Police Infrastructure, Police Performance, and Crime: Evidence from Austerity Cuts*

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Abstract

The effect of neighbourhood police on citizens' welfare is poorly understood. Yet, this is a key parameter in evaluating the impacts of reductions of police spending. I exploit a large wave of austerity cuts to police forces in London, which resulted in the closure of 70% of police stations and led to a major reshuffling of police workforce from closed to surviving stations. I combine novel granular data on reported crime, location of police stations and their closure, and information on individual crimes' judicial outcomes. I show that the reduced local police presence led to a persistent significant increase in violent crimes, consistent with lower deterrence, and reduced clearance rates, indicating lower police effectiveness. I also provide suggestive evidence consistent with reduced reporting of non-violent crimes, as citizens internalise a higher reporting cost. Overall, the policy led to a sizeable reduction in citizens' welfare, which I document by showing a decrease in house prices concentrated in high-crime and more deprived census blocks, further exacerbating already existing inequality. Together, the closures produced considerable distributional and efficiency losses, and generated costs that substantially outweigh the benefits in terms of lower public expenditure for the criminal justice system.

JEL Classification: H72, K40, R53.

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1 Introduction

Provision of public safety is a central responsibility of national governments. Because of this, establishing the optimal allocation of public funding for crime prevention is a major public policy challenge. In recent years, while in the US the public opinion was urging a reduction in police funding and a restructuring of police departments, many European governments had already slashed police budgets as a result of fiscal adjustments.[1] Yet, little research investigates the trade-offs that law enforcement agencies face between promoting crime prevention and social welfare, while keeping public budgets under control.

Comprehensively assessing the impacts of police spending reductions is difficult. The existing work employs city-level or police-force level sources of variation in funding to identify the reduced-form effect of police resources on crime (Evans and Owens, 2007; Machin and Marie, 2011). Focusing on aggregate areas, however, limits our understanding of the police production function *black box* (Cook and Ludwig, 2010; Durlauf and Nagin, 2011), and prevents us from drawing policy-relevant conclusions about which policing strategies best promote public safety and social welfare (Owens, 2020). This paper fills this void by examining whether a specific margin of police spending, which relates to the presence of police stations, can have both local and aggregate effects on community safety and social welfare.

I provide novel empirical evidence on the multi-dimensional impacts of police spending reductions, that result in the closure of local police stations, on crime prevention and local welfare. To do so, I study a natural experiment generated by a massive wave of police station closures in London. To comply with centrally-imposed austerity cuts, the London police shut down 70% of their police stations, while centralising police workforce from closed to surviving stations. As an immediate effect, the closures significantly increased the distance to the police stations, a key determinant of police deployment. Consequently, by means of increasing response time and reducing deterrence, they possibly harmed police effectiveness. Yet, stationing police resources together could also have produced potential efficiency gains. I shed light on the ex-ante ambiguous effects of such unprecedented cuts on crime and police performance and I measure the unexplored costs for the local communities that law enforcement agencies aim to protect.

Analysing the immediate impacts of police station closures on crime requires access to information on the exact location of offences and stations over time. I combine three extremely

[1] See `https://www.nytimes.com/2020/06/08/us/what-does-defund-police-mean.html`. Countries that reduced funding to police departments are Austria, Belgium, Denmark, France, Germany, Ireland, and the UK (Fyfe et al., 2013).

granular datasets to identify how distance to police matters for crime, police effectiveness and social welfare. First, I geo-code all London police stations and collect their dates of closure. Second, I complement them with geo-referenced information on the universe of criminal incidents recorded by the London Metropolitan Police Service (MPS), with their occurrence dates and crime type. Each closure is linked to a census block and to the total number of incidents recorded in that block.[2] I map each incident to the outcome of the criminal investigation and observe the actions taken by the police and the courts following the crime being reported. Lastly, I employ a database with the universe of geo-referenced house sales. This allows me to construct a block-level panel on the incidence of the closures, the number of reported incidents, clearance rates and judicial outcomes, and the local house prices.

Identifying the effects of police stations poses few empirical challenges. First, police stations are originally located in persistently high-crime areas. Second, the closures are not randomly distributed across neighbourhoods, affecting stations located in relatively less deprived, low-crime blocks. I overcome the potential endogeneity by adopting a difference-in-differences strategy. I compare the number and composition of crimes in blocks which experience a closure of the nearest police station (treatment) to blocks which do not (control), before and after the closure. I further control for changes in time-varying attributes at the neighbourhood level, such as in local funding and public service provision. I show that treatment and control areas, despite not having same level of crime intensity, follow similar trends in outcomes prior to the closures. Thanks to the granularity of my data, I compute the distance between the centroids of the census block and the stations. Because of the closures, the average distance to the nearest police station doubled, with the highest rise being experienced by areas located at baseline closer to the stations. I therefore further exploit the intensity of the treatment to investigate the non-linearities of the treatment effects.

I present four sets of results. First, I investigate the effect of police station closures on reported crime. I find that in treated blocks violent crimes, measured as assaults and murders, increase by 11% with respect to the baseline mean. Additional violent crime types, such as robberies, increase by 1%. This translates into a total of 4 more violent offences recorded per treated block per year. This effect is sudden, persists overtime, and is concentrated in blocks surrounding closed stations, suggesting that higher distance lowers police deterrence, and therefore increases the incidence of high-severity crimes, such as violent crimes. I further

[2] I define as census blocks the so-called Lower Layer Super Output Areas (LSOAs), small geographical areas constructed by the Office for National Statistics (ONS) and containing on average 700 households in London.

rule out that the results are driven by criminals' displacement. I finally test if relocation of front-line police officers to surviving stations contributes to the positive coefficients. Despite the existence of positive spillovers from control areas, I estimate a substantial positive aggregate effect on the total number of violent offences. This evidence illustrates the efficiency losses from blind reductions of police spending.

I further present suggestive evidence of a reduction in citizens' reporting. Greater distance to the stations increases the reporting cost, and discourages residents from reporting crime. Consistent with this mechanism, I document that total reported crime persistently drops following the closures. While I cannot directly measure reporting, I detect the reporting effect by exploiting the heterogeneity across crime types, and I focus on low-severity incidents, such as property offences, which typically have lower marginal returns to report. I find that in treated areas reported property crimes decline by 2%. In line with this interpretation, I further document an increase in burglaries, a particular type of property crime which is not affected by reporting, given that a police report is required to claim insurance coverage.

Second, I examine the net impact on police effectiveness. In the aftermath of the cuts, the MPS claimed that the closures would have led to a more efficient reallocation of resources, improving police ability to prosecute and investigate, i.e. to *clear*, crimes (MOPAC, 2015, 2017). More generally, in light of the reduced pool of reports, we could expect that the proportion of reported crimes that are cleared by the police would have increased, with no implied costs for the police effectiveness. Instead, I find that following the closures the police likelihood of clearing a crime falls by 0.6 percentage points, equivalent to a 3% drop with respect to the baseline clearance rate. This translates into 1 less cleared offence per treated block per year. I report a reduction in convictions for both violent and property offences that in the long term may reinforce the adverse effects on crime through lower incapacitation of offenders. This decline in clearance is exclusively attributed to a deterioration in police ability to investigate and collect evidence necessary to clear up crimes, and not to changes in reporting or in crime composition. To explore the mechanisms through which these effects are mediated, I exploit the intensity of the treatment and find stronger effects in blocks located closer to the stations at baseline, suggesting that distance affects clearance rate through slower response time.

Third, I show that shutting down police stations reduces the social welfare of local residents. Intuitively, house prices reflect both the direct costs, associated with crime changes, and the indirect costs (e.g. loss of local amenities and changes in perceptions of safety) that arise as a result of the closings (Rosen, 1974; Thaler, 1978). I find that the station closures decrease local house prices. In monetary terms this translates into a total loss for private home-owners of £220 million. These results suggest that station closures lower individuals' valuations of local areas, burdening local communities with part of the costs of police infrastructure loss. Such findings directly speak to a largely established fact in the economics of crime (Gibbons, 2004; Linden and Rockoff, 2008; Besley and Mueller, 2012), that is the inverse relationship between local crime and citizens' willingness to pay for local housing. Finally, I find that the distributional consequences are substantial: the average effect is driven by crime hot spots and deprived blocks, further exacerbating existing inequalities between poor and rich areas. Adopting a capitalisation approach, I compute that for every pound saved by the public authorities, up to 2 pounds are paid back by the local residents in terms of foregone house valuations.

Finally, I evaluate the cost-effectiveness of the cuts to police resources. While supporters of the closures point to the benefits that this reform generates for the public finances, the net welfare consequences are unclear. I first carry out a cost-benefit analysis and show that the accrued savings for the criminal justice system do not outweigh the fiscal and social costs induced by the closures because of lower deterrence. Finally, adopting the recent approach developed by Hendren and Sprung-Keyser (2020), I calculate the marginal value of public funds (MVPF) of these cuts. I find that for each pound saved by the public administration, £1.75 of additional costs are borne by the society. These findings help quantify the complex failures of austerity cuts to police and may assist policy-makers in designing better compensation schemes to implement financial consolidation policies more effectively.

This paper is related to the large existing literature that estimates the impacts of exogenous changes in police presence on crime. Most academic work focuses on measuring police-crime elasticities, exploiting sudden increases in police presence driven by terrorist attacks (Di Tella and Schargrodsky, 2004; Klick and Tabarrok, 2005; Draca et al., 2011).[3] Machin and Marie (2011), Evans and Owens (2007) and Mello (2019) exploit variation in funding to the police at the aggregate level to identify the reduced-form impacts on crime. Blattman et al. (2021) evaluate a street-level intervention in Bogotá and find that increasing police presence in high-crime streets has small direct effects on local crime, while significant spillover effects on neighbouring streets. Thanks to the granularity of my data, and to the very local level of variation that I

[3]Since Becker (1968), most scholars and policy-makers considered police manpower as the most effective input to deter crime. For a summary of the literature on police and crime, see Levitt and Miles (2006, 2007), Nagin (2013), Chalfin and McCrary (2017).

exploit, I offer a bridge between the geographically detailed data analysed in randomized experiments and the aggregate data usually available at the municipality level. Blanes i Vidal and Mastrobuoni (2018), Mastrobuoni (2019) and Weisburd (2021) are notable exceptions that use GPS level data on precise locations of patrolling units to measure the effect of police patrol on crime. I complement this work by further producing estimates of a policy-relevant parameter of distance to the station on a direct measure of police effectiveness. Similarly to Bindler and Hjalmarsson (2019), who evaluate the effect of creating a centralized professional police force, I study the consequences of a permanent change in the police resources.[4]

This paper adds to the emerging literature that specifically study the efficiency of the police in clearing crimes. Mastrobuoni (2020) looks at the impact of adoption of predicting policing on police productivity, measured as clearance rate. Ba et al. (2021) looks at how the allocation of police officers to districts, keeping resources constant, affect crime, arrests and use of force. Adda et al. (2014) show that the de-penalisation of cannabis possession allowed the police to efficiently relocate resources. My paper is most closely related to Blanes i Vidal and Kirchmaier (2018), where the authors exploit discontinuities in the distance to response stations to show that increased response time lowers the clearance rate by reducing the likelihood of an immediate arrest and of a suspect to be identified. Consistently with their results, I find that an increase in distance worsens police effectiveness. I complement their findings by exploring the complex relationship between crime and clearance that works through deterrence, reporting and incapacitation. Importantly, I also provide suggestive evidence on the extent to which citizens' reporting behaviour reacts to changes to the police organisation, an aspect largely neglected by the existing literature.[5]

Finally, this paper expands our knowledge on the adverse consequences of austerity policies by studying the complex impacts of budget cuts to policing.[6] Two recent studies focus on the welfare reforms targeting individual benefits in the UK and estimate the impacts on the Brexit vote (Fetzer, 2019), and on the the spatial concentration of crime (Giulietti and McConnell,

[4]Blesse and Diegmann (2018) similarly exploit a reorganisation of the police local administration in German municipalities, pointing at the deterrence effect of police stations. Morales-Mosquera (2019) exploits investment in new police stations in Bogotá to measure citizens' willingness to pay to avoid crime.

[5]There is a relatively small literature on the under-reporting of crime that measure the effectiveness of policies to increase an individual's willingness to report a crime (Jacome, 2018; Miller and Segal, 2019; Ang et al., 2021). Because I look at the reporting effect following an increase in the cost residents' face to report crime, I also contribute to the existing work that measures the impact of changes in the cost to access public services (Ba, 2018; Deshpande and Li, 2019) .

[6]Most existing research examines how austerity cuts affect political outcomes, such as political unrest (Ponticelli and Voth, 2020), voting outcomes (Dal Bó et al., 2018) and corruption (Daniele and Giommoni, 2021).

2020), showing that austerity cuts disproportionately hit already deprived areas. This paper complements previous research by evaluating a so far neglected but equally relevant aspect of austerity, that is, the austerity-driven place-based policy that reduced local police resources. Quantifying the total costs of cuts to police forces might indeed guide policy makers to appraise the shadow price of public savings and to better design compensation schemes for the losers.

The remainder of the paper is organised as follows. Section 2 illustrates the context of my analysis, giving details on the police structure in London and on the process of station closures. Section 3 describes the data sources and the sample used. Section 4 outlines the empirical strategy used for the identification of the causal effects of police closures on outcomes and discusses the threats to identification. Section 5, 6 and 7 present the main findings on crime, clearance, and house prices and the various channels of impact. Section 8 performs a cost-benefit analysis to quantify the impacts and Section 9 finally concludes.

2 Institutional Context

2.1 Police structure in London

The Metropolitan Police Service (henceforth, MPS) is the police force responsible for law enforcement in the metropolitan area of Greater London, serving around 8.9 million people. The MPS territory is organised into 32 divisions, called Borough Operational Command Units (BOCUs), which correspond to the 32 London Local Authorities (LAs), excluding City of London which is policed by the smaller City of London Police.[7] Each territorial division is responsible for the neighbourhood patrolling and the incident response functions, and is equipped with a Command Control Call Centre, responsible for taking telephone calls and for dispatching them to deployment units when necessary.[8] Each division is then organised in police stations from which the neighbourhood teams serving the local communities and the response teams, that patrol and respond to emergencies, operate. All operating police stations in London have front-counters, which offer face-to-face contact for reporting crimes or collecting and returning forms, and Safer Neighbourhoods bases which act as point of contact with the local communi-

[7] London has 33 Local Authorities (hereafter, LAs), including Westminster, which represent the local government units in England and are the main responsible for the provision of local public services, such as education, waste collection, social housing and policing. The basic street-level policing boundaries of each division exactly matched with the ones of London boroughs.

[8] The patrolling and emergency response functions are entirely managed within the division boundaries, with only 1% of police deployments being cross-border.

ties. Front-counter services are primarily provided in police stations by neighbourhood officers and back-office staff.

2.2 Budget cuts and closures of police stations

In 2010, the UK central government launched the Comprehensive Spending Review, which included an overall 20% real term reduction in central government funding grant to all police forces in England and Wales (HMIC, 2011).[9] Following these cuts, since 2010 more than 600 out of 900 police stations in England shut down. The MPS had also to undergo a large budgetary consolidation as the central government cut the funding by 29% . Consequently, the Mayor of London approved a number of plans aimed to curb policing expenses.[10] As a result, the MPS set the target of leaving only one front counter open per territorial division, and begun to drastically reduce the number of stations, closing several front counters and selling police buildings.[11] Figure 1 shows the stock of police stations operating in Greater London between 2008 and 2018: the number of stations dropped from 160 in 2008 to 45 in 2018. Around 80% of all closures took place in 2013. Figure 2 shows the boundaries of the 32 police divisions, overlapping with the borders of the LAs, and the geographical distribution of the police stations in Greater London: the average number of stations per LA declined from around 5 in 2010 to 2.4 in 2016, and to 1.3 in 2018, with all commands experiencing at least one closure in 2013.

I characterise the census blocks where police stations were initially located and then closed. I perform regressions of indicators for the presence, the closure or the sale of a police station respectively on crime rates and house prices measured in 2008-2010, before any closure happened. Table 1 displays the results of this exercise. First, police stations were initially located in areas with significantly and persistently higher levels of crime and house values than areas without police stations. Second, the MPS effectively chose to close stations located in relatively low-crime areas with respect to areas where stations remained opened.

The police authorities claimed that the plan had no detrimental impact on criminality, police

[9] 73% of the total fiscal tightening planned in the Spending Review came from cuts to public spending (Crawford et al., 2011). The Home Office and the Justice departments, together with the education, culture and communities departments, underwent the largest cuts to their budget, equal to around 26%, with respect to a total average cut of 12%.

[10] 30% of MPS funding comes from central government transfers, while the rest from local taxation (Morse, 2015). Between 2012 and 2016, the MPS made £600 million savings and needed to save additional £400 million by 2022 (MOPAC, 2013, 2017).

[11] Front counters are stations where the public can have face-to-face contact with the police. Prior to the closures, all police stations in London had a front counter. In this context, closing a front counter is equivalent to releasing the whole building.

performance nor confidence rates as front counters were untapped by residents to report crimes (MOPAC, 2013).[12] Rather, they argued that dismissing front counters would yield sizeable savings in infrastructure maintenance and operating costs, possibly allowing resources to be shifted to front-line policing (MOPAC, 2015, 2017).[13] Their argument was grounded on the consideration that, as only few people reported crimes by directly walking into the station, instead by phone or internet, closing front counters could have only marginally affected residents' reporting behaviour.[14] They did not expect any change in their response times given that, although deployment teams had specific parade sites where to start and finish their tour of duty, they were trained to respond to incidents from patrolling locations. As claimed by the police, most of the savings were in the form of foregone running costs. The MPS committed to keep the number of front-line officers constant, as shown in Figure 3, at the expenses of the back-office staff and, to a larger extent, of the infrastructure, under the assumption that the most effective strategy to fight crime is through the use of police manpower deployed on the streets, and not through the maintenance of police infrastructure.[15]

Figure 4 Panel A shows the yearly trend in reported crime rates per thousand of people between 2010 and 2016. The overall crime rate was on a downward trend before 2014, and then it started to rebound afterwards, while the violent crime rate increased by 40% between 2013 and 2016. Panel B displays instead respectively the total number of emergency calls recorded by the MPS police between 2010 and 2016, the average response time for all offences and for violent offences only. Despite a 20% reduction in the number of calls, the average response time to emergency calls started to increase after 2013 for all offences, and for violent offences only too. In spite of a lower demand for police services, proxied by a lower number of reported

[12]The police station closures triggered a harsh debate between the public opinion and the police authorities. Local communities were concerned that the closures of front counters would have reduced the speed of response from the police as well as the perceived presence of the police on the territory, leading to an increase in criminality and a loss of trust towards public authorities. See, for instance, https://commonslibrary.parliament.uk/police-stations-are-they-a-thing-of-the-past/.

[13]To offset the impact of the closures, the police put in place alternative measures to ensure public access to policing services, by improving Internet-based and telephone contact methods and launching contact points, located in the remaining existing buildings for non-urgent face-to-face communications.

[14]Around 8% of criminal incidents were reported via face-to-face contact in 2011. This share dropped to 6% by the end of 2016. Throughout the sample period share of incidents reported by phone remained stable and equal to around 90% (source: FOI).

[15]According to my calculation using MPS data at the LA level, between 2010 and 2016, only 1% of front-line officers employed by the MPS was dismissed, while 60% of police staff and police support officers was let go. While the former are mainly employed for response functions, the latter's main duties are: police high visibility patrolling, tackling anti-social behaviour, dealing with minor offences, crowd controlling, directing traffic. Back office roles include administrative or clerical jobs carried out by civilians such as training, finance and HR, middle office roles such as processing intelligence, call handlers, and preparing files for court.

incidents and emergency calls, police capacity to attend calls did not improve and response time spiked. Panel C of Figure 4 plots a measure of civilian crime reporting, borrowed from Ang et al. (2021): the ratio of emergency calls for violent incidents to the number of violent offences. This measure indicates, for a given violent offence, how likely a community is to call the police. After 2013, crime reporting dropped by 40%. These stylised facts motivate my subsequent empirical analysis.

3 Data and Descriptive Evidence

Police station closures. To study the impacts of police station closures, I construct a novel database including all the existing police stations between 2009 and 2018 in Greater London. I gather information from FOIA requests lodged to the MPS on the universe of police stations, with their exact location and their date of opening and closure. I then geo-locate all police stations and map them to their census blocks, that is, small-level geographies with a target population of about 800 households and an average size of just above 0.25 square miles.[16] For each station I also collect information on whether the building was sold and, if sold, the destination of the regenerated building. I therefore obtain a list of 168 police stations operating between 2009 and 2018. In the empirical analysis, I focus on the period 2011-2016. During this period, the number of police stations in London dropped by around 50%. I compute the geodesic distance between the centroid of a census block and each police station's exact geographical location. I measure such distance conditional on the police station being in the same LA as the census block, since throughout my sample period the law enforcement in London was managed at the police division, i.e. the LA, level. Figure 5 shows the distribution of the distance to the nearest station before and after the closures: between 2010 and 2016 the median distance to the closest police stations more than doubled, moving from 1.3 km to 3 km. Figure 2 displays the location of all operating and closed stations, which drives the variation that I will exploit to identify the causal effects of the police station closures.

Crime records and investigation outcomes. I employ the universe of criminal incidents recorded by the MPS between 2011 and 2016. Data include information on the monthly date,

[16]The census blocks considered in the analysis are the so-called Lower Layer Super Output Areas (LSOAs), geographical layers developed by the Office for National Statistics (ONS) for statistical purposes, which in London contains on average 700 households. There are 4,835 LSOAs in London, designed to fit the boundaries of the local authorities.

the type of offence and the geographical coordinates of each criminal incident.[17] The original dataset contains around 7 million criminal offences. All criminal incidents are geo-located, mapped and then mapped to their census block. To account for the prevalence of zeros in the types of crimes, I follow Bellemare and Wichman (2020) and use as transformation of the variables the inverse hyperbolic sine transformation (*asinh*). This linear monotonic transformation behaves similarly to a log-transformation, except for the fact that it is defined at zero. The interpretation of regression estimators in the form of *asinh* is similar to the interpretation of a log-transformed variables.[18]

From 2011 onwards, for each criminal incident, I also observe the outcome of the criminal investigation, which describes, if applicable, the action taken by the police or the court following a crime being reported.[19] The dataset contains around 3.5 million incidents with the final outcome of their prosecution process, that is assigned by the police or the court.[20]

Figure B1 charts how incidents are processed in the criminal justice system in England. The figures report percentages for the period before the closures, i.e. 2011-2013. As reported by the chart, almost 20% of incidents are cleared, i.e. they are charged by the police or the court at the of the police investigation.[21] Out of all the charged cases, 65% are charged with a formal court sentence, while the remaining are resolved with an informal (i.e. out-of-court) sanction, that applies in cases of minor offences, or offences which do not meet the public interest criterion of the CPS. Only the police (or the CPS) can assign informal sanctions in case of less severe offences. The clearance rate greatly varies across crime types, reflecting the severity of the offences, the difficulty in identifying a suspect and the amount of required evidence (Home Office, 2016). For instance, for crimes directly detected by the police, such as drugs and weapon possession, the offender is usually identified when the crime comes to the

[17] Appendix Table B3 reports the list of the 16 offence categories provided by the MPS with thei relative frequency.

[18] The inverse hyperbolic sine (*asinh*) is defined as $log[y+\sqrt{y^2+1}]$. Except for small values of y, $asinh(y) = log(2)+log(y)$. Bellemare and Wichman (2020) derive the elasticity of y with respect to x for a specification like $asinh(y) = \beta x+u$. For a continuous x this is $\frac{\beta x}{y}\sqrt{y2+1} \approx \beta x$ for $y \geq 2$. For empirical applications see, for instance, Bahar and Rapoport (2018) and Card et al. (2020).

[19] Appendix Table B4 shows the definition of all the outcome types that can be assigned to a criminal offence by the police or the court.

[20] 60% of criminal incidents report a valid investigation outcome. 95% of criminal incidents without a valid outcome are anti-social-behaviour incidents, which are never investigated by the police.

[21] According to the Home Office definition, a crime is considered cleared if all the following conditions are met: (1) a notifiable offence (crime) has been committed and recorded, (2) sufficient evidence is available to claim a detection, i.e. the Crime Prosecution Service (CPS) evidential test is satisfied; (3) a suspect has been identified and made aware that they will be responsible for committing that crime; (4) the suspect has been charged, reported for summons, or cautioned, been issued with a penalty notice for disorder or the offence has been taken into consideration when an offender is sentenced.

attention of the police.[22]

I follow Blanes i Vidal and Kirchmaier (2018) and define the main measure of police performance as the probability of a criminal incident to be investigated, prosecuted and solved, i.e. *cleared*, conditional on a police investigation taking place. This corresponds to the second node of the chart B1. I compute two additional indicators that I will use in the empirical analysis. First, I measure if the criminal investigation was solved with a court or a police decision (node 2 of chart B1). Second, I use convictions, defined as incidents that are sanctioned by the court to imprisonment, fines or other sentences, which exclude acquittals and discharges amount to 77% of all court sentences (node 3 of chart B1).

House prices. I use administrative records from the UK Land Registry on the universe of house transactions from 2010 to 2016. Every transaction records the date, price paid for the house.[23] All house transactions are geo-located and linked to their census blocks to build the average house price at the block level.

Sample selection and summary statistics My analysis is based on the universe of the census blocks located in Greater London. I exclude from the sample all blocks located in the boroughs of City of London and Westminster due to the very distinctive administrative features of these areas and to the fact that crime records without a physical locations are conventionally attributed to these LAs. Furthermore, I exclude periods subsequent to December 2016, as the MPS Police restructured the entire territorial governance into larger geographical areas.[24] The final sample consists of a monthly panel of 4,701 census blocks, observed between January 2011 and December 2016.

Table 2 and 3 show the summary statistics for all the variables used in the analysis. The average distance to the nearest station at baseline is 1.4 km, which increased to 2 km on average after 2013.

[22]For instance, while around 95% of thefts remain unsolved, more than 65% of drugs and possession of weapons offences are cleared. Appendix Figure B2 provides a full break-down of the investigation outcome by crime type.

[23]The UK Land registry also records for each property the house type (detached, semi-detached, terraced, flat), the house age (newly built or old), and the contract type (leasehold or freehold).

[24]The territorial division was changed at the end of 2016, whereby the 32 division system was substituted by a 12 larger Basic Command Units.

4 Empirical strategy

My identification strategy exploits the time and spatial variation in the police station closures in Greater London, which give rise to changes in the distance between census blocks and their nearest station. I define treated units as areas which experience an increase in the distance to the nearest police station solely induced by station closures. Control units are areas whose nearest station never closed. My design compares blocks whose distance from the closest police station remains unchanged to blocks which instead experience an increase in distance. In the empirical analysis, I will further exploit the variation in the intensity of the treatment using as continuous treatment variable the (log) distance from the closest police station.

My main estimating equation is:

$$y_{it} = \beta Closed_{it} + \phi_i + \phi_{lt} + \varepsilon_{it} \tag{1}$$

where $y_{i,t}$ is the outcome of interest of block i at time t. $Closed_{it}$ is an indicator variable equal to 1 if the closest police station to census block i shuts down at time t (and remains equal to one afterwards). ϕ_i are block fixed effects and ϕ_{lt} are LA-specific time dummies: while the former capture any time-invariant characteristics of the blocks, the latter absorb all time-varying changes that occur at the LA level, such as changes in the local policing strategy and in the local provision of public services, which are both administered at the LA level. The estimate of β provides the causal effect of police station closures on three sets of outcomes, namely crime, clearance and house prices. As in a standard difference-in-differences model, the common trend assumption is required to hold. I will discuss this assumption in the next sub-section 4.1. Standard errors are clustered at the census block level, allowing for serial correlation over time (Bertrand et al., 2004).[25]

A caveat in the treatment definition arises from the fact that in principle blocks might be treated more than once, if, for instance, after the closest station shuts down, also the second closest one is removed, and so on. Only 8% of treated blocks are treated more than once (specifically, 183 blocks were treated twice and 30 three times), however this might complicate the identification strategy. To address this issue, in the empirical analysis I adopt an Intention-to-Treat approach focusing on first closures only, and I define a block as treated if its nearest

[25] In Appendix B, I will also show results computing Conley Standard errors (Conley, 1999) to account for both spacial and serial correlation of the errors. The cut-off distance is set to up to 3 km, larger than the average radius of a LA.

police station at baseline closed.

To further exploit the variation in the intensity of the treatment, I first investigate the relationship between the binary treatment, the closure dummy, and the continuous variable, the (log) distance to the closest police station. First, the initial location of blocks predicts the extent of the subsequent changes in distance provoked by the closures: while the relationship between the initial and current distance is linear (Figure B3 Panel A), blocks at baseline closer to stations display a larger relative increase in distance (Figure B3 Panel B). To confirm this fact in a regression framework, I regress the (log) distance on the discrete treatment interacted with indicators for the quintiles of the baseline distance. I plot the estimates in Figure 6: initially closer census blocks display indeed greater jumps in distance after the closures, while for block originally located farther, the effect fades out.

Throughout my empirical analysis, I show and compare estimates using both the discrete and the continuous treatment variables. Although the binary treatment predicts on average more than 90% of the changes in distance, however, given that the treatment intensity largely varies depending on the pre-determined location of blocks, I will assess the existence of non-linearities along the distance distribution.[26]

4.1 Event study design

I estimate an event-study specification that allows for differential effects of closing a police station. Naively applying Equation 1 poses a number of empirical challenges that have been recently highlighted by a growing literature on the pitfalls of two-way fixed effects estimators with staggered adoption in the presence of heterogeneous treatment effects (Goodman-Bacon, 2018).[27] In particular, the β from Equation 1 is a weighted average of all the possible 2x2 comparisons in my sample, meaning that it is also estimated using comparisons among already-treated units and not-yet-treated units, where the already-treated units serve as control. This would induce a bias in presence of heterogeneous treatment effects across blocks experiencing closures at different points in time.

[26] The impact on (log) crime of the continuous treatment (log) distance can be written as: $\frac{\partial logcrime}{\partial logdistance} \approx \frac{\partial logcrime}{\partial treated_{i,t}} \frac{\partial treated_{i,t}}{\partial logdistance} = \frac{\beta}{\alpha}$, where α is the coefficient of a regression of (log) distance on the treatment indicator. If the effect is linear across the distance distribution and α is close to 1, then the two estimates are equivalent.

[27] A burgeoning and rapidly developing literature has recently emphasised that difference-in-differences designs with staggered treatment timing are likely to be biased in the presence of treatment effect heterogeneity. See, among others, Borusyak and Jaravel (2017), Goodman-Bacon (2018), De Chaisemartin and d'Haultfoeuille (2020), Callaway and Sant'Anna (2020) and Baker et al. (2021).

To assess the heterogeneity of the treatment effects across groups and time, I therefore implement an event study approach as follow. First, I define the control group as never treated census blocks, i.e. areas which never experienced a change in distance, which allows to separately identify *calendar date* and *time to event* fixed effects. Indeed, the main advantage of my design relative to a pure event study design is that the difference-in-differences model uses a control group to eliminate event time trends that do not appear in calendar time (see Borusyak and Jaravel (2017) for a clear description of the problem). In my context, this becomes crucial if, for instance, the MPS chooses which station to close in a given year based on crime trends in previous years. In that case, calendar time effects alone would not eliminate the pre-trends.

In my setting, there is no straightforward definition of "event" for control areas. To assign placebo events to control units I follow Deshpande and Li (2019) and construct my sample as described below. First, I create a separate dataset for each of the *treatment wave*, i.e. for each group of census blocks that is treated in the same period.[28] In each of these datasets, blocks that were subject to a change in distance in that period are considered treated, while blocks that never experienced the treatment serve as control. Event-time dummies are specified relative to the period of treatment, i.e. of closure.[29] Second, I append all datasets into one: the resulting dataset has 4,701 blocks (2,039 treated) and, in my main sample restricted to event quarters -9 to 11, a total of 450,901 block-quarter observations.

I therefore run a stacked event study (Deshpande and Li, 2019; Cengiz et al., 2019; Fadlon and Nielsen, 2019) that compares blocks which experienced a closure to blocks which never experienced a closure and I estimate the following equation:

$$y_{it} = \sum_{k=-B}^{T} \delta^k Treat_i \times D_{it}^k + \sum_{k=-B}^{T} \beta^k D_{it}^k + \phi_i + \phi_{lt} + \varepsilon_{it} \tag{2}$$

where y_{it} is the outcome of interest for block i in calendar month t. D_{it}^k's is a set of relative event-time dummies, that take value of 1 if period t is k periods after (or before, if k is negative) the event. The treatment indicator $Treat_i$ is equal to one if block i has ever experienced an increase in distance. Event-time dummies are assigned to both the treatment and the control group as explained above. I omit the period before the treatment and include B preceding and

[28]To estimate the event study specification I collapse the dataset at the quarterly level. Although there are 9 quarterly dates where at least one closure occurred, up to 80% and 85% of treated units got treated respectively in in the third quarter of 2013 only and throughout the year of 2013.

[29]Similarly to Equation 1, for blocks treated more than once, I assign only the first treatment period that they have been subject to as the event period.

T following periods. The use of a stacked-by-event design allows me to control separately for both event-time trends (the set of ϕ_k's dummies in Equation 2) and calendar-time trends (ϕ_t's). Standard errors are clustered at the census block level, allowing for serial correlation over time (Bertrand et al., 2004). This specification is robust to heterogeneous treatment effects, under which traditional event studies perform poorly (Baker et al., 2021).[30]

The coefficients of interest of Equation 2, the δ_k's, measure the change in the outcome of treated blocks k periods after treatment, relative to the pre-treatment period, compared to the change in the outcome of control areas. The graphs presented in the following sections plot the δ_k estimates in event time.

Albeit I cannot directly test my identifying assumption, the event study framework nests a set of placebo tests that can be used to assess its validity. In the figures presented in Section 5 and 6, I test for violations of the parallel trend assumption in the quarters leading up to the event by evaluating the event-study coefficients for $k < 0$ and I show that there are no pre-trends in the outcome variables.

4.2 Identifying Assumption and Threats to Identification

The identifying assumption of my design is that, in the absence of the closures, the number and composition of criminal incidents would have evolved similarly in treated and control blocks, i.e. in blocks whose distance from the police stations increased relative to blocks which never experienced a closure.

A key aspect of my identification strategy is that there exists variation across time in the distance to the closest station. Out of 4,701 census blocks in London, 2,039 experienced a closure of their nearest police station. Around 80% of the latter were treated in the third quarter of 2013. Yet, the timing of the closings could be determined by local conditions and, specifically, local criminality. Although the MPS does not disclose the official criteria to decide which buildings to close first, I demonstrate that the timing of the closings appears random. To do so, I estimate a regression where I measure whether baseline crime, measured in 2008-2010, predicts the timing of the treatment, conditional on closing. I restrict the sample to treated blocks and I build distinct indicators for the timing of the closures, that are equal to 1 in the period when a

[30]To further assess the presence of heterogeneous treatment effects across treatment groups, in Section 5 and 6 I estimate Equation 2 by *treatment group*, keeping all control blocks and as treated blocks only those experiencing a closure in the same period.

station closed.[31] Results are displayed in Table B1. The table shows that local crime rates do not consistently predict the timing of the treatment, conditional on the closures. These results suggest that the timing of the closures is effectively not predicted by observable characteristics, although the decision on which station to close is not, which motivate my main empirical strategy.

Police station closures are non-random by nature. This would pose a concern for my identification strategy if, for instance, the MPS may be dismissing buildings in areas where crime rate was already falling or where the composition of criminal incidents was already changing. To address this concern, I use all stations that were operating in 2010 and examine whether local characteristics, such as local crime rates and house prices, matter for the decision of which stations to close. Panel B of Table 1 shows that lower crime rates and higher local house prices predict the probability of closure, which suggests that the closings themselves are not as good as random.[32] As in all difference-in-differences designs, time-invariant unbalances in the outcomes between treatment and control units do not represent a threat to the empirical strategy as they are absorbed by the census block fixed effects. I test if the treated and the control group are on differential trends before the closures in the next sections.

A final caveat arises from the presence of contemporaneous confounding policies implemented at the local government level. The closures were enforced by the MPS and were unlikely to correlate with local changes in the census blocks, which are very small areas that do not overlap with any administrative boundary. Yet, a threat to the empirical strategy is that the timing of the police closures is correlated with other local time-varying factors that are unobservable to the econometrician and that occur in the same period, in a way that is correlated with the staggered closings of the stations. Suppose for instance that during the same period each MPS division decided to adopt policing strategies and tactics as a response to the budget cuts, or that the central government cut funding to other welfare expenditures, affecting the local

[31] Specifically, to examine whether local crime predicts the timing of the treatment conditional on closing, I estimate the following equation on the sample of "ever treated" blocks, i.e. blocks that ever experienced a closure, in 2008-2010:

$$ClosePeriod_i = X_i'\beta + \phi_{l,t} + \varepsilon_i$$

where $ClosePeriod_i$ is the period where the station closed and X_i' is a vector of local characteristics, specifically crime rates, and $\phi_{l,t}$ are LA-by-year fixed effects. I define as closure periods the periods where majority of closing occurred (namely, July 2013, September 2013, or the entire third quarter of 2013) and I pool together all the other closure periods.

[32] Appendix Table B2 provides additional summary statistics of several baseline characteristics of blocks with and without police stations, with and without a closure conditional on having a police station, and treated versus controls blocks.

provision of public services which are provided at the LA level.[33] To address this challenge, in the main specification, I non-parametrically control for the differential trends by LA, by including LA-by-monthly date fixed effects. In a robustness check I also include time-varying fixed effects for smaller areas, i.e. wards.[34]

5 Effects on reported crime

I begin the empirical analysis looking at the impacts of police station closures on crime prevention. I use violent offences as proxy for high-severity crimes, which are not sensitive to changes in the reporting cost.[35] I test if the elasticity of high-severity crime with respect to distance is positive, because of lower police deterrence. I then move to look at the effect on total reported crime and finally I exploit the heterogeneity across types of crimes to assess which channel between deterrence and reporting dominates.

5.1 Violent crimes

The first step of my empirical analysis is to estimate the impact of the police station closures on violent crimes, which are defined by the MPS as murders and assaults.[36]

Figure 7 plots the δs coefficients from Equation 2: before the closures, period-specific coefficients are not statistically different from zero, supporting the plausibility of the parallel trend assumption between treated and control blocks.[37] I further investigate the presence of heterogeneous treatment effects across treatment groups, that is, areas experiencing treatment at different points in time (Callaway and Sant'Anna, 2020; De Chaisemartin and d'Haultfoeuille, 2020). I estimate equation 2 keeping only units treated in the third quarter of 2013, where 80% of blocks got treated. Figure 8 shows very similar point estimates as in Figure 7: these results provide further support against the presence of heterogeneous treatment effects across treatment groups and therefore confirm the validity of my empirical design.

[33] This is actually documented by Fetzer (2019) and Giulietti and McConnell (2020) as a result of the Welfare Reform Act 2012. Examples of such measures undertaken are: reform of tax credits, changes to child benefit, the capping of council tax benefits, bedroom tax, change in disability allowance.

[34] Wards are the primary units for local elections in London and are defined for electoral purposes only. In London, each ward contains on average 7 census blocks.

[35] In Appendix A, I develop predictions to test the reporting versus the deterrence channels by compare low- versus high- severity offences.

[36] See https://www.met.police.uk/sd/stats-and-data/met/crime-type-definitions/

[37] Figure B4 plots the coefficients from estimating Equation 2 on the (log) distance to the closest police station. As expected, distance mechanically increases only after the closings and remains constant afterwards.

Table 4 shows the difference-in-dfferences estimates from Equation 1 on the number of violent offences.[38] The table shows the results using both the binary and the continuous treatment variable, as explained in Section 4. In column 1, I present the estimates from a regression with only block and monthly date fixed effects, while in column 2 I enrich the specification with year-specific LA dummies and in column 3 with monthly date-specific LA dummies. Controlling for the latter is especially important as not only it captures police division-specific changes in crime prevention and policing strategies, but more generally it absorbs any change in the provision of local public services that are managed at the LA level.

Results show that following an increase in the distance to the nearest police station the average number of recorded violent incidents increases. Considering the point estimate of my preferred specification in Column 3 Panel A, the violent offences in treated blocks increase by 11% per treated census block-period. Indeed, using the estimates on the continuous distance (see Panel B.II), I estimate a positive elasticity of violence with respect to distance of approximately 0.08, so that a 10% increase in distance increases violent crimes by 1%. To get an overall sense of the magnitude, the total increase in distance caused by the closures ($\sim 74\%$) decreases the number of assaults and murders by around 8.5%. Results are robust to the inclusion of a set of time-varying fixed effects for even smaller geographies (Column 4 and 5), i.e. wards, that allow me to further account for any change in policing and service provision at the ward level, corroborating my research design.[39] Guided by the fact that high-severity offences, such as violent crimes, are inelastic to changes in reporting (see Appendix A), I interpret these results as consistent with a reduction in deterrence determined by the police station closures.

I further exploit the intensive margin of the treatment. First, I explore whether the effect is driven by blocks initially located closer to or farther from the police stations. I estimate the main specification splitting the sample by quintiles of baseline distance. As shown in Figure 9, the average positive effect is driven by blocks which were originally located closer to police stations. This is consistent with the fact that these areas experience the greatest intensity of the treatment, that is, the largest increase in distance. Second, in Figure 10 I show that the effects are driven by crime hot spots, i.e. high-crime and high-violence areas. These findings provides supporting evidence to the deterrence channel. The *marginal areas*, those that are more likely

[38]The variable for violent crime is transformed using the inverse hyperbolic sine transformation (*asihn*) following Bellemare and Wichman (2020) and Card et al. (2020) as explained in Section 3 to accommodate the presence of zeros. Table B5 shows that the results are robust to alternative transformations of the outcome variable, such as $log(y+1)$, $log(y+0.01)$.

[39]All robustness checks are reported in Appendix B.

to suffer from an increase in the distance to the police station, are blocks that at baseline had higher opportunities for crime and, therefore, higher returns from police presence.

5.2 Total reported crime

As a second step I estimate the impact of police station closures on the total number of offences reported to the police (expressed in logs).[40] Results show that following an increase in the distance from the closest station the average number of recorded criminal incidents drops by 1.3% per treated census block-period (Column 3 Table 5). The elasticity of the total reported crime to the distance is approximately 0.01, so that a 10% increase in distance decreases crime by 0.1%. The overall increase in distance ($\sim 74\%$) leads to a reduction in total reported crime by around 1%. As for violent crimes, I test the robustness of the results by including a set of time-varying fixed effects for even smaller geographies (Column 4 and 5), i.e. wards, that account for all changes in policing and service provision at the ward level. Additionally, Column 6 of Table 5 shows that the average effect is driven by areas that at baseline were located in the vicinity of the police stations. In line with evidence from Figure 6, the greatest increase in distance, and therefore the largest intensity of the treatment, occurs in areas located nearby the stations.

Finally, Figure 11 plots the δs coefficients from Equation 2: in the periods before the closures, period-specific coefficients are not statistically different from zero, supporting the validity of the parallel trends assumption. Figure 12 shows the results from estimating equation 2 keeping only units treated in the third quarter of 2013, where 80% of blocks got treated. I find similar estimates to Figure 11, although less precise, providing further support against the presence of heterogeneous treatment effects across treatment groups.

5.3 Other types of crime

The results up to this point suggest that the police station closures increase violent crimes while decrease the total reported crime. I overcome the empirical challenge of disentangling the deterrence vis-à-vis the reporting channel by exploiting the heterogeneity across types of crimes.

I start by considering other types of violent offences. In addition to assaults and murders, I find that also robberies increase by respectively 1.6% (Table 6 Panel A, columns 4).[41] I then

[40] Note that only 0.4% of the observations have zero total crime, therefore there is no need to use alternative transformation. Still, as an additional robustness check, Table B6 shows that the results are robust to alternative transformations.

[41] Sexual offences report a significant and strong negative coefficient. They mainly include domestic violence

look at property crimes only, that I use as a proxy of low-severity offences. Table 7 Columns 1 and 2 pool together all components of property crimes, while columns 3-12 separately estimate the effect for each sub-category of property offences. Following an increase in the distance to the closest police station, recorded property crimes drop by 2.1% on average.[42] All components of property crimes drop, except for burglaries, which instead show a positive, although for the discrete variable insignificant, increase.

These findings are consistent with the fact that the increase in the distance to the police station raises the cost of reporting and arguably affects the incentives to report offences. Such cost rise is more likely to affect the propensity to report low-severity crimes, whose benefits from reporting are lower. At the same time, the police station closures also impair police ability to detect incidents: indeed, higher distance to stations causes a reduction in drugs-related crimes, which solely depend on direct police detection, as shown in Table B7. Consistently with the conceptual framework outlined in Appendix A, the deterrence effects can only be retrieved on violent crimes as the latter are less elastic to changes in the reporting costs, given that the benefits from reporting are likely to be higher. A notable exception is burglaries, that display a positive, although not always significant, effect. Such offences are less likely to be affected by changes in the reporting cost given that, to provide insurance coverage, insurers often require a police report as proof that the burglary has taken place.

5.4 Discussion on sources of displacement

Displacement of criminals A potential threat to my estimation strategy stems from the fact that some control blocks, despite not directly experiencing the closure of their nearest police station, border treated blocks. This might lead to under- or over-estimate the effect on crime: for instance, the negative effect on the total number of recorded crimes might actually be driven by an increase in reported crime in control bordering blocks, rather than an actual drop in treated blocks. Similarly, the positive effect on violent crimes could be due to criminals moving away from control bordering areas to treated blocks, which, after closure, have a relatively lower risk of apprehension than the former.

Figure B5 identifies 833 neighbouring blocks based on whether they share a border with treated blocks. Although I do not directly observe the behaviour of criminals, I can still as-

crimes that have distinct characteristics with respect to other types of violent crimes, such as, for instance, being strongly affected by reporting bias.

[42]Table B5 shows that the results are robust to alternative transformations $log(y+1)$, $log(y+0.01)$.

sess whether the effect picks up adjustments coming from bordering areas by excluding them from the estimation sample and repeating the analysis using as control group only "landlocked" blocks, i.e. blocks not bordering with any treated areas. Table B11 and B12 show that the estimates do not change when I exclude bordering blocks from the sample.

Displacement of front-line police officers Another source of displacement concerns police officers, moving from treated to control blocks. As shown in Figure 3, the number of front-line police officers, who are employed in patrolling and in attending emergencies, remains constant throughout the sample period. This implies that the front-line police workforce might be relocated to operating stations within the same police division (i.e. LA). Because a higher police presence deters crime, areas surrounding open police stations would therefore benefits from a greater number of front-line officers, and therefore experience lower crime rates. These indirect spillover effects generated by control areas might partially mitigate the direct positive impact of police station closures on violent crimes in treated blocks.

Although I do not directly observe police workforce relocation patterns within police divisions, I perform a number of exercises to quantify the magnitude of these spillovers. First, I restrict the sample to control blocks based on my main definition of treatment, i.e. blocks which never experienced the closure of their nearest police station at baseline (2,514 blocks). I thus exploit the variation in the distance between each control block and its nearest open police station within LA, which defines the boundaries of a police division. This exercise is based on the intuition that the influx of police officers to surviving stations occurs only once at least one police station under the same police command closes. I hence compare blocks located at different distances from open stations before and after the first closure within the same police division.[43] The underlying assumption is that, if anything, blocks located further away from open stations benefit less from greater police presence, and therefore police deterrence.[44]

To estimate the spillover effect, I run the following specification on the sample of blocks that

[43]On average, 80% of the closures within LA occurs in 2013. For 12 out of 31 LAs, all closures happen in 2013. For this reason, it's reasonable to use the first closure period to identify the period in which the majority of front-line officers were displaced.

[44]Such assumption is incidentally corroborated by the results outlined in Table B11: removing the control blocks located further away from open stations from the control group does not affect the estimates of the average treatment effect. These blocks are hence likely not to be subject to changes in crime prevention and police displacement.

did not directly experience any closure:

$$y_{it} = \delta Near_i * Post_{lt} + \phi_i + \phi_{tl} + \varepsilon_{it} \tag{3}$$

where y_it is the number of violent crimes in block i at monthly date t. $Near_i$ is a dummy variable equal to 1 if census block i is located in proximity to an open police station. $Post_{lt}$ is an indicator equal to 1 for the period when the first police station in the same police division l shuts down. I first define *near* blocks as those located closer than the median distance to the nearest open station. I then assess if the estimates are sensitive to alternative definitions of proximity or to the use of the continuous distance.

Table 8 shows the estimates of Equation 3. Nearby operating police stations, violent crimes decrease. These findings seem to confirm that upon the closure of stations, front-line police officers were relocated to surviving stations. They also emphasise the relevance of the deterrence mechanism, as greater police presence around stations effectively deters violent crimes.[45] To further explore the spatial distribution of the effects and account for spatial spillovers flexibly, I estimate Equation 3 interacting the *Post* dummy with indicators for whether each block is in the $q-th$ percentile of the baseline distance distribution, where $q = 1, ..., 5$. As shown in Figure 13, the deterrence effect is driven by blocks located very close to open stations and this spillover effect decays with distance. Overall, these findings highlight the strong gradient in distance of the spillover effects and emphasise how police presence is effective in deterring crime only in close proximity to stations.

Aggregate impacts I first use the estimates of Equation 3 to roughly assess the aggregate effects. I estimate the total number of non-deterred and deterred crimes as the product of (i) the estimated coefficients of the direct and spillover effect from respectively Table 6 and 8 and (ii) the number of treatment and spillover blocks in London. Treated blocks are defined as blocks which experienced a closure of a police stations, while spillover blocks as those located in the surroundings of open stations. This simple back-of-the-envelope calculation helps rule out large decreases in crime from the reallocation of police. These crude aggregates suggest that 614 more assaults and murders are committed city-wide because of police station closures in treated blocks, or 14% relative to the total number of violent crimes in London. Instead,

[45] To estimate spillover effects, I adopt a differences-in-difference design. To test the plausibility of the parallel trend assumption, I run an event study and demonstrate that blocks located near versus far open stations exhibit parallel trends before the closure of the first police station within the police division (See Appendix Figure B6).

441 violent crimes are deterred near to open police stations, equal to -10% of the total violent crimes.

Furthermore, I empirically identify the net aggregate impact by collapsing the dataset at the police division level. This approach has important advantages. First, it allows me to effectively identify the net effect of block-level direct and spillover effects. Indeed, ideally, one would define treatment and control areas such that the whole policy impact is confined within the treatment area. However, if, as in my setting, the treatment area is relatively small, part of the impact could possibly contaminate the control areas if those are adjacent. The resulting estimate for the treatment effect would be biased toward zero. I avoid this problem by defining a larger treatment area, within which most of the policy impact is indeed confined.[46] Furthermore, this approach allows me to implicitly account for all unobserved effects that might impact crime due to re-allocation of resources (e.g. economies of scale and organisational gains from centralisation, congestion effects, learning and productivity spillovers, etc.). I therefore regress the total number of crimes within LA on respectively the share of treated blocks and the average distance at the local authority level, and a full set of LA and date fixed effects.[47]

Table 9 shows that the aggregate net effect on violent crime is positive and significant at the 10% level. Using the estimates on the continuous distance, I estimate a positive elasticity of violence with respect to distance of approximately 0.6, so that a 10% increase in distance increases violent crimes by 6%, or 290 violent offences more per LA per year. The aggregate effects on total crime and other crime types are imprecisely estimated. Overall, my findings suggest that police station closures, and the consequent reallocation of resources, not only had unintended distributional consequences on crime. It also generated meaningful efficiency losses in terms of lower police ability to prevent crime.

6 Effects on police performance

In this section I move to estimate the impact of the closures on police performance. I measure police performance as the ability to effectively investigate, prosecute and solve, i.e. *clear*,

[46]This is related to the "ripple" effect outlined by Manning and Petrongolo (2017), who argue that defining small treatment areas around the shock location risks missing most of the impact and possibly contaminating control areas. In my case, the underlying assumption to causally identify the aggregate effect is that the re-allocation of resources and police displacement occurs within the boundaries of the local authorities.

[47]The LA average distance to police stations moved from .13 km in 2011 to .5 at the end of the sample period. By the end of 2016, on average more than 40% of blocks per local authority got treated.

crimes.[48]

The direction of the effect is a-priori unclear, as outlined in Appendix A. On the one hand, if police station cuts free wasteful resources and re-direct them towards a more efficient use, as claimed by the MPS, the police would improve on its effectiveness. On the other hand, the closure of the police stations increases the response time to attend the crime scene, therefore worsening the ability to collect evidence, which is needed to clear a crime, and to successfully identify a suspect. Furthermore, as shown in sub-section 5.3, station closings altered the composition of recorded incidents towards more severe and violent crimes. Again, the impact of changes in crime composition on the clearance rate is unclear: the overall effect might indeed vary according to the relative ability of the police to clear different types of crime. For instance, for some types of incidents, such as violent crimes, the burden of the proof might be larger: as the response time increases, the chances of proof contamination or destruction raise.[49] Yet, a lower volume of reported incidents might result in lower congestion and hence in the police concentrating all its resources to clear fewer crimes, resulting in a higher clearance rate. More generally, there is a clear complex relation between actual crimes, reported crimes and clearance rates. Lower clearance rates might increase incentives for criminals to commit a crime while reduce incentives for the public to report a crime, contributing to the effects found in the previous section.

In order to establish the overall direction of the effect, I measure the impact of shutting down stations on the probability of clearing a crime. I therefore estimate Equation 1 on the incident-level dataset restricting the sample to all incidents with a non-missing investigation outcome and run a linear probability model to assess whether police closures had any effect on clearance.[50]

Results are reported in Table 10. Following the closings, the probability of clearance, displayed in columns 1-3, significantly drops by 0.6 pp, equal to around 3% with respect to the baseline average clearance rate. Results are robust to the inclusion of crime type fixed effects, as shown in column 3, that absorb the different intrinsic degrees of difficulty to clear distinct

[48] Several economic studies have used clearances as a measure of police performance (see, among others, Mas (2006), Garicano and Heaton (2010), Blanes i Vidal and Kirchmaier (2018), Mastrobuoni (2020).

[49] Indeed, in my sample 70% of non-cleared robberies and 80% of non-cleared assaults are left non-cleared because of police failure to identify the suspect.

[50] To establish if the cuts to the police affected the recordings of the investigation outcomes, and therefore affected the sample selection of investigated incidents, I empirically check whether the the closures predict the probability of an incident to have a an investigation outcome, conditional on being reported. Appendix Table B13 shows that the the probability of having an investigation does not change as a result of police station closures. Furthermore, in Appendix Tables B14 and B15 I estimate the effect on crime on the sub-sample of incidents with non-missing investigation outcome and I show that the coefficients are unchanged.

types of crimes. I also measure the effect on the total volume of cleared incidents and find that a higher distance to the stations not only affects the total number of reports, as shows in sub-section 5.2, but also significantly decreases the number of offences cleared and brought to justice, i.e. convicted, by 7% and 5% respectively (columns 1-2 of Table 11).[51] To validate again my empirical strategy, I examine the existence of pre-trends in the probability of clearance and I estimate the event-study Equation 2. Figure 14 shows that, in the periods before the closures, period-specific coefficients on clearance are not statistically different from zero, supporting the validity of the identification assumption for clearance. Furthermore, I study the presence of heterogeneous treatment effects across treatment groups, that is, areas experiencing treatment at different points in time. I keep only units treated in the third quarter of 2013, where 80% of blocks got treated. Point estimates in Figure 15 are the same in magnitudes as in Figure 14, although less precise: the results provide further support against the presence of heterogeneous treatment effects across treatment groups (Callaway and Sant'Anna, 2020).

I provide evidence that the clearance effect in not due to changes in the composition of offences. First, I further explore the intensive margin of clearance, that is, if the closures of the stations caused a change in the composition of the sanctions charged. As outlined in Section 3, prosecuted incidents can be dealt with a formal court sentence or with an informal sanction, that can only be assigned by the police. As shown in columns 4-6 of Table 10, the decline in the overall clearance is driven by a lower likelihood of clearing cases with informal sanctions. These results suggest that the lower clearance rate is exclusively induced by a change in police behaviour, while, if anything, part of the clearance burden is shifted to the court.

Furthermore, I investigate whether the observed effect on clearance might partially reflect the change in composition of new incidents. Although Appendix Figure B2 shows that violent crimes are not necessarily the most difficult to clear, still the budget cuts may force the police to focus their investigations on more serious crimes, to the extent that they take longer to clear. As a result, the average decline in clearance might reflect a shift in the composition of the incidents towards more difficult ones. I therefore perform the following exercise to assess the presence of such selection effects. I compute the average baseline probability of clearing an incident by type of crime and by police command. I then compute the total number of criminal incidents recorded in a census block by clearance difficulty level, and I use it as outcome variable to estimate the effect of closures. As Table B17 shows, the closures do not alter the composition

[51] The evidence on convictions is also consistent with the fact that that the reduction in clearance reinforces the increase in crime through an incapacitation channel that works through fewer arrests and convictions.

of incidents towards more difficult ones. On the contrary, if anything, they actually increase the number of offences that are easier to solve.

6.1 Discussion on police performance, deterrence and reporting

I then move to investigate the importance of the three competing mechanisms for the clearance reduction: response time, deterrence and reporting. I first measure the impact on clearance splitting the sample by quintiles of baseline distance. The results of this exercise are reported in Figure 16 and show that areas that at baseline were located in the vicinity of stations, i.e. in the first quintile of the distribution of baseline distance, display a stronger negative coefficient. This is because distance matters most for incidents that were initially located in close proximity to a police station and are characterized by a higher effect of response time on the clearance rate relative to the average incident (Blanes i Vidal and Kirchmaier, 2018). The main consequence of an increase in distance is hence to prevent the police from arriving very fast at those crime scenes, hurting its ability to collect evidence and clear crimes. Ideally, one would want to validate this argument empirically, however I do not observe the incident-level response time. Nevertheless, I exploit information at a more aggregate level on the average response time for 999 emergency calls by LA provided by the MPS between 2012 and 2016 and document a positive relationship between response time and distance in Figure 17.[52] I hence interpret the evidence from Figure 16 as consistent with the fact that the average negative effect on clearance is driven by a deterioration in police performance, through higher response time, due to the closures.

Second, to disentangle the reporting from the police performance channel, I explore the heterogeneity by crime type, exploiting the fact that, as violent crimes bring higher benefits from reporting, victims of violent assaults if anything respond less to changes in the cost of reporting. The average effect could be partially caused by the reduced pool of the reports and not only by a worsened police ability to clear crimes. Furthermore, although the average incident-level probability of clearing a crime falls, the police might relocate the resources to prioritise the investigations of specific types of crime. Columns 3-6 of Table 11 shows the findings from this exercise. I aggregate all components of property crimes and violent crimes together, and I measure the effect on the total number of cleared and convicted property, or

[52] Blanes i Vidal and Kirchmaier (2018) document a strong positive relationship between the two, employing data from the Greater Manchester Police.

violent, crimes. I find that the closures significantly reduce not only the number of cleared and convicted property crimes, but also the ones of violent crimes, where reporting is likely not to matter. In light of the intuitions developed in Appendix A, the net effect on clearance seems to be driven by worse police performance, since if only deterrence was at play, we would have expected a positive coefficient.[53]

Combining these findings with results from Section 5, the overall picture that emerges contrasts the hypothesis of no relevance of police stations: although reported crimes dropped, still police effectiveness dramatically weakened after police station closures.[54]

7 Effects on local welfare

I finally analyse the effects of reducing police infrastructure on local welfare. I first measure the impacts on the real estate market and I quantify the total social welfare costs by adopting a capitalisation approach. The basic intuition of the approach is that the decrease in house prices relative to the savings in public expenditures reflects the extent to which households lose from the police station closures.

7.1 House prices

The inverse relationship between local property values and crime is widely documented in the crime literature (see, among others, Thaler (1978), Gibbons (2004), Linden and Rockoff (2008) and Besley and Mueller (2012)): as residents care about their exposure to crime risk, an upsurge in crime influences their perceptions of public safety and local amenities and affects their valuation of living in close proximity to the crime scenes. All the existing literature underlines the importance of measuring the willingness to pay of individuals to reduce their exposure to local deprivation in general, which is directly reflected in the local housing market. The goal of this analysis is thus to determine the effect on the willingness to pay of local residents, proxied by house prices, of a change in the local conditions solely induced by police station closures.

[53]To investigate whether changes in the reporting cost drive the whole effect, I construct the ratio between the number of charges and convictions vis-a-vis the number of reports. Table B16 shows a decrease in the ratio, suggesting that, although reporting still matters, police ability to clear incidents and convict criminals is likely worsened.

[54]Lower clearance rate might in turn reinforce the reporting channel outlined in the Section 5. Indeed, if citizens observe the higher response time, reporting rates may drop if the perceived likelihood of a crime being solved decreases.

From a conceptual point of view, the welfare implications of the closings are ambiguous: although they cause a fall in total reported incidents, they also change the composition of crimes, reducing the number of reported low-severity offences, while increasing violent crimes. More generally, the loss of police stations potentially reduces police visibility in the neighbourhood and alter communities' perceptions of police presence. To estimate the overall impact of the closings through both crime and non-crime channels, I estimate the effect of police station closures on house prices in blocks which experienced a closure versus blocks which did not. The underlying intuition is that the total costs of the closures, not just the direct cost related to crime, should be reflected in house prices (Rosen, 1974), which absorb changes in crime rates and crime composition, as well as in local amenities and residents' perceptions. Furthermore, this exercise provides a further test to challenge the interpretation of the results on reporting exposed in sub-section 5.2: if the observed decrease in recorded crime masks an actual decline in crime exposure, one would expect a subsequent increase in local house prices.

I exploit the universe of house transactions occurred in London from 2011 to 2016, I collapse the dataset at the quarterly level, and I estimate Equation 1 on the mean house prices in the census block, where observations are weighted by the number of transactions registered in the block during the quarter.[55] I further examine the presence of heterogeneous effects of closings on house prices. The heterogeneity relates to the distribution of crimes across census blocks. I define crime-intense areas as block that had a baseline higher than median overall crime or violent crime rate and I split the sample in more versus less crime-intense areas. I then split the sample based on a number of baseline socio-economic characteristics of the blocks.

Figure 18 reports the estimates on house prices, with 95% confidence intervals. An increase in the distance to the closest station produces a negative impact on house prices, although not statistically significant. Still, the average effect is non negligible: to give a sense of the magnitude, a 10% increase in distance (approximately around 150 meters) deteriorates house prices by 1.3% per quarter. House prices fall by 1.2% more per quarter, amounting to 5% yearly, in blocks were stations closed relative to blocks with still operating stations.[56] The estimates from the heterogeneity analysis are also reported in Figure 18. The police station closures appear to significantly reduce the willingness to pay to reside in high-crime neighbourhoods, in more deprived blocks and blocks with a higher disadvantage in terms of socio-economic characteris-

[55]I use sales of residential properties only.

[56]These effects are likely to correspond to a lower bound on welfare losses because they ignore any reduction in property values that are experienced by those residents who chose not to sell.

tics. These results suggest that police station closures likely increased the inequality in house values between high– and low– crime blocks. To reiterate, the negative effects on house prices occur despite the overall falls in the total recorded crime experienced in areas interested by stations' closures. They therefore suggests that the police station closures increases overall crime, although overall reported crime falls.[57] Considering how spatially persistent is crime (Kirchmaier et al., 2021), both the average and the heterogeneous effects provide additional evidence in favour of the interpretation that the reduction in local criminality masks a change in reporting behaviour, and not in the underlying local crime risk. Overall, these findings show that the cuts hit harder those areas that are already losing from austerity cuts (Fetzer, 2019; Giulietti and McConnell, 2020).

As a robustness check, I test whether the effects on house prices are driven by the sales of the police stations, which expanded the local housing supply by placing on the estate market new regenerated housing units, and not by the removal of stations themselves. I therefore exploit the fact that around 50% of the closed police stations were sold, and 28% were then transformed into new residential buildings and explore whether house prices vary differentially between non-sold and sold stations, and according to their intended estate use.[58] Appendix Table B19 shows the results of this triple-difference exercise and suggests that, if anything, the negative effects on prices originate from those blocks where the closed police station was either not replaced (i.e. those stations that were not sold) or replaced with another public amenity, such as education or community centres, contrasting the hypothesis that the price reduction was actually driven by a price effect due to a supply shock to the local estate market.[59]

The magnitude of these house price impacts can be compared relative to other studies linking house prices with crime. I find that an equivalent reduction in house prices could be generated by more than a one-tenth standard deviation increase in visible crime density in London (Gibbons, 2004).[60] Interpreting this coefficient as an implicit price in a hedonic function gives a

[57] Alternative explanations consistent with these findings are that either residents place disproportionate weight on the change in composition of reported crimes, or there exist other social costs beyond crime associated with the police station closures, related, for instance, to the loss in local amenities and to a lower perception of police presence.

[58] 55% out of the 2,039 treated blocks had their closest station closed and sold, while for 36% of them, the closest stations were then regenerated and transformed into new residential estates. The remaining sold stations were targeted to become public facilities, such as community and education centres.

[59] These negative effects absorb not only the change in crime patterns, but also the loss of local amenities represented by the closure of police stations, that might directly affect residents' valuation of the neighbourhood. An alternative interpretation of evidence from Appendix Table B19 speaks to the substitutability across local amenities and suggest that other local amenities do not offset the loss from closure of stations.

[60] Adda et al. (2014) show that in London a localised policy of cannabis de-penalization reduces house prices by 5%; for the United States, Linden and Rockoff (2008) show that the revelation of a sex offender residing next

mean price of around £4,800 for the closure of a local police station, which is more than twice the implicit price for a one-tenth standard deviation reduction in property incidents in London estimated by Gibbons (2004).

7.2 Capitalisation approach

I next turn to the question of how lower house prices compare to the lower public expenditure relying on the capitalisation approach.[61] The capitalisation approach captures both direct crime costs as well as indirect costs such as loss in local amenities and deterioration of residents' perceptions. Estimates of the cost-benefit ratio can be seen in Appendix Table C7. I first compute the closure costs in terms of house price depreciation. Using the estimates on house prices outlined in sub-section 7.1, I compute an average cost to a treated block from the closure program equal to approximately £5,404. I then quantify the benefits, that is, the public savings from the closures. According to the official estimates, the MPS made savings equal to £600 million between 2012 and 2016, which translates into around £16,000 saved per treated block-quarter. The ratio between costs and benefits yields a value of 0.34. Costs are disproportionately borne by high-crime blocks: for them I estimate a cost-benefit ratio of 0.42. Overall, it appears that for every £5 saved by the public authority, 1 to 2 are paid back by the local residents.

The capitalisation approach has a number of drawbacks. It includes only the valuation of marginal movers induced by the policy, not of all affected households. Therefore it does not fully quantify the costs associated to lower deterrence and police effectiveness which are perceived by the residents who choose not to sell. In light of these shortcomings, in the next section I perform two complementary approaches to quantify the cost and benefits of the policy.

8 Cost-effectiveness of police station closures

In the previous sections, I established that police station closures increase violent crimes and decrease clearance rate and house prices in the blocks experiencing the closures. In this section, I put the causal estimates into context considering the cost-effectiveness of closing the stations. Using the estimates produced in the previous sections, I perform two different exercises to compare the various costs associated to the closures to the benefits in terms of public expenditure

door reduces house prices by 12%.

[61] The pioneer of this approach is Brueckner (1979). Lafortune and Schönholzer (2021) adopt this approach to measure the benefits of school construction programs. I apply it to my context of police station closures.

savings arising from the closures.

8.1 Cost-benefit analysis

Costs I compute the costs associated to the additional crimes generated when the clearance rate decreases because of the police station closures. A lower clearance rate encourages potential criminals to offend because of lower deterrence, and therefore results in more crimes. I use the estimates of the total economic and social costs of crime from the Home Office Report (Heeks et al., 2018), which includes UK-based calculations of the costs in anticipation of crime (e.g. defensive expenditure and insurance), costs as a consequence of crime (e.g. physical and emotional harm, lost output, victims' services) and costs in response to crime (e.g. police and criminal justice costs). The average cost per crime is £7,106 (Appendix Table C1). In Appendix Table C2, I quantify the deterrence costs generated by the lower clearance rate resulting from the closures. Specifically, I measure the total costs following a lower likelihood of clearing crime equal to 0.6 percentage points, the coefficient estimated in Table 10. To do that, I first assume a conservative estimate of elasticity of crime on clearance rate equal to -0.1.[62] I then use the estimates of the unit cost of crime from Appendix Table C1. Overall, I estimate a cost of £13 million resulting from a decrease in the clearance rate equal to 0.006.

I also compute the incapacitation costs that stem from the impacts on crime types reported in Section 5.[63] Appendix Table C3 calculates the additional years of incarceration following the increase in crime. I adopt a conservative approach and take into account also the reductions in reported property crimes. Based on MPS figures, I account for the fact that only a fraction of crimes is convicted, and only a fraction of them lead to an incarceration sentence. Because the U.K. criminal justice system is relatively lenient, only a small fraction of convicted offenders are incarcerated and, conditional on incarceration, average custodial sentences are relatively short. Furthermore, I account for the fact that the transitions between different stages of the criminal justice system vary greatly across crime types. To illustrate, robberies comprise only a small proportion of crimes but lead to a high number of additional years of incarceration, while

[62]This is the same estimate used by Blanes i Vidal and Kirchmaier (2018) which is taken from Levitt (1998) and is at the lowest bound of estimates.

[63]In this section, I compute the fiscal costs of higher incarceration. I instead do not include all the social costs of higher incarceration, in terms of economic impact (i.e., reduced employment, greater reliance on public assistance) and post-release criminal behaviour. I therefore likely underestimate the actual costs of incarceration. Furthermore, given that I do not observe individuals, I assume that each criminal incident is associated to a different individual. Finally, I use estimates from the block-level regressions. If instead I used the estimates from the LA-level aggregate regressions, the computation of the total incarceration cost would be higher.

the opposite is true for criminal damage crimes. I calculate that the changes in crime would lead to an extra 23,170 years of incarceration and to a total cost of £770 million.

Savings Detecting and clearing less crimes results in greater savings due to a lower CJS expenditure. Appendix Table C4 calculates the CJS savings that result from decreasing the clearance rate by 0.6 percentage points. The calculations are also based on the figures from the Home Office Report (Heeks et al., 2018) and include costs associated to prosecution, courts' functioning and legal aid. Overall, I estimate a total saving for the CJS of £10.1 million resulting from the station closures.[64] I similarly compute the total savings for the police due to a fewer number of crimes to investigate. Appendix Table C5 calculates the police savings that result from decreasing the clearance rate by 0.6 percentage points. The savings would amount to roughly £21 million.

After comparing the savings of police station closures with the potential costs generated by greater criminal activity, I conclude that closing stations is not a cost-effective way to implement public spending cuts. Using moderately conservative estimates, these findings suggest that for every pound saved for closing stations, society loses £20 in additional fiscal costs. Table C6 summarizes the estimates from this analysis, showing that even in the most conservative approach, the estimated savings do not offset the higher costs associated with the greater number of crimes committed due to lower police effectiveness and lower deterrence.

8.2 Marginal Value of Public Funds

Finally, I consider an alternative cost-benefit framework to conduct welfare analysis. Specifically, I compute the marginal value of public funds (MVPF, Finkelstein and Hendren, 2020; Hendren and Sprung-Keyser, 2020). The MVPF is the ratio of society's willingness to pay for a policy to the net cost of the policy to the government. I adapt this approach to a context of cuts to public spending, computing a marginal *loss* of public funds. [65]

Willingness to pay I begin by calculating the numerator of the ratio, which measures the aggregate social willingness to pay for the policy change. The primary component of the numer-

[64] I also include in the calculations the savings from lower probation and prison expenditures, equal to £1.8 million (Column 7 of Table C2).

[65] To my knowledge, this is the first paper to quantify the MVPF of a policy related to crime or (dis)investments in infrastructure, as Hendren and Sprung-Keyser (2020) do not include them in their work. I therefore try to adopt it to my context, under the additional assumption that the MVPF is symmetric for increase and cuts to public spending.

ator is society's willingness to pay for additional crimes. I use estimates of the average social cost of crime, from Table C1. I combine them with the estimates of the additional number of crimes committed (Table C3) and the number of non deterred crimes due to lower clearance (Table C4). I compute a total social cost of the closures of approximately £230 million.

Next, I consider the willingness to pay for worsened labour market prospects, by the individuals who are now incarcerated following the increase in violent crimes. In other words, I compute the total loss in wages they experience from this policy change. I consider as population at risk of incarceration youth aged 19-25. To calculate this foregone income, I first use the Annual Population Survey to calculate the employment rate in 2012, the year before the closures, of individuals aged 16-25 (40.2%) and the HM Revenue & Customs for the median annual income of employed individuals aged 19-25 in 2012 (£16,550). I then calculate the total foregone income of affected individuals during incarceration, roughly equal £19 million.[66]

Net cost of government The denominator of the MVPF captures the cost to the government for this policy change, including both mechanical costs as well as fiscal externalities. In my context, the mechanical costs are the public savings from the police station closures and amount to £600 million. Regarding fiscal externalities, I subtract the foregone tax revenues from fewer house sales from the stamp duty land tax, a tax on house sales.[67] In calculating the total cost to the government, I also factor in the greater fiscal cost from more incarcerations, which is approximately equal to £770 million. I finally add the fiscal savings for the CJS.

Table C8 summarises the calculations of the MVPF of closing police stations. I compute a MPVF of 1.75.[68] One advantage of calculating this ratio is that it can be compared to the MVPF of other policy changes, thereby shedding light on its relative cost-effectiveness. My estimate of the MVPF is close to the MVPFs for policies targeting adults, such as health insurance expansion, food stamps, housing voucher and cash welfare programs for low income households (Hendren and Sprung-Keyser, 2020). Overall, the findings from this exercise suggest that £1 of savings accrued on this policy delivers £1.75 additional costs borne by the society.

[66]Specifically, I calculate this figure by multiplying the number of individuals who were incarcerated because of the higher crime rate as computed in Table C3 $\times$ the employment rate $\times$ the average annual income $\times$ the average sentence served as computed in Table C3.

[67]I do not consider fiscal externalities from the income tax due to a change in future earnings, nor tax savings from the council tax (analogous to a property tax) for rents.

[68]Note that a simple non-distortionary transfer from the government to an individual would have a MVPF of 1 as the cost to the government would be exactly equal to the individuals' willingness to pay (Hendren and Sprung-Keyser, 2020).

9 Conclusion

This paper shows that reductions to police spending contribute to a significant rise in violent crimes, and to a deterioration of police effectiveness and citizens' welfare.

I exploit a natural experiment generated by a massive wave of austerity cuts to police spending, that resulted in the closures of 70% of London police stations. I document that the closures of stations increase the total number of assaults and murders, consistent with lower police presence and, therefore, lower deterrence. I also provide suggestive evidence of a decrease in citizens' reporting of non-violent offences. Such evidence suggests that the mechanisms for curbing future crimes are damaged, as citizens become less likely to provide assistance or information to law enforcement. My findings further highlight how closures damage police effectiveness to clear crimes, possibly further exacerbating the consequences of the closures in terms of lower reporting and deterrence. Finally, I report a reduction in residents' willingness to pay, measured by house prices, plausibly due to higher crime, worse local perceptions and loss of local amenities, driven by deprived and high-crime areas, suggesting that austerity cuts to police feed the inequality across areas. In spite of the fact that the decision was based on the need for fiscal adjustment, closing police stations is not cost-effective, in the sense that the public financial resources saved do not compensate the additional costs induced by the closures.

Given the recent large debate on the role of police funding for crime prevention and for social welfare, this paper carries several compelling policy implications. First, policy makers may be inclined to cut funding and reorganise resources in order to promote public sector efficiency. I show that the un-targeted, "blind" cuts are not efficient nor cost-effective and produce distributional consequences that disproportionately hit the poor. Second, I shed light on the strategic role of police infrastructure for societies aiming at providing public safety. My findings emphasise that police stations matter for crime prevention. Third, I demonstrate that these measures exacerbate the detrimental impacts of austerity cuts to the welfare system and have knock-on consequences for local communities. I quantify the true shadow price of austerity by examining the so-far overlooked reductions to police funding. Finally, although in this paper I do not perform counter-factual analyses, however it's important to consider what the optimal decision for the policy maker would be in a context of limited resources. Indeed, suppose that the alternative viable policy option was to obtain the same budgetary savings by instead reducing the size of police workforce. Based on existing estimates of the police-crime elasticity, the latter solution might have been much more costly for the society.

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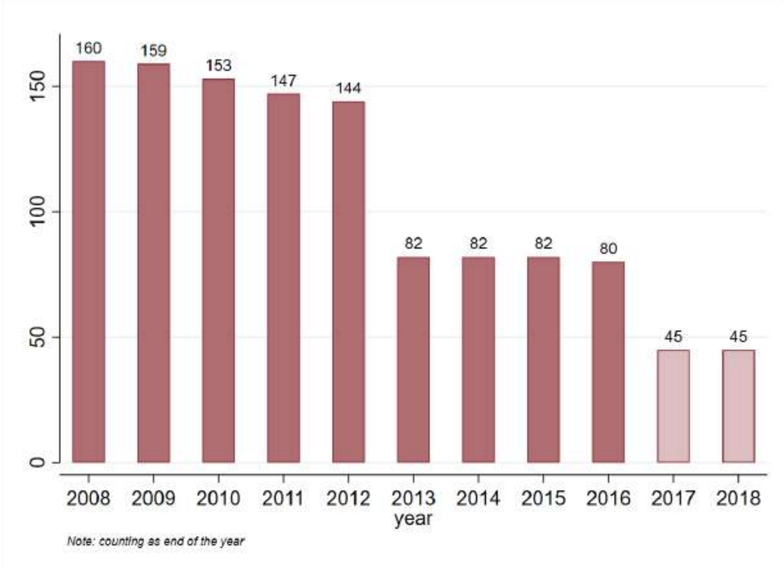
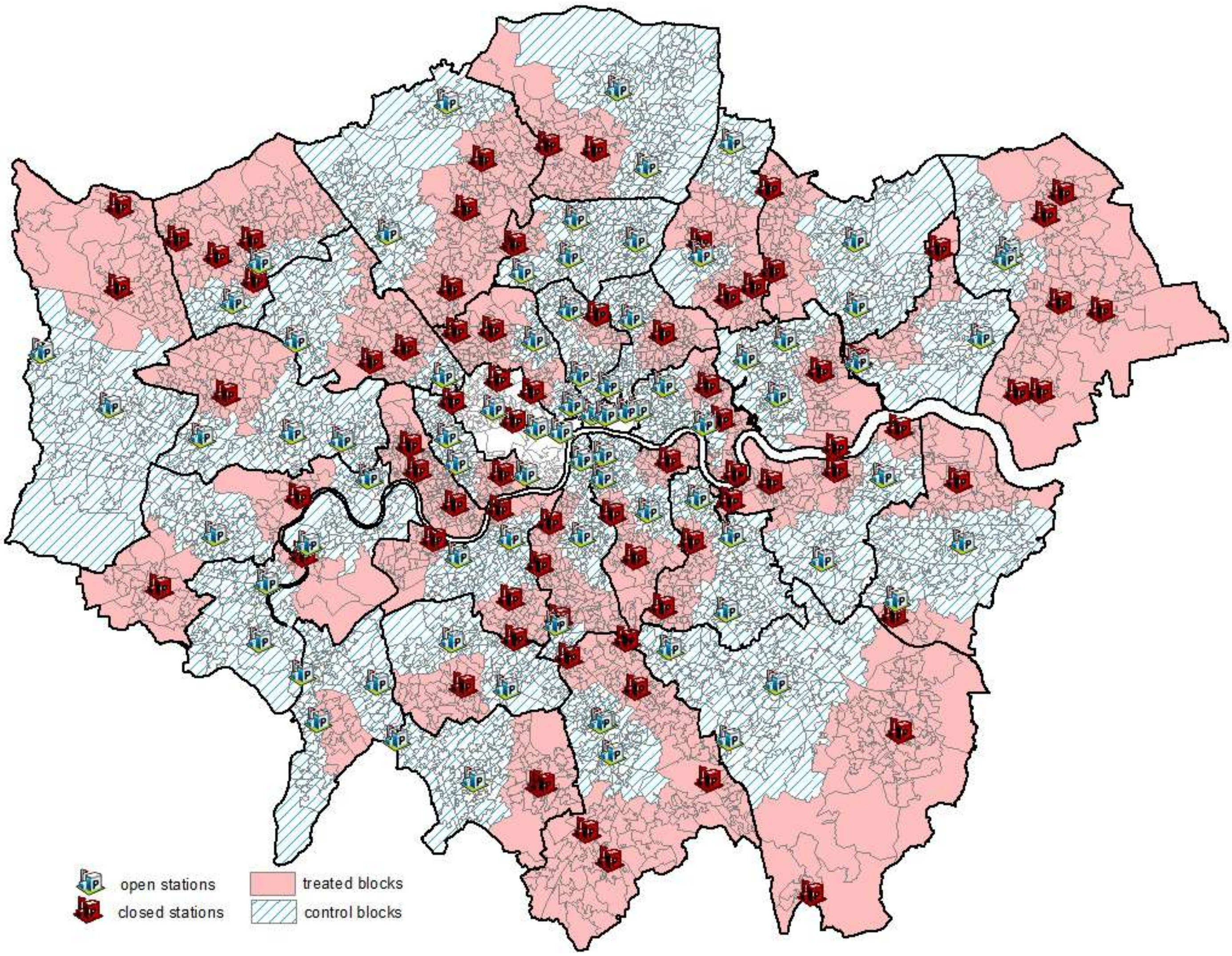
Figures

Figure 1: Number of police stations in Greater London



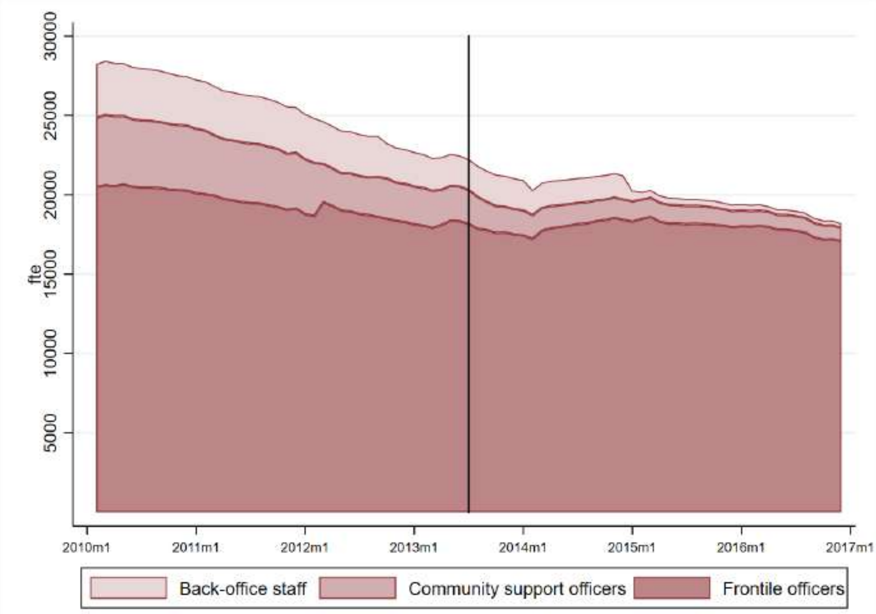
Note: The figure displays the total number of police stations operating in Greater London. The sample period of my empirical analysis includes years until 2016 only because of changes in the local policing structure that took place at the end of 2016.

Figure 2: Map of police station closings and treated blocks



Notes: The map plots the locations of police stations, including both open and closed stations, as of the end of 2016 (end of the sample period). In addition, the map codes different types of census blocks: blocks where the nearest station was closed ("treated" blocks) and blocks whose nearest station remained open ("control" blocks). Black borders correspond to boundaries to the 31 boroughs of London, excluding City of London and Westminster.

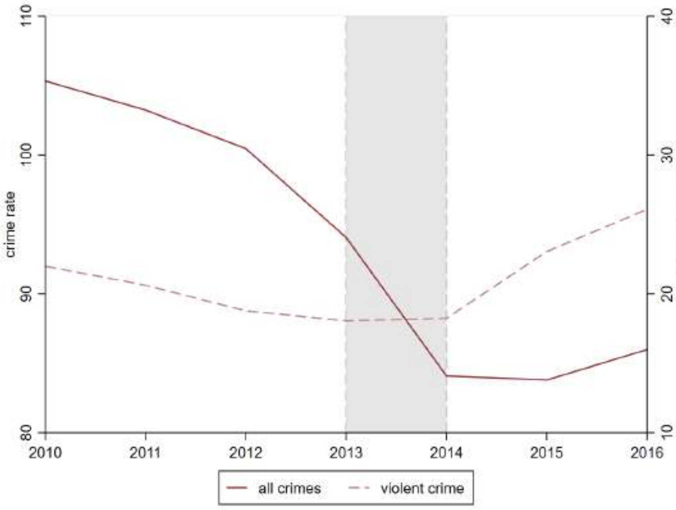
Figure 3: Police workforce



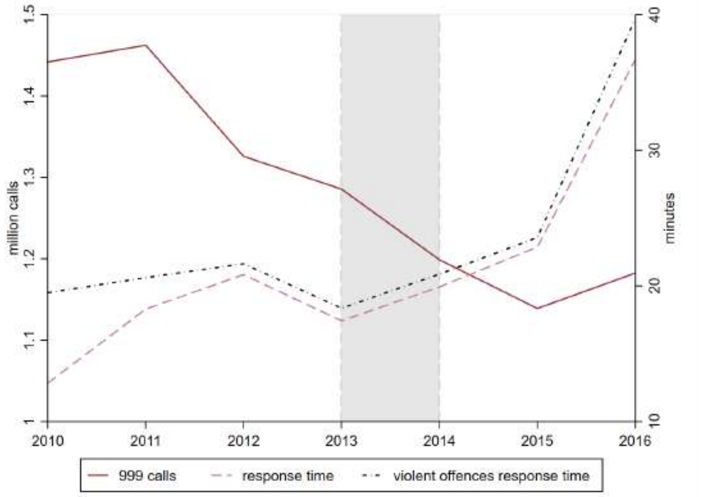
Note: This figure shows the MPS police workforce breakdown from January 2010 and December 2016. Front-line officers include police force employed in patrolling, such as police constables and detective constables. Community support officers (PCSOs) are uniformed members of police staff whose duties include high visibility patrolling, tackling anti-social behaviour, dealing with minor offences, crowd control, directing traffic. Back office roles include administrative or clerical jobs carried out by civilians such as training, finance and HR, middle office roles such as processing intelligence, call handlers, and preparing files for court. The vertical line corresponds to July 2013, where majority of closures occurred.

Figure 4: Crime and 999 calls in Greater London

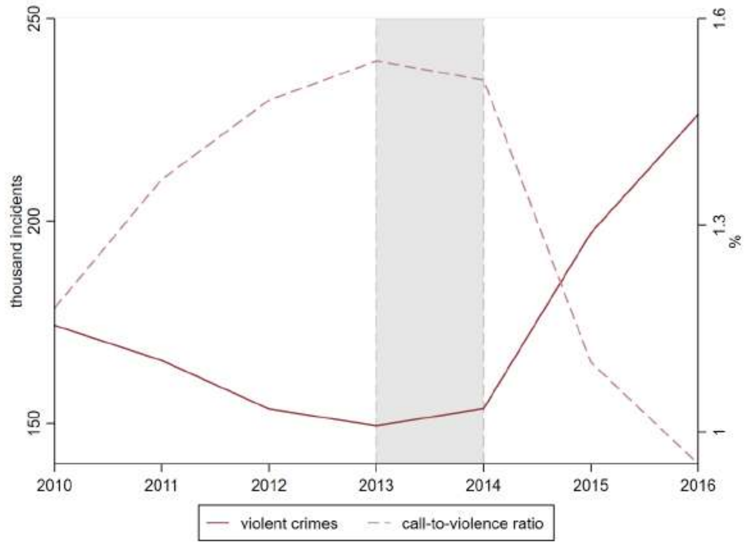
(a) Crime rates



(b) Emergency calls and response time

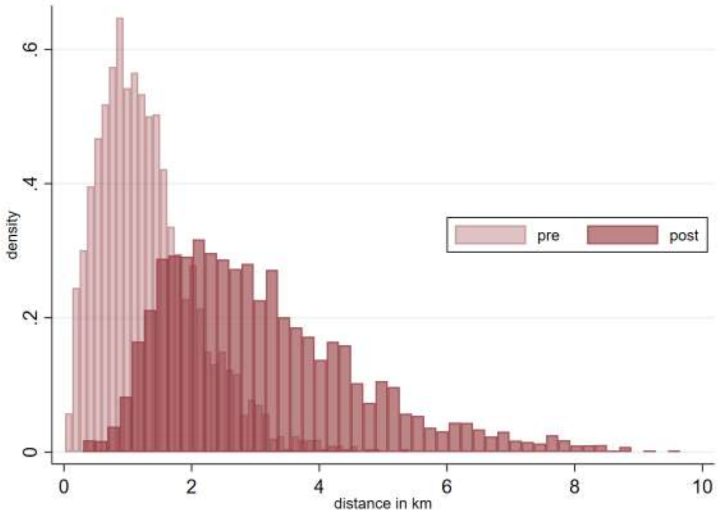


(c) Violence to emergency calls ratio



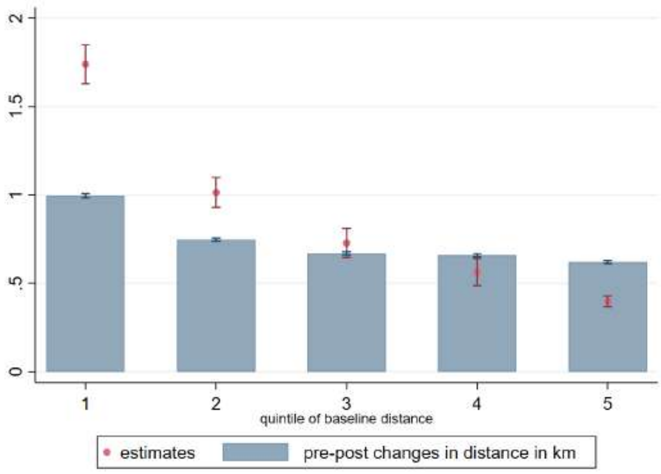
Note: Panel A displays the crime rate per 1,000 individuals. Panel B shows the number of emergency calls and the related average response time between January 2010 and December 2016 by the MET police. Average response time is for calls graded as "I" (Immediate calls) or "S" (Significant calls). Response time includes time spent on the telephone to the caller and travelling time. The average response time is computed for all offences and for violent offences only. Factors affecting response time include geography of the borough, prevailing traffic conditions, weather conditions. The shadow area corresponds to the period between 2013 and 2014, where majority of closures occurred. Panel C constructs the ratio between the number of 999 calls for violence offences only and the number of violent offence. Violent offences contains offences against the person. Source: author's calculations from Metropolitan Police Service.

Figure 5: Identifying variation in distance



Note: This graph displays the distribution of the distance to the closest police station across all census blocks whose nearest police station closed, i.e. treated blocks, before and after the closures. The sample contains all census blocks in London, excluding those located in City of London and Westminster.

Figure 6: Effect of police station closure on log distance by baseline distance

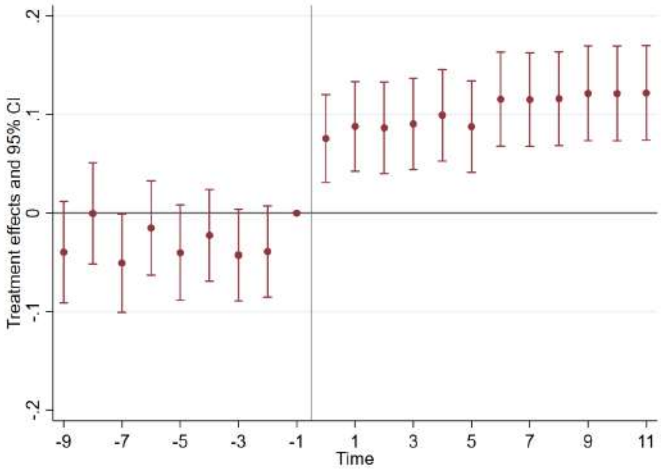


Note: The graph reports coefficients and 95% confidence intervals from the following regression:

$$logdist_{i,t} = \beta_1 Treated_{i,t} + \sum_{q=1}^{4} \gamma_q I\{Bin_i = q\} * Treated_{i,t} + \phi_i + \phi_{l,t} + \varepsilon_{i,t}$$

For each bin $q = 1, ..., 4$, I plot the coefficients $\beta_1 + \gamma_q$, using as reference category the highest quintile of baseline distance. Standard errors are clustered at the census block level. The dependent variable is the log distance from the closest station. All regressions include census block and LA-by-monthly date fixed effects. The gray bars plot the distance in the block-specific post-closure period minus the distance in the pre-period (in km).

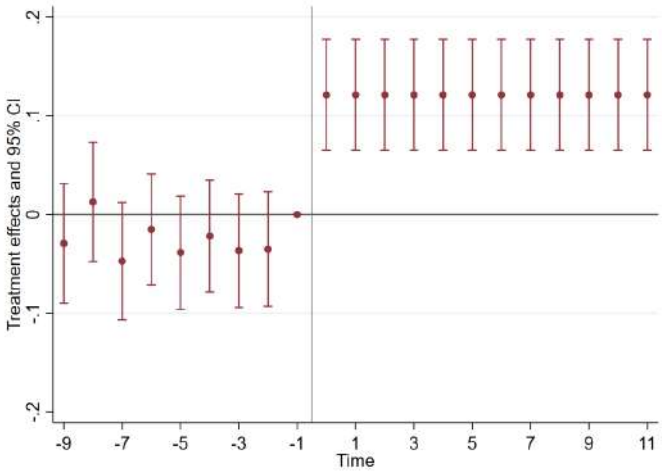
Figure 7: Event study for violent crimes



s

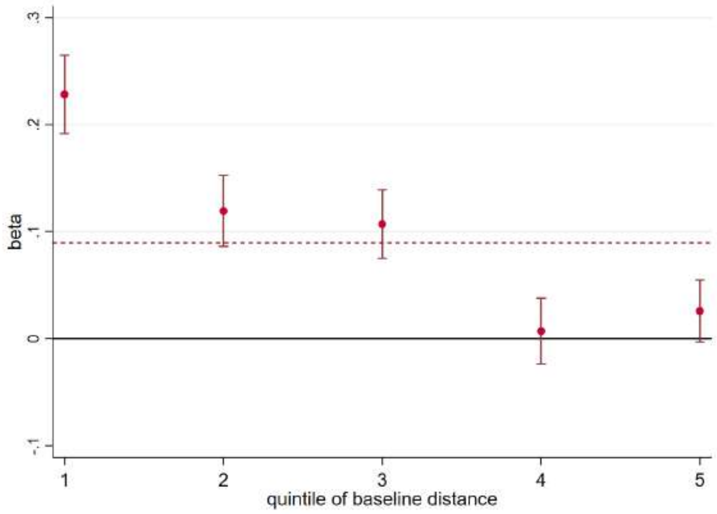
Note: The graph reports coefficients and 95% confidence intervals estimated according to Equation 2. Standard errors are clustered at the census block level. The dependent variable is the total number of violent crimes, defined as assaults and murders, recorded in a census block expressed in logs. I omit the dummy for the period before the closures. All regressions include census block, LA-by-quarterly date fixed effects, and relative time fixed effects.

Figure 8: Event study for violent crimes for the main treatment group



Note: The graph reports coefficients and 95% confidence intervals estimated according to Equation 2. Following Callaway and Sant'Anna (2020), I keep only units treated in the main treatment wave, the third quarter of 2013, when 80% of closures happened, and all control units. Standard errors are clustered at the census block level. The dependent variable is the total number of violent crimes, defined as assaults and murders, recorded in a census block expressed in logs. I omit the dummy for the period before the closures. All regressions include census block, LA-by-quarterly date fixed effects, and relative time fixed effects.

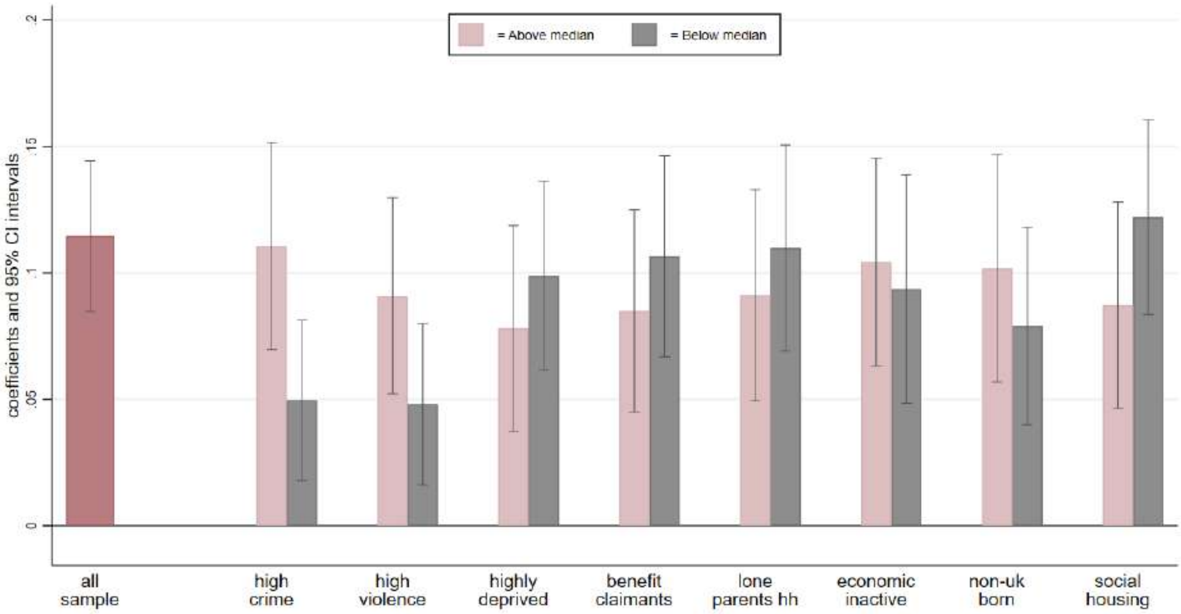
Figure 9: Effects on violent crimes by baseline distance



S

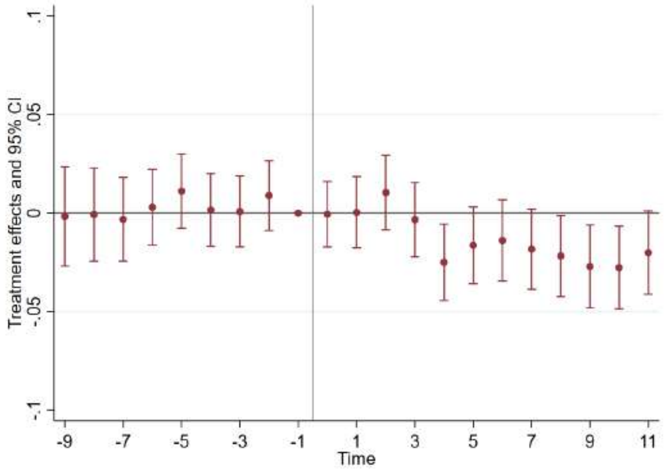
Note: The graph reports coefficients and 95% confidence intervals estimated based on Equation 1 for each quintile of baseline distance, i.e. distance measured before any change occurs. The dashed horizontal line reports the average effect. The dependent variable is the total number of violent crimes, defined as assaults and murders, recorded in a census block expressed using the hyperbolic sine transformation (*asinh*). All regressions include census block and LA-by-monthly date fixed effects. Standard errors are clustered at the census block level.

Figure 10: Effects on violent crime by baseline characteristics of the census blocks



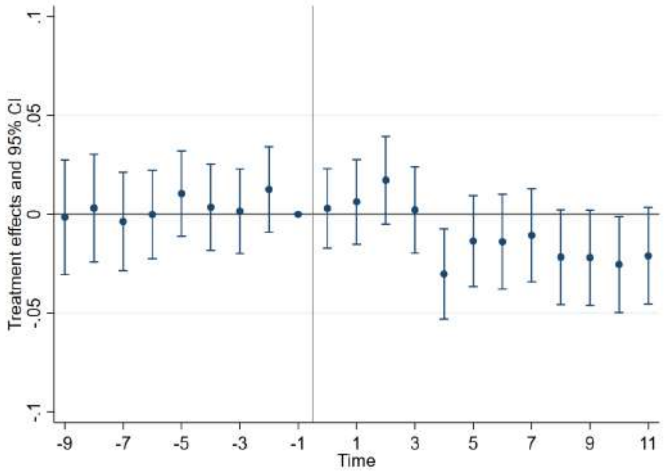
Note: The figure plots estimates from Equation 1 splitting the sample by baseline characteristics (above versus below the London median). The dependent variable is the total number of violent crimes, defined as assaults and murders, recorded in a census block expressed using the hyperbolic sine transformation (*asinh*). Baseline characteristics of the census blocks come from the Census (2011). Deprivation indices come from The English Indices of Deprivation in 2010, computed by the Ministry of Housing, Communities and Local Government. Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Figure 11: Event study for total reported crime



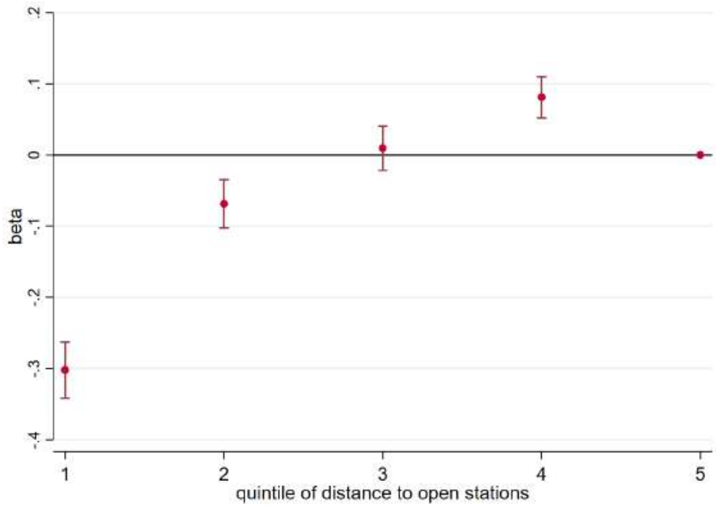
Note: The graph reports coefficients and 95% confidence intervals estimated according to Equation 2. Standard errors are clustered at the census block level. The dependent variable is the total number of crime recorded in a census block expressed in logs. I omit the dummy for the period before the closures. All regressions include census block, LA-by-quarterly date fixed effects, and relative time fixed effects.

Figure 12: Event study for total reported crime for the main treatment group



Note: The graph reports coefficients and 95% confidence intervals estimated according to Equation 2. Following Callaway and Sant'Anna (2020), I keep only units treated in the main treatment wave, the third quarter of 2013, when 80% of closures happened, and all control units. Standard errors are clustered at the census block level. The dependent variable is the total number of crime recorded in a census block expressed in logs. I omit the dummy for the period before the closures. All regressions include census block, LA-by-quarterly date fixed effects, and relative time fixed effects.

Figure 13: Spillover effects on control group

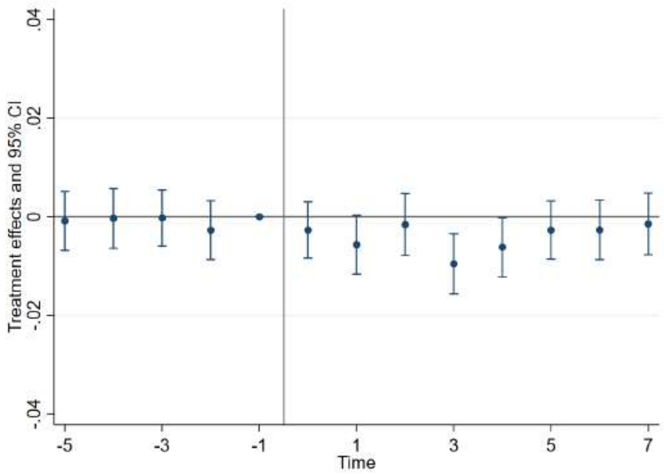


Note: The figure reports the coefficients and confidence intervals from estimating the following regression:

$$y_{i,t} = \sum_{q=1}^{4} \delta_q I\{Bin_i = q\} * Post_{l,t} + \phi_i + \phi_{t,l} + \varepsilon_{it}$$

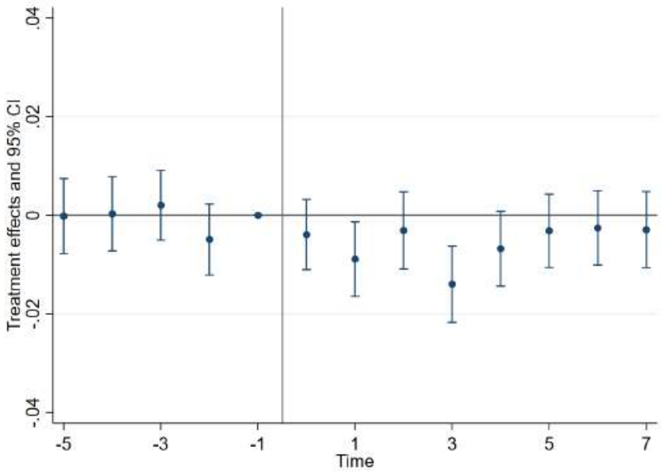
For each bin $q = 1,...,4$ I plot the coefficient δ_q, using as omitted category the highest quintile of baseline distance. Standard errors are clustered at the census block level. All regressions include census block and LA-by-monthly date fixed effects. Standard errors are clustered at the census block level. The dependent variables are transformed using the *asinh*.

Figure 14: Event study for clearance



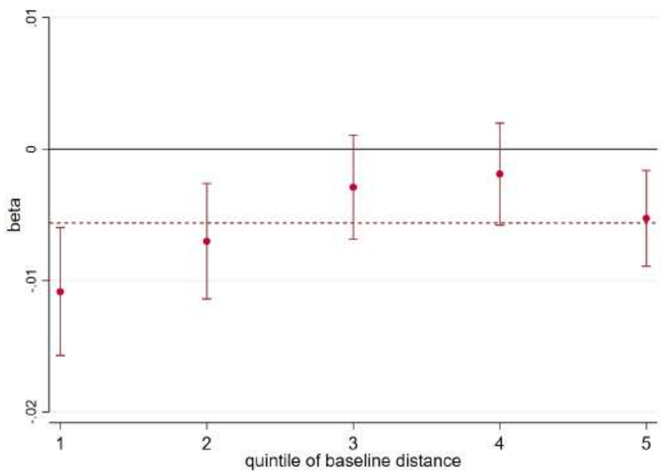
Note: The graph reports coefficients and 95% confidence intervals estimated according to Equation 2. Standard errors are clustered at the census block level. The dependent variable is an indicator for whether a criminal incident was cleared. I omit the dummy for the period before the closures. All regressions include census block, LA-by-quarterly date fixed effects, and relative time fixed effects.

Figure 15: Event study for clearance for the main treatment group



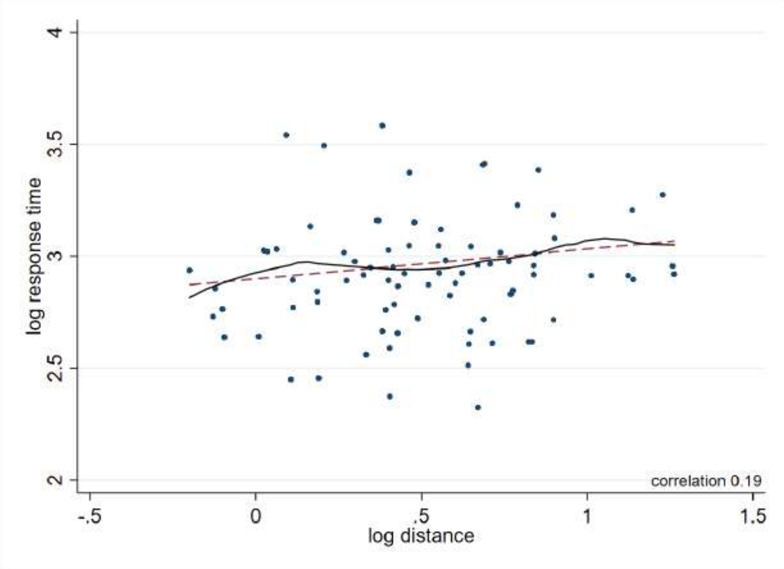
Note: The graph reports coefficients and 95% confidence intervals estimated according to Equation 2. Following Callaway and Sant'Anna (2020), I keep only units treated in the main treatment wave, the third quarter of 2013, when 80% of closures happened, and all control units. Errors are clustered at the census block level. The dependent variable is an indicator for whether a criminal incident was cleared. I omit the dummy for period before the closures. All regressions include census block, LA-by-quarterly date fixed effects, and relative time fixed effects.

Figure 16: Effect on clearance by baseline distance



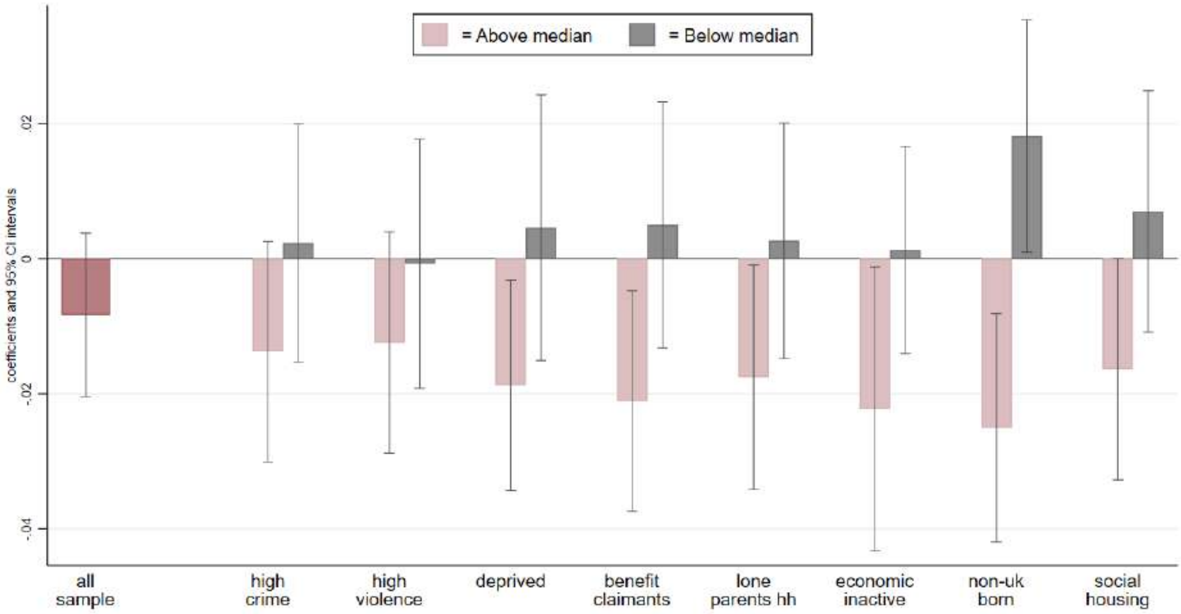
Note: The graph reports the coefficients and 95% confidence intervals of separate regressions estimated based on Equation 1 for each quintile of baseline distance, i.e. distance measured before any change occurs. The dashed horizontal line reports the average effect. The dependent variable is a dummy variable equal to 1 if the incident was cleared. All regressions include census block and LA-by-monthly date fixed effects. The sample is restricted to incidents with a non-missing investigative outcome. Errors are clustered at the census block level.

Figure 17: Relationship between distance and response time



Note: The figure shows the relationship between distance and response time using data at the Local Authority level between 2010 and 2016. Both variables are transformed in logarithms. I group distance into 500 equally-sized bins and I compute the average log response time per distance bin. The dark line is estimated using a local linear polynomial, the red dashed line shows the linear fit. Source: Metropolitan Police Service and author's calculations.

Figure 18: Effects on house prices by baseline characteristics of the census blocks



Note: The figure plots estimates from Equation 1, with 95% confidence intervals, splitting the sample by baseline characteristics (above versus below the London median). The dependent variable is the average (log) house prices computed in the census block, the explanatory variable is the dummy treatment as defined in Section 4. The dataset is collapsed at the quarterly level. Baseline characteristics of the census blocks come from the Census (2011). Deprivation indices come from The English Indices of Deprivation in 2010, computed by the Ministry of Housing, Communities and Local Government. High-crime blocks are defined as blocks whose baseline crime rate (computed in 2008) is above the London median. Crime rates for 2008 are computed from the MPS Historical Crime Data collection. All regressions include census block, and LAxdate fixed effects. *LA* refers to the 31 London Local Authorities, excluding City of London and Westminster. The observations are weighted by the number of sales in the census block during the quarter. Standard errors are clustered at the census block level.

Tables

Table 1: Local characteristics that predict police station presence and closure

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All census blocks						
Outcomes	police presence			station ever closed		
logcrime	0.068***			0.027***		
	[0.007]			[0.006]		
logviolcrime		0.021***	0.047***		0.010**	0.021***
		[0.004]	[0.008]		[0.004]	[0.007]
logpropcrime		0.035***	0.008		0.020***	0.008
		[0.008]	[0.008]		[0.006]	[0.006]
logdrugs		0.010***	0.012***		-0.002	-0.004
		[0.003]	[0.004]		[0.002]	[0.003]
log house prices			0.047***			0.012
			[0.011]			[0.008]
N blocks	4,701	4,701	4,701	4,701	4,701	4,701
Observations	13,584	12,993	7,718	13,575	12,993	7,718
FE	LAxYear	LAxYear	LA	LAxYear	LAxYear	LA
Panel B: Census blocks with any police station						
Outcomes	station ever closed			station ever sold		
logcrime	-0.241***			-0.134**		
	[0.057]			[0.064]		
logviolcrime		-0.080	-0.326***		-0.123	-0.337**
		[0.108]	[0.105]		[0.096]	[0.126]
logpropcrime		0.159	0.259**		0.129	0.145
		[0.100]	[0.097]		[0.088]	[0.093]
logdrugs		-0.246***	-0.267***		-0.109*	-0.076
		[0.056]	[0.055]		[0.056]	[0.057]
log house prices			-0.484***			0.024
			[0.145]			[0.220]
N blocks	148	148	148	148	148	148
Observations	417	415	251	417	415	251
FE	LAxYear	LAxYear	LA	LAxYear	LAxYear	LA

Note: Each column displays results from separate OLS regressions, where the dependent variables are in Panel A indicators for station presence and station closure, in Panel B indicators for station presence and sale of a police station. Panel A keeps the entire sample of census blocks, while Panel B restricts the sample to blocks with an operating police station at the beginning of the sample. The crime variables are computed on the 2008-2010 sample period, before any closure occurred. House prices are computed as average house prices in the census block between 2006 and 2010. *LA* refers to the 31 London Local Authorities, excluding City of London and Westminster. Column 1-2 add LA-by-year fixed effects, column 3 includes LA fixed effects. Crime data for years before 2010 are taken from the MPS Historical Crime Data collection. *** p<0.01, ** p<0.05 * p<0.1.

Table 2: Descriptive statistics for dataset at census block level

	Treated blocks	Control blocks
	(1)	(2)
Panel A: crime dataset		
Distance		
Distance from closest police station (in km)	2.34	1.45
=1 if distance increases	0.57	0.00
Crime		
All crimes	15.56	18.57
Violent crimes	2.91	3.52
Robbery	0.41	0.51
Sexual offences	1.66	2.01
Assaults and murders	0.84	1.00
Property crimes	6.25	7.23
Theft	0.39	0.63
Burglary	1.38	1.40
Criminal damage and arson	0.85	0.97
Shoplifting	0.49	0.64
Vehicle crime	1.55	1.51
Drugs	0.51	0.69
Public order offences	0.52	0.66
ASB	4.46	5.36
Observations	146,808	191,664
Panel B: house price dataset		
Distance		
Distance from closest police station (in km)	2.36	1.45
=1 if distance increases	0.58	0.00
House prices		
House prices	575,310.85	556,889.29
Number of transactions	11.29	9.60
Observations	48,936	63,888

Note: This table shows summary statistics for the census block (LSOA) level dataset for crime (Panel A) and house prices (Panel B) between 2011 and 2016. The crime dataset is at the monthly frequency, while the house price dataset is at the quarterly frequency. Treated blocks are blocks which experienced an increase in distance from the closest police station throughout the sample period (2010-2016).

Table 3: Descriptive statistics for dataset at incident level

	Treated blocks (1)	Control blocks (2)
Panel A: investigation outcome dataset		
Distance		
distance from closest police station (in km)	2.37	1.28
=1 if distance increases	0.69	0.00
Investigation outcomes		
Cleared	0.17	0.18
Not cleared	0.83	0.82
Out-of-court sanction	0.07	0.07
Court sentence	0.10	0.11
Convicted	0.08	0.09
No sufficient evidence	0.00	0.01
No suspect identified	0.47	0.47
Observations	1,262,299	1,969,379

Note: This table shows summary statistics for the census block (LSOA) characteristics at baseline. The crime dataset is at the monthly frequency, while the house price dataset is at the quarterly frequency. Treated blocks are blocks which experienced an increase in distance from the closest police station throughout the sample period (2010-2016).

Table 4: Effects of closings on violent crimes

	(1)	(2)	(3)	(4)	(5)
Dependent variable			Assaults and murders		
Panel A: binary treatment					
dummy distance	0.090***	0.111***	0.115***	0.037**	0.023
	[0.015]	[0.015]	[0.015]	[0.015]	[0.027]
Panel B: continuous treatment					
distance	0.086***	0.087***	0.087***	0.017	-0.042
	[0.016]	[0.016]	[0.016]	[0.017]	[0.036]
Observations	338,472	338,472	338,472	338,472	338,472
Calendar month FE	✓	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓	✓
FE		LAxYear	LAxDate	WardxYear	WardxDate

Note: Tee table displays results using as dependent variable the total number of violent criminal incidents recorded by the police and expressed using the hyperbolic sine transformation (*asinh*). Violent crimes include assaults and murders according to the MPS classification. In Panel A the explanatory variable is the dummy treatment as defined in Section 4; in Panel B the explanatory variable is the geodesic distance between the centroid of the census block and the closest police station, measured in km and transformed in logs. *LA* refers to the 31 London LAs (excluding Westminster and City of London). Wards are the primary electoral divisions used in England. Each ward contains a median number of 7 census blocks (LSOAs). Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Effects of closings on total reported crime

	(1)	(2)	(3)	(4)	(5)
Dependent variable			log Crime		
Panel A: binary treatment					
dummy distance	-0.015***	-0.011**	-0.013**	-0.014*	-0.025**
	[0.005]	[0.006]	[0.006]	[0.007]	[0.012]
Panel B: continuous treatment					
distance	-0.012***	-0.010**	-0.012***	-0.011**	-0.017**
	[0.004]	[0.004]	[0.004]	[0.006]	[0.009]
Observations	337,122	337,122	337,122	337,122	337,122
Calendar month FE	✓	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓	✓
FE		LAxYear	LAxDate	WardxYear	WardxDate

Note: The table displays results using as dependent variable the total number of criminal incidents recorded by the police and expressed in logs. In Panel A, the explanatory variable is the dummy treatment as defined in Section 4; in Panel B the explanatory variable is the geodesic distance between the centroid of the census block and the closest police station, measured in km and transformed in logs. *LA* refers to the 31 London LAs (excluding Westminster and City of London). Wards are the primary electoral divisions used in England. Each ward contains a median number of 7 census blocks (LSOAs). Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Effects of closings on violent crimes

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Assaults and murders		Robbery		Sexual offences	
Panel A: binary treatment						
dummy distance	0.090***	0.115***	-0.006	0.016***	-0.107***	-0.133***
	[0.015]	[0.015]	[0.005]	[0.005]	[0.016]	[0.016]
Panel B: continuous treatment						
distance	0.086***	0.087***	-0.002	0.008	-0.104***	-0.110***
	[0.016]	[0.016]	[0.005]	[0.005]	[0.017]	[0.017]
Observations	338,472	338,472	338,472	338,472	338,472	338,472
Calendar month FE	✓	✓	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓	✓	✓
FE		LAxDate		LAxDate		LAxDate

Note: In Panel A, the explanatory variable is the dummy treatment as defined in Section 4; in Panel B the explanatory variable is the continuous geodesic distance between the centroid of the census block and the closest police station, measured in km and transformed using the *asinh* transformation. The dependent variables are transformed using the *asinh*. *LA* refers to the 31 London LAs (excluding Westminster and City of London). Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Effects of closings on property crimes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dependent variable	Property crime		Burglary		Criminal damage and arson		Shoplifting		Theft		Vehicle crime	
Panel A: binary treatment												
dummy distance	-0.021***	-0.021***	0.002	0.003	-0.029***	-0.034***	-0.008	-0.010	-0.109***	-0.110***	-0.004	0.002
	[0.008]	[0.008]	[0.007]	[0.007]	[0.005]	[0.006]	[0.007]	[0.008]	[0.013]	[0.013]	[0.007]	[0.007]
Panel B: continuous treatment												
distance	-0.016**	-0.012	0.007	0.016**	-0.030***	-0.034***	0.004	0.002	-0.093***	-0.061***	-0.005	-0.001
	[0.008]	[0.008]	[0.007]	[0.007]	[0.006]	[0.006]	[0.008]	[0.009]	[0.013]	[0.013]	[0.007]	[0.007]
Observations	338,472	338,472	338,472	338,472	338,472	338,472	338,472	338,472	338,472	338,472	338,472	338,472
Calendar month FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FE		LAxDate		LAxDate		LAxDate		LAxDate		LAxDate		LAxDate

Note: In Panel A, the explanatory variable is the dummy treatment as defined in Section 4; in Panel B the explanatory variable is the continuous geodesic distance between the centroid of the census block and the closest police station, measured in km and transformed using the *asinh* transformation. The dependent variables are transformed using the *asinh* transformation. *LA* refers to the 31 London LAs (excluding Westminster and City of London). Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Spillover effects on violent crimes

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Assaults and murders					
Post	0.062***		0.018		0.018	
	[0.019]		[0.018]		[0.027]	
dummy Near*Post	-0.238***	-0.180***				
	[0.021]	[0.024]				
dummy Bottom Quartile*Post			-0.331***	-0.271***		
			[0.029]	[0.030]		
1/distance*Post					-0.071***	-0.059***
					[0.020]	[0.018]
Observations	181,008	181,008	181,008	181,008	181,008	181,008
Calendar month FE	✓	✓	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓	✓	✓
LA*Date FE		✓		✓		✓

Note: The table displays the estimates from Equation 3. *Dummy near* is equal to 1 if the census block is below the median distance to the nearest police station. Among the 2,514 control blocks, 1,257 are below the median. *Dummy Bottom Quartile* is equal to 1 if the census block is below the bottom quartile distance. The dependent variable is the number of violent crimes transformed using the *asinh* transformation. *LA* refers to the 31 London LAs (excluding Westminster and City of London). Regressions are run on the sample of control blocks (2,514). Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Aggregate regressions at local authority level

	(1)	(2)	(3)	(4)	(5)
Dependent variable	Assaults and murders	all crimes	property crimes	thefts	burglaries
Panel A: binary treatment					
share treated	0.527*	-0.026	-0.042	0.124	0.038
	[0.308]	[0.028]	[0.054]	[0.582]	[0.093]
Panel B: Continuous treatment					
distance	0.615*	-0.023	-0.043	-0.268	-0.023
	[0.360]	[0.023]	[0.049]	[0.592]	[0.089]
Observations	2,232	2,232	2,232	2,232	2,232
Calendar month FE	✓	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓	✓

Note: The table shows aggregate regressions on the local authority level dataset. In Panel A, the explanatory variable is the monthly share of treated blocks within the LA. In Panel B it's the LA-specific average distance of each block to the closest police station. The dependent variables are the total number of crimes transformed using the *asinh* transformation. *LA* refers to the 31 London LAs (excluding Westminster and City of London). Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Table 10: Effect of closings on clearance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sample	Incidents with non-missing investigation outcome								
Dependent variable	Pr(cleared)			Pr(informal sanction)			Pr(convicted)		
Panel A: binary treatment									
dummy distance	-0.006***	-0.003**	-0.003**	-0.004***	-0.003**	-0.003***	-0.001	-0.001	-0.000
	[0.002]	[0.002]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
Panel B: continuous treatment									
distance	-0.005***	-0.001	-0.003***	-0.003***	-0.002**	-0.002***	-0.001	0.000	-0.001
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
Observations	3,231,678	3,231,678	3,231,678	3,231,678	3,231,678	3,231,678	3,231,678	3,231,678	3,231,678
Calendar date FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Crime type FE			✓			✓			✓
Date x LA FE		✓			✓			✓	

Note: Each column displays results from a separate OLS regression restricting the sample to incidents with non-missing investigation outcomes. The outcome variables are an indicator equal to 1 if the incident has been cleared (Column 1-3), for an informal sanction (Column 4-6) or for a conviction (Column 7-9). For a detailed definition of the outcome categories see Appendix Table B4. Panel A uses as explanatory variable the dummy treatment as defined in Section 4, while Panel B uses the geodesic distance between the centroid of the census block and the closest police station, measured in km and transformed in logs. *LA* refers to the 31 London LAs (excluding Westminster and City of London). All regressions include census block and monthly date fixed effects. Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Table 11: Volume of cleared and convicted crimes

	(1)	(2)	(3)	(4)	(5)	(6)
Type of crime	All crimes		Property crimes		Violent crimes	
Panel A: Dependent variable: # cleared crimes						
I. Binary treatment						
dummy distance	-0.073***	-0.070***	-0.027***	-0.029***	-0.039***	-0.038***
	[0.009]	[0.009]	[0.006]	[0.007]	[0.005]	[0.006]
II. Continuous treatment						
distance	-0.063***	-0.050***	-0.018***	-0.018**	-0.037***	-0.032***
	[0.009]	[0.010]	[0.007]	[0.007]	[0.005]	[0.006]
Panel B: Dependent variable: # convictions						
I. Binary treatment						
dummy distance	-0.044***	-0.047***	-0.017***	-0.019***	-0.019***	-0.021***
	[0.007]	[0.007]	[0.005]	[0.005]	[0.003]	[0.004]
II. Continuous treatment						
distance	-0.035***	-0.030***	-0.011**	-0.012**	-0.016***	-0.014***
	[0.007]	[0.007]	[0.005]	[0.005]	[0.003]	[0.004]
Observations	338,472	338,472	338,472	338,472	338,472	338,472
Calendar date FE	✓	✓	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓	✓	✓
LA*Date FE		✓		✓		✓

Note: Each column displays results from a separate OLS regression estimated as Equation 1. Panel A uses outcome variable the the total number of cleared crimes, while panel B uses as outcome variable the total number of convicted crimes, where convictions refer to incidents declared guilty of a criminal offence by the verdict of a court (thus excluding acquittals and discharges). All outcome variables are transformed using the inverse hyperbolic sine transformation, following Bellemare and Wichman (2020) and Card et al. (2020). I use both binary and continuous definitions of the explanatory variable as defined in Section 4. *LA* refers to the 31 London boroughs. All regressions include census block and monthly date fixed effects. Standard errors are clustered at the census block level. See Appendix Table B3 for the definitions of violent and property crimes. *** p<0.01, ** p<0.05, * p<0.1.

Appendix A Conceptual framework to interpret the effects

I present here a stylised conceptual framework to guide the interpretation of the impacts of police station closures on reported crime, actual crime and clearance. The starting point is a standard economic model of crime (Becker, 1968; Ehrlich, 1973). I extend the simple case of homogeneity of agents to outline an intuitive model with heterogeneous agents that can be applied to my empirical setting.

Conditional on committing a crime, criminals decide which type of offence to commit. In particular, suppose for simplicity that there are two types of criminals, who can choose to commit either a high-severity, V, or a low-severity, P, type of crime.[69] For each type of crime j ($j \in \{P, V\}$), the total number of actual crimes C_j^* inversely depends on the offenders' probability of apprehension p, $C_j^* = C_j^*(p)$. Reported crime is defined as the fraction of actual crimes that are either reported by residents to the police or directly detected by the police, and therefore equals $C_j = q_j \cdot C_j^*$, where $q_j = Pr(report|j)$ is the probability of reporting crime j (Bhuller et al., 2013; Comino et al., 2020). The probability that victims report a crime, q_j, depends on the severity of the crime. I assume that more severe crimes are more likely to be reported than less severe crimes. I finally define the total number of reported offences as a function of the distance to the police station, where $C_j = q_j(d) \cdot C_j^*(p(d))$.

I derive the overall relationship between the reported crime j and the distance to the station as:

$$\frac{\partial C_j}{\partial d} = C_j^* \frac{\partial q_j}{\partial d} + q \frac{\partial C_j^*}{\partial p} \cdot \frac{\partial p}{\partial d} \qquad j = P, V \tag{4}$$

In the spirit of Becker (1968), I also postulate the relation between crime and the output of the police activity, measured as the number of cleared offences. The total number of cleared offences is equal to $A_j = \lambda_j C_j$, where λ_j is the probability of clearance and depends on the type of crime j.[70] Similarly to reported crime, I define the total number of cleared offences as a function of the distance to the police station, as $A_j = \lambda_j(d) \cdot q_j(d) \cdot C_j^*(p(d))$, and measure the

[69] The type of agent and the type of crime overlap. An illustrative example would be as follows. Suppose there are only two types of criminals: thieves (T) and murderers (M). Thieves only commit low-severity crimes (P) while murderers only high-severity crimes (V). The underlying assumption is that the $Pr(P|M) = 0$ and $Pr(V|T) = 0$, that is, types of offences are complementary and criminals fully "specialise" in a specific type of crime.

[70] I assume that the $\lambda_p < \lambda_v$, that is, the probability of clearing a low-severity crime is lower than a high-severity crime. This assumption is validated with the data. For instance, the clearance rate of thefts is around 3%, while of murders and assaults is more than 20%.

net impact of distance on clearance of crime type j as:

$$\frac{\partial A_j}{\partial d} = q_j C_j^* \frac{\partial \lambda_j}{\partial d} + \lambda_j C_j^* \frac{\partial q_j}{\partial d} + \lambda_j q_j \frac{\partial C_j^*}{\partial d} \cdot \frac{\partial p}{\partial d} \qquad j = P, V \tag{5}$$

Equation 4 highlights that the net effect of distance on reported crime depends of two distinct components, namely, reporting and deterrence.

First, the victims' propensity to report a crime depends on the type of offence j. Victims of more severe crimes have a higher propensity to report given that the marginal cost of non-reporting, including both monetary and non-monetary costs, is higher. In contrast, victims of low key crimes have lower marginal benefits to report.[71] For illustrative purposes, I assume in my setting that for the high-severity type V the propensity to report is inelastic to changes in distance, i.e. $\partial q_V / \partial d = 0$. In contrast, for the low-severity type an increase in distance translates into a higher cost of reporting, implying $\partial q_P / \partial d < 0$.[72]

Second, distance affects the number of crimes committed through deterrence: higher distance to police stations implies lower police presence on the ground. The second term of Equation 4 captures the effect on actual crime C^*, which can be decomposed into two channels, which together contribute to the overall positive impact on reported crime: the direct effect of distance on the risk of apprehension, $\frac{\partial p}{\partial d}$, and the deterrence effect on actual crime, $\frac{\partial C^*}{\partial p}$. The deterrence role of police is widely established in the literature (Glaeser, 1999; Chalfin and McCrary, 2017), implying both $\partial p / \partial d < 0$ and $\partial C^* / \partial p < 0$: the reduced-form effect of higher distance via deterrence therefore increase the total number of crimes, irrespectively of the type j.

Equation 5 emphasises that the net effect of distance on clearance consists instead of three distinct components. The last two terms incorporate the reporting and deterrence effects that directly stem from Equation 4. The first term shows in addition the direct effect on police effectiveness. Distance directly affects police ability to clear crimes, λ, as longer distances imply longer response time to attend crime scenes and hence lower chances to collect evidence

[71] Another way to look at it is that victims' benefits from reporting are a function of the damage suffered from the offence (Comino et al., 2020). Victims report the crime to the police when the monetary loss is larger than the cost of reporting, which in my case is a function of the distance to the police station. We can therefore think of the threshold for reporting, which determines the victim's propensity to report, being higher for low-severity than for high-severity crimes.

[72] The economic literature often uses distance or travelling time as a proxy for the opportunity-cost of the individual's willingness to travel in the context of, for instance, decisions related to education (Card, 1993; Duflo, 2001), to consumption (Davis et al., 2019), to disability programs (Deshpande and Li, 2019), or police complaints (Ba, 2018).

and information to clear crimes, implying that $\partial\lambda_j/\partial d < 0$.[73]

In light of such considerations, the reporting component of high-severity crimes is 0. I can therefore derive Equations 4 and 5 separately for for high-severity crimes V and I obtain:

$$\frac{\partial C_V}{\partial d} = \underbrace{q_V \frac{\partial C_V^*}{\partial p} \cdot \frac{\partial p}{\partial d}}_{\text{deterrence (>0)}} \tag{6}$$

$$\frac{\partial A_V}{\partial d} = \underbrace{q_V\, C_V^* \frac{\partial \lambda_V}{\partial d}}_{\text{police performance (<0)}} + \underbrace{\lambda_V\, q_V \frac{\partial C_V^*}{\partial d} \cdot \frac{\partial p}{\partial d}}_{\text{deterrence (>0)}} \tag{7}$$

This simple model allows me to derive some predictions for the empirical analysis.

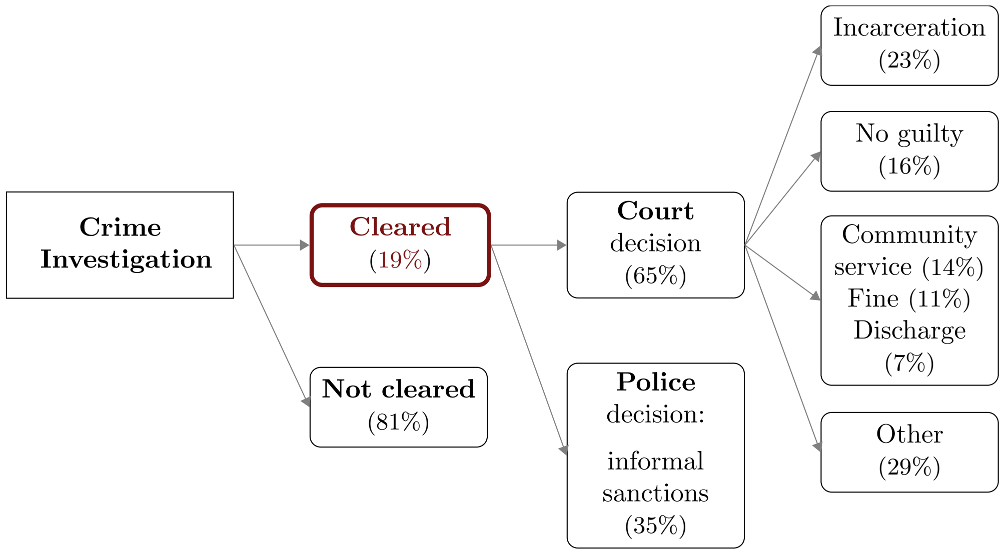
First, Equation 6 shows that the elasticity of crime with respect to distance is always positive, so that an increase in the distance to the closest police station generates an increase in the number of high-severity offences. Conversely, for low-severity offences, the effect on reported crime is a-priori uncertain as it depends on the size of the reduction in the reporting propensity. Assuming that the marginal deterrence is the same across different types of crimes (i.e. $\partial C^*/\partial d$ does not vary with j), the difference between high- and low-severity crimes can be interpreted as evidence of change in the reporting behaviour of residents.

Second, the net effect on clearance is a-priori ambiguous. However, comparing the effects on offences of various severity is informative on the most likely underlying mechanisms. For instance, if I observe a decrease in the number of high-severity offences, I attribute it the fact that the deterioration in police performance outweighs the relative importance of the deterrence channel. In the empirical analysis, I will therefore exploit the heterogeneity across types of crimes to estimate the relative importance of the various channels when looking at clearance as well. Given that I do not observe the intrinsic crime severity, I will use as a proxy the type of crime, and specifically compare results for violent versus property offences.

[73] Blanes i Vidal and Kirchmaier (2018) document that rapid response policing is a key determinant of clearance rate and use variation in distance as instrument for response time, reporting a strong and positive first stage.

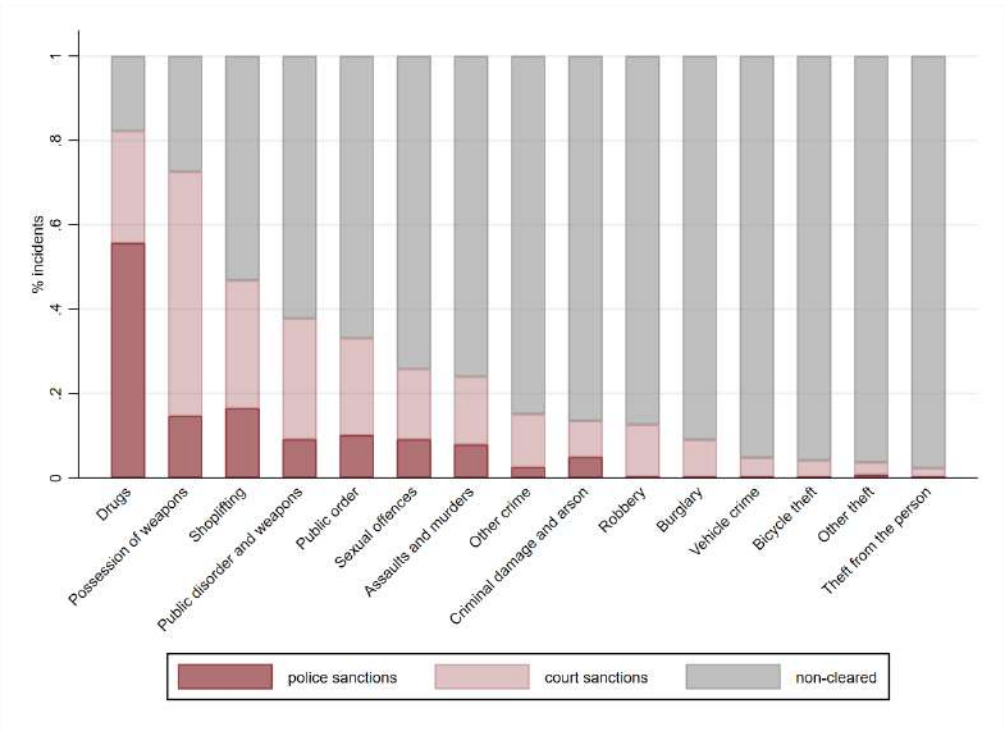
Appendix B

Figure B1: Processing of criminal investigations in England



Note: Sample consists of all criminal incidents investigated by the police before 2013, i.e. before any station closure. Source: author's calculations from Metropolitan Police Service.

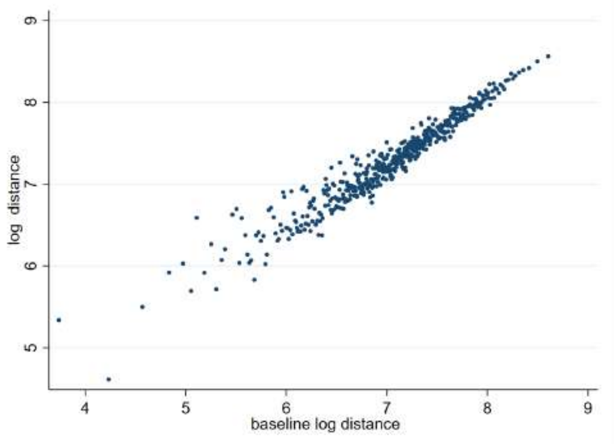
Figure B2: Share of incidents cleared by the police, the court or left uncleared



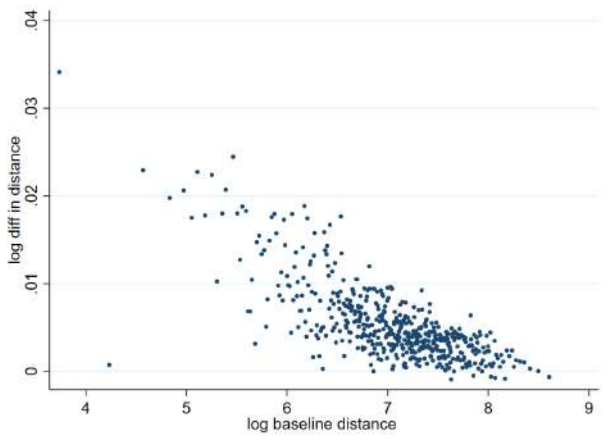
Note: The figure displays the share of incidents cleared, either charged with an informal or formal sanction, or left non-cleared by crime type. Police charges all informal (i.e. out-of-court) sanctions and decides over non-cleared cases. The sample is restricted to incidents with a non-missing investigation outcome and before 2013, i.e. before any closure occurred.

Figure B3: Intensity of the treatment

(a) Levels of distance

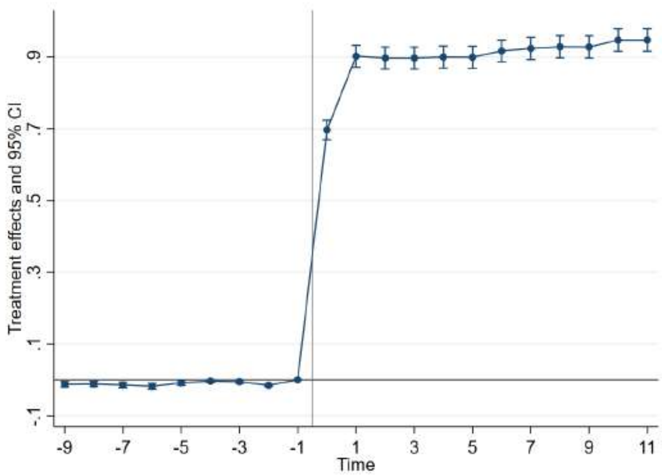


(b) Changes in distance



Note: Panel A displays the scatter plot between distance at baseline in 2008 and the actual distance. Panel B shows the scatter plot between baseline distance and changes in distance between t and $t-1$. Both variables are transformed in logarithms. I group baseline distance into 500 equally-sized bins.

Figure B4: Pre-trends on log distance



Note: The graph reports coefficients and 95% confidence intervals from an event-study regression of log distance estimated according to specification 2. Standard errors are clustered at the census block level. The regression includes census block, relative time and LA-by-quarterly date fixed effects.

Table B1: Local characteristics that predict the timing of the treatment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Sample: census blocks ever treated							
	=1 if treated in 2013q3		=1 if treated in 2013m7		=1 if treated in 2013m9		=1 if treated in other months	
Outcomes								
logcrime	0.032		0.026		0.002		-0.028	
	[0.022]		[0.022]		[0.004]		[0.022]	
logviolcrime		0.028		0.009		0.015		-0.024
		[0.025]		[0.022]		[0.017]		[0.025]
logpropcrime		-0.026		-0.010		-0.015		0.026
		[0.031]		[0.024]		[0.023]		[0.031]
logdrugs		0.021		0.024*		-0.001		-0.023
		[0.015]		[0.013]		[0.006]		[0.014]
# Treated blocks	1661		874		682		685	
Observations	6,333	5,995	6,333	5,995	6,333	5,995	6,333	5,995
FE	LAxYear	LAxYear	LAxYear	LAxYear	LAxYear	LAxYear	LAxYear	LAxYear

Note: Each column presents estimates from the following equation:

$$ClosePeriod_i = X_i'\beta + \phi_{l,t} + \varepsilon_i$$

where the dependent variable is equal to 1 is the closest station to block i shut down in 2013q3 (Column 1-2), 2013m7 (Column 3-4), 2013m9 (Column 5-6) or other periods (Column 7-8). X_i is a vector of local characteristics, specifically (log) crime. The sample is restricted to ever treated blocks, that is, blocks which ever experienced a closure. The explanatory variables are computed on the 2008-2010 sample period, before any closure occurred. *LA* refers to the 31 London LAs, excluding City of London and Westminster. Each column included LA-by-year fixed effects. Crime data for years before 2010 are taken from the MPS Historical Crime Data collection. *** p<0.01, ** p<0.05, * p<0.1.

Table B2: Descriptive statistics for baseline characteristics of census blocks

	Blocks with Police station	Blocks w/o Police station	Blocks with Closure	Blocks w/o Closure	Blocks Treated	Blocks Control
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Census block characteristics						
Crime rates						
Crime rate (2008)	0.18	0.09	0.15	0.23	0.08	0.09
Violent crime rate (2008)	0.05	0.02	0.04	0.06	0.02	0.03
Deprivation						
Economic deprivation index score (2010)	26.89	25.41	25.65	28.44	24.63	26.10
Children deprivation index score (2010)	0.32	0.31	0.30	0.33	0.30	0.32
Housing market						
Number of dwellings (2008)	785.93	690.89	784.33	787.92	698.91	690.03
House prices in 1000£ (2005-2008)	380.97	393.42	371.37	392.90	408.74	380.96
Population						
Population (2008)	1,737.30	1,659.34	1,722.13	1,756.14	1,658.90	1,664.02
Working age population (2008)	1,231.74	1,144.51	1,207.44	1,261.94	1,142.13	1,151.19
Share of asian (2001)	0.11	0.12	0.10	0.13	0.10	0.13
Share of black (2001)	0.11	0.11	0.10	0.11	0.11	0.11
Share of born outside UK (2001)	0.27	0.27	0.26	0.29	0.26	0.27
Share of claimants Income Support (2008)	6.78	6.78	6.50	7.12	6.50	7.00
Share in employment (2011)	0.87	0.87	0.87	0.86	0.87	0.86
N blocks	148	4,553	82	66	2,039	2,662

Note: This table shows summary statistics for the baseline characteristics of the census blocks. Treated blocks are blocks which experienced an increase in distance from the closest police station throughout the sample period (2012-2016).

Table B3: Types of crime provided by the MPS

Type of incident	**%**
Anti-social behaviour	0.288
Drugs and other crime	0.095
Bicycle thefts	0.011
Burglary	0.081
Criminal damage and arson	0.053
Other thefts	0.108
Shoplifting	0.033
Theft from the person	0.02
Vehicle crimes	0.089
Possession of weapons	0.002
Public disorder and weapons	0.009
Public order	0.024
Assault and murders	0.054
Robbery	0.027
Sexual offences	0.108
N	5,843,654

Source: Authors' calculation from Metropolitan Police Service

Table B4: Definition of investigation outcomes

Outcome group	Type of investigation outcome	%	Definition
Non cleared	Evidencial difficulties	0.005	Court case unable to proceed; Unable to prosecute suspect
	Suspect non identified	0.473	Investigation complete, no suspect identified
	Other	0.346	Action to be taken by another body/agency; Offender otherwise dealt with; Under investigation
Cleared	Informal sanction	0.033	Offender given a caution
		0.011	Offender given penalty notice
		0.006	Local resolution
		0.02	Offender given a drugs possession warning
	Court sanction	0.011	Offender fined
		0.023	Offender given suspended prison; Offender sent to prison
		0.007	Offender given absolute discharge; Offender given conditional discharge
		0.014	Offender given community sentence
		0.016	Defendant found not guilty
		0.001	Offender deprived of property; Offender ordered to pay compensation
		0.033	Awaiting court outcome; Court result unavailable; Defendant sent to Crown Court; Suspect charged as part of another case
	No public interest	0.000	Formal action is not in the public interest; Further investigation is not in the public interest
	N	3,231,678	

Source: Authors' calculation from Metropolitan Police Service

B1 Robustness checks for crime results

Alternative measures of crime To account for the prevalence of zeros in the types of crimes, I follow Bellemare and Wichman (2020) and use as transformation of the variables the inverse hyperbolic sine transformation (*asinh*). To further address concerns about blocks with 0 crimes, as a robustness check I also present models using $ln(1+y)$ and $ln(0.01+y)$ as the dependent variable. Table B5 and B6 show the results.

Additional crime types Appendix Table B3 reports the definition of the 16 offence categories used by the MPS. I measure the impacts of the closures on all types of reported crime, including drug-related offences, public order offences, and anti-social behaviour incidents in Table B7.

Definition of distance In the main empirical analysis, I compute the distance as the geodesic distance between the centroid of each census block and the closest police station within each LA, based on the fact that policing is independently managed by each LA. I empirically test whether this assumption holds and I compute as alternative explanatory variable the geodesic distance between the centroid of each census block and any closest police station, considering also stations located in different LA. In contrast to my main specification, this specification thus exploits also the variation in distance coming from blocks whose nearest station is located in another police division: these blocks, likely to lie at the border across LAs, would not be considered treated according to my main specification because they are not affected by any station closure within their division. Panel A and B of Table B8 provide evidence against such concern and show that the results do not change when computing the distance across LAs, rejecting the hypothesis that police readjusts across different divisions.

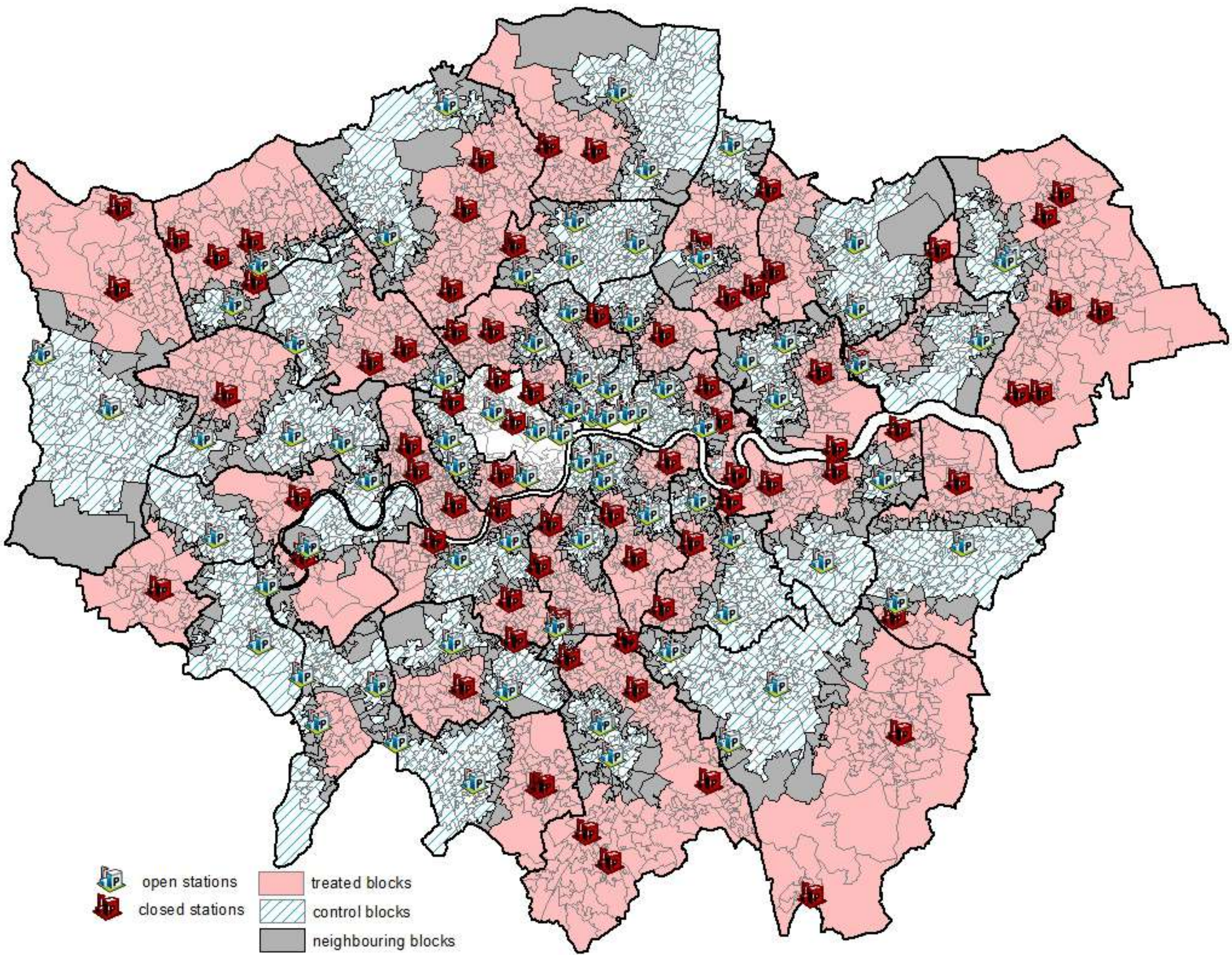
A final issue concerns the opening of new police stations: during the sample period only 7 police stations opened, and 148 census blocks out of 4,701 blocks experienced a decrease in the distance to the stations. As a final check, I attribute the baseline distance to blocks that experienced an opening and I estimate an Intention-to-Treat regression. Panel C of Table B8 show that the estimates on crime are unchanged.

Spatial Correlation Given the high spatial resolution of the data, I apply the method developed by Conley (1999) and estimate standard errors with a spatial heteroskedasticity and autocorrelation consistent correction (HAC), allowing for for both cross-sectional spatial correlation (across census blocks) and location-specific (within blocks across time) serial correlation, which decays as the distance from the block increases. Census blocks locations are specified in latitude-longitude degrees and the kernel cut-off is specified in kilometres. For the spatial kernel, I retain a radius varying from 250 meters to 3 km and I use a conical kernel that decays linearly in all directions to weight spatial correlations. I allow for serial correlation across 6 time periods (months). Appendix Table B9 reports estimates robust to these adjustments.

Potential exposure to station closure A further concern is related to the fact that census blocks might be differentially exposed to the treatment because of their location: for instance, central blocks are "potentially" closer to more police stations than peripheral blocks just because of their geographical position.[74] Census blocks fixed effects control for all time-invariant characteristics of the area, including its geography. However, this might be insufficient if a census block's treatment status, and thus if changes in the distance to the police station, are also determined by the initial potential exposure to the treatment. I address this concern by flexibly controlling for the initial potential exposure to police stations. In my setting, the potential exposure to the treatment is a function of the number of stations located in the surroundings of the area. Therefore, I first count the initial number of stations operating within a certain radius from the centroid of each census block. I then augment the baseline specification by controlling for the baseline potential exposure interacted with a linear time trend. Table B10 shows the results of this analysis. I use alternative cut-offs of potential exposure (namely, 2, 3, 4, 5 km) and interact them with either a linear yearly trend or a linear monthly trend. All results remain virtually unchanged.

[74]This concern is related to a form of non-random exposure to an exogenous shock (Borusyak and Hull, 2020), that gives rise to a peculiar type of omitted variable bias.

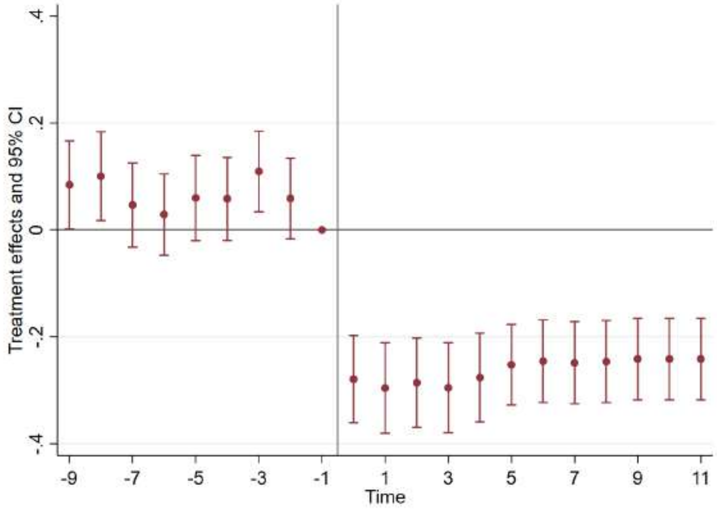
Figure B5: Map of police station closings, treated and neighbouring blocks



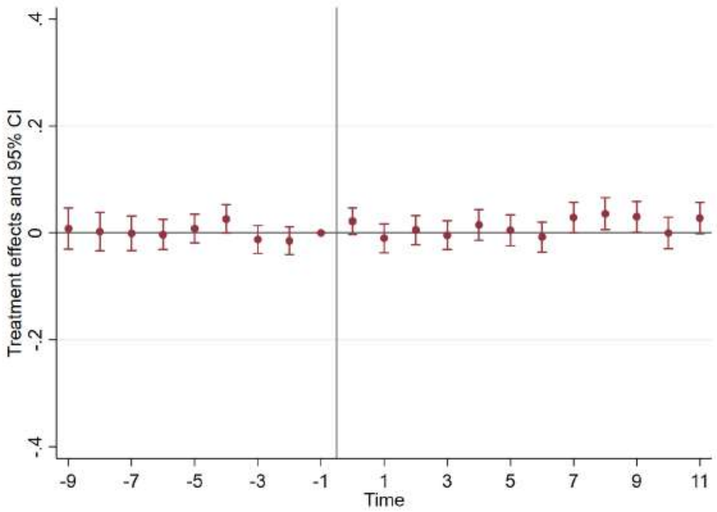
Notes: The map gives the locations of police stations, including both open and closed stations, as of the end of 2016 (end of the sample period). In addition, the map codes different types of census blocks: blocks where the nearest station was closed ("treated" blocks), blocks whose nearest station remained open ("control" blocks), control blocks bordering with treated blocks ("neighbouring" blocks). Black borders correspond to boundaries to the 31 boroughs of London, excluding City of London and Westminster.

Figure B6: Event study for spillover effects among control blocks

(a) Panel A: Violent crimes



(b) Panel B: All crimes



Note: The graphs report the coefficients and 95% confidence intervals estimated according to Equation 2. Standard errors are clustered at the census block level. The dependent variable is the total number of violent crimes, defined as assaults and murders, recorded in a census block expressed in logs. I omit the dummy for the period before the closures. All regressions include census block, LA-by-quarterly date fixed effects, and relative time fixed effects.

Table B5: Alternative transformations of crime type variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Violent crimes			Property crimes						Other types of crime		
Dependent variable	Assaults and murders	Robbery	Sexual offences	Property crime	Burglary	Criminal damage and arson	Shop lifting	Theft	Vehicle crime	Drugs	Public order	Anti-social behaviour
Panel A. Dependent variable: log(Y+1)												
I. Binary treatment												
dummy distance	0.091***	0.013***	-0.107***	-0.017***	0.002	-0.026***	-0.008	-0.086***	0.002	-0.018***	-0.029***	-0.006
	[0.012]	[0.004]	[0.013]	[0.006]	[0.005]	[0.004]	[0.006]	[0.010]	[0.005]	[0.004]	[0.005]	[0.008]
II. Continuous treatment												
distance	0.036***	0.001	-0.051***	-0.004	0.008*	-0.017***	0.005	-0.028***	0.000	-0.007**	-0.012***	-0.008
	[0.010]	[0.003]	[0.011]	[0.005]	[0.004]	[0.003]	[0.006]	[0.008]	[0.004]	[0.003]	[0.004]	[0.006]
Panel B. Dependent variable: log(Y+0.01)												
I. Binary treatment												
dummy distance	0.247***	0.039**	-0.242***	-0.022*	-0.002	-0.106***	-0.036	-0.308***	0.003	-0.083***	-0.110***	-0.045**
	[0.035]	[0.018]	[0.033]	[0.012]	[0.020]	[0.019]	[0.022]	[0.035]	[0.020]	[0.018]	[0.020]	[0.018]
II. Continuous treatment												
distance	0.106***	0.000	-0.121***	-0.006	0.025	-0.070***	0.005	-0.080***	-0.006	-0.025*	-0.055***	-0.026*
	[0.028]	[0.014]	[0.027]	[0.009]	[0.016]	[0.015]	[0.019]	[0.030]	[0.015]	[0.015]	[0.016]	[0.015]
Observations	338,472	338,472	338,472	338,472	338,472	338,472	338,472	338,472	338,472	338,472	338,472	338,472
Calendar month FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FE	LAxDate	LAxDate	LAxDate	LAxDate	LAxDate	LAxDate	LAxDate	LAxDate	LAxDate	LAxDate	LAxDate	LAxDate

Note: Panel A transforms the outcome variable in $log(Y+1)$, while Panel B in $log(Y+0.01)$. All explanatory variables are defined as in Table 4. *LA* refers to the 31 London LAs (excluding Westminster and City of London). Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Table B6: Alternative transformations of crime

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable		asinh(Crime)			log(Crime + 1)			log(Crime + 0.01)	
Panel A: Binary treatment									
dummy distance	-0.014**	-0.012**	-0.026**	-0.012**	-0.010*	-0.024**	-0.014**	-0.012*	-0.028**
	[0.006]	[0.006]	[0.012]	[0.005]	[0.005]	[0.011]	[0.006]	[0.007]	[0.014]
Panel B: Continuous treatment									
asinh(distance)	-0.017***	-0.015**	-0.038**						
	[0.006]	[0.006]	[0.015]						
log distance				-0.010***	-0.010**	-0.019**	-0.013***	-0.013**	-0.024**
				[0.004]	[0.004]	[0.008]	[0.005]	[0.005]	[0.010]
Observations	338,472	338,472	338,472	338,472	338,472	338,472	338,472	338,472	338,472
Calendar month FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
FE		LAxYear	LAxDate		LAxYear	LAxDate		LAxYear	LAxDate

Note: Column 1-3 use the inverse hyperbolic sine transformation (*asinh*) for crime (Bellemare and Wichman, 2020), Column 4-6 use $log(crime+1)$ and Column 7-9 use $log(crime+0.01)$. *LA* refers to the 31 London LAs (excluding Westminster and City of London). Wards are the primary electoral divisions used in England. Each ward contains a median number of 7 census blocks (LSOAs). Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Table B7: Effects of closings on other types of crime

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Drugs		Public order		Anti-social behaviour	
Panel A: binary treatment						
dummy distance	-0.031***	-0.024***	-0.034***	-0.037***	-0.007	-0.011
	[0.005]	[0.005]	[0.006]	[0.006]	[0.009]	[0.009]
Panel B: continuous treatment						
distance	-0.023***	-0.012**	-0.025***	-0.027***	-0.015*	-0.013
	[0.005]	[0.005]	[0.006]	[0.006]	[0.009]	[0.010]
Observations	338,472	338,472	338,472	338,472	338,472	338,472
Calendar month FE	✓	✓	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓	✓	✓
FE		LAxDate		LAxDate		LAxDate

Note: In Panel A, the explanatory variable is the dummy treatment as defined in Section 4; in Panel B the explanatory variable is the continuous geodesic distance between the centroid of the census block and the closest police station, measured in km and transformed using the *asinh* transformation. The dependent variables are transformed using the *asinh*. *LA* refers to the 31 London LAs (excluding Westminster and City of London). Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Table B8: Alternative definitions of distance

	(1)	(2)	(3)	(1)	(2)	(3)
Dependent variable	ihs (Violent crimes)			log (Crime)		
Panel A						
dummy distance across LAs	0.085***	0.101***	0.105***	-0.009*	-0.006	-0.007
	[0.015]	[0.014]	[0.015]	[0.005]	[0.006]	[0.006]
Panel B						
cotinuous distance across LAs	0.105***	0.100***	0.099***	-0.012***	-0.010**	-0.011**
	[0.018]	[0.017]	[0.018]	[0.004]	[0.004]	[0.005]
Panel C						
ITT distance	0.093***	0.089***	0.088***	-0.011***	-0.010**	-0.011**
	[0.016]	[0.016]	[0.017]	[0.004]	[0.004]	[0.004]
Observations	338,472	338,472	338,472	337,122	337,122	337,122
Calendar month FE	✓	✓	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓	✓	✓
FE		LAxYear	LAxDate		LAxYear	LAxDate

Note: LA refers to the 31 London Local Authorities, excluding City of London and Westminster. Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Table B9: Conley standard errors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: *Dependent variable = ihs (Violent crimes)*										
dummy distance	0.115***	0.115***	0.115***	0.115***	0.115***	0.115***	0.115***	0.115***	0.115***	0.115***
	[0.006]	[0.006]	[0.006]	[0.006]	[0.006]	[0.006]	[0.007]	[0.007]	[0.007]	[0.007]
distance	0.087***	0.087***	0.087***	0.087***	0.087***	0.087***	0.087***	0.087***	0.087***	0.087***
	[0.007]	[0.007]	[0.007]	[0.007]	[0.007]	[0.007]	[0.007]	[0.007]	[0.007]	[0.007]
Observations	338,472	338,472	338,472	338,472	338,472	338,472	338,472	338,472	338,472	338,472
Panel B: *Dependent variable = log Crime*										
dummy distance	-0.013***	-0.013***	-0.013***	-0.013***	-0.013***	-0.013***	-0.013***	-0.013***	-0.013***	-0.013***
	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]
distance	-0.012***	-0.012***	-0.012***	-0.012***	-0.012***	-0.012***	-0.012***	-0.012***	-0.012***	-0.012***
	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]
Observations	337,122	337,122	337,122	337,122	337,122	337,122	337,122	337,122	337,122	337,122
Calendar month FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
LA*Date FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cut-off (km)	0.25	0.3	0.4	0.5	0.75	1	1.5	2	2.5	3

Note: LA refers to the 31 London Local Authorities, excluding City of London and Westminster. Standard errors are clustered using Conley standard errors varying the cut-off for the variance covariance matrix from 250 meters to 3 kilometres. *** p<0.01, ** p<0.05, * p<0.1.

Table B10: Potential initial exposure to police stations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: *Dependent variable = ihs (Violent crimes)*								
dummy distance	0.112***	0.112***	0.107***	0.107***	0.105***	0.105***	0.108***	0.108***
	[0.015]	[0.015]	[0.015]	[0.015]	[0.015]	[0.015]	[0.015]	[0.015]
distance	0.087***	0.087***	0.080***	0.080***	0.077***	0.077***	0.079***	0.079***
	[0.016]	[0.016]	[0.016]	[0.016]	[0.016]	[0.016]	[0.016]	[0.016]
Observations	338,472	338,472	338,472	338,472	338,472	338,472	338,472	338,472
Panel B: *Dependent variable = log Crime*								
dummy distance	-0.012**	-0.012**	-0.011*	-0.011*	-0.011*	-0.011*	-0.010*	-0.010*
	[0.006]	[0.006]	[0.006]	[0.006]	[0.006]	[0.006]	[0.006]	[0.006]
distance	-0.012***	-0.012***	-0.012***	-0.011***	-0.011**	-0.011**	-0.010**	-0.010**
	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]
Observations	337,122	337,122	337,122	337,122	337,122	337,122	337,122	337,122
Calendar month FE	✓	✓	✓	✓	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓
LA*Date FE	✓	✓	✓	✓	✓	✓	✓	✓
# stations * Linear time trend	Yearly	Monthly	Yearly	Monthly	Yearly	Monthly	Yearly	Monthly
Cut-off (km)	2		3		4		5	

Note: LA refers to the 31 London Local Authorities, excluding City of London and Westminster. The initial number of stations is computed as the number of police stations operating in 2008 and located within respectively 2, 3, 4, 5 km from the centroids of the census blocks. Columns 1, 3, 5, 7 interact it with a linear annual time trend; columns 2, 4, 6, 8 interact it with a linear monthly time trend. *** p<0.01, ** p<0.05, * p<0.1.

Table B11: Effect on reported crime excluding bordering blocks from control group

	(1)	(2)	(3)
Dependent variable		log Crime	
Panel A: binary treatment			
dummy distance	-0.020***	-0.016***	-0.017***
	[0.006]	[0.006]	[0.006]
Panel B: continuous treatment			
distance	-0.014***	-0.012***	-0.013***
	[0.004]	[0.004]	[0.004]
Observations	277,372	277,372	277,372
Calendar month FE	✓	✓	✓
LSOA FE	✓	✓	✓
FE		LAxYear	LAxDate

Note: Sample includes treated blocks and "landlocked" blocks, excluding 833 bordering touching blocks. The dependent and independent variables are transformed is logs. *LA* refers to the 31 London LAs, excluding City of London and Westminster. Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Table B12: Effect on reported crime types excluding bordering blocks from control group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Violent crimes				Property crimes			
Dependent variable	Assaults and murders	Robbery	Sexual offences	Property crimes (all)	Burglary	Criminal damage and arson	Shoplifting	Theft	Vehicle crime
Panel A: binary treatment									
dummy distance	0.113***	0.014***	-0.132***	-0.028***	0.000	-0.035***	-0.010	-0.109***	-0.008
	[0.017]	[0.005]	[0.018]	[0.008]	[0.007]	[0.006]	[0.009]	[0.014]	[0.007]
Panel B: continuous treatment									
distance	0.078***	0.005	-0.101***	-0.017*	0.015**	-0.034***	0.005	-0.055***	-0.010
	[0.017]	[0.005]	[0.019]	[0.009]	[0.007]	[0.006]	[0.009]	[0.014]	[0.007]
Observations	278,496	278,496	278,496	278,496	278,496	278,496	278,496	278,496	278,496
Calendar month	✓	✓	✓	✓	✓	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
LA*Date FE	✓	✓	✓	✓	✓	✓	✓	✓	✓

Note: Sample includes treated blocks and "landlocked" blocks, excluding 833 bordering touching blocks. The dependent and independent variables are transformed using the *asinh*. *LA* refers to the 31 London Local Authorities, excluding City of London and Westminster. Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

B2 Robustness checks for clearance results

Table B13: Sample selection of investigative outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	All incidents					
Dependent variable	Pr(non-missing investigation outcome)					
Panel A: binary treatment						
dummy distance	-0.001	0.001	0.000	-0.001	0.001*	0.000
	[0.001]	[0.001]	[0.001]	[0.001]	[0.000]	[0.001]
Panel B: continuous treatment						
log distance	-0.001	-0.000	-0.001**	-0.001	-0.000	-0.001**
	[0.001]	[0.000]	[0.001]	[0.001]	[0.000]	[0.001]
Baseline outcome	0.825	0.825	0.825	0.825	0.825	0.825
Observations	3,356,506	3,356,506	3,356,506	3,356,506	3,356,506	3,356,506
Calendar date FE	✓	✓	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓	✓	✓
Crime type FE				✓	✓	✓
LA*Date FE		✓			✓	
Ward*Date FE			✓			✓

Note: Each column displays results from a separate OLS regression on the probability of displaying a non-missing investigation outcome on the universe of all incidents reported to the police. For a detailed definition of the outcome categories see Appendix Table B4. Panel A uses as explanatory variable the dummy treatment as defined in subsection 4, while Panel B uses the geodesic distance between the centroid of the census block and the closest police station, measured in km and transformed in logs.*LA* refers to the 31 London LAs (excluding Westminster and City of London). Wards are the primary electoral divisions used in England. Each ward contains a median number of 7 census blocks (LSOAs). Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Table B14: Effect on crime on sub-sample of non-missing outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample	Incidents with non-missing investigation outcome							
Dependent variable	All crimes		Assaults and murders		Robbery		Sexual offences	
Panel A: binary treatment								
dummy distance	-0.020***	-0.010*	-0.093***	-0.119***	-0.008*	0.016***	0.065***	0.089***
	[0.005]	[0.006]	[0.015]	[0.016]	[0.005]	[0.005]	[0.013]	[0.013]
Panel B: continuous treatment								
distance	-0.014***	-0.013**	-0.097***	-0.102***	0.000	0.011**	0.073***	0.069***
	[0.005]	[0.006]	[0.016]	[0.016]	[0.005]	[0.005]	[0.014]	[0.014]
Observations	277,113	277,113	277,113	277,113	277,113	277,113	277,113	277,113
Calendar month FE	✓	✓	✓	✓	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓
FE		LAxDate		LAxDate		LAxDate		LAxDate

Note: In Panel A, the explanatory variable is the dummy treatment as defined in Section 4; in Panel B the explanatory variable is the continuous geodesic distance between the centroid of the census block and the closest police station, measured in km and transformed using the *asinh* transformation. The dependent variables computed the number of crimes recorded in a census block transformed using the *asinh*. *LA* refers to the 31 London LAs (excluding Westminster and City of London). Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Table B15: Effect on property crime on sub-sample of non-missing outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Sample	Incidents with non-missing investigation outcome											
Dependent variable	Property crimes (all)		Burglary		Criminal damage and arson		Shoplifting		Theft		Vehicle crime	
Panel A: binary treatment												
dummy distance	-0.013*	-0.005	-0.010	-0.005	-0.016***	-0.018***	0.002	0.001	-0.102***	-0.102***	-0.011	-0.002
	[0.007]	[0.007]	[0.007]	[0.007]	[0.006]	[0.006]	[0.006]	[0.007]	[0.012]	[0.012]	[0.007]	[0.007]
Panel B: continuous treatment												
distance	-0.009	-0.007	-0.000	0.010	-0.021***	-0.025***	0.003	0.002	-0.090***	-0.059***	-0.008	-0.005
	[0.007]	[0.007]	[0.007]	[0.007]	[0.006]	[0.006]	[0.006]	[0.007]	[0.011]	[0.012]	[0.007]	[0.007]
Observations	277,113	277,113	277,113	277,113	277,113	277,113	277,113	277,113	277,113	277,113	277,113	277,113
Calendar month FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
LAxDate FE		✓		✓		✓		✓		✓		✓

Note: In Panel A, the explanatory variable is the dummy treatment as defined in Section 4; in Panel B the explanatory variable is the continuous geodesic distance between the centroid of the census block and the closest police station, measured in km and transformed using the *asinh* transformation. The dependent variables are transformed using the *asinh*. *LA* refers to the 31 London LAs (excluding Westminster and City of London). Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Table B16: Ratio of cleared and convicted crimes

	(1)	(2)	(3)	(4)	(5)	(6)
Type of crime	All crimes		Property crimes		Violent crimes	
Panel A: Dependent variable: cleared crimes / reported crimes						
I. Binary treatment						
dummy distance	-0.005***	-0.004***	-0.005***	-0.005***	-0.003	-0.002
	[0.001]	[0.001]	[0.001]	[0.001]	[0.002]	[0.002]
II. Continuous treatment						
distance	-0.004***	-0.002***	-0.003***	-0.002**	-0.005***	-0.002
	[0.001]	[0.001]	[0.001]	[0.001]	[0.002]	[0.002]
Panel B: Dependent variable: convictions / reported crimes						
I. Binary treatment						
dummy distance	-0.002***	-0.002***	-0.003***	-0.003***	0.000	0.000
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
II. Continuous treatment						
distance	-0.001**	-0.001	-0.001**	-0.001*	-0.001	-0.001
	[0.000]	[0.000]	[0.001]	[0.001]	[0.001]	[0.001]
Observations	337,122	337,122	324,306	324,306	269,500	269,500
Calendar date FE	✓	✓	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓	✓	✓
LA*Date FE		✓		✓		✓

Note: Each column displays results from a separate OLS regression estimated as Equation 1. Panel A uses as outcome variable the ratio between the total number of cleared crimes over the total number of reported crime. Panel B uses as outcome variable the ratio between the total number of convicted crimes over the total number of reported crime, where convictions refer to incidents declared guilty of a criminal offence by the verdict of a court (thus excluding acquittals and discharges). I use both binary and continuous definitions of the explanatory variable as defined in Section 4. *LA* refers to the 31 London boroughs. All regressions include census block and monthly date fixed effects. Standard errors are clustered at the census block level. See Appendix Table B3 for the definitions of violent and property crimes. *** p<0.01, ** p<0.05, * p<0.1.

Table B17: Effects on crime by baseline clearance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All crimes							
Dependent variable	Above median clearance rate		Below median clearance rate		Top 75 percentile clearance rate		Bottom 25 percentile clearance rate	
Panel A: binary treatment								
dummy distance	-0.011	-0.012	-0.026***	-0.013*	0.027***	0.025***	-0.025***	-0.016*
	[0.007]	[0.008]	[0.007]	[0.007]	[0.009]	[0.009]	[0.009]	[0.009]
Panel B: continuous treatment								
distance	-0.015**	-0.017**	-0.016**	-0.010	0.026***	0.024**	-0.021***	-0.013
	[0.007]	[0.008]	[0.007]	[0.007]	[0.010]	[0.009]	[0.008]	[0.008]
Observations	338,472	338,472	338,472	338,472	338,472	338,472	338,472	338,472
Calendar date FE	✓	✓	✓	✓	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓
LA*Date FE		✓		✓		✓		✓

Note: The outcome variables are defined as the number of crimes: above the median baseline clearance rate within LA (col. 1-2); below the median baseline clearance rate within LA (col. 3-4); above the 75th percentile baseline clearance rate within LA (col. 5-6); below the 25th percentile baseline clearance rate within LA (col. 7-8). Given the prevalence of zeros, the dependent variables and distance are transformed using the *asinh*. *LA* refers to the 31 London LAs (excluding Westminster and City of London). Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Table B18: Selection on baseline clearance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	Predicted Clearance							
dummy distance	-0.002*	-0.000	-0.002*	-0.000	-0.000	-0.000	-0.002*	-0.000
	[0.001]	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]
Observations	3,231,678	3,231,678	3,231,678	3,231,678	3,231,678	3,231,678	3,231,678	3,231,678
LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓
LA*Date FE	✓	✓	✓	✓	✓	✓	✓	✓
Crime type FE		✓		✓		✓		✓

Note: The outcome variable is the predicted probability of clearance, computed taking the predicted values from a regression of an indicator variable equal to 1 if the incident was cleared on: LAxDate, crime type fixed effects (col. 1-2); LAxDate, crime type fixed effects, baseline distance (col. 3-4); LSOA, LAxDate fixed effects (col. 5-6); LAxDate, LSOA, crime type fixed effects (col. 7-8). *LA* refers to the 31 London LAs (excluding Westminster and City of London). Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

B3 Robustness checks for results on house prices

Table B19: Effect on house prices by destination of the closed police station

	(1)	(2)	(3)	(4)
Outcome variable	log price lsoa (weighted by number of transactions)			
dummy distance	-0.018**	-0.009	-0.018**	-0.010
	[0.009]	[0.009]	[0.009]	[0.009]
dummy distance * 1[Sale]	0.009	0.001	-0.025*	-0.027*
	[0.010]	[0.010]	[0.014]	[0.014]
dummy distance * 1[Residential Destination]			0.048***	0.040***
			[0.013]	[0.015]
Observations	62,893	62,883	62,893	62,883
Date FE	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓
Date x LA FE		✓		✓

Note: Each column displays results from a separate OLS regression, where the dependent variable is the average (log) house prices computed in the census block. The dataset is collapsed at the quarterly level. The explanatory variables are the dummy treatment as defined in Section 4, and then the interaction of the treatment dummy with a dummy equal to 1 if the closest police station was sold, and if the closest police station was sold and then transformed into a new residential building. *LA* refers to the 31 London Local Authorities, excluding City of London and Westminster. The observations are weighted by the number of sales in the census block during the quarter. Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Appendix C Cost-benefit analysis

Table C1: Cost of crime

Crime category	(1) Unit cost of crime	(2) Number of offenses	(3) Average cost of crime
Violent crimes			10,793
- *Violence against the person*			10,761
- *Homicide*	3,217,740	570	
- *Violence with Injury*	14,050	1,104,930	
- *Violence without Injury*	5,930	852,900	
- *Rape*	39,360	121,750	
- *Other sexual offences*	6,520	1,137,320	
- *Robbery*	11,320	193,470	351
Property crimes			2,655
- *Domestic burglary*	5,930	695,000	5,930
- *Theft*			1,664
- *Theft of Vehicle*	10,290	68,000	
- *Theft from Vehicle*	870	574,110	
- *Theft from Person*	1,380	459,240	
- *Criminal damage*			1,505
- *Arson*	8,420	22,620	
- *Other criminal damage*	1,350	1,007,160	
All		6,237,070	7,106

Note: This table displays the calculations of the average cost of crime in England and Wales. The unit cost of crime for each of the crime categories in Column (1) is obtained from Table 1 of the Home Office report (Heeks et al., 2018) and the number of offences in Column (2) is obtained from Table 4 of the same report. Column (3) is the weighted average of the cost of crime, computed using the number of offences by crime category as weights. The average cost of crime for each crime group is computed instead using as weights the frequencies of each sub-component. The reports uses 2015/2016 prices.

Table C2: Deterrence costs from increasing clearance rate by β pp

Crime category	(1) Probability of crime	(2) Number offences	(3) Crimes non deterred	(4) Unit cost per crime (£)	(5) Cost of crime deterred (£)
Violent crimes	0.16	228,542	722	10,793	7,789,283
- *Violence against the person*	0.13	183,170	578	10,761	6,224,555
- *Robbery*	0.03	45,372	143	351	50,311
Property crimes	0.43	618,333	1953	2,655	5,184,958
- *Domestic burglary*	0.09	126,870	401	5,930	2,375,809
- *Theft*	0.00	4,926	16	1,664	25,889
- *Criminal damage and arson*	0.06	81,802	258	1,505	388,852
Total	1	1,441,951	4,554		12,974,241
Calculations			.1 × 0.006/0.19 × 1,441,951 × (1)		(3) × (4)

Note: This table shows the computation of the cost of crime for the additional offences generated when the clearance rate decreases by $\beta * 100$ percentage points (where $\beta = 0.006$ from Table 10). Column 1 and 2 display the proportion of crimes and the number of offences by crime category recorded by the MPS. Both columns are computed from my dataset restricting the sample to the pre-period (i.e. before June 2013). Column 4 reports the unit cost of crime computed in Table C1. Column 3 and 5 are calculated from previous columns, as indicated in the bottom row. We assume an elasticity of crime on the clearance rate of -.1 as in Blanes i Vidal and Kirchmaier (2018). To compute the additional number of crimes deterred, I multiply .1 (the assumed deterrence elasticity) by .006 (the β increase in the clearance rate) divided by .19 (the average clearance rate) and by the total number of incidents in the pre-period.

Table C3: Costs of incarceration from increasing crime by β pp

Crime category	(1) Number offences	(2) Extra crimes committed	(3) Probability of conviction	(4) Extra crimes convicted	(5) Probability of incarceration	(6) Extra crimes incarcerated	(7) Sentence lenght	(8) Extra years of incarceration	(9) Total costs
Violent crimes									
- *Violence against the person*	183,170	21,065	0.097	2,052	0.293	601	37	22,251	773,579,541
- *Robbery*	45,372	726	0.083	60	0.426	26	36	919	31,939,654
Property crimes									
- *Domestic burglary*	126,870	381	0.070	27	0.314	8	19	159	5,522,138
- *Theft*	4,926	-542	0.017	-9	0.153	-1	4	-6	-196,644
- *Criminal damage and arson*	81,802	-2,781	0.059	-164	0.319	-52	22	-1,151	-40,025,577
Total	442,140	18,848	0.027	1,965	0.027	582	15	23,170	770,819,112
Calculations		(1) × β		(2) × (3)		(4) × (5)		(6) × (7)	(8) × £34,766

Note: This table shows the computation of the incarceration costs resulting from changes in crime according to estimates of stations' closures on crime types from Tables 6 and 7. Column 1 is equivalent to Column 1 of Table C2. Column 2 computes the additional crimes using estimates from Tables 6 and 7. Column 3 and 5 display the probability of conviction (unconditional) and incarceration (conditional on conviction) recorded by the MPS. Both columns are computed from my dataset restricting the sample to the pre-period (i.e. before June 2013). Column 7 displays the England and Wales average custodial sentence length from Criminal Justice Statistics Quarterly Update (Ministry of Justice, 2013). Column 2, 4, 6, 8 are calculated from previous columns, as indicated in the bottom row. The cost per incarceration year is £34,766 and is computed by the Ministry of Justice (*Costs per place and costs per prisoner*, Ministry of justice, 2014).

Table C4: Criminal justice savings from increasing clearance rate by β pp

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Crime category	Probability of crime	Number offences	Crimes non deterred	Multiplier	Prison & probation costs	CJS costs	Total prison and probation costs saved	Total CJS costs saved
Violent crimes	0.16	228,542	722	7.08	100	949	511,168	4,852,614
- *Violence against the person*	0.13	183,170	578	7.25	314	861	1,318,253	3,610,143
- *Robbery*	0.03	45,372	143	4.30	1,260	2,420	776,291	1,490,972
Property crimes	0.43	618,333	1,953	3.11	213	863	1,294,517	5,246,596
- *Domestic burglary*	0.09	126,870	401	3.60	390	880	562,502	1,269,235
- *Theft*	0.00	4,926	16	3.86	487	3,238	29,289	194,693
- *Criminal damage and arson*	0.06	81,802	258	1.98	97	341	49,349	174,445
Total	1.00	1,441,951	4,554				1,805,686	10,099,210
Calculations			.1 × 0.006/0.19 × 1,441,951 × (1)				(3) × (4) × (5)	(3) × (4) × (6)

Note: This table shows the computation of the criminal justice savings resulting from decreasing the clearance rate by $\beta * 100$ percentage points (where $\beta = 0.006$ from Table 10). Column 1-3 are equivalent to Column 1-3 of Table C2. To obtain the cost per charged crime, I need to multiply the cost per crime by the inverse of the likelihood that a crime will be reported, cleared and charged. The multiplier of each crime categories (Column 4) is obtained from Table 4 of the Home Office report (Heeks et al., 2018). Column 5 and 6 are derived from Table 23 of Heeks et al. (2018). Prison and probation costs include costs related to: probation service, prison service and the National Offender Management Service headquarters. Criminal justice system costs include: costs in terms of prosecution, magistrates' court, crown court, jury service, legal aid, non legal-aid defence, youth justice board. Column 3, 7 and 8 are calculated from previous columns, as indicated in the bottom row.

Table C5: Police savings from increasing clearance rate by β pp

Crime category	(1) Probability of crime (proportion)	(2) Number offences	(3) Crimes non deterred	(4) Multiplier	(5) Police costs	(6) Total police costs saved
Violent crimes	0.16	228,542	722	7.08	2,976	15,213,482
- *Violence against the person*	0.13	183,170	578	7.25	3,094	12,977,110
- *Robbery*	0.03	45,372	143	4.30	1,010	622,265
Property crimes	0.43	618,333	1,953	3.11	993	6,032,824
- *Domestic burglary*	0.09	126,870	401	3.60	530	764,425
- *Theft*	0.00	4,926	16	3.86	5,244	315,248
- *Criminal damage and arson*	0.06	81,802	258	1.98	170	87,084
Total	1.00	1,441,951	4,554			21,246,306
Calculations			.1 × 0.006/0.19 × 1,441,951 × (1)			(3) × (4) × (5)

Note: This table shows the computation of the police savings resulting from decreasing the clearance rate by $\beta * 100$ percentage points (where $\beta = 0.006$ from Table 10). Column 1-3 are equivalent to Column 1-3 of Table C2. Column 5 is derived from Table 23 of Heeks et al. (2018). Police costs are estimates of the opportunity-cost of police time and resources taken up by investigating a certain crime rather than engaging in other activities, such as responding to non-crime activities (Dubourg et al., 2005). Because Column 5-6 display the cost per crime rather than the cost per charged crime, I need to multiply it by the inverse of the likelihood that a crime will be reported, cleared and charged. The multiplier of each crime categories (Column 4) is obtained from Table 4 of the Home Office report (Heeks et al., 2018). Column 3 and 6 are calculated from previous columns, as indicated in the bottom row.

Table C6: Summary of Savings and Costs of police station closures

	Value (£)
Savings	
Panel A: Savings from lower clearance	
- Savings of CJS	10,099,210
- Savings of Prison & probation	1,805,686
- Savings of Police	21,246,306
Total savings	33,151,202
Costs	
Panel B: Cost from non deterred crimes	
- Cost of deterrence	12,974,241
Panel C: Cost from higher incarceration	
- Cost of incarceration	770,819,112
Total costs	783,793,353

Note: This table reports the estimates from the cost-benefit analysis in Section 8.1. For a full derivation of these costs, see Table C1 to C3.

Table C7: Cost-benefit analysis using capitalisation approach

	(1)	(2)
Sample	All sample	High-crime areas
Program component	Value (£)	
Panel A: Police station closure cost		
Average house prices in treated blocks	450,369	445,316
Estimated decrease in price per treated block-quarter	5,404	6,680
Total cost for private owners	220,392,378	127,850,166
Panel B: Police station closure benefits		
Total savings	600 million	
Total savings per treated block	320,856	
Savings per treated block per treated block-quarter	16,043	
Cost/Benefit	0.34	0.42

Note: This table shows the cost-benefit comparison using house price estimates. In Panel A, I use house prices estimate outlined in sub-section 7.1. I compute the average house prices for the pre-period, i.e. before June 2013. In Panel B, I compute the total number of treated blocks between 2012 and 2016, the period where the MPS had to make the savings (MOPAC, 2013). I count 1870 treated blocks and 957 high-crime treated blocks between 2012 and 2016. Column 1 keeps all the sample, column 2 restricts the sample to blocks with higher than median crime rate, in terms of baseline (2008) crime rate.

Table C8: Calculation of marginal value of public funds

	Value (£)
Willingness to pay	
Society's willingness to pay for additional crimes	-233,166,403
Willingness to pay for worse labor market prospects by additional incarcerated individuals	-19,027,866
Aggregate WP	-252,194,269
Net cost to the government	
Mechanical savings from the closures	600,000,000
Fiscal externalities:	
- Fiscal cost of incarceration	-770,819,112
- Lower revenues from lower stamp duty land tax tax	-6,611,771
- Fiscal savings of Police	21,246,306
- Fiscal savings of Prison and Probation	1,805,686
- Fiscal savings of CJS	10,099,210
Net cost	-144,279,681
Aggregate WP / Net Cost	1.75

Note: This table shows the calculations of the marginal value of public funds (MVFP) in Section 8. For a full derivation of these costs, please refer to Table C1 to C7.