we conjecture that due to its scale the latter has more potential to reduce crime in the long-run.

3 Data

3.1 Funded Projects

Detailed information on funded projects was extracted from several sources (for a list, see Table A.3 in the Data Appendix) corresponding to important (sub-)distributors of National Lottery funds: the National Lottery Community Fund, Sports England, Power to Change Trust and the Big Local Trust. We restrict the extraction to projects granted during the period 2001 to 2019 to exclude the COVID-19 pandemic period, when mobility restrictions affected crime, and focus on England and Wales.[7] At the time of extraction, the complete sample of projects contained 292,717 records. Each record contained the name of the recipient organisation, the funding amount, the start date of the project, a project description and some geographic information regarding the location of the project.[8] To reduce noise and measurement error, we exclude projects that are worth less than £1,000. This yields a sample of 180,915 records.

Since our focus lies primarily with youth-related projects, we exclude projects that specifically target other demographic groups such as very young children and the elderly. We classify projects into youth and non-youth projects using chatGPT's large language model which we run based on the project descriptions. "Youth" projects corresponding are either targeted at young people directly, or benefit the wider community including young people. "Non-youth" projects subsume all other projects that either have no specific location or are not relevant to young people. For more information on the prompt used for this classification and a set of summary statistics see Appendix A. Approxi-

[7] Any data on projects prior to 2001 is available.

[8] For some records drawn from the National Lottery Community Fund, the location of the project is stated inaccurately using the wrong name or identifier code. For a description of the process of identifying the correct location of each ward, see the details in Appendix A.

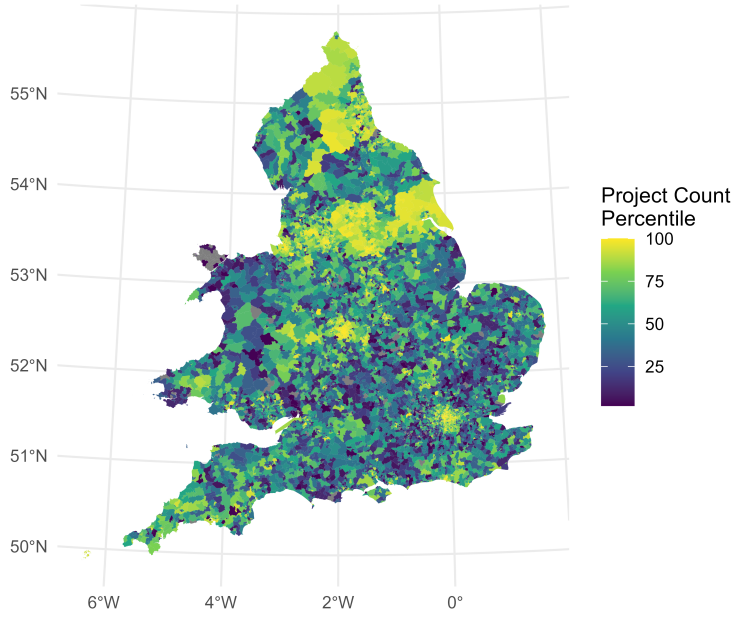
mately 63% of all projects fall into the "Youth" category and are preserved in the sample for analysis, leaving a final sample of 113, 248 projects.

We separately identify the effect of small and large projects in our analysis due to their differences in funding amounts, duration and typical focus. In our sample, approximately 79% of all projects are small. The median value of a small project is £8,000 with little variation in the funded amount. For large projects, the median amount is with a median funding of £108,000 more than 10 times larger, and has a large variation (the standard deviation is above one million pounds).

Next, we geocode project locations to (electoral) wards akin to neighbourhoods.[9] We will refer to ward and neighbourhood interchangeably. There is considerable variation in the quantity of awarded funded projects across neighbourhoods, as illustrated in Figure 2. Indeed, neighbourhoods at the 10th percentile receive approximately 9 times fewer projects that those at the 90th.

[9]An electoral ward is the primary unit of English electoral geography, with each ward representing about 5,500 electors, but ward population counts and size can vary substantially. In 2011, there were 8,546 wards in England and Wales. Periodically, boundary adjustments are made to keep ward sizes relatively constant. For consistency, we define all wards based on their 2011 borders using the 2011 Census Merged Wards (CMWD).

Figure 2: Heat Map of Percentiles of Number of Projects



Notes: Values are reported as the average rate per 1000 residents of each ward.

Projects differ not only by size, but also by their content and objectives. To take this into account, we perform two distinct classifications based on the descriptions available alongside each record. This classification is performed using ChatGPT 4.5.[10] The first of these, groups projects into four categories: capital, repeated, one-off and other. "Capital" projects are those primarily concerned with improvements to infrastructure, equipment purchases and amelioration of existing facilities (sports or otherwise). "Repeated" projects are those where an activity is repeated several times over a project's period of activity such as through a series of lectures/workshops. "One-off" projects instead operate for a short period of time within the activity window through a sport's day or field trip. Lastly, "Other" projects are the rest of the projects that could not be placed into any other of the categories. By differentiating between projects that bring youth together for different periods of time, we hope to disentangle the contributions of the "incapacitation effect" and "concentration effect" to the overall impact on crime.

The second classification focuses on the content more directly grouping projects into: sports, mental health, employment/education and other. "Sports"

[10]For more details on the prompt used and the distribution of projects by type see Appendix A.

projects are all those that organise or encourage greater participation in sporting activities either through sports days, equipment purchases or improvements to sports facilities. "Mental health" projects aim to improve the socio-emotional well-being of locals and young people by offering counselling, helping lonely and isolated individuals or organising socials. "Employment/Education" projects instead are the projects helping youths and others to find employment or provide new educational opportunities. As before, "Other" projects are the rest of the projects that could not be placed into any other of the categories. With this classification, we seek to pinpoint which types of projects are most effective at reducing crime. A complete set of summary statistics across both size and type is given in Table 1 below.

Table 1: Descriptive Statistics by Project Group

Group	N	Median (£)	SD (£)
Overall	113248	9590.0	494121.5
Small	89547	8000.0	2880.4
Large	23701	107654.0	1050025.5
Capital	37951	9525.0	382580.6
One-Off	14394	7432.0	233368.2
Repeated	39971	9725.0	637244.9
Other (Type 1)	20932	9808.0	490887.4
Sports	50984	9597.5	567933.3
Mental Health	13923	9928.0	323327.9
Employment	20305	9535.0	253761.5
Other (Type 2)	28036	9062.5	547419.9

Notes: A table of descriptive summary statistics across the different types of projects. Recall that projects worth over £10,000 are classified as large, while those worth £10,000 or below are small. The breakdown of the two classifications of projects are also included. All monetary values are given two one decimal place.

3.1.1 Local Crime

The main data source of recorded crime is the police crime records, available at `https://data.police.uk`, compiled for the 43 Police Force Areas (PFAs) in England Wales.[11] The raw data contains the monthly counts for 15 differ-

[11]Data from the British Transport Police has been excluded due to potential measurement error associated with the location of the reported crimes.

ent types of crime over the period 2011-2019. We restrict our sample to the end of 2019 as the subsequent COVID-19 pandemic generated a structural break in crime patterns. Though crime is recorded at the street-level, to preserve anonymity, the publicly available data is recorded at the LSOA-level, a slightly coarser geography. Crime reports are available for 35,750 out of the 35,753 LSOAs that are active during this period.[12] The LSOA-level information is aggregated to the electoral ward-level using 2011 ward code identifiers in a way that is consistent with those used for the funded projects.[13]

To reduce measurement error, the raw data are aggregated into 6 main groups.[14] Hence, 'total crime' is simply the sum across all 15 crime types; 'total crime without ASB' is the sum of all crime excluding anti-social behaviour; 'property crime' includes burglary, shoplifting, vehicle crime, bike theft, persons theft and other theft; 'violent crime' includes violent & sexual offences, robbery, arson, weapons, public order and disorder incidents, while 'substance crime' is kept unchanged. Moreover, due to changes in Home Office Counting Rules that took effect in April 2019 for crimes relating to stalking and harassment, some of the broader categories are not comparable over time. Hence, we discard all observations for 'total crime', 'total crime without ASB' and 'violent crime' from that date. Similarly, due to a large jump in the number of recorded violent crimes, observations for the first eight months of 2011 are also excluded. Table 2 provides summary statistics for these crime categories expressed per 1,000 residents per month at the ward-level.[15]

The average crime rate is roughly 7 instances per month per 1000 residents. However, this hides large variation between wards whereby neighbourhoods at the 90th percentile see 5.6 times more crime than those at the 10th percentile. The largest crime categories are anti-social behaviour and violent crimes, representing 33% and 30% of all crimes respectively. We focus primarily on anti-social behaviour, which is largely a youth crime. For example, out of all Anti-Social Behaviour Orders issued between 1999 and 2013, more

[12]The remaining three LSOAs have almost no reported crime. Moreover, data is not available for all LSOAs that fall into the Manchester PFA after July 2019 due to misreporting.

[13]See Appendix A for more details about the aggregation process and linking procedures.

[14]For a summary of the disaggregated crime types see the relevant section in Appendix A. Note also that collectively these groups are not exhaustive of all crime offences. A category for ' all other crimes' has been left as it is difficult to interpret and is an amalgamation of many heterogeneous types of offences.

[15]Population data is taken from the 2011 UK Census available in the NOMIS data repository.

Table 2: Crime Summary Statistics (2011-2019)

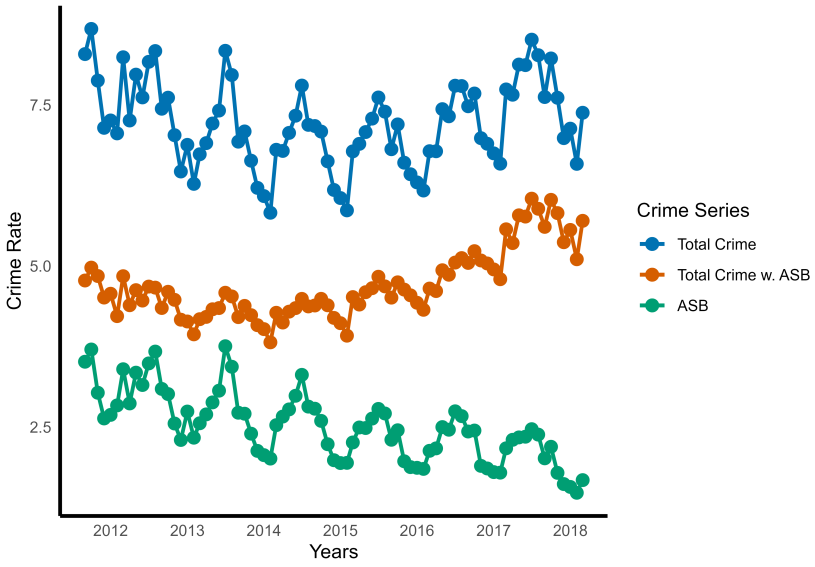
Statistic	N	Mean	St. Dev.	Min	Max
Total Crime	674,265	7.21	7.67	0.00	509.66
Total Crime (exc. ASB)	674,265	4.70	5.70	0.00	409.66
Anti-social Behaviour	920,490	2.45	2.64	0.00	100.00
Violent Crime	674,265	2.18	2.28	0.00	151.24
Property Crime	920,490	2.13	3.34	0.00	293.75
Substance Crime	920,490	0.18	0.50	0.00	121.33

Notes: A table of summary statistics for the broad types of crime used in the analysis for the period 2011-2019. Note that the number of observations differs for some types due to changes in the Home Office counting rules. For more information see the discussion in the Data Appendix. All values of crime are given at the monthly-level and per 1000 residents.

than 1/3rd were given to under-18 highlighting that this is predominantly a young person's crime.[16] In Figure 3, we report the trends by type of crime. Anti-social behaviour is highly seasonal and has been declining over the sampled period. This is in contrast to all remaining crimes, which has been rising over the period 2011 to 2019.

[16]From 1998 to 2014, anti-social behaviour was punishable with Anti-Social Behaviour Order using civil orders which restricted behaviour or access to locations. From October 2014, ASBOs were replaced with Community Protection Notice, a civil injunction, and Criminal Behaviour Orders, which are issued by magistrate or youth courts. For more details, see Government's the Statistical Bulletin on crime.

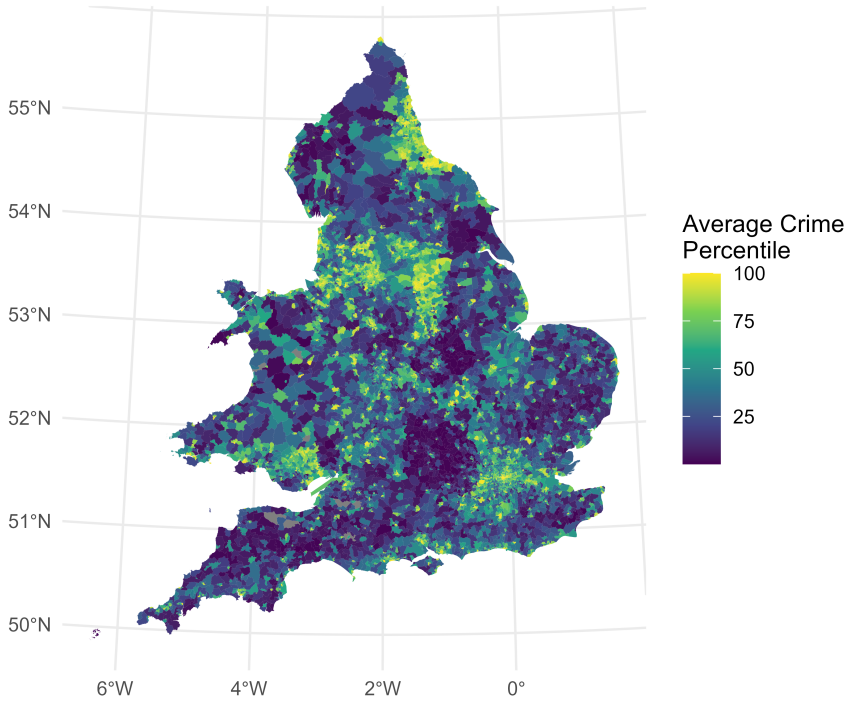
Figure 3: Crime Rate Time Series (2011-2019)



Notes: Values are reported as the average rate per 1000 residents of each ward.

Spatially, as illustrated in Figure 3.1.1 like with the projects, anti-social behaviour is concentrated in urban areas with most neighbours around England and Wales seeing very low rates of ASB (and other crimes). That being said, this inequality is not quite as large as the variation in the number of small projects. For ASB, on average, the 90th percentile neighbourhood sees only 5.6 times more incidents (per 1000 residents) than one at the 10th percentile.

Figure 4: Location of Crime (2011-2019)



Notes: Percentiles are calculated based on the average crime rate for each ward and the total number of projects started over the years 2011-2019. Brighter colours represent a higher percentile.

3.1.2 Control Variables

We also collected a set of time-invariant control variables from the 2011 Census, available at the NOMIS repository, to characterise the social and economic characteristics of wards at the beginning of the period of our study. These variables account for the wealth, education and poverty-levels of local areas, as well as their respective degrees of urbanisation.[17] Since Giulietti and McConnell (2020a) estimates that areas subject to deeper austerity cuts subsequently experienced a greater increase in crime, especially violent one, we also include a measure of the size of local austerity cuts, taken from Beatty and Fothergill (2013).[18] Summary statistics on these control variables are provided in Table 3. Since these variables are fixed over time, they will only be used in the analysis of heterogeneity of the effects by wards initial

[17]Table A.6 in the Data Appendix provides a brief explanation of each variable.

[18]These represent the simulated per person losses (GDP) from the 2012-13 cuts to welfare measured at the local authority level.

characteristics.

Table 3: Summary Statistics for Control Variables

Statistic	N	Median	St. Dev.
Population	8,546	5,565	4,961.80
Youth Share	8,546	0.27	0.06
Population Density	8,546	13.30	27.96
L2 Education Share	8,546	0.52	0.10
LR Unemployed Share	8,546	0.34	0.13
Social Rent Share	8,546	0.11	0.11
Urban/Rural	8,546	1	0.47
Median Income	8,377	481.00	72.71
Austerity Loss	8,365	434	120.92

Notes: A table of summary statistics for the control variables used for heterogeneity analysis. The variables are taken from the results of the 2011 Census (or the ONS) which are available at the NOMIS repository. Note that median income here refers to median weekly wages which are given at the constituency-level, while austerity loss are per resident monetary losses from austerity cuts as calculated in Beatty and Fothergill (2013) and are given at the local authority district level. For more details on each variable, see the descriptions in the Data Appendix.

4 Empirical Strategy

To help explain our approach to identifying the effect of projects on local crime, consider first this naive two-way fixed effects (TWFE) model in equation 1:

$$y_{it} = \eta_i + \theta_t + \beta_1 D_{it}^S + \beta_2 D_{it}^L + \epsilon_{it} \tag{1}$$

where y_{it} is monthly crime per 1000 persons in ward i at time t, η_i and θ_t are the fixed effects for ward and time, respectively, D_{it}^S and D_{it}^L are treatment dummy variables denoting the receipt of a small or a large project respectively, and ϵ_{it} is the error term.

For large projects received during our sample period, we assume that once a project is received, the neighbourhood remains treated irreversibly. Formally, we assume that large projects have a permanent effect on the ward, and define the treatment as $D_{it}^{L} = 1$ from the start date of the earliest (in the period 2011-19), large project allocated to a ward i. However, following National Lottery Funds regulations, we will assume that small projects last at most 24 months, and define the treatment period (i.e. $D_{it}^{S} = 1$) accordingly. Hence, assuming that no other project begins in the neighbourhood, after the treatment period has elapsed, the ward returns to its untreated state. Mote that this implies that there could be more than one project operating concurrently in a ward at a time. Hence, D_{it}^{S} will have two interpretations: (i) a dummy variable denoting whether there is at least one small project active and (ii) a set of dummy variables corresponding to different quantities of funds being actively used in the neighbourhood. We define the following categories: less than £999, £1000-£10,000, £10,0001-£20,000, £20,001-£30,000, and more than £30,000, which are roughly equivalent to the number of projects operating, since the median funding for a project is close to £10,0000.[19]

There are several issues with this naive approach. Firstly, recent work by Goodman-Bacon (2021), Sun and Abraham (2021), Borusyak et al. (2022) and others has shown that under staggered treatment timing and heterogeneous treatment effects, the TWFE approach fails to recover the average treatment effect on the treated (ATT). The reason for this is that the OLS estimator leverages "forbidden comparisons" between 'already treated' and 'not yet treated' units. Secondly, in designs with multiple treatments, the estimated coefficients will be cross-contaminated by the two treatment effects. Intuitively, if the two treatments are not mean-independent and treatment effects are heterogeneous it becomes difficult to disentangle the contributions of each treatment to the evolution of the outcome.[20]To address these concerns, we estimate the treatment effects of each type of project separately using sample restrictions to mitigate against contamination problems. The details of our identification strategy are discussed more formally below.

[19]We show that the estimates are not significantly affected if the number of projects is used instead. For the full set of results of this, see Table B.46.

[20]In our case, the number of large and small projects a ward receives is indeed positively correlated (see Figure B.1 in Appendix B).

4.1 Small Projects

To avoid the aforementioned contamination bias from larger projects, we make two sample restrictions: 1) we exclude all periods after a ward receives a large project in the 2011-2019 period, and 2) we exclude all wards that receive a large project within 24 months before the start of our sample (i.e. in the 2009-2011 period). The restrictions address both contemporaneous cases of contamination, as well as those that arise due to the effect of previous large projects that occurred outside our sample period (i.e before 2011). Implicitly, the second restriction is based on the assumption that the dynamic treatment effects of large projects stabilise within two years. The results are not sensitive to this second assumption.[21]

In practice, we impose that crime rates follows the process in equation 2. Note that the variables have the same interpretation as above.

$$y_{it} = \eta_i + \theta_t + \beta_{it} D_{it}^S + \epsilon_{it} \tag{2}$$

Notice that a typical parallel trends assumption is embedded as a combination of the functional form of equation 2 and the exogeneity of the error term. Formally, we assume that $E[\epsilon_{it}|D_{it}^S, \eta_i, \theta_t] = 0$. This implies that the trends in untreated potential outcomes are parallel for all wards. Compared to the naive approach, D_{it}^L has dropped out from the equation as we are working with a sample unaffected by large projects.

Since projects often differ in their objective and may have an effect at different times throughout their 24 months of activity, we allow for treatment effects to be heterogeneous across both unit and time dimensions. As noted above, in this context, OLS cannot be used to estimate equation 2. Fortunately, a large number of heterogeneity-robust estimators have been proposed.[22] Due its flexibility in handling different types of treatment variables, we use the two-stage difference-in-differences approach proposed by Gardner (2022). This works in two steps. In the first stage, the time and unit fixed effects are recovered using the sample of untreated observations. It is this step that relies on the parallel trends assumption which implies that these untreated observations can be used for the imputation. The dependent variables are then "adjusted" by removing the time and unit fixed effects. In

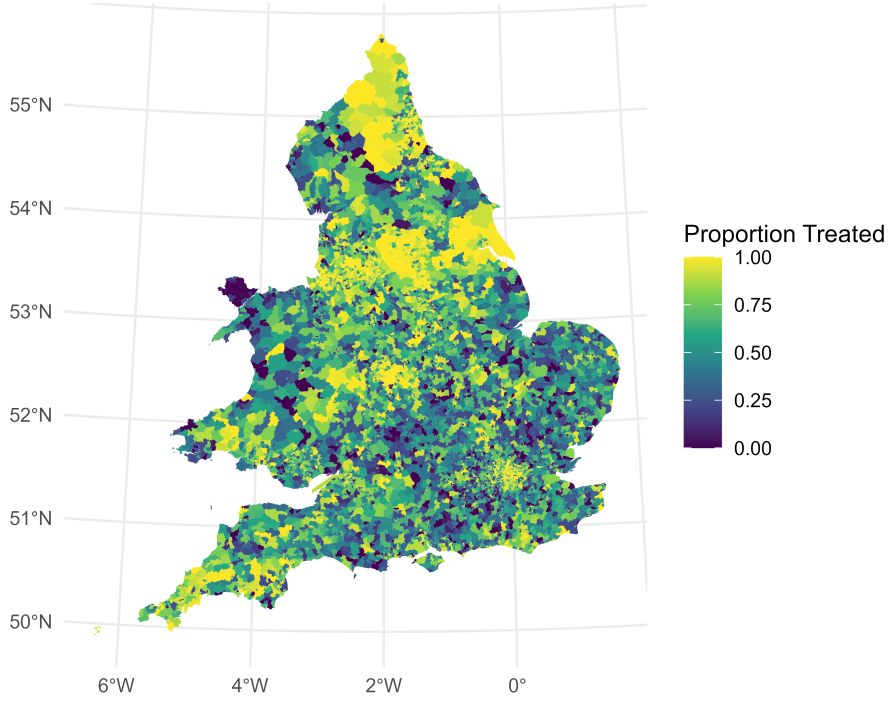
[21] The results are largely unaffected by either removing the second restriction altogether, or increasing it to five years, see Tables B.47 and B.48 in Appendix B.

[22] See the suggestions in Borusyak et al. (2021); Callaway and Sant'Anna (2021); De Chaisemartin and d'Haultfoeuille (2023); De Chaisemartin and D'Haultfœuille (2020); Dube et al. (2023); Roth et al. (2023); Wooldridge (2021).

the second, "adjusted" outcomes are regressed against a treatment variable. This yields numerically identical estimates to those of the "imputation" estimator by Borusyak et al. (2021), though differing standard errors.

Intuitively, this estimator relies on exogenous variation in the projects' start times; i.e. we compare treated ward with those where no projects are active. Over our sample period, on average, neighbourhoods have at least one project active 57% of the time though with considerable geographic heterogeneity. This variation is well-illustrated in Figure 4.1 which shows differences between wards in the proportion of time that these neighbourhoods remain "treated". For identification, conditional on time and ward fixed effects, we assume that the start times of these projects are orthogonal to trends in potential crime outcomes, as they are initiated due to chance events. For example, in a sports club, damage to old equipment could lead to an application. In such a case, the timing of the decision to apply and the start and end dates of the project itself would be unrelated to the crime rate. Treatment effects of individual projects can then be estimated in a difference-in-difference design with staggered adoption which compares treated wards to those that have either not yet received a project or for whom the dynamic effect of a previous treatment has vanished, i.e. they were last treated more than 24 months ago.

Figure 5: Proportion with an Active Project (2011-2019)



Notes: Proportions based on the number of periods a ward has at least one active project over the 2011-2019 period. Brighter colours represent a higher proportion.

This approach relies on assumptions on unobservable counterfactual trends which are not directly testable. However, one can provide some evidence in favour of its plausibility. In particular, one can leverage the periods immediately before the start of any given small project and verify whether pre-treatment trends in crime rates are indeed parallel between wards. In our case, this is a particularly powerful test since the repeat treatment means that we can use pre-treatment periods throughout our sample period. Formally, we follow Borusyak et al. (2021) in validating our design by performing a pre-trend test on the sample of (yet) untreated observations. To do so, we estimate the following regression by OLS:

$$y_{it} = \theta_t + \eta_i + \sum_{p=-P}^{-1} \gamma_p 1\{t = \tau_{ij} + p\} + \epsilon_{it} \tag{3}$$

where p denotes the number of months before a unit receives treatment in relative time, τ_{ij} is a treatment date.
The estimation uses all available periods before the earliest time captured by

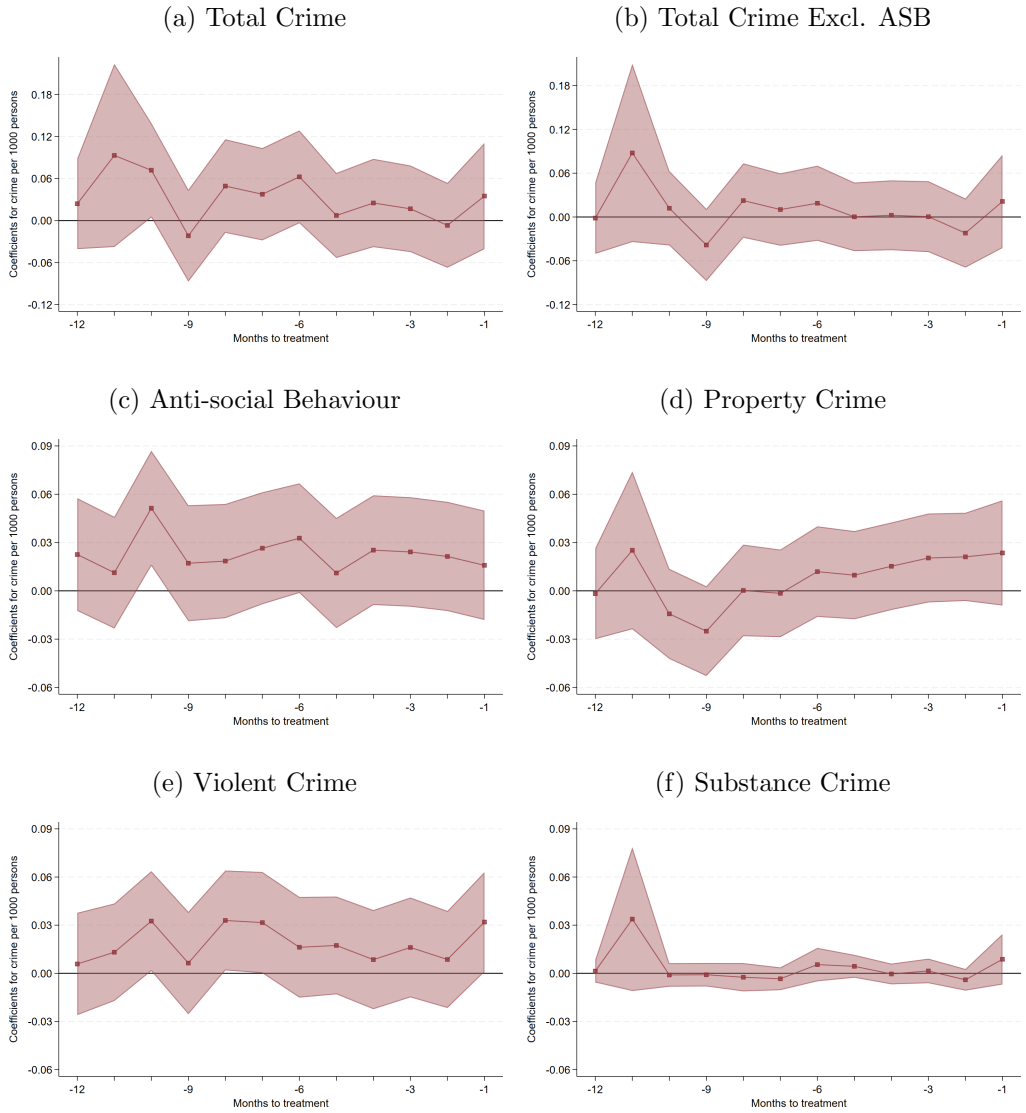
the model (12 months) as the reference group. We conduct a conventional F-test to test the joint significance of all pre-treatment coefficients. The advantage of this approach is that it is external to our choice of estimator, and hence avoids introducing correlations between our estimates and the pre-trend coefficients that, as argued in Roth (2022), can bias results. The results of this exercise are given in Figures 4.1, each panel illustrating for a type of crime the set of pre-treatment coefficients for the previous 12 months. The associated F-statistics for each regression are given in Table B.1 in Appendix B, where the test fails to reject the null hypothesis that all coefficients are jointly zero for all crimes. This supports they assumption that prior to receiving treatment, treated and control wards experienced similar trends in crime.

Having established some support for our preferred identification strategy, we now lay out our planned analysis for small projects. To start with, we consider the causal effect at the extensive margin: does at least one small project reduce crime? Next, since multiple projects, and hence monetary amounts, can be active in a neighbourhood at a time, a natural extension to this question is to ask whether increasing the "dose" produces a greater effect, the intensive margin. To do this rather than just using an indicator of treatment, we simply modify the second-stage regression, and add a set of dummy variables that model the quantity of funding in use in a local area at a given time. This allows us to non-parametrically identify the effect of having different numbers of projects active in a ward relative to having none at all. It must be noted that without making additional parallel trends assumptions on 'dosage', we cannot identify the marginal effect on the crime rate of increasing funding from say £20,000 to £30,000, but only the difference between these dosages and having no projects active at all Callaway et al. (2024). In both cases, we perform heterogeneity analysis by urban versus rural status and several other socio-economic indicators, as well as by comparing the impact of projects across neighbourhoods at different quantiles of the crime distribution. Furthermore, we use the classification on projects above to determine which types of projects are most effective at reducing crime.

Moreover, we assess whether neighbourhoods significantly affected by the UK Welfare Reform Act 2012 and subsequent austerity policies respond differently compared to less affected communities. To do so , we estimate equation 4:

$$y_{it} = \eta_i + \theta_t + \beta_{it} s_{it} + \gamma_{it} \text{Aust}_{it} + \tau_{it} (\text{Aust}_{it} \times s_{it}) + \epsilon_{it} \tag{4}$$

Figure 6: Pre-Trend Plots by Type of Crime



Notes: Event study plots estimated using the testing approach suggested in Borusyak et al. (2021). The dependent variable is reported by type of crime per 1,000 residents. A 95% confidence interval based on standard errors clustered at the ward-level is included around the estimates.

where the variables have the same interpretation as above, but s_{it} is the total sum of funds, expressed in £10,000s, active in a neighbourhood at a given time, while Aust_{it}, following Giulietti and McConnell (2020b), is a treatment indicator for whether a ward has highly affected by austerity cuts coming into effect post-2013. Cross-sectional variation comes from differential exposure to austerity cuts at the local authority district-level as calculated by Beatty and Fothergill (2013). In particular, we split local authority by whether these costs were above or below the median. The policy comes into force in April 2013. As above estimation proceeds using the two-stage difference-in-differences approach. Importantly, our comparison group is the set of ward-period combination that neither have an active project nor are highly affected by austerity. We then calculate the marginal effects for each state of austerity illustrating the magnitude of the interaction.

Finally, we conduct a battery of robustness checks. First, we assess whether local interventions reduce crime or simply displace it to other neighbourhoods. Hence, we test for the presence of spatial spillover effects by including dummy variables in our second stage regression for whether a neighbouring ward is in receipt of a small or large project.[23] Second, we evaluate the sensitivity of the main results to changes in the sample restrictions (removing wards that received large projects in the 60 months prior to the start of our observation period, or the removal of this restriction altogether), the defined period of 'active' treatment (18 or 30 months) or the type of projects considered (all projects type).

4.2 Large Projects

The effect of large projects (those worth more than £10,000) is more difficult to identify as they can overlap with smaller projects and there are no prolonged periods of time when a large project occurs and no small projects occur). Moreover, given the size of each project, its prospects and predicted impact on local communities are subject to greater scrutiny. For this reason, it is unlikely that the timing of such interventions would be completely exogenous to trends in crime. This means that the former estimation approach would be biased. To see this consider Table B.2 in Appendix B where for all types of the crime (except violent and substance), F-test rejects the null

[23]Note that this is only a test of whether the presence of a treatment effect in a given ward can be explained by the treatment status of a neighbouring ward. A major shortcoming of our approach is that we cannot model whether there is spatial contamination of control wards. This is because there are few periods when both a given ward and its neighbouring ward are untreated at the same time.

hypothesis that all pre-trend coefficients are zero. Therefore, instead, we pursue a synthetic difference-in-difference (SDID) approach, which combines the advantages of the synthetic control and difference-in-differences designs, allowing for failures of the standard parallel trends assumption. Moreover, by "matching" on pre-treatment outcomes the SDID compares similar wards similarly exposed to small projects allowing us to mitigate concerns that estimated effects are driven by the smaller interventions.

Formally, this is done by solving the minimisation problem in equation 5 for each large project adoption/start date a, then aggregating the result (Arkhangelsky et al., 2021; Clarke et al., 2023).[24]

$$\left(\hat{\tau}_a, \hat{\mu}, \hat{\theta}, \hat{\eta}\right) = \arg\min_{\tau_a, \mu, \theta, \eta} \left\{ \sum_{i=1}^{n} \sum_{t=1}^{T} (y_{it} - \mu - \theta_i - \eta_t - \tau_a D_{it}^L)^2 \hat{\omega}_i \hat{\lambda}_t \right\} \tag{5}$$

where τ_a is the average treatment effect of a large project started at date a, μ is the common intercept, θ_i and η_t are the ward and time fixed effects, D_{it}^L is the treatment variable that takes the value unity after a large project first begins in ward i, $\hat{\omega}_i$ and $\hat{\lambda}_t$ are the estimated ward and time weights respectively.[25] The results are aggregated into an overall estimate:

$$\hat{\tau} = \sum_{a} \frac{T_{\text{post}}^a}{T_{\text{post}}} \times \hat{\tau}_a \tag{6}$$

where $\hat{\tau}$ is the overall average treatment effect on the treated (ATT), a is a given adoption date (as above), T_{post} is the total number of post-treatment periods observed in treated units, and $\hat{\tau}_a$ is the estimated ATT for a given adoption date. To perform inference on these estimates, we use "placebo" standard errors that are constructed by assigning placebo treatments to control units. We do this because this inference procedure is preferred when, for some adoption dates, the number of treated units is small (Clarke et al., 2023).

In addition, we make several additional modifications of our sample. Firstly, as SDID is sensitive to random fluctuations in the data, we smooth the crime data using a 3-month moving average process. [26] Secondly, the estimation requires a strongly balanced sample of treatment and control observations

[24]Note that $\hat{(.)}$ denotes an estimated parameter.

[25]To derive these weights, we use the standard recommended procedure suggested in Arkhangelsky et al. (2021).

[26]As a robustness check, we show that this adjustment would not significantly alter the

with no missing values. As before, we stick to our sample restriction that only wards that did not receive a large project at least 24 months before the start of our sample are admissible. 'Control' wards are those that do not receive a large project throughout our sample period; i.e. never treated. We limit 'Treated' wards to wards that are observed for 12 months before and 24 months after the earliest large project received in the sample period. This ensures that a balanced sample can be constructed from our set of observations. The approach outlined above can then be used to estimate the overall average treatment effect of receiving a large project on each crime type.

To perform heterogeneity analysis, we instead perform the SDID estimation on each large project individually yielding a set of ward-level treatment effects which can then be regressed against a set of ward-level characteristics similar to those evaluated for small projects as shown in equation 7. Due to the high computational cost of this procedure, it is not feasible to use the full sample of controls for these estimations. Instead, for each ward-level estimate, we use a random sample of 100 control units. To form an overall ATT estimate, the individual effects are aggregated using a weighted least squares (WLS) regression which places more weight on individual-level estimates with lower standard errors.

$$\hat{\tau}_i = \delta_1 \mathbf{x_i} + \varepsilon_i \tag{7}$$

where $\hat{\tau}_i$ are the ward-level estimated treatment effects, $\mathbf{x_i}$ is a vector of ward-level characteristics (and a constant), while ε_i is the error term.

Lastly, we conduct a number of robustness checks for this analysis of large projects. Firstly, we check to see if the effects of large projects spillover to neighbouring wards. In an initial test, we estimate a two-way fixed effects model for crime on small and large project neighbourhood treatment dummies using the sample of control units. This gives preliminary information on which spillovers to control for.[27] Next, we re-estimate the baseline SDID model with the large project neighbourhood spillover dummy as a covariate. Finally, we redefine treatment in our SDID model to be either the receipt of a large project or a neighbour receiving a large project. This derives a total effect which takes any spillovers into account.

interpretation of our main results. These estimates are given in Table B.54 in Appendix B.

[27] As noted in Clarke et al. (2023), this is also the method by which the estimates for 'projected' covariates are obtained in SDID estimation.

Secondly, as above, we evaluate the sensitivity of the main results to changes in the sample restrictions (removing wards that received large projects in the 60 months prior to the start of our observation period, or the removal of this restriction altogether), and the type of projects considered (all projects type).

5 Results for Small Projects

Consistent with the structure above, we explore the results for small and large projects separately. We also note that crime rate is defined as the number of reported incidents per 1000 residents per month.

5.1 Extensive Margin

We start by presenting the results for the effect of having at least one small project active in a local area (extensive margin) on all six types of crime. In doing so, we are comparing changes in the rate of crime in areas with at least one small project to those without one over the same time period. The two-stage difference-in-difference estimates are given in Table 4. Over the 24 month period of time in which the projects are considered active, we find no effect on total crime, total crime excl. anti-social behaviour and anti-social behaviour implying that, on their own, the micro-interventions do little to reduce criminal activity. Perhaps more surprisingly, there is a small yet significant positive effect on property crime which rises by 1.62% relative to the monthly median.[28]

[28]Herein, whenever coefficients are being interpreted, they will be stated as percentages relative to the monthly median of the respective crime rate.

Table 4: Extensive Margin of Small Projects

	(1) Total	(2) Total (excl. ASB)	(3) ASB	(4) Property	(5) Violent	(6) Substance
Small	-0.001	0.001	0.001	0.023***	-0.005	0.003*
	(0.018)	(0.014)	(0.011)	(0.008)	(0.009)	(0.002)
Median	5.200	3.310	1.739	1.419	1.470	0.000
Rural	3.546	2.301	1.070	1.011	0.912	0.000
Urban	6.470	4.059	2.226	1.682	1.855	0.000
Percentile	49.984	50.020	50.026	50.853	49.839	61.705
N	330059	330059	451897	451897	330059	451897

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level. The crime rates running from models (1) to (6) are total crime, total crime excluding anti-social behaviour (ASB), anti-social behaviour (ASB), property crime, violent crime and substance crime respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect. Standard errors are given in parentheses and are clustered at the ward-level.

We investigate the heterogeneity of these results by ward's initial characteristics and redo the analysis separately by whether the neighbourhood is classified as rural or urban, and subsequently whether it is below or above the median in population density, share of young people in local population, long-run unemployment, social housing, education, weekly wages and share of residents not born in the UK. A summary of these estimates is given in Table 5 below.

Table 5: Summary of Extensive Margin Heterogeneity Estimates

	(1) Total	(2) Total excl. ASB	(3) ASB	(4) Property	(5) Violent	(6) Substance
Baseline	-0.001	0.001	0.001	0.023**	-0.005	0.003
	(0.018)	(0.014)	(0.011)	(0.008)	(0.009)	(0.002)
Rural	-0.009	-0.011	0.004	0.025*	-0.013	0.002
	(0.024)	(0.019)	(0.013)	(0.010)	(0.012)	(0.002)
Urban	0.004	0.009	-0.002	0.021*	0.001	0.004
	(0.025)	(0.018)	(0.015)	(0.010)	(0.012)	(0.002)
Low Pop. Dens	-0.013	-0.017	0.013	0.016	-0.023*	0.002
	(0.021)	(0.017)	(0.013)	(0.010)	(0.011)	(0.002)
High Pop. Dens.	0.015	0.022	-0.007	0.027*	0.015	0.003
	(0.030)	(0.021)	(0.016)	(0.012)	(0.014)	(0.003)
High Educ.	0.017	0.005	0.009	0.019	-0.001	0.004
	(0.021)	(0.016)	(0.012)	(0.010)	(0.010)	(0.002)
Low Educ.	-0.021	-0.005	-0.006	0.026*	-0.011	0.002
	(0.029)	(0.021)	(0.016)	(0.011)	(0.015)	(0.002)
Low LR Unemp.	0.010	0.009	0.002	0.026*	-0.001	0.006*
	(0.022)	(0.017)	(0.013)	(0.011)	(0.010)	(0.002)
High LR Unemp	-0.015	-0.005	-0.001	0.020	-0.008	0.000
	(0.029)	(0.021)	(0.016)	(0.010)	(0.015)	(0.002)
Low Soc. Rent	-0.006	-0.002	0.004	0.015	-0.006	-0.001
	(0.022)	(0.016)	(0.012)	(0.009)	(0.010)	(0.002)
High Soc. Rent	-0.004	0.002	-0.001	0.029*	-0.004	0.006*
	(0.028)	(0.022)	(0.017)	(0.012)	(0.014)	(0.003)
Low Prop. Young	-0.012	-0.013	-0.001	0.026*	-0.011	0.002
	(0.025)	(0.018)	(0.015)	(0.011)	(0.011)	(0.002)
High Prop. Young	0.009	0.014	0.001	0.020	0.001	0.004
	(0.026)	(0.020)	(0.015)	(0.011)	(0.013)	(0.003)
Low Non-UK-Born	-0.035	-0.036	0.007	0.012	-0.027*	0.003
	(0.024)	(0.018)	(0.014)	(0.010)	(0.013)	(0.002)
High Non-Uk-Born	0.037	0.043*	-0.005	0.037**	0.017	0.003
	(0.027)	(0.020)	(0.016)	(0.012)	(0.012)	(0.003)
Low Wage	-0.028	-0.016	-0.006	0.013	-0.014	0.003
	(0.027)	(0.020)	(0.016)	(0.010)	(0.014)	(0.002)
High Wage	0.027	0.023	0.005	0.033**	0.005	0.004
	(0.024)	(0.018)	(0.014)	(0.011)	(0.011)	(0.003)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level split by various ward-level characteristics. The crime rates running from columns (1) to (6) are total crime, total crime excluding anti-social behaviour (ASB), anti-social behaviour (ASB), property crime, violent crime and substance crime respectively. Standard errors are given in parentheses and are clustered at the ward-level.

However, in each of these cases, we find few significant effects and a lack of heterogeneity across ward-level characteristics. For example, focusing on urban vs rural wards the estimated marginal effects on overall crime, crime excluding ASB and ASB are all not significant, and do not differ by degree of urbanisation. The important exception is property crime which increases by 1.5% in rural and 1.8% in urban areas (relative to the unconditional median). We similarly observe larger positive effects in areas of high population density (1.9%), high share of those living in social housing (2%), high share of those that were not born in the UK (2.6%), above median weekly wage (2.3%), and low levels of education (1.8%). The full results for these remaining heterogeneity tests are available in Tables B.3 to B.18 in the Online Appendix.

This suggests that, in general, simply receiving a project is not associated with any crime reduction and in some cases may lead to greater criminality. Since this is mainly driven by increases in property crime, particularly in urban neighbourhoods, it could be that projects in these areas, especially those involving capital investments of equipment purchases, provide existing thieves with new targets and opportunities to offend. To test this, we check whether receiving at least one project of a particular type has a greater impact on the crime rate. These estimates are given in Tables B.19 and B.20 below.

In this case, we find that property crime is indeed partly driven by 'other' (2%), 'capital' (1.6%) and to a lesser extent 'repeated' (1.4%) projects. Although little can be said about the first category, the last two appear to support the contention that the rise in property crime is driven by the new targets provided by equipment purchases or facility improvements. However, turning to the second classification, we find that the positive effect is largely dominated by 'mental health' projects (5.2%) where the magnitude of the effect is just over 3 times the size of the effect of 'capital' projects. Since these projects are primarily about providing new opportunities for socialisation amongst those already at risk, this could be evidence of a "concentration effect" caused by a higher number of interactions amongst crime-prone individuals. This pattern is observed again with total crime (2.6%) with the primary driver being violent crime that increases by 6.1%.

On a more optimistic note, we find that 'one-off' interventions significantly reduce overall crime (-1.7%). This is driven by a large fall in violent crime (-4.8%). This appears to be driven by a strong "incapacitation effect" which keeps youths busy for a short-time while the project is active. This interpre-

tation is supported by the finding that 'sports' projects reduce violent crime by 2.3%.

Table 6: Extensive Margin - Project Type 1

	(1) Total	(2) Total (excl. ASB)	(3) ASB	(4) Property	(5) Violent	(6) Substance
Capital	0.015	0.005	0.019	0.023**	-0.000	0.003
	(0.025)	(0.019)	(0.015)	(0.010)	(0.013)	(0.003)
One-Off	-0.086**	-0.084**	-0.030	0.012	-0.070***	0.003
	(0.042)	(0.034)	(0.028)	(0.021)	(0.024)	(0.005)
Repeated	0.006	0.002	-0.001	0.020*	0.001	-0.000
	(0.026)	(0.020)	(0.015)	(0.011)	(0.013)	(0.002)
Other	0.034	0.056**	-0.015	0.028**	0.022	0.002
	(0.032)	(0.024)	(0.019)	(0.013)	(0.016)	(0.003)
Median	5.200	3.310	1.739	1.419	1.470	0.000
Rural	3.546	2.301	1.070	1.011	0.912	0.000
Urban	6.470	4.059	2.226	1.682	1.855	0.000
N	330059	330059	451897	451897	330059	451897

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level for different types of projects. The crime rates running from models (1) to (6) are total crime, total crime excluding anti-social behaviour (ASB), anti-social behaviour (ASB), property crime, violent crime and substance crime respectively. The project types considered here are: capital, one-off, repeated and other. The median values for each type of crime, and their values in rural and urban wards are also reported. Standard errors are given in parentheses and are clustered at the ward-level.

Table 7: Extensive Margin - Project Type 2

	(1) Total	(2) Total (excl. ASB)	(3) ASB	(4) Property	(5) Violent	(6) Substance
Sports	-0.036	-0.042**	0.012	0.007	-0.034***	0.002
	(0.023)	(0.018)	(0.013)	(0.010)	(0.011)	(0.003)
Mental	0.133***	0.143***	-0.029	0.074***	0.090***	-0.002
	(0.041)	(0.032)	(0.024)	(0.018)	(0.023)	(0.004)
Emp.	0.046	0.030	0.025	0.025*	-0.004	0.010**
	(0.034)	(0.027)	(0.023)	(0.014)	(0.019)	(0.004)
Other	-0.021	0.002	-0.022	0.019*	-0.000	-0.002
	(0.027)	(0.020)	(0.016)	(0.011)	(0.014)	(0.002)
Median	5.200	3.310	1.739	1.419	1.470	0.000
Rural	3.546	2.301	1.070	1.011	0.912	0.000
Urban	6.470	4.059	2.226	1.682	1.855	0.000
N	330059	330059	451897	451897	330059	451897

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level for different types of projects. The crime rates running from models (1) to (6) are total crime, total crime excluding anti-social behaviour (ASB), anti-social behaviour (ASB), property crime, violent crime and substance crime respectively. The project types considered here are: sports, mental health, employment & education and other. The median values for each type of crime, and their values in rural and urban wards are also reported. Standard errors are given in parentheses and are clustered at the ward-level.

Next, to understand further the interaction between project types and ward-level characteristics, we simply re-estimate the project-type regressions for urban and rural wards separately. The results of this can be found in Tables B.21 to B.24. We find that most of the effects of "one-off" projects, which reduced violent crime, are concentrated in urban areas, finding no significant effects for rural neighbourhoods. Similarly, the positive impact of "capital" projects on property crime is once again only found in urban areas. The impact of "mental health" projects on property and violent crime is positive in both rural and urban areas. The effects only differ in magnitude: 4.4% in rural, 5.3% in urban for property crime. This difference, however, may be explained by simple there being more crime, in general, in urban areas. Interestingly, ASB falls by 3.6% from a "mental health" project but only in rural neighbourhoods.

Overall, these results paint a mixed picture. It is clear that simply organising a project without careful attention to its type and objectives, is unlikely to cause significant reductions in the local crime, and may even increase it if it provides already crime-prone individuals to interact and socialise with those that can push them into committing new offences (through "concentration effects"). On the hand, similar to Jacob and Lefgren (2003), we find that one-off sporting events have the potential to create large "incapacitation" effects with the potential to reduce even violent offences.

5.2 Intensive Margin

Turning now to the intensive margin, we consider whether increased investment, i.e funding more (small) projects in a ward, has a cumulative effect on crime. In doing so, we compare areas that receive different quantities of funding to those that are in receipt of nothing at all. As mentioned above, we consider four main categories, each corresponding to changes in £10,000 which approximately corresponds to an additional small project. Thus, when a ward receives over £30,000 in funding this typically implies that at least three projects are active in that area. The estimates on intensive margins are reported in Table 8. Note that the estimates become less precise as the amount of funding in a ward increases since the number of periods where wards receive that many project decreases.

Table 8: Intensive Margin of Small Projects

	(1) Total	(2) Total Excl. ASB	(3) ASB	(4) Property	(5) Violent	(6) Substance
£10k	-0.011	-0.017	0.013	0.014*	-0.023***	0.003**
	(0.017)	(0.013)	(0.010)	(0.007)	(0.008)	(0.002)
£10k-£20k	0.013	0.030	-0.000	0.035***	0.018	0.001
	(0.032)	(0.024)	(0.018)	(0.013)	(0.016)	(0.003)
£20k-£30k	0.017	0.034	-0.056*	0.041*	0.049	0.004
	(0.054)	(0.044)	(0.029)	(0.022)	(0.030)	(0.006)
> £30k	0.094	0.169**	-0.141***	0.105***	0.164***	0.006
	(0.089)	(0.072)	(0.050)	(0.037)	(0.055)	(0.007)
Median	5.200	3.310	1.739	1.419	1.470	0.000
Rural	3.546	2.301	1.070	1.011	0.912	0.000
Urban	6.470	4.059	2.226	1.682	1.855	0.000
Percentile	51.086	52.946	46.261	53.516	55.000	61.705
N	330059.000	330059.000	451897.000	451897.000	330059.000	451897.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in monetary terms on the monthly crime rate at the ward-level. The respective monetary categories considered here are: upto £10,000; £10,001-£20,000; £20,001-£30,000; Over £30,000. The crime rates running from models (1) to (6) are total crime, total crime excluding anti-social behaviour (ASB), anti-social behaviour (ASB), property crime, violent crime and substance crime respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect. Standard errors are given in parentheses and are clustered at the ward-level.

The intensive margin effects are ambiguous, and seem to imply that projects cause compositional changes in the crime rate. Notice that irrespective of the quantity of funds in use, total crime remains unchanged, yet at when over £30,000 are invested, total crime excluding ASB rises by 5.1%, while ASB falls by 8.1%. The increase is driven by by property and violent crime that rise by 7.4% and 11.2% respectively. Clearly organising more projects involves both 'incapacitation" and "concentration" effects found above, but to a larger magnitude. To understand the sources of these effects, we once again consider the heterogeneity of these effects by both ward-level and project characteristics.

Table 9: Summary of Intensive Margin Heterogeneity Estimates

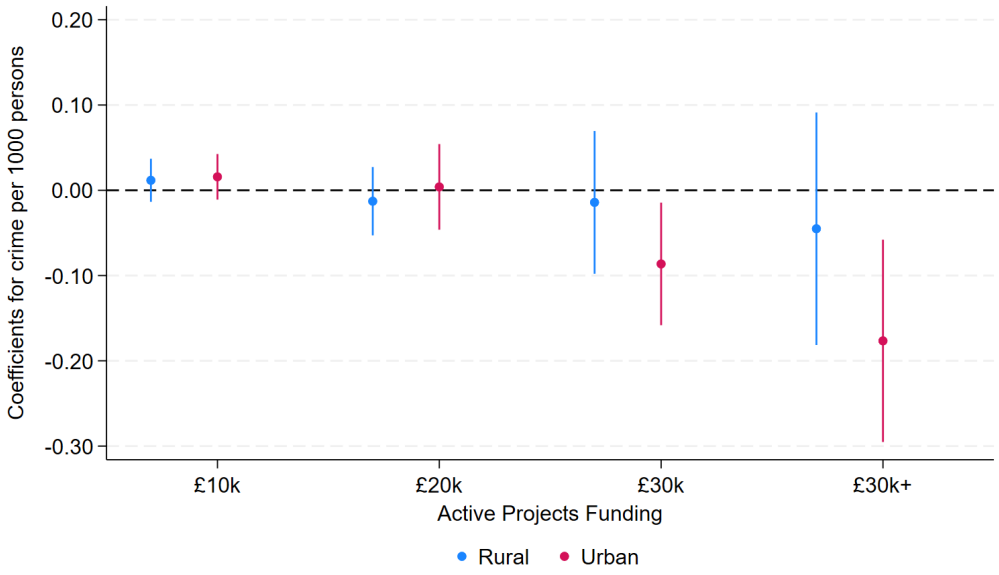
	(1) Total	(2) Total excl. ASB	(3) ASB	(4) Property	(5) Violent	(6) Substance
Baseline	0.094	0.169*	-0.141**	0.105**	0.164**	0.006
	(0.089)	(0.072)	(0.050)	(0.037)	(0.055)	(0.007)
Rural	-0.229	-0.282**	-0.045	0.050	-0.129**	-0.003
	(0.132)	(0.095)	(0.070)	(0.057)	(0.044)	(0.008)
Urban	0.186	0.302***	-0.177**	0.117**	0.253***	0.008
	(0.107)	(0.084)	(0.061)	(0.045)	(0.066)	(0.009)
Low Pop. Dens	-0.145	-0.160	-0.084	0.021	-0.059	-0.001
	(0.112)	(0.085)	(0.074)	(0.048)	(0.048)	(0.007)
High Pop. Dens.	0.211	0.333***	-0.161*	0.133**	0.277***	0.006
	(0.122)	(0.095)	(0.063)	(0.050)	(0.076)	(0.010)
High Educ.	0.065	-0.012	-0.056	0.087	0.000	0.009
	(0.108)	(0.088)	(0.074)	(0.062)	(0.055)	(0.009)
Low Educ.	0.117	0.301**	-0.202**	0.114*	0.280***	0.003
	(0.134)	(0.102)	(0.068)	(0.046)	(0.081)	(0.010)
Low LR Unemp.	0.046	0.033	-0.071	0.086	0.050	0.002
	(0.104)	(0.082)	(0.069)	(0.061)	(0.055)	(0.008)
High LR Unemp	0.129	0.269*	-0.190**	0.121**	0.243**	0.009
	(0.134)	(0.107)	(0.070)	(0.047)	(0.083)	(0.010)
Low Soc. Rent	-0.069	-0.011	-0.095	-0.012	0.114	0.006
	(0.105)	(0.089)	(0.065)	(0.038)	(0.064)	(0.008)
High Soc. Rent	0.155	0.250*	-0.141*	0.151**	0.183*	0.002
	(0.126)	(0.097)	(0.066)	(0.052)	(0.073)	(0.009)
Low Prop. Young	0.051	0.035	-0.108	0.081	0.076	-0.003
	(0.108)	(0.096)	(0.063)	(0.059)	(0.071)	(0.008)
High Prop. Young	0.109	0.252*	-0.173*	0.114*	0.222**	0.010
	(0.130)	(0.099)	(0.071)	(0.048)	(0.076)	(0.010)
Low Non-UK-Born	-0.203	-0.025	-0.185**	0.066	0.039	0.003
	(0.131)	(0.097)	(0.063)	(0.040)	(0.068)	(0.008)
High Non-Uk-Born	0.368**	0.350***	-0.104	0.142*	0.278***	0.009
	(0.112)	(0.102)	(0.077)	(0.061)	(0.083)	(0.011)
Low Wage	-0.058	0.181	-0.238***	0.099*	0.220**	0.003
	(0.123)	(0.101)	(0.060)	(0.044)	(0.079)	(0.008)
High Wage	0.337**	0.151	0.014	0.124	0.072	0.013
	(0.120)	(0.089)	(0.087)	(0.066)	(0.063)	(0.013)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in monetary terms on the monthly crime rate at the ward-level. This summary table presents estimates for receipt of over £30,000 split by various ward-level characteristics. The crime rates running from models (1) to (6) are total crime, total crime excluding anti-social behaviour (ASB), anti-social behaviour (ASB), property crime, violent crime and substance crime respectively. Standard errors are given in parentheses and are clustered at the ward-level.

These effects are highly heterogeneous - see Table 9 above for a summary. For example, consider again the rural vs. urban distinction in wards. Figure 5.2 plots the intensive margin estimates for anti-social behaviour, and illustrates the significant divergence in effects between urban areas, where the reduction becomes as large as 10.2%, and rural ones where no significant effect can be observed. This pattern repeats itself for population density where highly dense areas see a fall by 9.3%, while low density areas seen none. Similarly areas of greater share of youths and low levels of education see larger effects: just under 10% and 11.6% respectively. This pattern is not surprising: wards with higher degrees of urbanisation yet low socio-economic performance have a larger population of crime-prone young people that can be prevented from committing crimes through incapacitation.

Figure 7: Intensive Margin of Small Projects for ASB by Urban/Rural



Notes: A plot of coefficients for the effect of varying quantities of concurrent project funding on the rate of anti-social behaviour. The results are split by whether the receiving ward is classified as rural or urban. A 95% confidence interval based on standard errors clustered at the ward-level is included around the estimates.

The situation with other types of crime is more complex as the divergence between rural and urban wards is reversed and is much greater in magnitude. Striking are the estimates for total crime excl. ASB, which falls by 8.5% in a rural setting but rises by 9.1% in urban wards! This pattern, as

before, is driven by property and violent crime both of which increase significantly, but only in an urban setting, by 8.2% and 17.2% respectively. The explanation for this difference likely lies in where the "concentration" effect exceeds the "incapacitation effect". Urban areas provide more opportunities for inter-personal interaction, and although some more minor offences can be avoided when projects are running, each one runs the risk of creating conflict between youths that could become violent. In addition, by providing potentially crime-prone young people with more contacts from similar backgrounds, it allows for an exchange in information, particularity of new crime targets. For the full set of heterogeneity analysis estimates see Tables B.25 to B.38 in the Online Appendix.

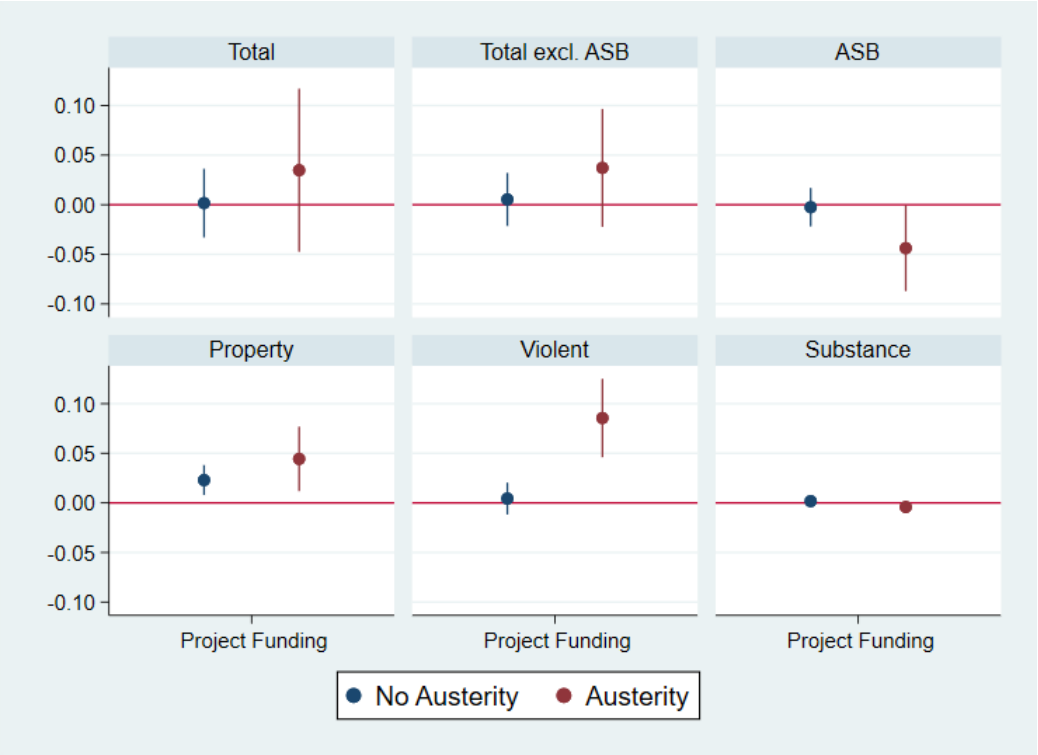
Once again, we assess the role that project types and objectives play in shaping these differential effects. Table B.39 reports the estimates for the first classification. Although not much is revealed for violent and property crime, we find that 'one-off' projects reduce ASB by 4.4%, perhaps suggesting that incapacitation effects are an effective way of curtailing anti-social behaviour. The results for the second classification, reported in Table B.40 are richer. Interestingly, we find a weakly significant 2.6% reduction in ASB associated with 'mental health' projects, yet this is the opposite of what can be observed for property and violent crime, which increase by 4.9% and 6.3% respectively. Tables B.41 to B.44 report the estimates for both classifications split by rural and urban wards. Notably, most of the observed effects are driven by urban neighbourhoods. For example, an additional £10,000 of funding for 'one-off' projects reduces ASB by 5% in urban areas, with no significant effects observed in rural ones. Equally, as before, 'mental health' projects increase both property and violent crime across both rural and urban communities. Overall, by considering the intensive margin of small projects, we find that concurrent funding of over £30,000 can have beneficial effects on the rate of anti-social behaviour that are particularly large in urban areas. It is likely that this effect stems from the incapacitation of crime-prone and vulnerable youths through after-school activities and similar interventions aiming at providing them with more opportunities for socialisation and support. Yet, these opportunities often increase both property and violent crime. Similar to Jacob and Lefgren (2003), this is likely through concentration and potentially networking effects.

5.3 Projects and Austerity

Policies of fiscal consolidation, exemplified by austerity, often lead to cuts to funding for public services, community centres and local clubs. Areas more

affected by measures may be more responsive to funded projects that could potentially mitigate against any harmful effects of austerity. We therefore investigate the interaction between the size of austerity cuts and the quantity of funding active in a ward. In particular, we check whether the marginal effect of an additional £10,000 worth of funding is greater in areas where the cost of austerity was below or above the median. The results of this are illustrated in Figure 5.3. The full set of estimates can be found in Table B.45 in the Online Appendix.

Figure 8: Marginal Effects of Small Projects by Austerity Costs



Notes: A marginal effects plot representing the impact of an additional £10,000 worth of funding for small projects on the monthly crime rate split by whether a ward was significantly affected by austerity or not. A 95% confidence interval based on standard errors clustered at the ward-level is included around the estimates.

The results may at first glance appear to be at odds with what we found earlier, namely that more funding lead to greater effects. This can be explained by the fact that the relationship between funding and crime is non-linear which is not modelled here. This explains why the magnitude and significance of estimated effects is lower than previously estimated when there is no

austerity. Yet, we do find important interaction effects with austerity policies. Namely that an extra £10,000 areas used in wards exposed to austerity see larger negative effects on ASB, but larger positive effects on property and violent crime. This pattern fits well with what we have established so far. The impact of projects is ambiguous, as it is never clear whether incapacitation or concentration effects will be stronger. Austerity measures lead to closures of youth and community clubs often leading to falls in socialisation opportunities which are sometimes provided by these projects. As we have seen above, these reduce ASB, but raise other types of crime instead.

5.4 Robustness Checks

We evaluate the robustness of the main results to changes in the sample restrictions, the assumed length projects are classified as active, and the sample of projects used in the analysis. In addition, we test for whether our results are driven by spatial spillover effects.

First, we are concerned that past large projects might affect the effect of current small project. In Tables B.47 and B.48 available in the Online Appendix we compare the intensive margin estimates when we restrict the sample to wards that did not receive a large project in the two years prior to the beginning of the sample, when there is no such restriction or when we exclude all wards that received a large project in the five years prior to the sample period. The results are largely insensitive to variations in these restrictions.In general, the less restrictive the restrictions, the larger and more precise the effects as the sample size increases. The differences in the size of the treatment effects are not very large and could be driven by greater contamination from the larger projects, or selection in the wards that are removed. Wards that received funding for larger projects are typically in urban areas, and imposing stronger restriction disproportionally increase the share of rural wards, for which the effects on crime are generally weaker, biasing the estimates towards zero. However, any contamination or selection bias is small. For example, when considering overall crime in wards that received less than £10k of funding, the estimates range from -0.006 to -0.016. We thus conclude that the estimates are largely insensitive to restrictions on having been a recipient of large project funding in the past.

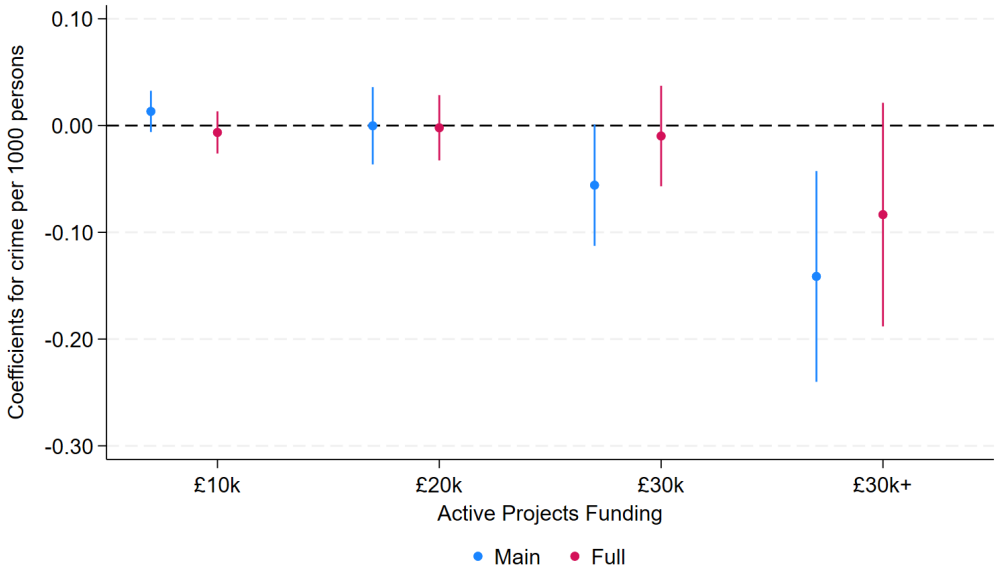
Secondly, we consider the robustness of the estimates to variations in the duration that a ward is defined as treated. The baseline of 24 months following the start of a project was based on the maximum allowed by the NLCF/SE, but we consider alternative durations of 18 or 30 months. The

duration of projects also directly impacts on the intensive margin, since a longer treatment window increases the chance that projects overlap. Estimates for alternative effective duration of treatment are reported in Tables B.49 and B.50 in the Online Appendix. The predicted pattern emerges; greater duration generally reduces the estimated effects. For example, a 18-month window implies that having funding of over £30,000 reduces the rate of ASB by 7.2%, but only by an insignificant 3.6% when a 30-month window is used. Overall, the estimates are broadly consistent across duration definitions.

Thirdly, we evaluate the robustness of the estimates to the sample of projects considered. Remember, for the baseline analysis, we are using a sample consisting only of projects that are relevant to youth and are therefore expected to have an effect on the crime rate. By using the full sample, we expect estimates to be numerically smaller and biased towards zero as many of the projects left out aim to help groups such as the elderly. Moreover, the inclusion of more projects will impact the overall number of projects active at any given moment in time impacting estimates at the intensive margin as well.The estimates for the different definitions of a treated ward are represented in Figure 5.4 in the case of anti-social behaviour below.[29] Overall, the estimates are consistent to alternative definitions of treatment increasing in magnitude as the quantity of concurrent funding increases. The more expansive definitions result in estimates that are closer to zero, but the patterns are similar. This is in line with our *a priori* expectations, and hence the interpretation of our main result is left unchanged.

[29]The full set of estimates is available in Tables B.51 and B.52 in the Online Appendix.

Figure 9: Intensive Margin of Small Projects for ASB by Project Samples



Notes: A plot of coefficients for the effect of varying quantities of concurrent project funding on the rate of anti-social behaviour split by the use of different sample of projects. A 95% confidence interval based on standard errors clustered at the ward-level is included around the estimates.

Lastly, we consider whether our estimates are in part driven by spatial spillovers. To do so we add a dummy to the second stage regression for whether a neighbouring ward either has at least one small project active or has received a large project in our sample period. This provides a simple test to see whether any residual differences between the actual and counter-factual crime rates in a ward can be explained by the treatment status of neighbouring wards. The results of this exercise are given in Table B.53. We find no significant effects of either large or small projects. Note that this does not imply that large projects cannot in principle have spillover effects, just that our sample restrictions have minimised their impact and mitigated contamination problems.

Altogether we have found that small projects results in almost no reduction in overall crimes. This is driven by urban wards that experience large drops of just over 10% in anti-social behaviour but nearly identical increases in more serious offences such as property and violent crimes. We also find no evidence of spatial spillovers. We now move our discussion to the estimation of the larger projects.

6 Results for Large Projects

6.1 ATT of Large Projects

In terms of funding, large projects represent 92% of funds attributed by the NLCF/SE, but only 21% of projects. Indeed, while all wards receive at least one small projects, many wards receive only one large project during the 10-years sample period. To recap, contrary to small projects, we assume that large projects have a (close to) permanent effect on crime in the neighbourhood. As noted above, due to the greater selectivity with which funding for large projects is given out, wards that receive such an investment are on different counterfactual crime trends to those that did not. This was detected by our pre-trends tests that showed violations in periods before wards first received a large project. Hence we use a synthetic difference-in-difference (SDID) approach to treatment effect estimation.

The SDID results for all crime types are available in Table 10 below. These reveal a 4.1% increase in property crime and a 4.6% increase in violent crime. This follows the same pattern that was observed for small projects though given the monetary size of the larger projects, the per pound effect is considerably smaller.

Table 10: SDID Estimates for Large Projects

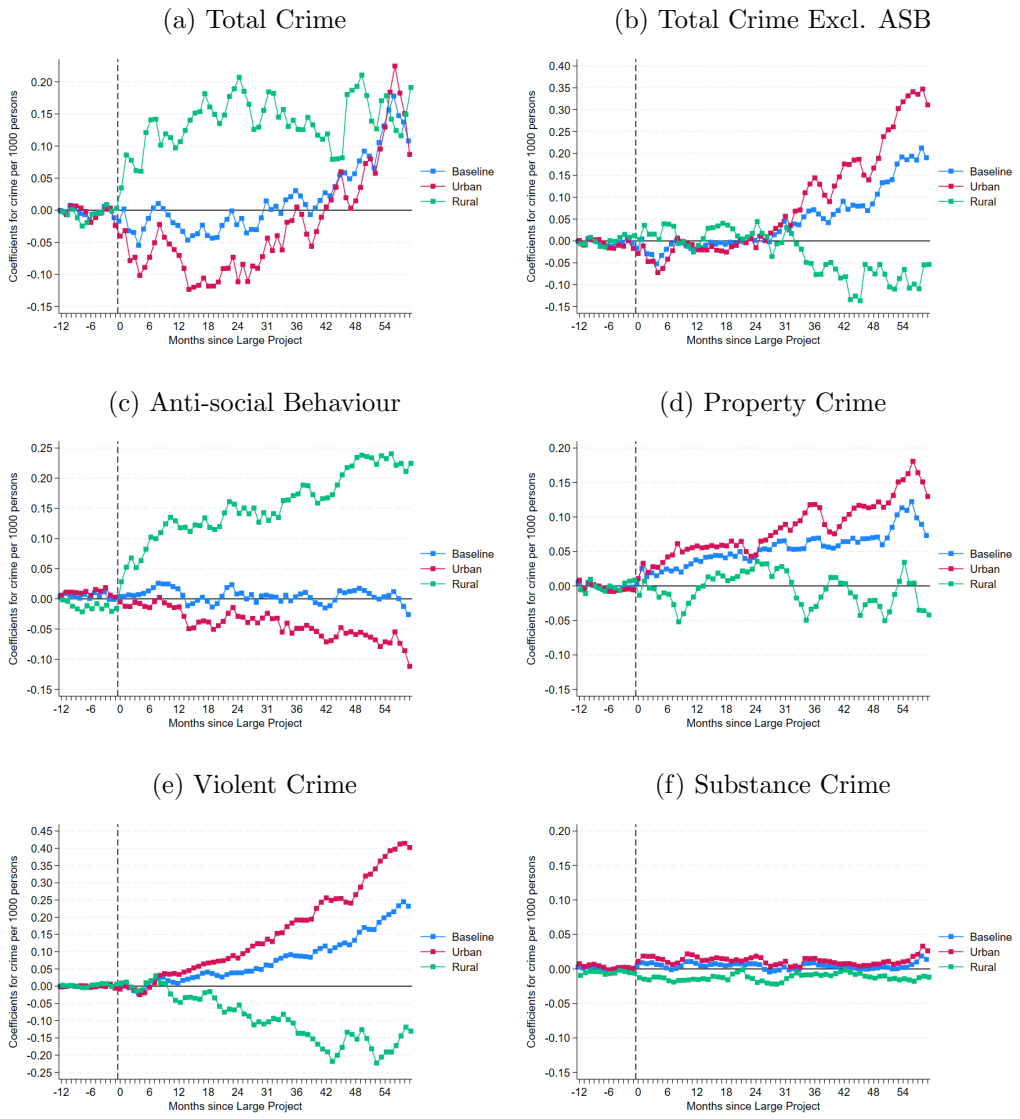
	(1) Total	(2) Total Excl. ASB	(3) ASB	(4) Property	(5) Violent	(6) Substance
Large	0.010	0.043	-0.014	0.063***	0.073***	0.005
	(0.043)	(0.028)	(0.027)	(0.017)	(0.017)	(0.004)
Median	5.395	3.479	1.724	1.552	1.591	0.091
Rural	3.612	2.389	1.087	1.110	0.994	0.000
Urban	6.760	4.341	2.182	1.877	2.030	0.126
Percentile	50.123	50.758	49.599	52.219	52.337	51.254
N	451528.000	451528.000	621584.000	621584.000	451528.000	621584.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the synthetic difference-in-difference estimation results illustrating the average treatment effect of receiving a large project on the monthly crime rate at the ward-level. Crime rates have been smoothed using a 3-month moving average. Placebo standard-errors clustered at the ward-level are reported in parentheses.

Next, using the approach suggested by Ciccia (2024), we perform event-study analysis based on the SDID estimates allowing us to derive the dynamic treatment effects across the pre-treatment period and a 60-month post-treatment period. These are also split by whether the receiving neighbourhood is rural or urban. These estimates are illustrated in Figure 6.1.

Figure 10: SDID Event Study Plots



Notes: Synthetic difference-in-difference event study plots, derived using the approach suggested in Ciccia (2024), showing the dynamic effect estimates of the impact of large projects on crime. Results are also split by whether the receiving ward is rural (green) or urban (red). The dependent variable is reported by type of crime per 1,000 residents. Confidence intervals have been suppressed.

The event study estimates provide additional information not only about how effects vary over time, but also what types of neighbourhoods see the largest effects. There is a clear divergence that can be seen between ASB and other

types of crime. In particular, ASB falls in urban areas, but rises in rural ones, while the opposite occurs for property and violent crime. Hence, total crime excluding ASB rises at most by just over 10% in rural neighbourhoods, and falls by up to 4.2% in rural ones. That being said, these larger effects only appear just over 2.5 years since the receipt of the large project, perhaps being driven by the completion of a new building or a larger capital investment.

Overall, we find evidence that large projects have mainly positive effects on crime mainly being driven by an increase in both property and violent crimes. That being said, unlike for small projects, these effects differ between rural and urban areas, falling in the latter, but rising in the former. We now consider in more detail how area characteristics impact static effects.

6.2 Heterogeneity Analysis

To conduct heterogeneity analysis for large projects, we derive a set of unit-level estimates by conducting the SDID analysis at the ward-level. In particular, due to the computational complexity of running a SDID model with several thousand control wards, we take a random sampling approach. In particular, each ward-level estimate is derived using a 100 randomly sampled control units. We then regress these estimates on a set of ward-level and project-level characteristics to identify the sources of heterogeneity.

The set of aggregated ATTs derived from this procedure are given in Table B.55. In terms of magnitude, they compare favourably to the original estimates and are not statistically different from them. The differences that do exist are an artifact of the sampling process: different samples of control units will yield slightly different results.[30]

Table B.56 in the Online Appendix reports the results from heterogeneity analysis regressions for each type of crime. As with the dynamic effects above there are clear differences between urban and rural neighbourhoods. Indeed, large projects have a negative effect on ASB, but positive effects on property and violent crime in urban areas. A similar pattern emerges for levels of education. Interestingly, the type of large project appears to explain little of the variation in treatment effects. The only exception are

[30]Note (for a more detailed discussion see the Empirical Strategy section above) that, to obtain more efficient estimates, these have been aggregated using a weighted least squares regression on a constant with weights equal to the normalised squared inverse of the standard errors of each estimate. Figure B.4 illustrates this variation through a set of funnel plots.

weakly significant negative impacts of 'capital', 'one-off' and even 'repeated' projects on total crime (relative to other projects).

6.3 Robustness Checks

Next, we check for spatial spillovers. Table B.57 provides the two-way fixed effects estimates that test for the presence of spillovers on the sample of control units. Consistent with our previous findings, we find little evidence that small projects have any spillovers effects - only the coefficient for substance crime is weakly significant. However, strong spillover effects are detected for larger projects. This suggests that our baseline SDID results could be driven by these effects.

Table B.58 reports the SDID results after a spillover dummy for whether a neighbouring ward has received a large project was included as a covariate. However, the relative to our main results, the interpretation is unchanged: large projects still have a significant positive effect on both property and violent crime. However, this result may itself be still be driven by spillovers: controlling for the treatment status of a neighbouring ward is typically insufficient to purge the estimates of contamination bias. Hence, in our next approach we use control units that neither receive a large project nor have a neighbour that receives one, and redefine treatment accordingly.

The results of this final exercise are provided in Table B.59. Here the effects are different from before. It is clear that when direct and spillovers effects are taken together, a large project increases property crime by nearly 8%. Equally, it appears that ASB may increase too although the coefficient is only weakly significant. Interestingly, the coefficient on violent crime is not significant. This may imply that violent crime is simply spatially displaced, occurring close to where a large project is being organised, and remains unchanged overall.

In sum, although considerations of spatial spillovers do affect our estimated coefficients, the interpretation of our results is still largely unchanged: large projects still appear to have a positive effect on property crime and potentially violent crime and ASB as well, though the results appear more ambiguous for the latter two.

Finally, similar to our analysis for small projects, we evaluate the robustness of our results to various changes in our sample restrictions. Recall that these

are: restrictions on whether a large project was received pre-2011 and the sample of projects used. The results of these checks are given in Tables B.60 to B.62. Each crime type is presented together where the first column provides the baseline estimates for comparison. Remarkably, the large project effects remain very close across all types of restrictions with minor variations across each model. For example, the effect of property crime in the range from 0.051 to 0.067, and remains significant throughout.

Overall, the robustness checks considered do not alter the basic interpretation of the results though spatial spillover considerations certainly enrich the baseline effects. We find pretty consistently that property crime rises after a large project is organised. This is likely due to the provision of new targets for potential offenders. The effect on other types of crime appears to be more mixed and less robust.

7 Conclusion

In this paper, we present the first causal estimates of the effect of small grassroot lead interventions on local crime rates in England and Wales over the 2011-2019 period. We rely on data from the National Lottery Community Fund and Police Reporting Archive (among other data sources) to construct a large panel of projects and monthly crime rates at the electoral ward-level, and exploit differences in the timing of projects to estimate the average treatment effect of small projects, worth less than £10,000 each.

Though relatively small individually, these projects aim to unite communities, improve neighbourhoods and provide services to residents. As such, they provide a unique opportunity to study the impact of place-based policies and bring to light causal evidence of their impact. Our analysis focuses on projects that mostly benefit/target young people and thus might affect local crime. We thus contribute to the debate on optimal public policy responses to crime.

The results of our analysis are mixed: small projects often cause compositional changes in the crime rate and although the total number of offences often remains unchanged, more serious offences such as property and violent crimes tend to rise. For example, we estimate that at the extensive margins, interventions costing as little as £10,000 increase property crime by 1.6% over a two-year period. A greater number of concurrent projects leads to an even larger effect. Happily, when over £30,000 is received in funding

(corresponding to at least three projects), anti-social behaviour declines by just over 8%. Unfortunately, this is matched by an increase in property and violent crime leading to no effect overall. Equally, large projects follow this pattern with one such investment increasing property crime by 4.1%. However, given that the median funding for such a project is 13 times larger than for the median small project the per pound effects are unlikely to be economically significant. Importantly for these types of interventions, their impact is often shaped by both the the characteristics of the projects themselves as well as the local communities where they are organised with large level of heterogeneity, especially between urban and rural neighbourhoods.

The picture suggested by our results is that small grassroot-lead local interventions have the potential to prevent crime, but greater attention must be paid to the type of project being funded: one-off interventions seem more effective in only reducing ASB without causing compositional changes in the crime rate. Although few studies have discussed the efficacy of similar interventions, our estimates compare favourably to those evaluating the various after-effects of austerity. For instance, Giulietti and McConnell (2020b) find that districts exposed to welfare reforms - which resulted to drop in public services, see a 3.5% increase in total crime and a 3.9% rise in violent crime. Equally Villa (2024), finds that as a result of youth centre closures in London, juveniles committed 15% more crimes.

Overall, the implication for public policy is that community-led intentions, if targetted and chosen appropriately, can be very cost effective at reducing some type of crime. Moreover, projects that focus on young people with the aim of helping with career aspirations and providing sporting opportunities may be an effective means of reducing anti-social behaviour and disrupting behavioural patterns that lead to more serious forms of crime.

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A Data Appendix

In this *Data Appendix*, we detail the process by which the crime and funded project datasets are constructed, as well as any underlying problems in the data.

A.1 Local Crime Data

A.1.1 Sources

The main source of crime data for the analysis is the police reported data available at the Police Data Archive. Further details available in Table A.1 below.

Table A.1: Raw Crime Data Sources

Name	Link
Police Recorded	Source

Notes: This table reports the type of crime data used and the link to where it can be accessed.

A.1.2 LSOA to Ward Aggregation

The LSOA-level police recorded data is aggregated to the ward-level. However, there is no publicly available "best-fit" look-up table for LSOAs to Wards (2011). Therefore, one has been constructed by matching the population weighted centroids of LSOAs to the fully clipped 2011 census ward boundaries. Geographically, LSOAs are not constructed in such a way as to aggregate into wards. Hence, the matching process is imperfect. This is because, though there are 8,546 census merged wards, LSOA population weighted centroids fall into 8,535 of them. This means that there are 11 wards such that no LSOA population centroids are inside them. This has potential to introduce some spatial-based measurement error into the aggregated crime data in that some counts of crime are misattributed geographically. Prima facie, this may appear to bias results since it implies that some wards where projects have been undertaken will not be observed. However, the population weighted centroid adjusts for the distribution of the population across each LSOA, and so areas which are 'discarded' in the matching process are unlikely to have a significant number of occupants.

A.1.3 Crime Classifications

The raw police recorded crime data is summarised below. As stated in the main text these raw values are aggregated into six broad categories. Hence, 'total crime' is simply the sum across all 15 crime types; 'total crime without ASB' is the sum of all crime excluding anti-social behaviour; 'property crime' includes burglary, shoplifting, vehicle crime, bike theft, persons theft and other theft; 'violent crime' includes violent & sexual offences, robbery, arson, weapons, public order and disorder incidents, while 'substance crime' is kept unchanged. Note that the values reported in Table A.2 below are for the full sample before any restrictions are based due to changing counting rules by the Home Office.

Table A.2: Summary Statistics for Raw Crime Types (2011-19)

Statistic	N	Mean	St. Dev.	Min	Max
Total Crime	674,265	7.21	7.67	0.00	509.66
Anti-social Behaviour	920,490	2.45	2.64	0.00	100.00
Violent & Sexual	920,490	1.37	1.57	0.00	149.31
Arson	920,490	0.64	0.68	0.00	23.11
Burglary	920,490	0.54	0.56	0.00	32.38
Drugs	920,490	0.46	0.53	0.00	27.23
Vehicle Crime	920,490	0.37	0.98	0.00	63.22
Shop-lifting	920,490	0.24	0.51	0.00	40.76
Public Order	920,490	0.18	0.50	0.00	121.33
Bike Theft	920,490	0.08	0.29	0.00	34.56
Robbery	920,490	0.06	0.17	0.00	14.21
Person Theft	920,490	0.06	0.59	0.00	72.28
Disorder	920,490	0.03	0.18	0.00	25.52
Weapons	920,490	0.03	0.11	0.00	14.43
Other Theft	920,490	0.63	1.62	0.00	237.47
Other Crime	920,490	0.31	1.39	0.00	282.55

A.2 Social Project Data

A.2.1 Sources

Data for funded social projects is taken from two major distributors: the National Lottery Community Fund and Sports England. The former makes up the majority of projects, but includes funding for trusts that themselves

redistribute funds nationwide. The largest of these are the (Big) Local Trust and the Power to Change Trust. All data is publicly available and links are provided in Table A.3 below.

Table A.3: Raw Funded Project Data Sources

Name	Link
National Lottery Community Fund	Source
Sports England	Source
(Big) Local Trust	Source
Power to Change Trust	Source

Notes: This table reports the distributor for the funded projects and the link to where project data can be accessed.

A.2.2 Data Cleaning

The raw data from all four sources yields a sample of 292,717 projects: 249,488 by the National Lottery Community Fund; 40,258 by Sports England; 2,821 by the Power to Change Trust, and 150 by the Big Local Trust. Initially, we remove all projects not taking place in England and Wales, and subsequently proceed to matching each project to the respective ward in which it was organised. The raw data collected by the National Lottery Community Fund for projects undertaken across England and Wales detailed 160,517 funded projects containing any ward-level geographic identifiers. However, in some entries, this information was erroneous and a geographic matching procedure was undertaken to recover the ward-level location of these projects. The Sport England, Big Local and Power to Change Trust data contained the exact co-ordinates for the project beneficiary, and hence no matching procedure was required. Instead, Full Extent Boundary shape-files were used to construct a one-to-one matching between the co-ordinates and ward code.

As a result, out of the 160,517 National Lottery Community Fund projects, the locations of 157,902 were identified, corresponding to a 98% success rate. The matching procedure itself followed a number of steps:

1. Using a look-up table from 2011 Wards to 2011 Census Merged Wards, matching was performed on the basis of ward names and local authority names (to avoid duplicates)

2. A partial match function was used to identify ward codes for those wards that had undergone slight name changes or whose name were inputted with error while being in the same local authority

3. An (imperfect) correspondence of 2011 wards was created with ward codes from 2015, 2016, 2017, 2018, 2019, 2020 and 2021 by overlaying the respective shape-files onto the 2011 geometry and matching where at least 90% of newer ward lay inside the 2011 one

4. Using the School Information Service[31], educational establishment post-codes could be identified on the basis of school or education facility names, and thus combined with data from the August 2011 NSPL to derive ward code information

The final sample covering the years 2001-2019 contains 180,915 projects.

A.2.3 Sample Restrictions

To reduce noise and avoid bias, we introduce two sample restrictions on our funded project data. Firstly, we drop projects worth less that £1,000 as some projects are recorded have received funds smaller the permitted lower bound of £300, thus avoiding possible measurement error problems. Moreover, many of these projects are too small to have any non-negligible effect on the crime rate. Secondly, to reduce noise and make sure that our estimates reflect youth-related projects only, we restrict our attention to those that affect youths and young people, or could benefit them in some way. To do this, we provide ChatGPT 4.5, a large language model (LLM) developed by OpenAI, with the prompt below. The basic idea is to exclude the projects that either have no specific location or target demographic groups that are unlikely to be involved in crime such as young children or the elderly. Hence, in our analysis, we only use the projects classified as"Youth". Note that when performing robustness checks, we re-run our main analysis on the full sample to show that our interpretation is left unchanged even if the magnitude of estimated effects falls due to the presence of superfluous projects. As a result of this classification, 63% of projects were retained in the sample.

Use this Excel document which contains the descriptions of various funded projects. Create a new categorical variable called "project_type" which denotes the type of each project. There are two mutually exclusive types: "Youth" and

[31]Downloaded from `https://www.get-information-schools.service.gov.uk/Downloads` on 06/04/2023

"Non-youth". "Youth" projects are those where young people or youths would be among the beneficiaries (or those involved) of the project. This is a broad category that could include sporting events/activities, counselling/mentoring sessions, the establishment of a new club (sports or otherwise) or the running of socials where young people would be involved. In addition, these projects could be capital investments that benefit the wider community alongside young people, for example when a sports facility or community centre is built/repaired/refurbished (e.g. sport participation/capacity building/capital/tackling inactivity projects by Sports England or Big Local community grants). On the other hand, "Non-youth" projects are all other projects that would likely not benefit or involve young people. For example, these could be projects that specifically target other (non-youth) demographic groups such as very young children or the elderly, or ones that have no specific location (i.e. those that described as nationwide or England-wide). Save this Excel file as "updated_projects_classification_gpt.xlsx" so that it can be downloaded.

A.2.4 Project Classifications

The two prompts used to classify projects into their respective types are given below. In the first classification, projects are placed into four categories: "Capital" for projects related to equipment purchases, infrastructure upgrades or improvements to local facilities (sports-related or otherwise); "One-Off" for projects that bring youths together or occupy them over a short time period usually via single event; "Repeated" projects are those that benefit the community through a set of repeated interventions or through things that can be used repeatedly. A simple example is the setting up for a collection of workshops or talks. All other potentially relevant projects are placed in the "Other" category. Summary statistics are given in Table A.4.
In the second classification, as before, projects are placed into four categories: "Sports" for projects involving sports activities, "Mental Health" for projects that aim to improve the mental health of local youths for either those suffering from an existing condition or simply those that are lonely and/or isolated, "Employment" are for projects aimed at improving the education or employment opportunities for youths or members of the local community, and "Other", as above, contains all other projects. Summary statistics are given in Table A.5.

Prompt for Classification 1

Use this Excel document which contains the descriptions of various funded

Table A.4: Proportion of Projects by Type and Size

Project Type	Small Projects	Large Projects
Capital	0.354	0.330
Repeated	0.408	0.338
One-Off	0.083	0.139
Other	0.155	0.193

Notes: A table summarising the proportion of each type of of project from the first classification. Recall that projects worth over £10,000 are classified as large, while those worth £10,000 or below are small.

Table A.5: Proportion of Projects by Type and Size

Project Type	Small Projects	Large Projects
Employment	0.120	0.195
Mental Health	0.133	0.120
Other	0.128	0.279
Sports	0.619	0.405

Notes: A table summarising the proportion of each type of of project from the second classification. Recall that projects worth over £10,000 are classified as large, while those worth £10,000 or below are small.

projects. Create a new categorical variable called "project_type_2" which denotes the type of each project. There are three mutually exclusive types: "Capital", "One-Off", "Repeated" and "Other". "Capital" projects are those where an investment is made into infrastructure such as building repairs, equipment purchases or the construction of new facilities (sports or otherwise). "One-Off" projects are all non-capital projects that fund an event that takes place over a short period of time such as a sports day or a field trip or any other similar activity. "Repeated" projects are all non-capital where community members benefit over a longer period such as through a series of workshops, talks, or sporting events, or any other similar event which are repeated. Lastly, "Other" are those projects that cannot be placed into any of the other categories (e.g. if there is insufficient information). Save this Excel file as "updated_projects_classification_gpt_2.xlsx" so that it can be downloaded.

Prompt for Classification 2

Use this Excel document which contains the descriptions of various funded projects. Create a new categorical variable called "project_type_3" which denotes the type of each project. There are four mutually exclusive types: "Sports", "Mental Health", "Employment" and "Other". "Sports" projects are those where the project aims to improve local sports facilities (through building renovations or equipment purchases), support a local sports club, increase participation in sports activities or organize some sort of sporting event, or something else sports-related. "Mental Health" projects are those that aim to improve the mental health of locals such as providing social opportunities, counselling or advice. Next, "Employment" projects are which try to improve either the education or employment opportunities available to locals either through additional tutoring, learning opportunities or workshops. Lastly, "Other" are those projects that cannot be placed into any of the other categories. Save this Excel file as "updated_projects_classification_gpt_3.xlsx" so that it can be downloaded.

A.3 Control Variables

Control variables were selected to represent various dimensions of local-area development such as wealth, education, extent of urbanisation and poverty. Table A.6 provides a brief explanation of each variable.

Table A.6: Control Variable Descriptions

Variable	Explanation
Population	The number of residents of a ward .
Youth Share	The share of residents aged 0-24.
Population Density	The number of residents of a ward per square kilometre.
L2 Education Share	The share of individuals who have achieved at most an L2-level (GCSE) of education.
Social Rent Share	Share of individuals living in socially-rented accommodation.
LR Unemployed Share	The share of those that have been unemployed for more than five years
Urban/Rural	ONS classification of whether a given ward is urban or rural
Median Income	Median weekly wage of the local authority district
Austerity Loss	Estimated per person loss (GBP) from austerity cuts calculated by Beatty and Fothergill (2013)

B Online Appendix

B.1 Additional Tables

Table B.1: Small Projects - Pre-trend Test Estimates

	(1) Total	(2) Total Excl. ASB	(3) ASB	(4) Property	(5) Violent	(6) Substance
-12	0.024	-0.001	0.023	-0.002	0.006	0.001
	(0.033)	(0.025)	(0.018)	(0.014)	(0.016)	(0.004)
-11	0.093	0.088	0.011	0.025	0.013	0.034
	(0.067)	(0.062)	(0.018)	(0.025)	(0.015)	(0.023)
-10	0.072**	0.012	0.051***	-0.014	0.033**	-0.001
	(0.034)	(0.026)	(0.018)	(0.014)	(0.016)	(0.004)
-9	-0.022	-0.038	0.017	-0.025*	0.006	-0.001
	(0.033)	(0.025)	(0.018)	(0.014)	(0.016)	(0.004)
-8	0.049	0.023	0.018	0.000	0.033**	-0.002
	(0.034)	(0.026)	(0.018)	(0.014)	(0.016)	(0.004)
-7	0.038	0.010	0.026	-0.002	0.032**	-0.003
	(0.034)	(0.025)	(0.018)	(0.014)	(0.016)	(0.004)
-6	0.062*	0.019	0.033*	0.012	0.016	0.005
	(0.034)	(0.026)	(0.017)	(0.014)	(0.016)	(0.005)
-5	0.007	0.000	0.011	0.010	0.017	0.004
	(0.031)	(0.024)	(0.017)	(0.014)	(0.016)	(0.004)
-4	0.025	0.002	0.025	0.015	0.009	-0.000
	(0.032)	(0.024)	(0.017)	(0.014)	(0.016)	(0.003)
-3	0.017	0.000	0.024	0.020	0.016	0.001
	(0.032)	(0.025)	(0.017)	(0.014)	(0.016)	(0.004)
-2	-0.007	-0.022	0.021	0.021	0.009	-0.004
	(0.031)	(0.024)	(0.017)	(0.014)	(0.015)	(0.003)
-1	0.035	0.021	0.016	0.024	0.032**	0.009
	(0.039)	(0.033)	(0.017)	(0.017)	(0.016)	(0.008)
F	1.310	0.897	0.993	1.055	1.003	0.866
p	0.205	0.549	0.452	0.395	0.443	0.582
N	174137.000	174137.000	233472.000	233472.000	174137.000	233472.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the estimates from event study regressions, following Borusyak et al. (2024) and run on the sample of untreated units, that test for pre-trend violations of the parallel trends assumptions. Standard errors are given in parentheses and are clustered at the ward-level. The overall F-statistics for joint significance of the pre-trend coefficients and the corresponding p-value are also reported.

Table B.2: Large Projects - Pre-trend Test Estimates

	(1) Total	(2) Total Excl. ASB	(3) ASB	(4) Property	(5) Violent	(6) Substance
-12	-0.074*	-0.033	-0.062***	0.039**	0.003	-0.004
	(0.040)	(0.026)	(0.023)	(0.018)	(0.014)	(0.004)
-11	-0.081**	-0.036	-0.073***	0.030	0.010	-0.003
	(0.041)	(0.027)	(0.023)	(0.019)	(0.014)	(0.004)
-10	-0.080*	-0.058**	-0.055**	0.027	0.009	-0.003
	(0.042)	(0.028)	(0.024)	(0.019)	(0.015)	(0.004)
-9	-0.120***	-0.082***	-0.058**	0.016	0.007	-0.002
	(0.042)	(0.028)	(0.024)	(0.019)	(0.014)	(0.004)
-8	-0.126***	-0.095***	-0.048*	0.024	0.001	-0.003
	(0.043)	(0.028)	(0.025)	(0.019)	(0.015)	(0.005)
-7	-0.135***	-0.086***	-0.064***	0.032*	-0.002	-0.002
	(0.042)	(0.028)	(0.024)	(0.019)	(0.015)	(0.004)
-6	-0.101**	-0.061**	-0.060**	0.050**	0.003	0.002
	(0.043)	(0.030)	(0.024)	(0.020)	(0.016)	(0.004)
-5	-0.078*	-0.034	-0.079***	0.071***	0.007	0.010*
	(0.044)	(0.029)	(0.024)	(0.021)	(0.015)	(0.005)
-4	-0.070	-0.030	-0.085***	0.086***	0.007	0.014**
	(0.044)	(0.029)	(0.025)	(0.021)	(0.016)	(0.006)
-3	-0.068	-0.029	-0.095***	0.099***	0.008	0.015***
	(0.045)	(0.030)	(0.025)	(0.021)	(0.016)	(0.005)
-2	-0.103**	-0.065**	-0.094***	0.085***	-0.002	0.009*
	(0.045)	(0.030)	(0.026)	(0.021)	(0.016)	(0.005)
-1	-0.103**	-0.056	-0.103***	0.098***	-0.004	0.011*
	(0.048)	(0.034)	(0.026)	(0.024)	(0.017)	(0.006)
F	1.631	2.045	2.148	2.580	0.430	1.497
p	0.076	0.017	0.012	0.002	0.952	0.117
N	316268.000	316268.000	431598.000	431598.000	316268.000	431598.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the estimates from event study regressions, following Borusyak et al. (2024) and run on the sample of untreated units, that test for pre-trend violations of the parallel trends assumptions. Standard errors are given in parentheses and are clustered at the ward-level. The overall F-statistics for joint significance of the pre-trend coefficients and the corresponding p-value are also reported.

Table B.3: Heterogeneity - Extensive Margin of Small Projects - Urban/Rural - I

	(1) Total	(2) Total	(3) Total excl. ASB	(4) Total excl. ASB	(5) ASB	(6) ASB
Small	0.004	-0.009	0.009	-0.011	-0.002	0.004
	(0.025)	(0.024)	(0.018)	(0.019)	(0.015)	(0.013)
Median	3.546	6.470	2.301	4.059	1.070	2.226
Percentile	28.670	62.884	30.700	61.775	30.672	61.238
N	203228.000	126831.000	203228.000	126831.000	278521.000	173376.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level. Results are split by the whether a ward is classified as rural or urban (respectively) by the ONS. The crime rates running from models (1) to (6) in pairs are total crime, total crime excluding anti-social behaviour (ASB) and anti-social behaviour (ASB) respectively. The median values for each type of crime are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.4: Heterogeneity - Extensive Margin of Small Projects - Urban/Rural - II

	(1) Property	(2) Property	(3) Violent	(4) Violent	(5) Substance	(6) Substance
Small	0.021**	0.025**	0.001	-0.013	0.004*	0.002
	(0.010)	(0.010)	(0.012)	(0.012)	(0.002)	(0.002)
Median	1.011	1.682	0.912	1.855	0.000	0.000
Percentile	35.754	59.332	30.262	60.890	61.705	61.705
N	278521.000	173376.000	203228.000	126831.000	278521.000	173376.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level. Results are split by the whether a ward is classified as rural or urban (respectively) by the ONS. The crime rates running from models (1) to (6) in pairs are property crime, violent crime and substance crime respectively. The median values for each type of crime are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.5: Heterogeneity - Extensive Margin of Small Projects - Education - I

	(1) Total	(2) Total	(3) Total excl. ASB	(4) Total excl. ASB	(5) ASB	(6) ASB
Small	0.017	-0.021	0.005	-0.005	0.009	-0.006
	(0.021)	(0.029)	(0.016)	(0.021)	(0.012)	(0.016)
Median	4.128	6.506	2.726	3.956	1.299	2.316
Rural	3.152	4.388	2.102	2.683	0.887	1.482
Urban	5.264	7.415	3.444	4.488	1.734	2.688
Percentile	36.690	63.106	39.097	60.403	37.975	62.809
N	167225.000	162834.000	167225.000	162834.000	228067.000	223830.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level. Results are split by the by whether a ward is below or above the median (respectively) for the proportion of residents with at most a GCSE-level education as recorded in the 2011 Census. The crime rates running from models (1) to (6) in pairs are total crime, total crime excluding anti-social behaviour (ASB) and anti-social behaviour (ASB) respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.6: Heterogeneity - Extensive Margin of Small Projects - Education - II

	(1) Property	(2) Property	(3) Violent	(4) Violent	(5) Substance	(6) Substance
Small	0.019*	0.026**	-0.001	-0.011	0.004	0.002
	(0.010)	(0.011)	(0.010)	(0.015)	(0.002)	(0.002)
Median	1.304	1.532	1.079	1.932	0.000	0.000
Rural	1.015	1.000	0.740	1.278	0.000	0.000
Urban	1.588	1.746	1.431	2.191	0.000	0.078
Percentile	46.637	54.644	36.442	62.926	61.705	61.705
N	228067.000	223830.000	167225.000	162834.000	228067.000	223830.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level. Results are split by the by whether a ward is below or above the median (respectively) for the proportion of residents with at most a GCSE-level education as recorded in the 2011 Census. The crime rates running from models (1) to (6) in pairs are property crime, violent crime and substance crime respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.7: Heterogeneity - Extensive Margin of Small Projects - Weekly Wage - I

	(1) Total	(2) Total	(3) Total excl. ASB	(4) Total excl. ASB	(5) ASB	(6) ASB
Small	-0.028	0.027	-0.016	0.023	-0.006	0.005
	(0.027)	(0.024)	(0.020)	(0.018)	(0.016)	(0.014)
Median	5.536	4.932	3.375	3.261	1.963	1.551
Rural	3.484	3.597	2.183	2.411	1.137	1.004
Urban	7.158	5.911	4.293	3.872	2.629	1.916
Percentile	53.435	47.151	50.869	49.564	55.348	45.121
N	159367.000	164246.000	159367.000	164246.000	218811.000	224316.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level. Results are split by the by whether the local authority in which the ward is located is below or above the median (respectively) of the weekly wage distribution. The crime rates running from models (1) to (6) in pairs are total crime, total crime excluding anti-social behaviour (ASB) and anti-social behaviour (ASB) respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.8: Heterogeneity - Extensive Margin of Small Projects - Weekly Wage - II

	(1) Property	(2) Property	(3) Violent	(4) Violent	(5) Substance	(6) Substance
Small	0.013	0.033***	-0.014	0.005	0.003	0.004
	(0.010)	(0.011)	(0.014)	(0.011)	(0.002)	(0.003)
Median	1.351	1.488	1.586	1.385	0.000	0.000
Rural	0.864	1.187	0.940	0.884	0.000	0.000
Urban	1.705	1.675	2.039	1.711	0.000	0.000
Percentile	48.071	53.409	53.099	47.275	61.705	61.705
N	218811.000	224316.000	159367.000	164246.000	218811.000	224316.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level. Results are split by the by whether the local authority in which the ward is located is below or above the median (respectively) of the weekly wage distribution. The crime rates running from models (1) to (6) in pairs are property crime, violent crime and substance crime respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.9: Heterogeneity - Extensive Margin of Small Projects - Foreign Born - I

	(1) Total	(2) Total	(3) Total excl. ASB	(4) Total excl. ASB	(5) ASB	(6) ASB
Small	-0.035	0.037	-0.036*	0.043**	0.007	-0.005
	(0.024)	(0.027)	(0.018)	(0.020)	(0.014)	(0.016)
Median	4.528	5.983	2.785	3.947	1.592	1.900
Rural	3.476	3.689	2.199	2.508	1.109	0.991
Urban	5.869	6.869	3.470	4.481	2.191	2.249
Percentile	41.324	58.781	39.461	60.984	46.291	53.887
N	169215.000	160844.000	169215.000	160844.000	232034.000	219863.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level. Results are split by the by whether a ward is below or above the median (respectively) for the proportion of locals that were not born in the UK as recorded in the 2011 Census. The crime rates running from models (1) to (6) in pairs are total crime, total crime excluding anti-social behaviour (ASB) and anti-social behaviour (ASB) respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.10: Heterogeneity - Extensive Margin of Small Projects - Foreign Born - II

	(1) Property	(2) Property	(3) Violent	(4) Violent	(5) Substance	(6) Substance
Small	0.012	0.037***	-0.027**	0.017	0.003*	0.003
	(0.010)	(0.012)	(0.013)	(0.012)	(0.002)	(0.003)
Median	1.147	1.726	1.246	1.720	0.000	0.000
Rural	0.926	1.200	0.899	0.943	0.000	0.000
Urban	1.372	1.903	1.648	2.000	0.000	0.095
Percentile	40.568	60.989	41.402	58.020	61.705	61.705
N	232034.000	219863.000	169215.000	160844.000	232034.000	219863.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level. Results are split by the by whether a ward is below or above the median (respectively) for the proportion of locals that were not born in the UK as recorded in the 2011 Census. The crime rates running from models (1) to (6) in pairs are property crime, violent crime and substance crime respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.11: Heterogeneity - Extensive Margin of Small Projects - Population Density - I

	(1) Total	(2) Total	(3) Total excl. ASB	(4) Total excl. ASB	(5) ASB	(6) ASB
Small	-0.013	0.015	-0.017	0.022	0.013	-0.007
	(0.021)	(0.030)	(0.017)	(0.021)	(0.013)	(0.016)
Median	3.903	6.895	2.503	4.336	1.237	2.374
Rural	3.495	5.333	2.278	3.107	1.045	1.967
Urban	5.071	6.938	3.122	4.371	1.772	2.384
Percentile	33.303	66.823	34.268	66.065	36.180	63.869
N	172421.000	157638.000	172421.000	157638.000	235963.000	215934.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level. Results are split by the by whether a ward is below or above the median (respectively) in the population density distribution as recorded in the 2011 Census. The crime rates running from models (1) to (6) in pairs are total crime, total crime excluding anti-social behaviour (ASB) and anti-social behaviour (ASB) respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.12: Heterogeneity - Extensive Margin of Small Projects - Population Density - II

	(1) Property	(2) Property	(3) Violent	(4) Violent	(5) Substance	(6) Substance
Small	0.016*	0.027**	-0.023**	0.015	0.002	0.003
	(0.010)	(0.012)	(0.011)	(0.014)	(0.002)	(0.003)
Median	1.105	1.772	1.012	2.007	0.000	0.081
Rural	1.009	1.042	0.893	1.669	0.000	0.000
Urban	1.339	1.793	1.356	2.017	0.000	0.085
Percentile	39.232	62.041	33.266	65.461	61.705	63.270
N	235963.000	215934.000	172421.000	157638.000	235963.000	215934.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level. Results are split by the by whether a ward is below or above the median (respectively) in the population density distribution as recorded in the 2011 Census. The crime rates running from models (1) to (6) in pairs are property crime, violent crime and substance crime respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.13: Heterogeneity - Extensive Margin of Small Projects - Youth Share - I

	(1) Total	(2) Total	(3) Total excl. ASB	(4) Total excl. ASB	(5) ASB	(6) ASB
Small	-0.012	0.009	-0.013	0.014	-0.001	0.001
	(0.025)	(0.026)	(0.018)	(0.020)	(0.015)	(0.015)
Median	4.091	6.567	2.600	4.148	1.345	2.213
Rural	3.380	4.098	2.190	2.645	1.003	1.272
Urban	5.207	7.155	3.211	4.510	1.853	2.452
Percentile	35.810	63.934	36.301	63.404	39.008	60.922
N	170507.000	159552.000	170507.000	159552.000	232912.000	218985.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level. Results are split by the by whether a ward is below or above the median (respectively) for the proportion of locals that are aged 0-24 as recorded in the 2011 Census. The crime rates running from models (1) to (6) in pairs are total crime, total crime excluding anti-social behaviour (ASB) and anti-social behaviour (ASB) respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.14: Heterogeneity - Extensive Margin of Small Projects - Youth Share - II

	(1) Property	(2) Property	(3) Violent	(4) Violent	(5) Substance	(6) Substance
Small	0.026**	0.020*	-0.011	0.001	0.002	0.004
	(0.011)	(0.011)	(0.011)	(0.013)	(0.002)	(0.003)
Median	1.151	1.700	1.083	1.919	0.000	0.000
Rural	0.973	1.124	0.851	1.134	0.000	0.000
Urban	1.387	1.838	1.424	2.099	0.000	0.097
Percentile	41.218	59.724	36.221	62.906	61.705	61.705
N	232912.000	218985.000	170507.000	159552.000	232912.000	218985.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level. Results are split by the by whether a ward is below or above the median (respectively) for the proportion of locals that are aged 0-24 as recorded in the 2011 Census. The crime rates running from models (1) to (6) in pairs are property crime, violent crime and substance crime respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.15: Heterogeneity - Extensive Margin of Small Projects - LR Unemployment - I

	(1) Total	(2) Total	(3) Total excl. ASB	(4) Total excl. ASB	(5) ASB	(6) ASB
Small	0.010	-0.015	0.009	-0.005	0.002	-0.001
	(0.022)	(0.029)	(0.017)	(0.021)	(0.013)	(0.016)
Median	4.676	5.916	3.029	3.651	1.512	2.036
Rural	3.418	3.710	2.266	2.352	0.984	1.179
Urban	5.616	7.416	3.605	4.536	1.892	2.638
Percentile	43.445	57.549	44.775	55.688	43.849	57.060
N	171592.000	158467.000	171592.000	158467.000	234563.000	217334.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level. Results are split by the by whether a ward is below or above the median (respectively) in the long-term unemployment rate distribution as recorded in the 2011 Census. The crime rates running from models (1) to (6) in pairs are total crime, total crime excluding anti-social behaviour (ASB) and anti-social behaviour (ASB) respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.16: Heterogeneity - Extensive Margin of Small Projects - LR Unemployment - II

	(1) Property	(2) Property	(3) Violent	(4) Violent	(5) Substance	(6) Substance
Small	0.026**	0.020*	-0.001	-0.008	0.006**	0.000
	(0.011)	(0.010)	(0.010)	(0.015)	(0.002)	(0.002)
Median	1.375	1.467	1.277	1.722	0.000	0.000
Rural	1.076	0.951	0.838	0.997	0.000	0.000
Urban	1.572	1.788	1.584	2.164	0.000	0.076
Percentile	49.157	52.347	43.235	57.360	61.705	61.705
N	234563.000	217334.000	171592.000	158467.000	234563.000	217334.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level. Results are split by the by whether a ward is below or above the median (respectively) in the long-term unemployment rate distribution as recorded in the 2011 Census. The crime rates running from models (1) to (6) in pairs are property crime, violent crime and substance crime respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.17: Heterogeneity - Extensive Margin of Small Projects - Share Social Housing - I

	(1) Total	(2) Total	(3) Total excl. ASB	(4) Total excl. ASB	(5) ASB	(6) ASB
Small	-0.006	-0.004	-0.002	0.002	0.004	-0.001
	(0.022)	(0.028)	(0.016)	(0.022)	(0.012)	(0.017)
Median	3.932	6.918	2.561	4.260	1.248	2.427
Rural	3.118	4.427	2.054	2.760	0.893	1.465
Urban	4.750	7.900	3.058	4.878	1.586	2.835
Percentile	33.802	66.848	35.732	64.756	36.259	64.970
N	173050.000	157009.000	173050.000	157009.000	236672.000	215225.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level. Results are split by the by whether a ward is below or above the median (respectively) in distribution of the proportion of locals that live in socially tented accomodation as recorded in the 2011 Census. The crime rates running from models (1) to (6) in pairs are total crime, total crime excluding anti-social behaviour (ASB) and anti-social behaviour (ASB) respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.18: Heterogeneity - Extensive Margin of Small Projects - Share Social Housing - II

	(1) Property	(2) Property	(3) Violent	(4) Violent	(5) Substance	(6) Substance
Small	0.015	0.029**	-0.006	-0.004	-0.001	0.006**
	(0.009)	(0.012)	(0.010)	(0.014)	(0.002)	(0.003)
Median	1.191	1.673	1.025	2.050	0.000	0.000
Rural	0.953	1.112	0.751	1.238	0.000	0.000
Urban	1.401	1.904	1.277	2.350	0.000	0.117
Percentile	42.318	59.154	34.366	66.012	0.000	61.705
N	236672.000	215225.000	173050.000	157009.000	236672.000	215225.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level. Results are split by the by whether a ward is below or above the median (respectively) in distribution of the proportion of locals that live in socially tented accomodation as recorded in the 2011 Census. The crime rates running from models (1) to (6) in pairs are property crime, violent crime and substance crime respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.19: Extensive Margin - Project Type 1

	(1) Total	(2) Total (excl. ASB)	(3) ASB	(4) Property	(5) Violent	(6) Substance
Capital	0.015	0.005	0.019	0.023**	-0.000	0.003
	(0.025)	(0.019)	(0.015)	(0.010)	(0.013)	(0.003)
One-Off	-0.086**	-0.084**	-0.030	0.012	-0.070***	0.003
	(0.042)	(0.034)	(0.028)	(0.021)	(0.024)	(0.005)
Repeated	0.006	0.002	-0.001	0.020*	0.001	-0.000
	(0.026)	(0.020)	(0.015)	(0.011)	(0.013)	(0.002)
Other	0.034	0.056**	-0.015	0.028**	0.022	0.002
	(0.032)	(0.024)	(0.019)	(0.013)	(0.016)	(0.003)
Median	5.200	3.310	1.739	1.419	1.470	0.000
Rural	3.546	2.301	1.070	1.011	0.912	0.000
Urban	6.470	4.059	2.226	1.682	1.855	0.000
N	330059	330059	451897	451897	330059	451897

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level for different types of projects. The crime rates running from models (1) to (6) are total crime, total crime excluding anti-social behaviour (ASB), anti-social behaviour (ASB), property crime, violent crime and substance crime respectively. The project types considered here are: capital, one-off, repeated and other. The median values for each type of crime, and their values in rural and urban wards are also reported. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.20: Extensive Margin - Project Type 2

	(1) Total	(2) Total (excl. ASB)	(3) ASB	(4) Property	(5) Violent	(6) Substance
Sports	-0.036	-0.042**	0.012	0.007	-0.034***	0.002
	(0.023)	(0.018)	(0.013)	(0.010)	(0.011)	(0.003)
Mental	0.133***	0.143***	-0.029	0.074***	0.090***	-0.002
	(0.041)	(0.032)	(0.024)	(0.018)	(0.023)	(0.004)
Emp.	0.046	0.030	0.025	0.025*	-0.004	0.010**
	(0.034)	(0.027)	(0.023)	(0.014)	(0.019)	(0.004)
Other	-0.021	0.002	-0.022	0.019*	-0.000	-0.002
	(0.027)	(0.020)	(0.016)	(0.011)	(0.014)	(0.002)
Median	5.200	3.310	1.739	1.419	1.470	0.000
Rural	3.546	2.301	1.070	1.011	0.912	0.000
Urban	6.470	4.059	2.226	1.682	1.855	0.000
N	330059	330059	451897	451897	330059	451897

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level for different types of projects. The crime rates running from models (1) to (6) are total crime, total crime excluding anti-social behaviour (ASB), anti-social behaviour (ASB), property crime, violent crime and substance crime respectively. The project types considered here are: sports, mental health, employment & education and other. The median values for each type of crime, and their values in rural and urban wards are also reported. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.21: Extensive Margin - Project Type 1 - Rural/Urban - I

	(1) Total	(2) Total	(3) Total Excl. ASB	(4) Total Excl. ASB	(5) ASB	(6) ASB
Capital	-0.018	0.037	-0.014	0.014	0.018	0.018
	(0.032)	(0.037)	(0.024)	(0.028)	(0.017)	(0.022)
One-Off	-0.095	-0.087*	-0.083	-0.088**	-0.039	-0.026
	(0.070)	(0.052)	(0.053)	(0.042)	(0.040)	(0.036)
Repeated	0.001	0.008	-0.020	0.013	-0.005	0.001
	(0.040)	(0.032)	(0.033)	(0.024)	(0.021)	(0.020)
Other	0.037	0.035	0.036	0.070**	-0.003	-0.018
	(0.042)	(0.043)	(0.031)	(0.032)	(0.022)	(0.026)
Median	5.200	5.200	3.310	3.310	1.739	1.739
N	126831.000	203228.000	126831.000	203228.000	173376.000	278521.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level for different types of projects. The results are split by whether a ward is classified as rural or urban respectively. The crime rates in each pair of models are total crime, total crime excluding anti-social behaviour (ASB), and anti-social behaviour (ASB). The project types considered here are: capital, one-off, repeated and other. The median values for each type of crime are also reported. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.22: Extensive Margin - Project Type 1 - Rural/Urban - II

	(1) Property	(2) Property	(3) Violent	(4) Violent	(5) Substance	(6) Substance
Capital	0.008	0.033**	-0.009	0.002	0.004	0.004
	(0.013)	(0.015)	(0.015)	(0.020)	(0.005)	(0.003)
One-Off	0.019	0.006	-0.056	-0.076**	0.007	0.001
	(0.025)	(0.028)	(0.038)	(0.030)	(0.006)	(0.007)
Repeated	0.033*	0.013	-0.018	0.011	-0.002	0.000
	(0.019)	(0.013)	(0.021)	(0.015)	(0.003)	(0.003)
Other	0.043**	0.018	0.013	0.029	-0.005	0.005
	(0.017)	(0.017)	(0.019)	(0.022)	(0.004)	(0.005)
Median	1.419	1.419	1.470	1.470	0.000	0.000
N	173376.000	278521.000	126831.000	203228.000	173376.000	278521.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level for different types of projects. The results are split by whether a ward is classified as rural or urban respectively. The crime rates in each pair of models are property crime, violent crime, and substance crime. The project types considered here are: capital, one-off, repeated and other. The median values for each type of crime are also reported. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.23: Extensive Margin - Project Type 2 - Rural/Urban - I

	(1) Total	(2) Total	(3) Total Excl. ASB	(4) Total Excl. ASB	(5) ASB	(6) ASB
Sports	-0.013	-0.047	-0.039	-0.046**	0.016	0.014
	(0.033)	(0.029)	(0.026)	(0.023)	(0.018)	(0.017)
Mental	0.012	0.173***	0.070	0.162***	-0.063**	-0.014
	(0.065)	(0.050)	(0.052)	(0.039)	(0.031)	(0.031)
Emp.	-0.020	0.071*	-0.028	0.056*	0.011	0.026
	(0.055)	(0.042)	(0.039)	(0.033)	(0.032)	(0.028)
Other	-0.010	-0.030	0.006	0.000	-0.001	-0.042*
	(0.032)	(0.041)	(0.025)	(0.029)	(0.017)	(0.024)
Median	5.200	5.200	3.310	3.310	1.739	1.739
N	126831.000	203228.000	126831.000	203228.000	173376.000	278521.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level for different types of projects. The results are split by whether a ward is classified as rural or urban respectively. The crime rates in each pair of models are total crime, total crime excluding anti-social behaviour (ASB), and anti-social behaviour (ASB). The project types considered here are: sports, mental health, employment & education and other. The median values for each type of crime are also reported. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.24: Extensive Margin - Project Type 2 - Rural/Urban - I

	(1) Property	(2) Property	(3) Violent	(4) Violent	(5) Substance	(6) Substance
Sports	0.021	-0.003	-0.042**	-0.031**	0.006	-0.001
	(0.013)	(0.013)	(0.017)	(0.014)	(0.005)	(0.003)
Mental	0.063**	0.075***	0.064**	0.096***	-0.004	-0.001
	(0.029)	(0.022)	(0.028)	(0.030)	(0.005)	(0.005)
Emp.	0.003	0.033*	-0.025	0.009	0.007	0.011**
	(0.021)	(0.018)	(0.025)	(0.024)	(0.009)	(0.005)
Other	0.025*	0.016	0.005	-0.005	-0.007**	0.003
	(0.013)	(0.016)	(0.015)	(0.021)	(0.003)	(0.004)
Median	1.419	1.419	1.470	1.470	0.000	0.000
N	173376.000	278521.000	126831.000	203228.000	173376.000	278521.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level for different types of projects. The results are split by whether a ward is classified as rural or urban respectively. The crime rates in each pair of models are property crime, violent crime, and substance crime. The project types considered here are: sports, mental health, employment & education and other. The median values for each type of crime are also reported. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.25: Heterogeneity - Intensive Margin of Small Projects - Rural/Urban - I

	(1) Total	(2) Total	(3) Total Excl. ASB	(4) Total Excl. ASB	(5) ASB	(6) ASB
£10k	-0.006	-0.011	-0.009	-0.020	0.012	0.016
	(0.024)	(0.023)	(0.019)	(0.017)	(0.013)	(0.014)
£10k-£20k	-0.001	0.017	0.016	0.034	-0.013	0.004
	(0.041)	(0.044)	(0.032)	(0.032)	(0.020)	(0.026)
£20k-£30k	-0.025	0.027	-0.072	0.079	-0.014	-0.086**
	(0.079)	(0.070)	(0.063)	(0.056)	(0.043)	(0.037)
¿£30k	-0.229*	0.186*	-0.282***	0.302***	-0.045	-0.177***
	(0.132)	(0.107)	(0.095)	(0.084)	(0.070)	(0.061)
Median	3.546	6.470	2.301	4.059	1.070	2.226
Percentile	25.448	64.614	24.770	66.110	29.319	57.374
N	126831.000	203228.000	126831.000	203228.000	173376.000	278521.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in monetary terms on the monthly crime rate at the ward-level. Results are split by the whether a ward is classified as rural or urban (respectively) by the ONS. The respective monetary categories considered here are: upto £10,000; £10,001-£20,000; £20,001-£30,000; Over £30,000. The crime rates running from models (1) to (6) in pairs are total crime, total crime excluding anti-social behaviour (ASB) and anti-social behaviour (ASB) respectively. The median values for each type of crime are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect for investments worth of over £30,000. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.26: Heterogeneity - Intensive Margin of Small Projects - Rural/Urban - II

	(1) Property	(2) Property	(3) Violent	(4) Violent	(5) Substance	(6) Substance
£10k	0.019*	0.010	-0.019	-0.024**	0.003	0.003*
	(0.011)	(0.010)	(0.012)	(0.011)	(0.003)	(0.002)
£10k-£20k	0.036**	0.033*	0.014	0.018	-0.002	0.003
	(0.017)	(0.017)	(0.019)	(0.021)	(0.003)	(0.004)
£20k-£30k	0.058**	0.031	0.001	0.071*	-0.001	0.006
	(0.028)	(0.029)	(0.041)	(0.039)	(0.005)	(0.008)
¿£30k	0.050	0.117***	-0.129***	0.253***	-0.003	0.008
	(0.057)	(0.045)	(0.044)	(0.066)	(0.008)	(0.009)
Median	1.011	1.682	0.912	1.855	0.000	0.000
Percentile	36.871	62.014	25.319	67.490	0.000	61.705
N	173376.000	278521.000	126831.000	203228.000	173376.000	278521.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in monetary terms on the monthly crime rate at the ward-level. Results are split by the whether a ward is classified as rural or urban (respectively) by the ONS. The respective monetary categories considered here are: upto £10,000; £10,001-£20,000; £20,001-£30,000; Over £30,000. The crime rates running from models (1) to (6) in pairs are property crime, violent crime and substance crime respectively. The median values for each type of crime are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect for investments worth of over £30,000. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.27: Heterogeneity - Intensive Margin of Small Projects - Education - I

	(1) Total	(2) Total	(3) Total Excl. ASB	(4) Total Excl. ASB	(5) ASB	(6) ASB
£10k	0.013	-0.036	0.002	-0.037*	0.016	0.013
	(0.021)	(0.026)	(0.016)	(0.019)	(0.011)	(0.016)
£10k-£20k	0.006	0.014	0.014	0.042	0.001	-0.006
	(0.036)	(0.054)	(0.027)	(0.039)	(0.024)	(0.028)
£20k-£30k	0.077	-0.047	0.021	0.042	-0.015	-0.100**
	(0.062)	(0.088)	(0.047)	(0.072)	(0.035)	(0.045)
¿£30k	0.065	0.117	-0.012	0.301***	-0.056	-0.202***
	(0.108)	(0.134)	(0.088)	(0.102)	(0.074)	(0.068)
Median	4.128	6.506	2.726	3.956	1.299	2.316
Rural	3.152	4.388	2.102	2.683	0.887	1.482
Urban	5.264	7.415	3.444	4.488	1.734	2.688
Percentile	37.349	64.337	38.751	64.674	35.972	58.785
N	167225.000	162834.000	167225.000	162834.000	228067.000	223830.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in monetary terms on the monthly crime rate at the ward-level. Results are split by the by whether a ward is below or above the median (respectively) for the proportion of residents with at most a GCSE-level education as recorded in the 2011 Census. The respective monetary categories considered here are: upto £10,000; £10,001-£20,000; £20,001-£30,000; Over £30,000. The crime rates running from models (1) to (6) in pairs are total crime, total crime excluding anti-social behaviour (ASB) and anti-social behaviour (ASB) respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect for investments worth of over £30,000. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.28: Heterogeneity - Intensive Margin of Small Projects - Education - II

	(1) Property	(2) Property	(3) Violent	(4) Violent	(5) Substance	(6) Substance
£10k	0.008	0.019*	-0.008	-0.040***	0.003	0.003
	(0.009)	(0.011)	(0.009)	(0.013)	(0.002)	(0.002)
£10k-£20k	0.031*	0.038**	0.011	0.021	0.003	-0.001
	(0.018)	(0.018)	(0.016)	(0.026)	(0.005)	(0.004)
£20k-£30k	0.060*	0.018	0.033	0.060	0.010	-0.002
	(0.035)	(0.027)	(0.027)	(0.052)	(0.007)	(0.008)
¿£30k	0.087	0.114**	0.000	0.280***	0.009	0.003
	(0.062)	(0.046)	(0.055)	(0.081)	(0.009)	(0.010)
Median	1.304	1.532	1.079	1.932	0.000	0.000
Rural	1.015	1.000	0.740	1.278	0.000	0.000
Urban	1.588	1.746	1.431	2.191	0.000	0.078
Percentile	49.005	57.457	36.516	69.818	61.705	61.705
N	228067.000	223830.000	167225.000	162834.000	228067.000	223830.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in monetary terms on the monthly crime rate at the ward-level. Results are split by the by whether a ward is below or above the median (respectively) for the proportion of residents with at most a GCSE-level education as recorded in the 2011 Census. The respective monetary categories considered here are: upto £10,000; £10,001-£20,000; £20,001-£30,000; Over £30,000. The crime rates running from models (1) to (6) in pairs are property crime, violent crime and substance crime respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect for investments worth of over £30,000. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.29: Heterogeneity - Intensive Margin of Small Projects - Foreign-Born - I

	(1) Total	(2) Total	(3) Total Excl. ASB	(4) Total Excl. ASB	(5) ASB	(6) ASB
£10k	-0.034	0.019	-0.043***	0.016	0.023	0.004
	(0.022)	(0.025)	(0.017)	(0.019)	(0.014)	(0.014)
£10k-£20k	0.007	0.020	0.010	0.052	0.006	-0.006
	(0.041)	(0.049)	(0.032)	(0.035)	(0.023)	(0.028)
£20k-£30k	-0.128	0.139*	-0.109	0.161***	-0.069*	-0.050
	(0.081)	(0.071)	(0.067)	(0.057)	(0.041)	(0.040)
¿£30k	-0.203	0.368***	-0.025	0.350***	-0.185***	-0.104
	(0.131)	(0.112)	(0.097)	(0.102)	(0.063)	(0.077)
Median	4.528	5.983	2.785	3.947	1.592	1.900
Rural	3.476	3.689	2.199	2.508	1.109	0.991
Urban	5.869	6.869	3.470	4.481	2.191	2.249
Percentile	39.151	61.914	39.653	65.222	40.831	51.454
N	169215.000	160844.000	169215.000	160844.000	232034.000	219863.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in monetary terms on the monthly crime rate at the ward-level. Results are split by the by whether a ward is below or above the median (respectively) for the proportion of locals that were not born in the UK as recorded in the 2011 Census. The respective monetary categories considered here are: upto £10,000; £10,001-£20,000; £20,001-£30,000; Over £30,000. The crime rates running from models (1) to (6) in pairs are total crime, total crime excluding anti-social behaviour (ASB) and anti-social behaviour (ASB) respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect for investments worth of over £30,000. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.30: Heterogeneity - Extensive Margin of Small Projects - Foreign Born - II

	(1) Property	(2) Property	(3) Violent	(4) Violent	(5) Substance	(6) Substance
Small	0.012	0.037***	-0.027**	0.017	0.003*	0.003
	(0.010)	(0.012)	(0.013)	(0.012)	(0.002)	(0.003)
Median	1.147	1.726	1.246	1.720	0.000	0.000
Rural	0.926	1.200	0.899	0.943	0.000	0.000
Urban	1.372	1.903	1.648	2.000	0.000	0.095
Percentile	40.568	60.989	41.402	58.020	61.705	61.705
N	232034.000	219863.000	169215.000	160844.000	232034.000	219863.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of receiving at least one small project on the monthly crime rate at the ward-level. Results are split by the by whether a ward is below or above the median (respectively) for the proportion of locals that were not born in the UK as recorded in the 2011 Census. The crime rates running from models (1) to (6) in pairs are property crime, violent crime and substance crime respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.31: Heterogeneity - Intensive Margin of Small Projects - Population Density - I

	(1) Total	(2) Total	(3) Total Excl. ASB	(4) Total Excl. ASB	(5) ASB	(6) ASB
£10k	-0.008	-0.007	-0.016	-0.013	0.024**	0.007
	(0.021)	(0.026)	(0.017)	(0.019)	(0.012)	(0.015)
£10k-£20k	-0.008	0.035	0.007	0.047	0.002	0.001
	(0.036)	(0.052)	(0.029)	(0.037)	(0.024)	(0.027)
£20k-£30k	-0.051	0.055	-0.071	0.106*	-0.051	-0.067*
	(0.071)	(0.077)	(0.058)	(0.062)	(0.040)	(0.040)
¿£30k	-0.145	0.211*	-0.160*	0.333***	-0.084	-0.161**
	(0.112)	(0.122)	(0.085)	(0.095)	(0.074)	(0.063)
Median	3.903	6.895	2.503	4.336	1.237	2.374
Rural	3.495	5.333	2.278	3.107	1.045	1.967
Urban	5.071	6.938	3.122	4.371	1.772	2.384
Percentile	31.482	68.403	31.335	69.959	33.215	60.887
N	172421.000	157638.000	172421.000	157638.000	235963.000	215934.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in monetary terms on the monthly crime rate at the ward-level. These results are split by whether a ward has low or high population density as recorded in the 2011 Census. The respective monetary categories considered here are: upto £10,000; £10,001-£20,000; £20,001-£30,000; Over £30,000. The crime rates running from models (1) to (6) in pairs are total crime, total crime excluding anti-social behaviour (ASB) and anti-social behaviour (ASB) respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect for investments worth of over £30,000. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.32: Heterogeneity - Intensive Margin of Small Projects - Population Density - II

	(1) Property	(2) Property	(3) Violent	(4) Violent	(5) Substance	(6) Substance
£10k	0.010	0.017*	-0.028***	-0.015	0.004	0.003
	(0.010)	(0.010)	(0.011)	(0.013)	(0.002)	(0.002)
£10k-£20k	0.033**	0.033	-0.006	0.035	-0.002	0.005
	(0.015)	(0.020)	(0.018)	(0.024)	(0.003)	(0.005)
£20k-£30k	0.036	0.039	-0.011	0.093**	0.002	0.005
	(0.023)	(0.033)	(0.035)	(0.044)	(0.008)	(0.007)
¿£30k	0.021	0.133***	-0.059	0.277***	-0.001	0.006
	(0.048)	(0.050)	(0.048)	(0.076)	(0.007)	(0.010)
Median	1.105	1.772	1.012	2.007	0.000	0.081
Rural	1.009	1.042	0.893	1.669	0.000	0.000
Urban	1.339	1.793	1.356	2.017	0.000	0.085
Percentile	39.411	64.992	31.890	71.335	0.000	63.528
N	235963.000	215934.000	172421.000	157638.000	235963.000	215934.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in monetary terms on the monthly crime rate at the ward-level. These results are split by whether a ward has low or high population density as recorded in the 2011 Census. The respective monetary categories considered here are: upto £10,000; £10,001-£20,000; £20,001-£30,000; Over £30,000. The crime rates running from models (1) to (6) in pairs are property crime, violent crime and substance crime respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect for investments worth of over £30,000. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.33: Heterogeneity - Intensive Margin of Small Projects - Youth Share - I

	(1) Total	(2) Total	(3) Total Excl. ASB	(4) Total Excl. ASB	(5) ASB	(6) ASB
£10k	-0.010	-0.010	-0.015	-0.017	0.009	0.016
	(0.023)	(0.024)	(0.017)	(0.018)	(0.013)	(0.015)
£10k-£20k	-0.023	0.044	0.007	0.045	-0.021	0.020
	(0.045)	(0.046)	(0.031)	(0.036)	(0.028)	(0.024)
£20k-£30k	-0.019	0.040	-0.077	0.128**	-0.002	-0.111***
	(0.071)	(0.080)	(0.058)	(0.064)	(0.039)	(0.041)
¿£30k	0.051	0.109	0.035	0.252**	-0.108*	-0.173**
	(0.108)	(0.130)	(0.096)	(0.099)	(0.063)	(0.071)
Median	4.091	6.567	2.600	4.148	1.345	2.213
Rural	3.380	4.098	2.190	2.645	1.003	1.272
Urban	5.207	7.155	3.211	4.510	1.853	2.452
Percentile	36.649	64.284	37.229	63.684	35.747	58.571
N	170507.000	159552.000	170507.000	159552.000	232912.000	218985.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in monetary terms on the monthly crime rate at the ward-level. Results are split by the by whether a ward is below or above the median (respectively) for the proportion of locals that are aged 0-24 as recorded in the 2011 Census. The respective monetary categories considered here are: upto £10,000; £10,001-£20,000; £20,001-£30,000; Over £30,000. The crime rates running from models (1) to (6) in pairs are total crime, total crime excluding anti-social behaviour (ASB) and anti-social behaviour (ASB) respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect for investments worth of over £30,000. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.34: Heterogeneity - Intensive Margin of Small Projects - Youth Share - II

	(1) Property	(2) Property	(3) Violent	(4) Violent	(5) Substance	(6) Substance
£10k	0.016	0.012	-0.018	-0.027**	0.002	0.005**
	(0.010)	(0.011)	(0.011)	(0.012)	(0.002)	(0.003)
£10k-£20k	0.047**	0.023	0.006	0.025	0.003	-0.000
	(0.019)	(0.017)	(0.019)	(0.024)	(0.004)	(0.005)
£20k-£30k	0.040	0.038	-0.031	0.117***	0.007	0.001
	(0.035)	(0.027)	(0.042)	(0.041)	(0.008)	(0.007)
¿£30k	0.081	0.114**	0.076	0.222***	-0.003	0.010
	(0.059)	(0.048)	(0.071)	(0.076)	(0.008)	(0.010)
Median	1.151	1.700	1.083	1.919	0.000	0.000
Rural	0.973	1.124	0.851	1.134	0.000	0.000
Urban	1.387	1.838	1.424	2.099	0.000	0.097
Percentile	43.308	61.504	39.317	64.817	0.000	0.000
N	232912.000	218985.000	170507.000	159552.000	232912.000	218985.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in monetary terms on the monthly crime rate at the ward-level. Results are split by the by whether a ward is below or above the median (respectively) for the proportion of locals that are aged 0-24 as recorded in the 2011 Census. The respective monetary categories considered here are: upto £10,000; £10,001-£20,000; £20,001-£30,000; Over £30,000. The crime rates running from models (1) to (6) in pairs are property crime, violent crime and substance crime respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect for investments worth of over £30,000. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.35: Heterogeneity - Intensive Margin of Small Projects - LR Unemployment - I

	(1) Total	(2) Total	(3) Total Excl. ASB	(4) Total Excl. ASB	(5) ASB	(6) ASB
£10k	0.008	-0.033	0.001	-0.034*	0.008	0.016
	(0.021)	(0.026)	(0.016)	(0.019)	(0.012)	(0.016)
£10k-£20k	-0.002	0.026	0.022	0.040	-0.002	0.001
	(0.039)	(0.052)	(0.030)	(0.038)	(0.025)	(0.028)
£20k-£30k	0.063	-0.027	0.040	0.029	-0.031	-0.078*
	(0.075)	(0.077)	(0.056)	(0.066)	(0.037)	(0.043)
¿£30k	0.046	0.129	0.033	0.269**	-0.071	-0.190***
	(0.104)	(0.134)	(0.082)	(0.107)	(0.069)	(0.070)
Median	4.676	5.916	3.029	3.651	1.512	2.036
Rural	3.418	3.710	2.266	2.352	0.984	1.179
Urban	5.616	7.416	3.605	4.536	1.892	2.638
Percentile	44.243	59.022	45.464	59.920	41.880	52.679
N	171592.000	158467.000	171592.000	158467.000	234563.000	217334.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in monetary terms on the monthly crime rate at the ward-level. Results are split by the by whether a ward is below or above the median (respectively) in the long-term unemployment rate distribution as recorded in the 2011 Census. The respective monetary categories considered here are: upto £10,000; £10,001-£20,000; £20,001-£30,000; Over £30,000. The crime rates running from models (1) to (6) in pairs are total crime, total crime excluding anti-social behaviour (ASB) and anti-social behaviour (ASB) respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect for investments worth of over £30,000. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.36: Heterogeneity - Intensive Margin of Small Projects - LR Unemployment - II

	(1) Property	(2) Property	(3) Violent	(4) Violent	(5) Substance	(6) Substance
£10k	0.015	0.013	-0.009	-0.037***	0.005**	0.002
	(0.010)	(0.010)	(0.010)	(0.013)	(0.002)	(0.002)
£10k-£20k	0.040**	0.030*	0.005	0.032	0.007*	-0.005
	(0.019)	(0.017)	(0.018)	(0.026)	(0.004)	(0.005)
£20k-£30k	0.080**	0.009	0.042	0.054	0.017*	-0.006
	(0.038)	(0.023)	(0.030)	(0.049)	(0.009)	(0.006)
¿£30k	0.086	0.121***	0.050	0.243***	0.002	0.009
	(0.061)	(0.047)	(0.055)	(0.083)	(0.008)	(0.010)
Median	1.375	1.467	1.277	1.722	0.000	0.000
Rural	1.076	0.951	0.838	0.997	0.000	0.000
Urban	1.572	1.788	1.584	2.164	0.000	0.076
Percentile	51.472	55.610	45.209	64.049	61.705	61.705
N	234563.000	217334.000	171592.000	158467.000	234563.000	217334.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in monetary terms on the monthly crime rate at the ward-level. Results are split by the by whether a ward is below or above the median (respectively) in the long-term unemployment rate distribution as recorded in the 2011 Census. The respective monetary categories considered here are: upto £10,000; £10,001-£20,000; £20,001-£30,000; Over £30,000. The crime rates running from models (1) to (6) in pairs are property crime, violent crime and substance crime respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect for investments worth of over £30,000. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.37: Heterogeneity - Intensive Margin of Small Projects - Social Housing - I

	(1) Total	(2) Total	(3) Total Excl. ASB	(4) Total Excl. ASB	(5) ASB	(6) ASB
£10k	-0.006	-0.018	-0.005	-0.028	0.010	0.014
	(0.021)	(0.026)	(0.016)	(0.020)	(0.011)	(0.016)
£10k-£20k	-0.021	0.030	0.003	0.050	-0.004	0.006
	(0.040)	(0.049)	(0.029)	(0.038)	(0.022)	(0.029)
£20k-£30k	0.074	-0.049	0.020	0.026	-0.001	-0.081*
	(0.071)	(0.079)	(0.054)	(0.066)	(0.034)	(0.042)
¿£30k	-0.069	0.155	-0.011	0.250**	-0.095	-0.141**
	(0.105)	(0.126)	(0.089)	(0.097)	(0.065)	(0.066)
Median	3.932	6.918	2.561	4.260	1.248	2.427
Rural	3.118	4.427	2.054	2.760	0.893	1.465
Urban	4.750	7.900	3.058	4.878	1.586	2.835
Percentile	32.922	68.124	35.550	68.045	33.200	62.330
N	173050.000	157009.000	173050.000	157009.000	236672.000	215225.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in monetary terms on the monthly crime rate at the ward-level. Results are split by the by whether a ward is below or above the median (respectively) in distribution of the proportion of locals that live in socially tented accommodation as recorded in the 2011 Census. The respective monetary categories considered here are: upto £10,000; £10,001-£20,000; £20,001-£30,000; Over £30,000. The crime rates running from models (1) to (6) in pairs are total crime, total crime excluding anti-social behaviour (ASB) and anti-social behaviour (ASB) respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect for investments worth of over £30,000. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.38: Heterogeneity - Intensive Margin of Small Projects - Social Housing - II

	(1) Property	(2) Property	(3) Violent	(4) Violent	(5) Substance	(6) Substance
£10k	0.012	0.016	-0.015	-0.030**	-0.000	0.007**
	(0.010)	(0.011)	(0.009)	(0.013)	(0.002)	(0.003)
£10k-£20k	0.023	0.042**	0.008	0.026	-0.004	0.005
	(0.016)	(0.020)	(0.018)	(0.025)	(0.003)	(0.005)
£20k-£30k	0.037	0.037	0.021	0.057	0.003	0.004
	(0.026)	(0.033)	(0.033)	(0.046)	(0.006)	(0.009)
¿£30k	-0.012	0.151***	0.114*	0.183**	0.006	0.002
	(0.038)	(0.052)	(0.064)	(0.073)	(0.008)	(0.009)
Median	1.191	1.673	1.025	2.050	0.000	0.000
Rural	0.953	1.112	0.751	1.238	0.000	0.000
Urban	1.401	1.904	1.277	2.350	0.000	0.117
Percentile	41.296	62.744	38.645	70.243	61.705	61.705
N	236672.000	215225.000	173050.000	157009.000	236672.000	215225.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in monetary terms on the monthly crime rate at the ward-level. Results are split by the by whether a ward is below or above the median (respectively) in distribution of the proportion of locals that live in socially tented accommodation as recorded in the 2011 Census. The respective monetary categories considered here are: upto £10,000; £10,001-£20,000; £20,001-£30,000; Over £30,000. The crime rates running from models (1) to (6) in pairs are property crime, violent crime and substance crime respectively. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect for investments worth of over £30,000. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.39: Intensive Margin - Project Type 1

	(1) Total	(2) Total Excl. ASB	(3) ASB	(4) Property	(5) Violent	(6) Substance
Capital	0.012	0.011	0.008	0.018*	0.018	0.002
	(0.026)	(0.021)	(0.015)	(0.010)	(0.014)	(0.002)
One-Off	-0.053	-0.035	-0.076**	0.025	-0.027	-0.001
	(0.049)	(0.040)	(0.032)	(0.022)	(0.032)	(0.005)
Repeated	0.025	0.022	-0.000	0.026**	0.008	0.000
	(0.026)	(0.019)	(0.016)	(0.010)	(0.013)	(0.002)
Other	0.038	0.070***	-0.025	0.028**	0.035**	0.004
	(0.034)	(0.025)	(0.020)	(0.013)	(0.017)	(0.004)
Median	5.200	3.310	1.739	1.419	1.470	0.000
Rural	3.546	2.301	1.070	1.011	0.912	0.000
Urban	6.470	4.059	2.226	1.682	1.855	0.000
N	330059.000	330059.000	451897.000	451897.000	330059.000	451897.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in continuous monetary terms (£10,000s) on the monthly crime rate at the ward-level for different types of projects. The crime rates running from models (1) to (6) are total crime, total crime excluding anti-social behaviour (ASB), anti-social behaviour (ASB), property crime, violent crime and substance crime respectively. The project types considered here are: capital, one-off, repeated and other. The median values for each type of crime, and their values in rural and urban wards are also reported. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.40: Intensive Margin - Project Type 2

	(1) Total	(2) Total Excl. ASB	(3) ASB	(4) Property	(5) Violent	(6) Substance
Sports	-0.021	-0.021	-0.000	0.011	-0.012	0.001
	(0.023)	(0.019)	(0.013)	(0.009)	(0.012)	(0.002)
Mental	0.120***	0.140***	-0.046*	0.070***	0.092***	-0.002
	(0.043)	(0.034)	(0.026)	(0.019)	(0.025)	(0.004)
Emp.	0.032	0.042	-0.005	0.020	0.012	0.008**
	(0.035)	(0.028)	(0.024)	(0.015)	(0.022)	(0.004)
Other	0.009	0.017	-0.010	0.020**	0.015	-0.000
	(0.029)	(0.022)	(0.014)	(0.010)	(0.013)	(0.002)
Median	5.200	3.310	1.739	1.419	1.470	0.000
Rural	3.546	2.301	1.070	1.011	0.912	0.000
Urban	6.470	4.059	2.226	1.682	1.855	0.000
N	330059.000	330059.000	451897.000	451897.000	330059.000	451897.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in continuous monetary terms (£10,000s) on the monthly crime rate at the ward-level for different types of projects. The crime rates running from models (1) to (6) are total crime, total crime excluding anti-social behaviour (ASB), anti-social behaviour (ASB), property crime, violent crime and substance crime respectively. The project types considered here are: sports, mental health, employment & education and other, and are treated as continuous. The median values for each type of crime, and their values in rural and urban wards are also reported. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.41: Intensive Margin - Project Type 1 - Urban/Rural - I

	(1) Total	(2) Total	(3) Total Excl. ASB	(4) Total Excl. ASB	(5) ASB	(6) ASB
Capital	-0.038	0.044	-0.036	0.044	0.020	-0.007
	(0.030)	(0.039)	(0.024)	(0.031)	(0.014)	(0.022)
One-Off	-0.036	-0.062	-0.075	-0.031	-0.012	-0.087**
	(0.068)	(0.060)	(0.052)	(0.050)	(0.047)	(0.038)
Repeated	0.045	0.020	0.014	0.023	0.001	0.000
	(0.045)	(0.030)	(0.035)	(0.022)	(0.022)	(0.019)
Other	0.013	0.050	0.025	0.090***	-0.027	-0.022
	(0.049)	(0.044)	(0.036)	(0.032)	(0.025)	(0.026)
Median	5.200	5.200	3.310	3.310	1.739	1.739
N	126831.000	203228.000	126831.000	203228.000	173376.000	278521.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

*Notes:*This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in continuous monetary terms (£10,000s) on the monthly crime rate at the ward-level for different types of projects. The results are split by whether a ward is classified as rural or urban respectively. The crime rates in each pair of models are total crime, total crime excluding anti-social behaviour (ASB), and anti-social behaviour (ASB). The project types considered here are: capital, one-off, repeated and other. The median values for each type of crime are also reported. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.42: Intensive Margin - Project Type 1 - Urban/Rural - II

	(1) Property	(2) Property	(3) Violent	(4) Violent	(5) Substance	(6) Substance
Capital	0.002	0.031**	-0.012	0.038*	0.001	0.003
	(0.013)	(0.015)	(0.014)	(0.022)	(0.004)	(0.003)
One-Off	0.050	0.012	-0.060	-0.020	0.003	-0.004
	(0.041)	(0.026)	(0.038)	(0.039)	(0.006)	(0.006)
Repeated	0.044**	0.019	0.003	0.010	0.000	-0.000
	(0.021)	(0.011)	(0.022)	(0.015)	(0.003)	(0.003)
Other	0.037**	0.022	0.012	0.045**	-0.002	0.007
	(0.018)	(0.017)	(0.020)	(0.022)	(0.003)	(0.005)
Median	1.419	1.419	1.470	1.470	0.000	0.000
N	173376.000	278521.000	126831.000	203228.000	173376.000	278521.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

*Notes:*This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in continuous monetary terms (£10,000s) on the monthly crime rate at the ward-level for different types of projects. The results are split by whether a ward is classified as rural or urban respectively. The crime rates in each pair of models are property crime, violent crime, and substance crime. The project types considered here are: capital, one-off, repeated and other. The median values for each type of crime are also reported. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.43: Intensive Margin - Project Type 2 - Urban/Rural - I

	(1) Total	(2) Total	(3) Total Excl. ASB	(4) Total Excl. ASB	(5) ASB	(6) ASB
Sports	-0.004	-0.028	-0.044	-0.014	0.020	-0.005
	(0.036)	(0.028)	(0.028)	(0.023)	(0.019)	(0.016)
Mental	-0.002	0.153***	0.065	0.153***	-0.089**	-0.023
	(0.071)	(0.052)	(0.057)	(0.041)	(0.035)	(0.032)
Emp.	-0.076	0.062	-0.049	0.066**	-0.007	-0.004
	(0.057)	(0.041)	(0.044)	(0.032)	(0.038)	(0.028)
Other	0.015	0.005	0.007	0.031	0.016	-0.040*
	(0.031)	(0.047)	(0.025)	(0.033)	(0.016)	(0.022)
Median	5.200	5.200	3.310	3.310	1.739	1.739
N	126831.000	203228.000	126831.000	203228.000	173376.000	278521.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in continuous monetary terms (£10,000s) on the monthly crime rate at the ward-level for different types of projects. The results are split by whether a ward is classified as rural or urban respectively. The crime rates in each pair of models are total crime, total crime excluding anti-social behaviour (ASB), and anti-social behaviour (ASB). The project types considered here are: sports, mental health, employment & education and other, and are treated as continuous. The median values for each type of crime are also reported. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.44: Intensive Margin - Project Type 2 - Urban/Rural - II

	(1) Property	(2) Property	(3) Violent	(4) Violent	(5) Substance	(6) Substance
Sports	0.023	0.004	-0.030*	-0.006	0.005	-0.001
	(0.016)	(0.011)	(0.018)	(0.015)	(0.005)	(0.002)
Mental	0.077**	0.063***	0.056*	0.097***	-0.002	-0.002
	(0.035)	(0.022)	(0.030)	(0.031)	(0.004)	(0.005)
Emp.	0.001	0.024	-0.045	0.029	0.002	0.008**
	(0.024)	(0.018)	(0.027)	(0.026)	(0.009)	(0.004)
Other	0.019	0.024	0.011	0.021	-0.004*	0.003
	(0.012)	(0.016)	(0.014)	(0.020)	(0.002)	(0.004)
Median	1.419	1.419	1.470	1.470	0.000	0.000
N	173376.000	278521.000	126831.000	203228.000	173376.000	278521.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in continuous monetary terms (£10,000s) on the monthly crime rate at the ward-level for different types of projects. The results are split by whether a ward is classified as rural or urban respectively. The crime rates in each pair of models are property crime, violent crime, and substance crime. The project types considered here are: sports, mental health, employment & education and other, and are treated as continuous. The median values for each type of crime are also reported. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.45: Small Projects and Austerity Estimates

	(1) Total	(2) Total Excl. ASB	(3) ASB	(4) Property	(5) Violent	(6) Substance
GBP Active	0.002	0.005	-0.003	0.023***	0.004	0.002
	(0.018)	(0.014)	(0.010)	(0.008)	(0.008)	(0.002)
Aust. Loss	-0.148***	0.095***	-0.341***	0.021	0.117***	0.008
	(0.051)	(0.035)	(0.036)	(0.023)	(0.023)	(0.006)
Active x Loss	0.033	0.032	-0.041*	0.021	0.081***	-0.006*
	(0.044)	(0.032)	(0.023)	(0.018)	(0.022)	(0.003)
Median	5.200	3.310	1.739	1.419	1.470	0.000
Rural	3.546	2.301	1.070	1.011	0.912	0.000
Urban	6.470	4.059	2.226	1.682	1.855	0.000
Percentile						
N	272356.000	272356.000	395417.000	395417.000	272356.000	395417.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in continuous monetary terms (£10,000s) and whether the ward is significantly exposed to welfare cuts as a result of austerity measures on the monthly crime rate at the ward-level. The crime rates running from models (1) to (6) are total crime, total crime excluding anti-social behaviour (ASB), anti-social behaviour (ASB), property crime, violent crime and substance crime respectively. The median values for each type of crime, and their values in rural and urban wards are also reported. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.46: Robustness Checks - Number of Small Projects Active

	(1) Total	(2) Total Excl. ASB	(3) ASB	(4) Property	(5) Violent	(6) Substance
1.active_projects_cat	-0.003	-0.008	0.012	0.014*	-0.013*	0.003**
	(0.016)	(0.012)	(0.010)	(0.007)	(0.008)	(0.002)
2.active_projects_cat	-0.000	0.019	0.001	0.038***	0.001	0.002
	(0.030)	(0.022)	(0.018)	(0.011)	(0.015)	(0.003)
3.active_projects_cat	0.031	0.017	-0.020	0.027	0.021	0.003
	(0.046)	(0.037)	(0.026)	(0.018)	(0.026)	(0.005)
4.active_projects_cat	-0.042	0.013	-0.118***	0.072**	0.048	0.006
	(0.082)	(0.070)	(0.045)	(0.035)	(0.052)	(0.007)
N	330059.000	330059.000	451897.000	451897.000	330059.000	451897.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects on the monthly crime rate at the ward-level. The respective categories considered here are: 1,2,3 and 3+ active projects. The crime rates running from models (1) to (6) are total crime, total crime excluding anti-social behaviour (ASB), anti-social behaviour (ASB), property crime, violent crime and substance crime respectively. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.47: Robustness Checks - Pre-period Sample Restrictions - I

	(1) Total	(2) Total	(3) Total	(4) Total Excl. ASB	(5) Total Excl. ASB	(6) Total Excl. ASB	(7) ASB	(8) ASB	(9) ASB
£10k	-0.011	-0.006	-0.016	-0.017	-0.018	-0.020	0.013	0.015	0.013
	(0.017)	(0.016)	(0.018)	(0.013)	(0.012)	(0.013)	(0.010)	(0.010)	(0.010)
£10k-£20k	0.013	0.016	-0.007	0.030	0.025	0.022	-0.000	0.003	-0.002
	(0.032)	(0.031)	(0.035)	(0.024)	(0.023)	(0.025)	(0.018)	(0.018)	(0.018)
£20k-£30k	0.017	0.005	-0.015	0.034	0.033	0.029	-0.056*	-0.086***	-0.091***
	(0.054)	(0.055)	(0.060)	(0.044)	(0.044)	(0.048)	(0.029)	(0.031)	(0.031)
¿£30k	0.094	0.048	-0.048	0.169**	0.172**	0.096	-0.141***	-0.186***	-0.217***
	(0.089)	(0.092)	(0.109)	(0.072)	(0.071)	(0.084)	(0.050)	(0.049)	(0.055)
N	330059.000	360469.000	274930.000	330059.000	360469.000	274930.000	451897.000	493948.000	376421.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in monetary terms on the monthly crime rate at the ward-level. The respective monetary categories considered here are: up to £10,000; £10,001-£20,000; £20,001-£30,000; Over £30,000. In addition, we compare the robustness of our main results to changes in the initial sample restrictions for large projects that occur before 2011. In the baseline specifications this is 24 months (i.e. all wards where a large project occurs within 24 months of Jan 2011 are not included in the estimation), but we compare this to estimates that either drop this restriction or assume a 5-year restriction instead. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.48: Robustness Checks - Pre-period Sample Restrictions - II

	(1) Property	(2) Property	(3) Property	(4) Property	(5) Violent	(6) Violent	(7) Violent	(8) Substance	(9) Substance
£10k	0.014*	0.015**	0.010	-0.023***	-0.025***	-0.025***	0.003**	0.004***	0.004**
	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.009)	(0.002)	(0.002)	(0.002)
£10k-£20k	0.035***	0.040***	0.034**	0.018	0.007	0.014	0.001	0.004	0.002
	(0.013)	(0.013)	(0.014)	(0.016)	(0.015)	(0.016)	(0.003)	(0.003)	(0.003)
£20k-£30k	0.041*	0.076**	0.045*	0.049	0.041	0.060*	0.004	0.007	0.003
	(0.022)	(0.031)	(0.024)	(0.030)	(0.029)	(0.031)	(0.006)	(0.006)	(0.006)
¿£30k	0.105***	0.108***	0.117***	0.164***	0.164***	0.083	0.006	0.006	0.001
	(0.037)	(0.037)	(0.043)	(0.055)	(0.051)	(0.058)	(0.007)	(0.008)	(0.007)
N	451897.000	493948.000	376421.000	330059.000	360469.000	274930.000	451897.000	493948.000	376421.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in monetary terms on the monthly crime rate at the ward-level. The respective monetary categories considered here are: up to £10,000; £10,001-£20,000; £20,001-£30,000; Over £30,000. In addition, we compare the robustness of our main results to changes in the initial sample restrictions for large projects that occur before 2011. In the baseline specifications this is 24 months (i.e. all wards where a large project occurs within 24 months of Jan 2011 are not included in the estimation), but we compare this to estimates that either drop this restriction or assume a 5-year restriction instead. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.49: Robustness Checks - Small Project Activity Window - I

	(1) Total	(2) Total	(3) Total	(4) Total Excl. ASB	(5) Total Excl. ASB	(6) Total Excl. ASB	(7) ASB	(8) ASB	(9) ASB
£10k	-0.011	-0.019	-0.017	-0.017	-0.013	-0.023*	0.013	0.008	0.004
	(0.017)	(0.016)	(0.018)	(0.013)	(0.012)	(0.014)	(0.010)	(0.009)	(0.011)
£10k-£20k	0.013	0.028	0.017	0.030	0.057**	0.028	-0.000	-0.028	0.008
	(0.032)	(0.033)	(0.037)	(0.024)	(0.025)	(0.026)	(0.018)	(0.017)	(0.018)
£20k-£30k	0.017	-0.008	-0.013	0.034	0.015	-0.011	-0.056*	-0.062**	-0.030
	(0.054)	(0.053)	(0.062)	(0.044)	(0.041)	(0.051)	(0.029)	(0.030)	(0.031)
¿£30k	0.094	0.162	0.100	0.169**	0.260***	0.108	-0.141***	-0.125**	-0.063
	(0.089)	(0.107)	(0.096)	(0.072)	(0.091)	(0.075)	(0.050)	(0.058)	(0.048)
N	330059.000	330059.000	309559.000	330059.000	330059.000	309559.000	451897.000	451897.000	430374.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in monetary terms on the monthly crime rate at the ward-level. The respective monetary categories considered here are: up to £10,000; £10,001-£20,000; £20,001-£30,000; Over £30,000. In addition, we compare the robustness of our main results to changes in window of time a project is treated as 'active'. In the baseline specifications this is 24 months, but we compare this to estimates that assume a 18 and 24 month window respectively. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.50: Robustness Checks - Small Project Activity Window - II

	(1) Property	(2) Property	(3) Property	(4) Property	(5) Violent	(6) Violent	(7) Violent	(8) Substance	(9) Substance
£10k	0.014*	0.004	0.008	-0.023***	-0.008	-0.018**	0.003**	0.004**	0.003*
	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.009)	(0.002)	(0.002)	(0.002)
£10k-£20k	0.035***	0.036***	0.033**	0.018	0.046***	0.021	0.001	0.002	0.004
	(0.013)	(0.013)	(0.014)	(0.016)	(0.016)	(0.017)	(0.003)	(0.003)	(0.003)
£20k-£30k	0.041*	0.027	0.025	0.049	0.053*	0.036	0.004	0.003	0.003
	(0.022)	(0.021)	(0.023)	(0.030)	(0.028)	(0.031)	(0.006)	(0.005)	(0.005)
¿£30k	0.105***	0.151***	0.080**	0.164***	0.242***	0.118**	0.006	0.009	0.002
	(0.037)	(0.048)	(0.032)	(0.055)	(0.069)	(0.052)	(0.007)	(0.008)	(0.007)
N	451897.000	451897.000	430374.000	330059.000	330059.000	309559.000	451897.000	451897.000	430374.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in monetary terms on the monthly crime rate at the ward-level. The respective monetary categories considered here are: up to £10,000; £10,001-£20,000; £20,001-£30,000; Over £30,000. In addition, we compare the robustness of our main results to changes in window of time a project is treated as 'active'. In the baseline specifications this is 24 months, but we compare this to estimates that assume a 18 and 24 month window respectively. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.51: Robustness Checks - Small Project Sample Restrictions - I

	(1) Total	(2) Total Excl. ASB	(3) ASB	(4) Property	(5) Violent	(6) Substance
£10k	-0.011	-0.042**	-0.017	-0.026	0.013	-0.006
	(0.017)	(0.019)	(0.013)	(0.016)	(0.010)	(0.010)
£10k-£20k	0.013	-0.043	0.030	-0.037*	-0.000	-0.002
	(0.032)	(0.029)	(0.024)	(0.022)	(0.018)	(0.016)
£20k-£30k	0.017	-0.129**	0.034	-0.082*	-0.056*	-0.010
	(0.054)	(0.057)	(0.044)	(0.049)	(0.029)	(0.024)
¿£30k	0.094	-0.013	0.169**	0.041	-0.141***	-0.083
	(0.089)	(0.087)	(0.072)	(0.071)	(0.050)	(0.053)
N	330059.000	285858.000	330059.000	285858.000	451897.000	401723.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in monetary terms on the monthly crime rate at the ward-level. The respective monetary categories considered here are: up to £10,000; £10,001-£20,000; £20,001-£30,000; Over £30,000. The results compare the outcomes across two different samples. The first estimate uses our main sample containing only youth-relevant projects. The second estimate is for the full sample containing all projects. Each pair of models measures the impact on total crime, total crime excluding anti-social behaviour (ASB) and anti-social behaviour. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.52: Robustness Checks - Small Project Sample Restrictions - II

	(1) Total	(2) Total Excl. ASB	(3) ASB	(4) Property	(5) Violent	(6) Substance
£10k	0.014*	0.003	-0.023***	-0.016	0.003**	0.003
	(0.007)	(0.008)	(0.008)	(0.011)	(0.002)	(0.002)
£10k-£20k	0.035***	0.007	0.018	-0.023	0.001	0.000
	(0.013)	(0.011)	(0.016)	(0.015)	(0.003)	(0.003)
£20k-£30k	0.041*	-0.003	0.049	-0.027	0.004	-0.000
	(0.022)	(0.020)	(0.030)	(0.032)	(0.006)	(0.005)
¿£30k	0.105***	0.016	0.164***	0.118***	0.006	-0.005
	(0.037)	(0.035)	(0.055)	(0.045)	(0.007)	(0.007)
N	451897.000	401723.000	330059.000	285858.000	451897.000	401723.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in monetary terms on the monthly crime rate at the ward-level. The respective monetary categories considered here are: up to £10,000; £10,001-£20,000; £20,001-£30,000; Over £30,000. The results compare the outcomes across two different samples. The first estimate uses our main sample containing only youth-relevant projects. The second estimate is for the full sample containing all projects. Each pair of models measures the impact on property crime, violent crime and substance crime. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.53: Robustness Checks - Small Project Spatial Spillovers

	(1) Total	(2) Total Excl. ASB	(3) ASB	(4) Property	(5) Violent	(6) Substance
£10k	-0.019	-0.024*	0.013	0.013*	-0.025***	0.004**
	(0.017)	(0.013)	(0.010)	(0.007)	(0.008)	(0.002)
£10k-£20k	0.004	0.023	-0.000	0.034***	0.016	0.002
	(0.032)	(0.024)	(0.019)	(0.013)	(0.016)	(0.003)
£20k-£30k	0.008	0.027	-0.056*	0.039*	0.047	0.005
	(0.054)	(0.044)	(0.029)	(0.022)	(0.030)	(0.006)
¿£30k	0.086	0.162**	-0.141***	0.103***	0.161***	0.007
	(0.089)	(0.072)	(0.050)	(0.037)	(0.055)	(0.007)
Neigh. Small	0.008	0.003	0.007	-0.006	-0.004	-0.001
	(0.013)	(0.009)	(0.008)	(0.005)	(0.006)	(0.001)
Neigh. Large	0.001	0.005	-0.009	0.010	0.007	-0.000
	(0.016)	(0.011)	(0.011)	(0.007)	(0.008)	(0.002)
N	330059.000	330059.000	451897.000	451897.000	330059.000	451897.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in monetary terms on the monthly crime rate at the ward-level. The respective monetary categories considered here are: up to £10,000; £10,001-£20,000; £20,001-£30,000; Over £30,000. To check robustness to spatial spillovers, dummies for whether neighbouring wards receive small or large projects are also included in the second-stage regressions. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.54: Robustness Checks - Smoothed Moving Average Crime Series

	(1) Total	(2) Total Excl. ASB	(3) ASB	(4) Property	(5) Violent	(6) Substance
£10k	-0.002	-0.011	0.015	0.007	-0.018**	0.003*
	(0.016)	(0.012)	(0.009)	(0.007)	(0.008)	(0.002)
£10k-£20k	0.027	0.042*	-0.001	0.026**	0.019	0.002
	(0.029)	(0.022)	(0.018)	(0.012)	(0.015)	(0.003)
£20k-£30k	0.021	0.039	-0.052*	0.026	0.048	0.001
	(0.051)	(0.040)	(0.030)	(0.018)	(0.030)	(0.005)
¿£30k	0.144*	0.201***	-0.123**	0.107***	0.163***	-0.001
	(0.079)	(0.065)	(0.051)	(0.036)	(0.052)	(0.006)
N	318507.000	318507.000	438796.000	438796.000	318507.000	438796.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the two-stage difference-in-difference estimation results illustrating the average treatment effect of being exposed to different numbers of concurrently active small projects in monetary terms on the monthly crime rate at the ward-level. The respective monetary categories considered here are: upto £10,000; £10,001-£20,000; £20,001-£30,000; Over £30,000. These estimates used a 3-month moving average to smooth the crime series. Standard errors are given in parentheses and are clustered at the ward-level.

Table B.55: ATTs from Heterogeneity Analysis

	(1) Total	(2) Total Excl. ASB	(3) ASB	(4) Property	(5) Violent	(6) Substance
ATT	0.041	0.080***	-0.020	0.058***	0.081***	0.006**
	(0.031)	(0.024)	(0.017)	(0.014)	(0.015)	(0.002)
Median	5.395	3.479	1.724	1.552	1.591	0.091
Rural	3.612	2.389	1.087	1.110	0.994	0.000
Urban	6.760	4.341	2.182	1.877	2.030	0.126
F	0.000	0.000	0.000	0.000	0.000	0.000
N	2850.000	2850.000	2850.000	2850.000	2850.000	2850.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: A table presenting the results from the individual-level synthetic difference-in-difference estimations for the effect of large projects on local crime. Crime rates have been smoothed using a 3-month moving average. The individual-level results are aggregated using a simple WLS approach where weights are the normalised standard errors from the synthetic DID estimations. The median values for each type of crime, and their values in rural and urban wards are also reported. Standard errors are clustered at the ward-level and are reported in parentheses.

Table B.56: Heterogeneity Analysis for Large Projects

	(1) Total	(2) Total Excl. ASB	(3) ASB	(4) Property	(5) Violent	(6) Substance
Rural/Urban	0.033	0.193***	-0.123***	0.116***	0.123***	0.003
	(0.076)	(0.046)	(0.038)	(0.030)	(0.029)	(0.005)
Pop. Dens.	-0.006***	-0.002*	-0.001	0.001	-0.000	0.000***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
Prop. Young	-1.220	-0.271	-0.891**	-1.003***	0.848***	0.127**
	(0.779)	(0.463)	(0.405)	(0.357)	(0.295)	(0.054)
Educ.	1.072***	1.685***	-1.058***	0.311**	1.320***	-0.021
	(0.408)	(0.236)	(0.216)	(0.157)	(0.150)	(0.026)
LR Unemp.	0.489	0.423**	0.086	0.029	0.204	0.006
	(0.320)	(0.215)	(0.211)	(0.126)	(0.135)	(0.024)
Log Inc.	0.287	0.001	-0.057	0.004	0.355***	-0.015
	(0.405)	(0.225)	(0.187)	(0.132)	(0.127)	(0.027)
Log Loss	0.547***	0.264**	0.084	0.074	0.312***	-0.006
	(0.198)	(0.126)	(0.101)	(0.073)	(0.068)	(0.013)
Log Amount	0.031	0.026	-0.027	0.031	0.004	0.004
	(0.050)	(0.037)	(0.021)	(0.029)	(0.020)	(0.003)
Capital	-0.342**	-0.160	-0.092	-0.069	-0.057	0.005
	(0.157)	(0.121)	(0.079)	(0.071)	(0.069)	(0.010)
One-Off	-0.562**	-0.148	-0.006	-0.025	0.017	-0.011
	(0.253)	(0.152)	(0.113)	(0.086)	(0.097)	(0.014)
Repeated	-0.341**	-0.148	-0.088	-0.056	-0.034	0.008
	(0.160)	(0.124)	(0.085)	(0.074)	(0.071)	(0.010)
Sports	-0.051	-0.188	0.071	-0.098	-0.080	0.020
	(0.185)	(0.117)	(0.111)	(0.070)	(0.068)	(0.012)
Mental Health	-0.264	-0.198	-0.154	-0.132	-0.020	0.038**
	(0.260)	(0.180)	(0.116)	(0.116)	(0.104)	(0.015)
Emp./Educ.	-0.179	-0.189	-0.078	-0.270**	-0.057	0.026*
	(0.238)	(0.162)	(0.134)	(0.123)	(0.092)	(0.015)
Constant	-5.297	-2.562	1.049	-0.603	-5.039***	0.028
	(3.520)	(1.911)	(1.652)	(1.143)	(1.077)	(0.222)
Median	5.395	3.479	1.724	1.552	1.591	0.091
Rural	3.612	2.389	1.087	1.110	0.994	0.000
Urban	6.760	4.341	2.182	1.877	2.030	0.126
F	4.850	11.412	8.809	3.858	20.109	4.034
N	2764.000	2764.000	2764.000	2764.000	2764.000	2764.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: A table presenting the heterogeneity analysis results for the effect of large projects on local crime. The individual-level treatment effects are regressed against ward and project-level characteristics to investigate how these impact the magnitude of estimated effects. Type 1 projects can be: capital, one-off, repeated and other. Type 2 projects can be: sports, mental health, employment & education and other. The median values for each type of crime, and their values in rural and urban wards are also reported. Standard errors are clustered at the ward-level and are reported in parentheses.

Table B.57: Large Projects - Spatial Spillovers Test

	(1) Total	(2) Total excl. ASB	(3) ASB	(4) Property	(5) Violent	(6) Substance
Neigh. Small	0.047	0.009	0.036	0.004	-0.010	-0.008*
	(0.039)	(0.029)	(0.024)	(0.014)	(0.018)	(0.005)
Neigh. Large	-0.052*	-0.071***	0.074***	-0.002	-0.070***	-0.007
	(0.031)	(0.023)	(0.022)	(0.013)	(0.015)	(0.004)
Constant	5.577***	3.639***	1.863***	1.613***	1.771***	0.141***
	(0.045)	(0.034)	(0.028)	(0.017)	(0.021)	(0.007)
F	2.203	4.739	6.784	0.052	11.747	1.739
N	250866.000	250866.000	345348.000	345348.000	250866.000	345348.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the spatial spillover regressions illustrating the effect that a neighbouring ward receiving a small/large project has on the monthly crime rate at the ward-level on control units that never receive a large project in our sample period. Crime rates have been smoothed using a 3-month moving average. Standard-errors clustered at the ward-level are reported in parentheses.

Table B.58: Large Projects - Spatial Spillovers - I

	(1) Total	(2) Total excl. ASB	(3) ASB	(4) Property	(5) Violent	(6) Substance
Large	0.009	0.040	-0.012	0.063***	0.071***	0.005
	(0.039)	(0.029)	(0.021)	(0.019)	(0.016)	(0.004)
N	451528.000	451528.000	621584.000	621584.000	451528.000	621584.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the synthetic difference-in-difference estimation results illustrating the average treatment effect of receiving a large project on the monthly crime rate at the ward-level, adjusted for neighbouring large projects. Crime rates have been smoothed using a 3-month moving average. Placebo standard-errors clustered at the ward-level are reported in parentheses.

Table B.59: Large Projects - Spatial Spillovers - II

	(1) Total	(2) Total excl. ASB	(3) ASB	(4) Property	(5) Violent	(6) Substance
Large	0.090	-0.060	0.104*	0.123***	0.018	0.003
	(0.064)	(0.046)	(0.058)	(0.041)	(0.032)	(0.008)
Median	5.395	3.479	1.724	1.552	1.591	0.091
Rural	3.612	2.389	1.087	1.110	0.994	0.000
Urban	6.760	4.341	2.182	1.877	2.030	0.126
Percentile	51.028	48.964	52.939	54.304	50.586	50.716
N	309001.000	309001.000	552684.000	552684.000	309001.000	552684.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the synthetic difference-in-difference estimation results illustrating the average treatment effect of either receiving or having a neighbouring ward receive a large project on the monthly crime rate at the ward-level. Crime rates have been smoothed using a 3-month moving average. The median values for each type of crime, and their values in rural and urban wards are also reported alongside the change in the percentile of the crime distribution that a ward would see given the estimated treatment effect. Bootstrap standard-errors clustered at the ward-level are reported in parentheses.

Table B.60: Robustness: Large Projects - Part I

	(1) Total	(2) Total	(3) Total	(4) Total	(5) Total excl. ASB	(6) Total excl. ASB	(7) Total excl. ASB	(8) Total excl. ASB
Large	0.010	-0.019	-0.028	0.068	0.043	0.031	0.024	0.041
	(0.043)	(0.067)	(0.045)	(0.037)	(0.028)	(0.045)	(0.041)	(0.035)
N	451528.000	445676.000	514822.000	357357.000	451528.000	445676.000	514822.000	357357.000

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: A table presenting a set of robustness checks for the synthetic difference-in-difference estimates of the effect of large projects on local crime. Crime rates have been smoothed using a 3-month moving average. For each type of crime, the baseline model is compared to several alternative estimates. First, we analyse the effect of large projects on the full sample containing all projects. Second, we compare our main results to changes in the initial sample restrictions for large projects that occur before 2011. In the baseline specifications this is 24 months (i.e. all wards where a large project occurs within 24 months of Jan 2011 are not included in the estimation), but we compare this to estimates that either drop this restriction or assume a 5-year restriction instead. Placebo standard-errors clustered at the ward-level are reported in parentheses.

Table B.61: Robustness: Large Projects - Part II

	(1) ASB	(2) ASB	(3) ASB	(4) ASB	(5) Property	(6) Property	(7) Property	(8) Property
Large	-0.014	-0.042	-0.036	0.039	0.063***	0.066*	0.067**	0.051**
	(0.028)	(0.034)	(0.042)	(0.024)	(0.017)	(0.031)	(0.025)	(0.018)
N	621584.000	613528.000	708716.000	491946.000	621584.000	613528.000	708716.000	491946.000

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: A table presenting a set of robustness checks for the synthetic difference-in-difference estimates of the effect of large projects on local crime. Crime rates have been smoothed using a 3-month moving average. For each type of crime, the baseline model is compared to several alternative estimates. First, we analyse the effect of large projects on the full sample containing all projects. Second, we compare our main results to changes in the initial sample restrictions for large projects that occur before 2011. In the baseline specifications this is 24 months (i.e. all wards where a large project occurs within 24 months of Jan 2011 are not included in the estimation), but we compare this to estimates that either drop this restriction or assume a 5-year restriction instead. Placebo standard-errors clustered at the ward-level are reported in parentheses.

Table B.62: Robustness: Large Projects - Part III

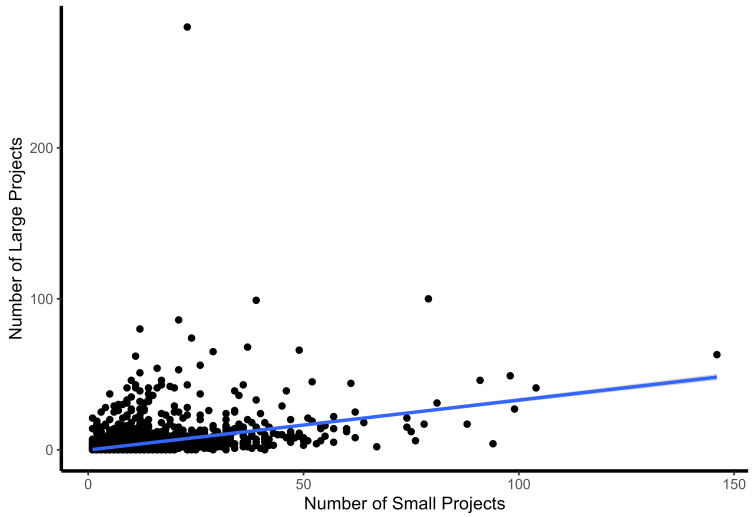
	(1) Violent	(2) Violent	(3) Violent	(4) Violent	(5) Substance	(6) Substance	(7) Substance	(8) Substance
Large	0.073***	0.067*	0.104***	0.046*	0.005	0.008	0.009*	0.001
	(0.017)	(0.031)	(0.023)	(0.019)	(0.004)	(0.005)	(0.004)	(0.004)
N	451528.000	445676.000	514822.000	357357.000	621584.000	613528.000	708716.000	491946.000

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: A table presenting a set of robustness checks for the synthetic difference-in-difference estimates of the effect of large projects on local crime. Crime rates have been smoothed using a 3-month moving average. For each type of crime, the baseline model is compared to several alternative estimates. First, we analyse the effect of large projects on the full sample containing all projects. Second, we compare our main results to changes in the initial sample restrictions for large projects that occur before 2011. In the baseline specifications this is 24 months (i.e. all wards where a large project occurs within 24 months of Jan 2011 are not included in the estimation), but we compare this to estimates that either drop this restriction or assume a 5-year restriction instead. Placebo standard-errors clustered at the ward-level are reported in parentheses.

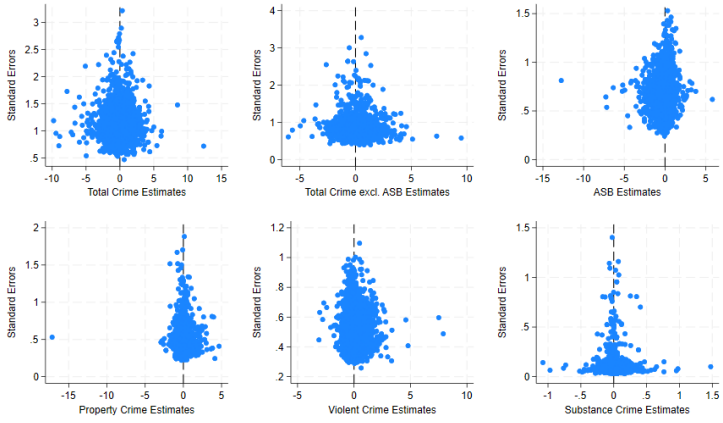
B.2 Additional Figures

Figure B.1: Correlation between Large and Small Ward-level Awards (2011-19)



Notes: Scatter plot of the relationship between the number of small and large projects funded in each ward over the years 2011-2019.

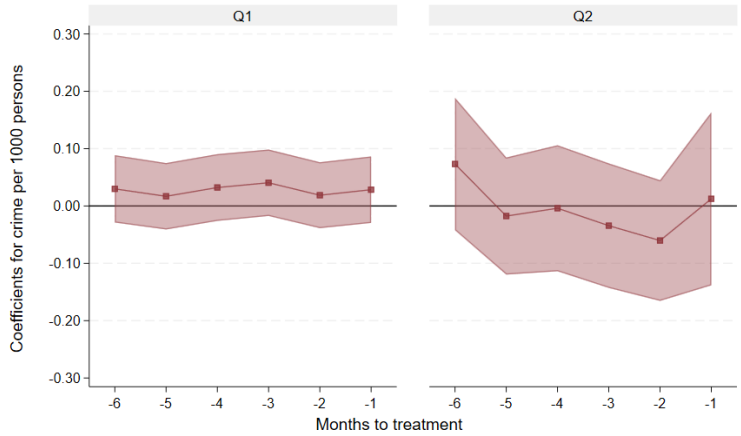
Figure B.4: Correlation between Large and Small Ward-level Awards (2011-19)



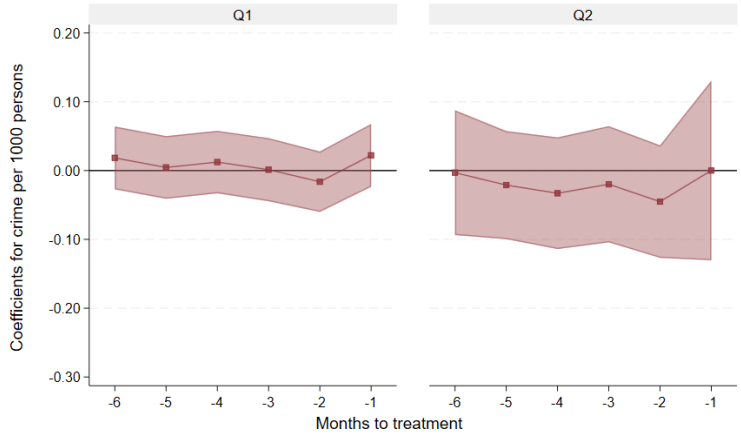
Notes: Funnel plots for the ward-level synthetic difference-in-difference estimates for the ATT of large projects.

Figure B.2: Pre-Trend Plots by Type of Crime by Quartile - I

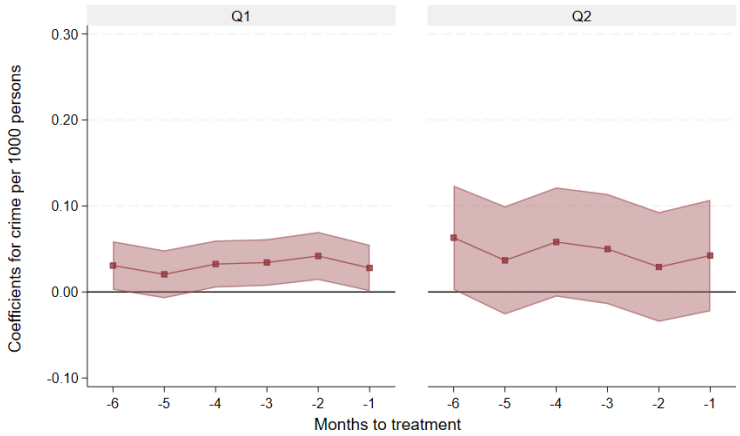
(a) Total Crime



(b) Total Crime Excl. ASB



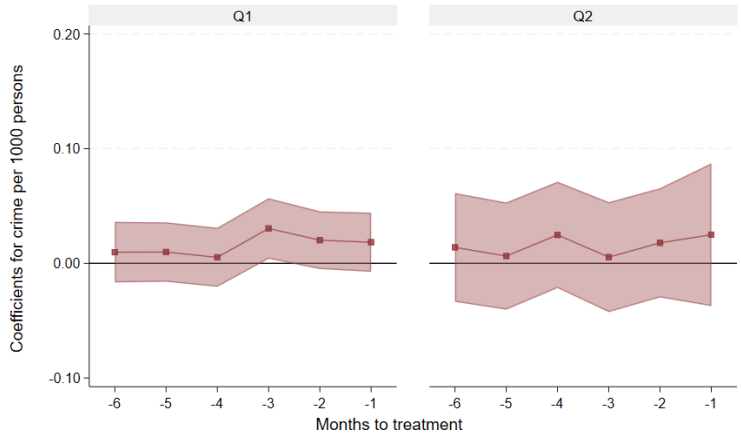
(c) Anti-social Behaviour



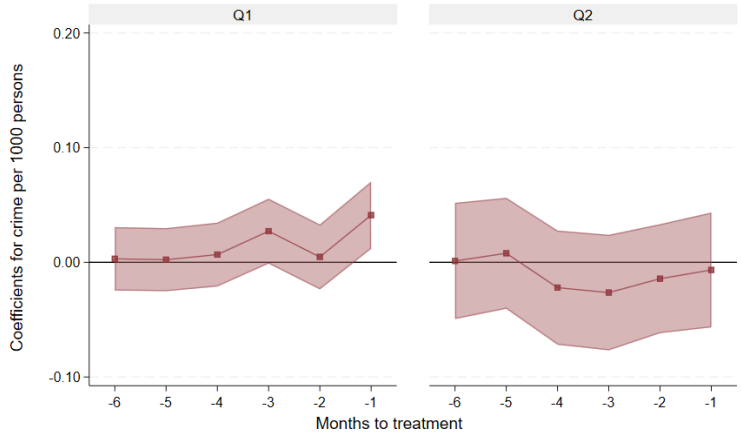
Notes: Event study plots estimated using the testing approach suggested in Borusyak et al. (2021) for wards above or below the median in average monthly crime. The dependent variable is reported by type of crime per 1,000 residents. A 95% confidence interval based on standard errors clustered at the ward-level is included around the estimates.

Figure B.3: Pre-Trend Plots by Type of Crime by Quartile - II

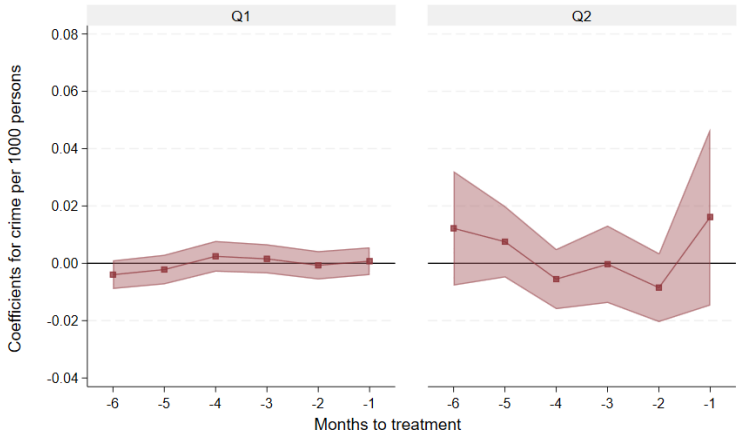
(a) Property Crime



(b) Violent Crime



(c) Substance Crime



Notes: Event study plots estimated using the testing approach suggested in Borusyak et al. (2021) for wards above or below the median in average monthly crime. The dependent variable is reported by type of crime per 1,000 residents. A 95% confidence interval based on standard errors clustered at the ward-level is included around the estimates.