IFS

Institute for Fiscal Studies

Elisa Facchetti

Working paper

25/11

Police infrastructure, police performance, and crime: evidence from austerity cuts

Economic and Social Research Council

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

Elisa Facchetti[†]

This Version: March 1, 2025

First Version: February 2021

Abstract

Leveraging unprecedented austerity cuts, this paper studies how the closure of over 70% of local police stations – while keeping officer numbers constant – affects crime, police effectiveness, civilian cooperation, and residents' welfare. Combining geo-referenced crime records and victimization survey data, I find that reduced proximity to stations persistently increases violent crime and reduces police effectiveness. A spatial model of crime and policing highlights heterogeneous shifts in criminal activity. Concurrently, I document reduced reporting of non-violent offenses, diminished trust in policing, and lower local house prices in the most deprived areas. A counterfactual exercise indicates alternative closure policies could have mitigated these adverse consequences.

JEL Classification: D29, K42, R53, H72.

Keywords: Austerity; Crime; Police production function; Reporting.

[*]I thank Jan David Bakker, Sebastian Blesse, Anna Bindler, André Diegmann, Gianmarco Daniele, Magadalena Domínguez, Francesco Fasani, Matteo Gamalerio, François Gerard, Randi Hjalmarsson, Tom Kirchmaier, Jens Ludwig, Rocco Macchiavello, Stephen Machin, Marco Manacorda, Giovanna Marcolongo, Giovanni Mastrobuoni, Brendon McConnell, Stelios Michalopoulos, Michael Mueller-Smith, Lorenzo Neri, Aurelie Ouss, Barbara Petrongolo, Paolo Pinotti, Imran Rasul, Maddalena Ronchi, Andrea Tesei, and participants at several seminars and conferences. The views expressed here are those of the author alone.

[†]Tor Vergata University of Rome, Institute for Fiscal Studies, IZA. Email: elisa.facchetti@uniroma2.it

1 Introduction

Public safety provision is a central responsibility of national governments. Therefore, establishing the optimal allocation of public funds for crime prevention is a major policy challenge. In recent years, while public opinion in the United States was urging a reduction in police budgets and a restructuring of police departments, austerity measures across Europe have led to significant cuts in police funding.[1] The literature on crime prevention strategies underscores the critical importance of how police resources are allocated (Cook and Ludwig, 2010; Owens, 2020). Yet, there is little empirical evidence on the trade-offs faced by law enforcement when allocating limited resources to promote public safety and social welfare, while keeping public budgets under control.

To address this gap, in this paper I leverage a natural experiment generated by unprecedented austerity cuts. To comply with saving needs, the London police undertook a massive wave of police station closures, shutting down more than 70% of stations and redistributing officers to the remaining facilities. As an immediate *direct* effect, the average distance to the nearest police station doubled, and aggregate response time slowed, reducing police deterrence. Yet, the consolidation of resources into fewer stations had the potential to generate indirect local gains, with police presence at the remaining stations increasing by 80%. Beyond operational changes, the closures had the potential to influence public perceptions too, through changes in victims' willingness to report and attitudes towards the police. In this paper, I shed light on the ex-ante ambiguous net effects of such closures on crime prevention, police performance, victim cooperation with law enforcement, and the broader welfare consequences for local communities.

I combine four extremely granular datasets on offenses, victims, and police stations. First, I geo-code all London police stations and collect their dates of closure. Second, I complement them with geo-referenced information on seven million incidents recorded by the London Metropolitan Police Service (henceforth, MPS), with their occurrence

[1] See the *New York Times* article "What Would Efforts to Defund or Disband Police Departments Really Mean?", 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).

dates, crime type, and criminal investigation outcome. Each closure is linked to a census block and to the total number of incidents recorded in that block. Furthermore, I employ a database on the universe of geo-referenced house sales. I therefore construct a block-level panel on the incidence of the closures, the number of reported incidents, clearance rates, and the local house prices. Lastly, I supplement administrative data with victimization survey data that reports exact residential locations as well as respondents' crime reporting and attitudes towards the police.

One challenge to measuring the causal effect of police station closures is that these policy changes are not randomly assigned. In London, police stations were originally located in persistently high-crime areas, and the closures affected stations in relatively less-deprived, low-crime blocks. Furthermore, surviving stations experienced an increase in officer concentration, further complicating identification. To overcome these challenges, I adopt a difference-in-differences strategy that compares local outcomes in census blocks that experience a closure of the nearest police station to blocks that do not, before and after the closure. In order to ensure that the impacts are not influenced by simultaneous reductions in local funding for other public services, I further control for time-varying attributes at the neighborhood level. I show that treated and control areas follow similar trends in outcomes prior to the closures, and that results are robust to the exclusion of areas near operating stations. To separately identify the indirect effects around surviving stations, I compare outcomes in areas near them to those farther away.

I present three sets of results. First, police station closures lower police deterrence and increase violence near to closed stations. I estimate that, in treated blocks, violent crimes, measured as assaults and murders, increase by 11%. This impact is sudden and persists over time, and shows that higher distance lowers police deterrence. At the same time, police effectiveness declines. The police likelihood of clearing a crime falls by 0.7 percentage points, a 3.7% drop with respect to the baseline clearance rate. I document a decline in the number of cleared incidents for both violent and property offenses, indicating that the deterioration in police effectiveness stems from a diminished ability to collect evidence necessary to solve crimes, rather than changes in the volume or nature of crime reports. Both the rise in violent crime and the drop in clearance are non-linear in distance: they are concentrated in blocks surrounding closed stations, and gradually decay as distance increases. This evidence indicates that the impact is driven not only by reduced police visibility around closed stations but also by longer response times. I rule out that the results are driven by criminals' displacement. Conversely, the concentration of front-line officers at the remaining stations reduces violence in their immediate vicinity. Despite this, I estimate a significant net increase in violent offenses when excluding areas near surviving stations from the control group.

To rationalize the implications of the two contrasting effects, I develop a quantitative spatial model of crime and police. Building on recent developments in economic geography (Redding and Rossi-Hansberg, 2017), the model incorporates the endogenous decisions of individuals on whether and where to engage in criminal activities. Central to the framework, the police production function hinges on two local inputs, proximity and police workforce at the police station. Exploiting the variation induced by the closures, I identify the model's structural parameters, with the resulting elasticities aligning closely with the reduced-form evidence. This framework allows me to quantify both distributional and aggregate impacts. While the model predicts a 2% overall increase in violent crime, this aggregate effect hides significant spatial heterogeneity, where some areas experience sharp increases, while others see minimal change. Finally, I consider a counterfactual scenario where station closures are chosen to minimize local expected utility from crime. Under this optimal allocation, the police could have achieved net gains by prioritizing closures that minimize changes in police proximity.

Second, police station closures discourage citizens from cooperating with law enforcement. Employing victimization survey data, I estimate a 9.5% drop in the probability of reporting crimes from a baseline rate of 37%, which is concentrated in low-severity offenses only. I find that, although police records show a decrease in low-severity property crimes and anti-social behavior (ASB), victimization data indicate an increase in those incidents. This pattern confirms that station closures exacerbate under-reporting for less severe crimes, while the rise in recorded violent crime reflects genuine increases in criminal activity. Beyond reporting, station closures also erode trust in law enforcement. Victims' satisfaction with how police handled investigations and overall confidence in police effectiveness decline, implying that lower proximity to the police erodes community engagement.

Finally, shutting down police stations reduces the welfare of local residents. I focus on house prices as a synthetic measure of community valuation of local areas. Intuitively, house prices not only reflect the direct costs associated with crime changes (e.g. Gibbons, 2004; Linden and Rockoff, 2008; Besley and Mueller, 2012), but also the indirect costs, such as the loss of local amenities and changes in perceptions of safety, that may arise as a result of the closures (Rosen, 1974; Thaler, 1978). I document an average reduction in local house prices, driven by high-crime and deprived blocks, exacerbating pre-existing inequalities, as well as in neighborhoods where police stations were left abandoned. Adopting a capitalization approach, I compute that for every £5 saved by the public authorities, £3 to £4.5 are paid back by local residents in terms of foregone house valuations. Computing the marginal value of public funds (MVPF; Hendren and Sprung-Keyser, 2020), I quantify that for each pound saved by the public administration, society bears £3 to £10 of additional costs.

This paper contributes to the vast literature that studies the relationship between crime and policing.[2] Early research identifies negative reduced form impacts on crime from city-level changes in police manpower and police resources (Evans and Owens, 2007; Machin and Marie, 2011; Chalfin and McCrary, 2017; Mello, 2019). Because of the aggregate nature of these policy shocks, isolating the mechanisms behind police deterrence becomes challenging. A related set of papers leverages natural experiments, exploiting sudden shifts in police deployment following terrorist attacks (Di Tella and Schargrodsky, 2004; Klick and Tabarrok, 2005; Draca et al., 2011), and highlights the preventative role of visible and static police presence. Likewise, Blesse and Diegmann (2022) study the deterrence effect of a police consolidation reform in German municipalities, estimating increases in crime attributed to lower police visibility. Recent studies have also considered specific determinants of police effectiveness more closely (Adda et al., 2014; Blanes i Vidal and Mastrobuoni, 2018; Mastrobuoni, 2020). For instance, Mastrobuoni (2019) exploits temporary disruption in police patrolling,

[2]This body of work started with the ground-breaking work of Becker (1968). For the most recent comprehensive reviews of the literature, see Durlauf and Nagin (2011) and Chalfin and McCrary (2017).

and shows that clearance is lower during patrol shift changes conditional on a crime being committed. Similarly, Weisburd (2021) shows that, when patrolling officers leave their beats, lower police presence increases crime through deterrence. Finally, Blanes i Vidal and Kirchmaier (2018) exploit discontinuities in the distance to response stations to show that increased response time lowers the clearance rate.

This paper advances the current literature in several ways. First, it leverages a unique natural experiment in which a major police force underwent a large-scale territorial reorganization, closing the majority of its stations. These closures generated localized shifts in proximity to police while leaving patrol patterns unchanged. Thanks to the richness of the data, I estimate the impacts on both clearance and crime, two outcomes that are often studied in isolation, operating through both police visibility and response times. Second, despite the inherently spatial nature of crime, few papers have modeled it using a spatial framework (e.g. Adda et al., 2014; Fu and Wolpin, 2018). I develop a spatial structural model that formally integrates the interplay of multiple policing inputs at the local level. By exploiting variation in station proximity and local police presence, the model isolates proximity as a key input in the police production function. Finally, this paper takes a broader view by simultaneously examining all actors involved in the law enforcement ecosystem: police, offenders, victims, and the local community. This holistic approach highlights how shifts in the spatial organization of policing can produce far-reaching and unintended consequences.

In addition, this paper emphasizes the critical role of civilian cooperation with law enforcement. As first highlighted by Levitt (1998a), if policy interventions affect *both* crime occurrence *and* reporting, ignoring changes in underlying reporting may lead to biased estimates on recorded crime. Furthermore, reporting has a crucial role for crime deterrence, as law enforcement ultimately relies on victims' willingness to report incidents (Acemoglu and Jackson, 2017; Owens and Ba, 2021). Despite this, the existing literature largely overlooks the influence of private attitudes on law enforcement.A few exceptions are Vollaard and Hamed (2012), who identify a positive correlation between reporting and police workforce size, and Ang et al. (2024), who use emergency calls to gunshots ratio to measure changes in civilian reporting after acts of police violence.

Gauthier (2022) takes a different approach by exploiting variation in reporting delays to distinguish crime occurrence from reporting in the context of gender-based violence. This paper extends this literature by directly measuring both crime incidence and victim reporting, leveraging a unique combination of police records and geo-referenced victimization survey data. My findings show that lower police presence significant undermines police–civilian interactions through erosion of community trust in policing, potentially compromising the long-term effectiveness of law enforcement.

Finally, this paper contributes to our understanding of the unintended consequences of austerity policies. Recent studies have examined the impacts of welfare reforms targeting individual benefits in the UK on the Brexit vote (Fetzer, 2019), and on the spatial concentration of crime (Giulietti and McConnell, 2020), showing that austerity measures disproportionately hit already deprived areas. I complement previous research by estimating the aggregate cost of police station closures, offering valuable insights for policymakers to assess the shadow price of public savings and to design optimal policies for reallocating scarce resources in the context of law enforcement.

2 Institutional context and data

2.A Territorial policing in London

The MPS is the police force responsible for law enforcement in the metropolitan area of Greater London, serving around 8.9 million people. The MPS is organized into 32 territorial divisions, each independently managing neighborhood patrolling and incident response functions.[3] Police deployments rarely cross divisional boundaries, with fewer than 1% of officers deployed outside their assigned division. All emergency calls are handled in centralized locations by First Contact operators, who classify incidents by severity. Once classified, calls are routed to dispatch operators within the relevant police division, who then determine which police response units to deploy. Shifts are

[3]During the study period, the boundaries of each police division in London overlapped with those of Local Authorities (hereafter, LAs), the local government units that in England are responsible for the provision of local public services (e.g. education, waste collection, social housing). Figure 1 shows the borders of the 32 London LAs. City of London does not fall under the jurisdiction of the MPS and is policed by the City of London Police.

typically divided into early, late, and night rotations, each lasting approximately eight hours. At the start of each shift, officers depart from their assigned police station to their designated patrolling zone or to respond to incidents. When not attending calls, officers are deployed on neighborhood patrols.

During the shifts, officers often return to police stations. First, each station includes a canteen where officers take their lunch breaks. Additionally, whenever an arrest is made, officers must return to the station with the detained individuals to complete the required documentation and process the incident.[4] This procedure, which includes fingerprinting, conducting criminal record checks, and completing the necessary paperwork, takes an average of 3.5 hours regardless of the severity of the crime. It may take longer if additional steps are involved, such as consulting with solicitors, coordinating interpreters, or reviewing CCTV footage. Once an arrest is fully processed, it often marks the end of the officer's shift.

2.B Budget cuts to police

In 2010, the UK government launched the Comprehensive Spending Review, leading to a 20% real-term reduction of funding to all police forces (HMIC, 2011a). The MPS saw its budget cut by 29%.[5] Consequently, the Mayor of London approved a plan aimed to curb expenses for policing. As a result, the MPS began to drastically reduce the number of stations, closing several front counters and selling police buildings.[6] Most of the savings were in the form of foregone running costs. The police authorities argued that dismissing stations would yield sizable savings in infrastructure maintenance and operating costs (MOPAC, 2015).[7]

[4]Tablets, which likely improved efficiency, were not introduced until 2017. Prior to this, officers had to return to the station to complete all the paperwork.

[5]Following the cuts, more than 600 out of 900 police stations across police forces in England shut down. Between 2012 and 2016, the MPS made £600 million in savings and needed to save an additional £400 million by 2022 (MOPAC, 2013, 2017).

[6]A police station is defined as an operational building with a front counter 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. Figure A1 provides examples through pictures of some closed stations.

[7]They argued that, as only few people reported crimes by directly walking into the station, the reduction in stations would have minimally affected residents' reporting. According to a Freedom of Information Act (FOIA) request filed to the MPS, face-to-face reporting of criminal incidents declined from

Figure 1 shows the geographical distribution of police stations closures across London. Between 2008 and 2018, the number of operating stations dropped from 160 to 45, with 80% of all closures taking place in 2013 (Figure 2). The number of residents served by each station increased from one station per 50,000 people to one station per 200,000 people. The closures were evenly distributed, and all police divisions lost at least one station in 2013. The average number of stations per division declined from 5 in 2010 to 2.4 in 2016, and to 1.3 in 2018.

The MPS committed to maintain the previous levels of front-line officers, those responsible for patrolling and incident response, at the expense of the back-office staff and, to a larger extent, of the infrastructure. They kept the distribution of officers across patrolling areas unchanged (MOPAC, 2017), reflecting their priority to preserve active police presence on the streets over stations. Between 2010 and 2016, the number of front-line officers remained largely stable, with only a 1% reduction, while 60% of police staff and police support officers were let go, as shown in Figure 3(a).[8]

The direct and indirect impacts of police station closures. Given these institutional changes, the police station closures had two distinct components affecting police deployment: a *direct* impact, stemming from reduced proximity to stations, and an *indirect* impact, due to the increased concentration of officers at the remaining stations.

First, the closures increased the distance between stations and crime scenes, while the allocation of officers to patrolling areas did not change.[9] Consequently, as front-line officers needed to travel back and forth from the stations to start and end their shifts, to respond to calls, and to report the evidence collected during on-site investigations, their

8% in 2011 to 6% by 2016, while phone reporting remained steady at around 90%. Conversely, local communities were concerned that station closures might worsen police response times, reduce visibility, and harm trust in public authorities (Pratt, 2019).

[8]Police support officers are responsible for addressing ASB, handling minor offenses, crowd control, and directing traffic. Civilian staff manage back-office functions essential to organizational operations, including finance, IT, and human resources (HMIC, 2011b). Reductions in back-office staff could plausibly create bottlenecks, diminishing administrative capacity. Because these functions are common to the entire police division, and thus are not station-specific, any capacity loss would likely affect policing within the division uniformly. The empirical analysis accounts for this by including police division-by-time fixed effects.

[9]In addition, the MPS did not change their use of other inputs (e.g. capital or technology) nor their patrolling strategy, which aimed to minimize the response time in all areas. The literature has examined how adopting different types of technology, such as IT (Garicano and Heaton, 2010; Mastrobuoni, 2020) or body-worn cameras (Barbosa et al., 2021), affects police performance.

response times increased. Figure 4(a) shows that the average distance to the nearest station doubled from 1.3 to 3 km between 2011 and 2016. Figure 4(b) plots aggregate MPS response time data. Following the closures, the median response time increased by 45%, from 13 minutes to 19 minutes. This constitutes the direct effect of the closures.

Second, although the total number of front-line officers per police division remained constant, the consolidation of stations mechanically increased the average number of officers per station by over 80% after the closures, from around 100 to more than 180 (see Figure 3(b)).This concentration may yield indirect gains at the remaining facilities, such as increased police visibility near active stations and stronger community relations thanks to the higher concentration of officers. These indirect effects may lead to localized improvements in police output around the surviving stations.

Using the two difference-in-differences strategies outlined in Section 3, I separately estimate the direct and indirect impacts of the closures on crime and police effectiveness in Section 4.A and Section 4.B. Then, in Section 4.D, I present a spatial model of police and crime to integrate the two opposing effects into a unified framework. The model delivers two structural equations that I use to estimate the input elasticities. I conclude by quantifying the aggregate impact of the closures.

2.C Data

The main dataset combines detailed information on police stations with granular data on incidents, victims, and housing transactions, and aggregates them at the census-block level. These census areas, called Lower Super Output Areas (LSOAs), are small-level geographies with a target population of about 700 households and an average size of just above 0.25 square miles. They are homogeneous geographical layers developed by the Office for National Statistics (ONS) for statistical purposes. There are 4,835 LSOAs in London, nested within the boundaries of the LAs. Below, I briefly describe the main data sources.

Police station closures. I construct a novel dataset on all police stations in Greater London between 2009 and 2018. I gathered data from FOIA requests submitted to the MPS on the universe of police stations, including their exact locations and dates of opening and closure, whether the building was sold, and, if so, the subsequent use of the property. This effort resulted in a comprehensive dataset of 168 police stations that were operational between 2009 and 2018. For the empirical analysis, I focus on the period 2011–2016.[10] During this period, the number of police stations decreased by 50%. To measure the proximity of police stations to neighborhoods, I compute the geodesic distance from the centroid of each census block to the exact geographical location of each police station, conditional on the station being located within the same LA as the census block, as each division independently manages law enforcement.

Police crime records and investigation outcomes. I employ the universe of criminal incidents recorded by the MPS between January 2011 and December 2016. Data include information on each incident's monthly date, type of offense, and geographical coordinates. The original dataset contains around seven million police records. Each incident is geo-located, and then mapped to its census block.[11] The main outcome of the empirical analysis is violent crimes, defined by the MPS as aggravated assaults and murders. This choice is motivated by the fact that serious violent crimes provide a reliable measure of the actual crime impact, as these are less prone to under-reporting (Pinotti, 2020). I also include in the analysis other types of police-recorded crimes, such as property crimes, burglaries, thefts, drug offenses, and ASB, to evaluate the extent of changes in reporting by offense type.

From January 2012 onwards, for each criminal incident, I also observe the outcome of the criminal investigation, which describes the action taken by the police or the court following a crime being recorded. The dataset contains approximately 3.2 million incidents with a valid investigation outcome.[12] I use as a measure of police performance the police ability to solve (i.e. clear) an incident, conditional on an investigation taking

[10] I exclude periods after December 2016, as the MPS undertook a territorial division restructuring.

[11] The MPS anonymization process involves slightly shifting the actual crime location, potentially creating measurement error when calculating the distance to the nearest police station. Aggregating the data at the LSOA level and computing the distance from each LSOA centroid mitigates this error.

[12] ASB incidents are never investigated by the police, while 95% of non-ASB incidents report a valid outcome. To address concerns about potential endogenous sample selection on outcomes, Table A2 shows that the probability of having a valid investigation is unaffected by the police station closures.

place.[13] At baseline, 19% of incidents are cleared at the end of an investigation. The clearance rate greatly varies across crime types, reflecting the severity of the offenses, the difficulty in identifying a suspect, and the amount of evidence required (Home Office, 2016). I use convictions as a measure of court punishment, which constitute 77% of all court sentences and include crimes sanctioned to imprisonment, fines, or other sentences by the court, excluding acquittals and discharges. To account for the prevalence of zeros in the types of crimes and outcomes, I transform the variables using the inverse hyperbolic sine transformation (asinh).[14]

Crime Survey for England and Wales. I use the Crime Survey for England and Wales (hereafter, CSEW) for the survey years 2010/11–2016/17. The CSEW is conducted on a nationally representative sample of approximately 35,000–45,000 respondents each year. It directly asks respondents whether they have experienced crime in the previous year and whether they have reported the offenses to the police. Additionally, for a subset of respondents, it asks questions about their attitudes towards the police and the criminal justice system. Online Appendix C provides further details on variable selection. I use the restricted-access geo-coded version of the CSEW, which includes information on the census block of residence of the respondents. I restrict the sample to all respondents living in London (21,409), among whom 5,087 individuals report being victims of crime. These victims experienced a total of around 7,000 incidents during the study period. Table C1 presents the descriptive statistics on respondents and incidents. The average incident-level reporting rate is 37% and it greatly varies across crime types, ranging from 30% for thefts to 70% for serious assaults and burglaries.

[13] From Thaler (1977) onwards, many studies have used clearance to measure police performance (e.g. Mas, 2006; Garicano and Heaton, 2010; Blanes i Vidal and Kirchmaier, 2018; Blanes i Vidal and Mastrobuoni, 2018; Mastrobuoni, 2020). The Home Office definition states that a crime is considered cleared when: (i) a notifiable offense has been committed and recorded; (ii) a suspect has been identified; (iii) sufficient evidence is available and the victim is informed of the detection; and (iv) the suspect is charged, cautioned, or otherwise held accountable through appropriate legal measures.

[14] The inverse hyperbolic sine (asinh) is defined as $\log(y+\sqrt{y^2+1})$. Except for small values of y, asinh is approximately equal to $\log(2)+\log(y)$. This linear monotonic transformation can therefore be interpreted in the same way as standard log-transformed variables, except for the fact that it is defined at zero (Bellemare and Wichman, 2020).

House prices. I use administrative records from the UK Land Registry on the universe of house transactions from 2011 to 2016. The original dataset contains more than 400,000 transactions in London. Every transaction records the date and price paid for the house, the house type (detached, semi-detached, terraced, flat), the house age (newly built or old), and the contract type (leasehold or freehold). All transactions are geo-located and linked to their census blocks to build the average house price at the block level (weighed by the number of transactions in the same census block–period).

Descriptive evidence. Table 1 shows the summary statistics for all the variables used in the analysis. The sample includes the universe of the census blocks in Greater London, excluding blocks located in the boroughs of City of London and Westminster due to their very distinctive administrative features and to the fact that crime records without a physical location are conventionally attributed by the MPS to these LAs. The resulting dataset consists of a monthly panel of 4,701 census blocks, observed between January 2011 and December 2016. Treated areas have lower crime rates and higher house prices than control areas, reflecting the fact that the police stations were shut down in areas with more favorable local conditions than those where police stations were left open. Indeed, station closures and their initial locations are non-random by nature. Panel A of Table A1 shows that police stations were initially located in census blocks with significantly and persistently higher levels of crime and house values than blocks without police stations. The MPS effectively chose to close stations in areas with relatively lower crime levels and higher house values than areas where stations remained open (Panel B). The identification strategy does not require the presence or closure of stations to be random or treated and control units to be similar in levels before the closures. It only assumes that, absent the closures, outcomes in treated and control blocks would have followed similar trends.

Figure 5(a) presents the yearly trend in reported crime rates from 2010 to 2016. While the overall crime rate declined until 2013, it subsequently rebounded, with a 40% increase in the violent crime rate. Figure 5(b) confirms this trend using CSEW aggregate victim data, and similarly shows an uptick starting in 2014. By the end of 2016, the average response time doubled for violent crimes, and tripled for all incidents

(Figure 5(c)). This is in spite of a lower demand for police services, with fewer reported incidents and a 20% reduction in emergency calls. Notably, the ratio of emergency calls for violent incidents to violent offenses plummeted by 40% after 2013. These stylized facts motivate the subsequent empirical analysis.

3 Empirical strategy

Difference-in-differences specification My identification strategy exploits the time and spatial variation in police station closures in London, which give rise to changes in the distance between each census block and their nearest police station. I define treated units as areas that experience an increase in the distance to the nearest police station induced by station closures. Out of 4,701 census blocks, 2,039 experienced a closure of their nearest station during the sample period. Control units are areas whose nearest station never closed. 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, the second closest is also removed, and so on. Only 8% of treated blocks are treated more than once: 183 blocks were treated twice, and 30 three times. Still, this might complicate the identification strategy. To address this, I adopt an Intention-to-Treat (ITT) approach focusing on first closures only, and I define blocks as treated if their baseline nearest police station closed.

The most straightforward design to identify the direct effects outlined in Section 2.B compares blocks with unchanged distance to those with increased distance from the nearest station. I estimate the following equation:

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

where y_{it} is the outcome of interest for census block i at calendar date t. $Closed_{it}$ is a dummy variable equal to 1 if the nearest police station to census block i closes at time t (and remains equal to 1 afterwards). It is computed as the interaction between the treatment indicator $Closure_i$, equal to 1 if the nearest station to block i was ever shut down, and $Post_{it}$, equal to 1 in the period t when the nearest police station to census block i was closed. ϕ_i are block fixed effects and capture all time-invariant characteristics of the blocks, while ϕ_t are calendar date dummies. Standard errors are clustered at the census block level, allowing for serial correlation over time (Bertrand et al., 2004). Under the assumption that, in the absence of the closures, the number and composition of criminal incidents would have evolved similarly in treated and control blocks, the parameter β provides the causal effect of police station closures on three sets of outcomes: crime, clearance, and house prices.

I also leverage variation in the intensity of treatment by estimating Equation (1) with the continuous (log or asinh) distance to the nearest police station as the main regressor. Furthermore, although the binary treatment captures over 90% of the changes in distance on average, the treatment intensity largely varies depending on the pre-determined location of blocks. After the closures, initially nearer census blocks experience greater changes in distance than blocks initially farther away. For this reason, I examine the existence of non-linearities in treatment effects along the baseline distance distribution.

A potential challenge in interpreting these effects is that surviving stations experienced an increased concentration of officers, which may generate indirect effects. To isolate the net impact of reduced station proximity, I also employ alternative definitions of the control group that exclude areas near operating stations, thereby restricting the control to blocks unaffected by station closures. This approach provides an estimate of the direct effect of closures, net of any resource reallocation.

A threat to the empirical strategy may arise from the presence of contemporaneous local confounding policies. Although the closures were enforced by the MPS, there may be other local time-varying factors correlated with the staggered closings of the stations. Suppose, for instance, that during the same period each police division decided to adopt different policing tactics as a response to the budget cuts, or that the lay-off of back-office staff increased congestion and administrative burdens within each division. Alternatively, suppose the central government cut funding to other welfare items, affecting the provision of other public services provided by the LAs (e.g. welfare, housing, education). To address this, I add to the estimating regressions the interactions between LA-specific indicators and time dummies to non-parametrically control for any (observed and unobserved) time-varying local change. Having point estimates virtually unchanged suggests that the impacts are not driven by the presence of contemporaneous confounding factors at the LA level.[15]

Dynamic specification I employ a generalized difference-in-differences specification to test the validity of the common trend assumption and to estimate the dynamic impacts of closing a police station. Recent econometric literature has raised concerns regarding two-way fixed effects (TWFE) estimators in the presence of variation in the treatment timing and heterogeneous treatment effects.[16] In this context, the vast majority of treated blocks are treated in the same period (78% in the third quarter of 2013 and 85% throughout 2013), which mitigates the concern of heterogeneous treatment effects across different treatment waves. Nevertheless, to deal with pitfalls in the TWFE estimation, I adopt a stacked-by-event design. The main advantage of a stacked design relative to a pure event study design is that the former uses a never-treated, control group, which allows the removal of event-time trends that do not appear in calendar time. In my context, this becomes crucial if, for instance, the MPS chooses which station to close first based on crime trends in previous years. In that case, calendar date fixed effects alone would not eliminate the pre-trends.

Given that in my setting there is no straightforward definition of "event" for control blocks, I adopt the approach first introduced by Deshpande and Li (2019) and I build placebo events for control areas. First, I create a separate dataset for each treatment wave (i.e. for each group of census blocks that experience the first closure in the same period). There are nine treatment waves in total.[17] In each of these datasets, all blocks

[15]Because census blocks are small areas that do not overlap with administrative boundaries, it is unlikely that any other policy changes took place at such a granular level. There may be other changes in unobserved factors (e.g. motivation of police officers). While I cannot empirically test this, leveraging variation in the continuous distance mitigates this concern, as such unobservables would have to correlate with continuous changes in the treatment intensity.

[16]A burgeoning literature, including, among others, De Chaisemartin and d'Haultfoeuille (2020), Goodman-Bacon (2021), Callaway and Sant'Anna (2021), and Borusyak et al. (2024), has emphasized that difference-in-differences designs with staggered treatment timing are likely to be biased in the presence of heterogeneous treatment effects. As the TWFE coefficient is a weighted average of all the possible 2×2 comparisons in my sample, it is also estimated using comparisons among already-treated units and not-yet-treated units, where the already-treated units serve as controls. In the presence of heterogeneous treatment effects across blocks experiencing closures at different points in time, this would induce a bias.

[17]To estimate the event study specification, I collapse the dataset at the quarterly level.

experiencing a closure in the considered period form the treatment group, while blocks that never experienced the treatment serve as the control group. Second, in each dataset I define event-time dummies relative to the period of closure. Finally, I stack all datasets into one. In this procedure, the same never-treated block serves as control multiple times (i.e. for each treatment wave). I restrict my main sample to event quarters −9 to 11, for a total of 450,901 block–quarter observations. I estimate the following equation:

$$y_{it} = \sum_{k=-B}^{T} \delta_k Closure_i \times D_t^k + \sum_{k=-B}^{T} \beta_k D_t^k + \phi_i + \phi_t + \varepsilon_{it} \quad (2)$$

Here, y_{it} is the outcome of interest for census block i in calendar date t, and D_t^k is a set of relative event-time dummies, each taking the value of 1 if period t is k periods after (or before, if k is negative) the event. The treatment indicator $Closure_i$ is equal to 1 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 $B = 9$ preceding periods and $T = 11$ subsequent periods. The stacked design allows me to separately identify calendar date (ϕ_t) and event-time (ϕ_k) fixed effects, eliminating event-time trends that do not appear in calendar time. This specification is robust to heterogeneous treatment effects, under which traditional event studies perform poorly. The coefficient δ_k identifies treatment effects k periods from the closure, with $k = -1, \ldots, -B$ indicating pre-treatment, placebo estimates. Figure 6 plots the point estimates of δ_k for violent crimes obtained using the stacked-by-event design, both when all control units are included and when those near surviving stations are excluded. Estimates of pre-treatment coefficients are close to zero and statistically not significant for all main outcomes. We cannot reject that pre-treatment coefficients are jointly equal to zero, supporting the validity of the identifying assumption. This aligns with the observation that station closures were decided on the basis of local crime trends.

Indirect effects To estimate the indirect impacts of the closures generated by the greater concentration of front-line officers near surviving stations, I restrict the sample to census blocks near surviving stations, blocks that never experienced the closure of their nearest police station at baseline (2,514 blocks). Although I do not directly observe police workforce relocation patterns within police divisions, I exploit the fact that police officers may be relocated to the surviving stations only after at least one police station under the same police division closes. Because of this, I use the first closure period within each police division to identify the relevant time in which the majority of front-line officers were displaced. As a source of cross-sectional variation, I exploit the distance from the nearest open police station. The underlying assumption is that blocks located farther away from open stations benefit little or nothing from greater police presence, and therefore police deterrence. Such an assumption is incidentally corroborated by evidence in Figure 7, which shows that the areas farthest from the stations do not experience any change in crime and police displacement.

This design compares blocks closer and farther from the open stations, before and after the first closure within the same police division. I estimate the following equation:

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

where y_{it} is the number of assaults and murders recorded in block i at monthly date t, $Post_{lt}$ is equal to 1 in the period when the first police station in the same police division l shuts down, and ϕ_i and ϕ_t are block and date fixed effects, respectively. The dummy $Near_i$ is equal to 1 for blocks located closer than the median distance to the nearest open station. To further exploit the variation in the intensity of the treatment, I employ the continuous distance, expressed as the inverse of distance, as the main regressor, and I examine whether there are non-linear treatment effects along the baseline distance distribution. Under the parallel trend assumption between treated (near) and control (far) areas, the coefficient δ identifies the indirect effect of the station closures.

4 Effects on violent crime and police effectiveness

4.A Direct effects

Violence increases as a result of police station closures. Table 2 shows regression results for violent crimes, defined using the MPS category of aggravated assaults and murders. Column 1 includes monthly date fixed effects, column 2 includes block and date fixed effects, while column 3 further adds LA $\times$ date fixed effects, which absorb all time-varying changes occurring within a police division. This includes all the potential impacts due to the reallocation of police resources within divisions (e.g. unobserved changes in organization, congestion effects). Columns 4 and 5 exclude blocks near surviving stations or those within the first quintile of baseline distance to the nearest surviving station. Conditional on date fixed effects, the correlation between the police station closures and violent crimes is negative; this fact is consistent with the non-random nature of the initial location of the police stations and their closures. Once conditioning on block fixed effects, the coefficient becomes positive. Estimates are barely affected by the inclusion of LA $\times$ date fixed effects. This last specification shows that following an increase in the distance to the nearest police station, the average number of recorded violent offenses increases by 11% per treated census block-month. In Panel B, I estimate a positive elasticity of violence with respect to distance of approximately 0.09, so that a 10% increase in distance increases violent crimes by roughly 1%. This effect translates into three additional assaults and murders in each block per year. Figure 6 plots the δ coefficients from Equation (2). Violence starts diverging across treated and control blocks right after the wave of police station closures starts. The increase in violent offenses is significant and persistent up to four years after the intervention.[18]

To assess the effects of closures on police effectiveness, I estimate Equation (1) on the incident-level dataset, restricting the sample to all incidents with a non-missing investigation outcome, as discussed in Section 2.C. I define police effectiveness as the police ability to effectively investigate, and solve (i.e. clear) crimes. The relationship

[18] Results are robust to binning distant periods together; using a balanced sample of areas in a $-7/+7$ period from/since the closure of the nearest police station or implementing the estimator proposed by Borusyak et al. (2024) (see Figure A2(a)).

between police station closures and police clearance is *a priori* ambiguous. On the one hand, station closures increase the distance and the response time to attend the crime scene. The later the police are brought in, the lower the chances are to gather the evidence and to successfully identify a suspect. On the other hand, fewer reports reduce congestion and free up resources to clear crime, thus improving clearance, conditional on reporting.

Police effectiveness worsens as a result of the closures. Table 3 shows that the incident-level probability of clearance significantly drops by 0.7 percentage points, equivalent to 3.7% from the baseline clearance rate of 19%. Results are robust to the inclusion of crime type fixed effects (column 2), which absorb the unobserved complexities associated with investigating different types of offenses.[19] Columns 4 and 7 of Table 3 indicate that higher distance to the stations significantly decreases the total volume of offenses cleared and brought to justice (i.e. convicted) in the census blocks by 7% and 5%, respectively.

Mechanisms Why is distance critical for police effectiveness and deterrence? First, closures reduce police visibility and perceptions of police presence in the surroundings of the stations. This is in line with previous findings from the literature (e.g. Di Tella and Schargrodsky, 2004; MacDonald et al., 2016; Blesse and Diegmann, 2022), that show that police visibility – through police buildings or police on guard – affects deterrence. Second, an increase in distance will prevent police from arriving very fast at those crime scenes, hurting their ability to collect evidence and clear crimes (Blanes i Vidal and Kirchmaier, 2018).[20]

To distinguish between the two channels, while incident-level response time data are unavailable, I leverage the intensive margin of the treatment and estimate Equation (1) on each subsample of quintile of baseline distance. This allows me to identify whether

[19]Figure A2(c) shows that before the closures, period-specific coefficients on clearance are not statistically different from zero, supporting the validity of the parallel trend assumption.

[20]A third potential channel is patrolling intensity. While I lack data on patrolling to test this channel, prior evidence for similar contexts (Blanes i Vidal and Mastrobuoni, 2018) suggests that small and temporary changes in patrolling intensity have little, if any, impact on crime. Mastrobuoni (2019) further underlines the incapacitation effects of police clearance. In light of this, such reduction in convictions may reinforce the adverse effects on crime through lower incapacitation of offenders, and may further discourage the public from reporting such incidents.

the observed effects are concentrated in blocks initially closer to or farther from the closed stations. Figure 7 reveals a stark gradient relative to distance: blocks located closer to the closed stations experience the largest change in both violent crime and police effectiveness. The effect on violent crime diminishes steadily and becomes negligible beyond the fourth quintile (1.65 km). In addition, Figure 8(a) further shows that the effects are driven by crime hotspots (i.e. areas with higher than median baseline crime rates). These findings together confirm that areas most affected by increased distance are those with higher opportunities for crime and greater potential returns from police presence. The impacts of station closures are strongest in the nearest distance quintile and gradually lessen, underscoring the importance of not just visibility near stations but also police response time for effective crime deterrence.

4.B Indirect effects

Table 4 presents the main indirect effect results from estimating Equation (3). Census blocks located in close proximity to an open station experience a reduction in violent crimes after the first closure within their police division. The specification from the last column quantifies a total decrease of five violent offenses per year for each block close to the operating stations. The remaining columns show that the indirect effect holds when interacting the Post dummy with the inverse of the continuous distance. Figure A2(b) shows no pre-trends before the first closure within each police division, and that the impacts are persistent over time, supporting the claim of no further reorganization of the police workforce in response to the closures.

Figure 9 shows a strong gradient of the indirect effects relative to proximity from surviving stations. The average effect is driven by blocks located very close to open stations and it quickly decays to zero as the distance increases. As *by design* response time is constant, these results can only be driven by the increased police visibility in the close vicinity of the operating stations.

Columns 4 and 5 of Table 2 exclude areas with operating stations from the control group. Both specifications yield a net increase in violent crime of similar magnitudes (5–10%). Figure 6 presents the dynamic impacts after removing surviving stations from the sample. Period-specific estimates are not statistically different from the main results using the whole sample. These findings suggest that the increased concentration of police around surviving stations does not fully offset the negative consequences of reduced proximity. To quantify the aggregate effect, the next section develops a spatial model of police and crime that formally integrates both direct and indirect effects within a unified framework.

4.C Robustness checks

I provide a range of robustness checks to various concerns. First, Table A4 in the Online Appendix varies the definition of distance to the nearest station. In columns 1–4 I replace straight-line distance with commuting distance and commuting response time by car, calculated using OpenStreetMap data. Because I lack incident-level response time data, I approximate travel distances and response times based on London road network commuting patterns. The average commuting distance computed using this approach is 1.9 km before the closures, and 2.24 km after the closures (slightly larger than distance computed as the crow flies). In columns 5–8, I compute distance to the nearest police station across LAs. This specification exploits variation in distance of census blocks whose nearest station is located in another LA, which in Equation (1) were considered not treated. In columns 9 and 10, I attribute the baseline distance to blocks that experienced an opening, estimating an ITT regression. During the sample period, seven police stations opened, and 148 census blocks out of 4,701 blocks experienced a decrease in the distance. Results are virtually unchanged.

Second, a threat to identification is the potential for local spillovers from the control group. Such spillovers may occur if criminals from neighboring control blocks relocate their activities to nearby treated blocks, which now have lower police presence and a lower risk of apprehension (Blattman et al., 2021). These spillovers would contribute to the estimated increase in violence. To address this, I identify 833 neighboring control blocks that share a border with treated blocks, I exclude them from the sample, and I repeat the analysis using as a control group only "landlocked" blocks (i.e. blocks not bordering with any treated areas). Table A5 shows that the estimates are unchanged.

Third, an additional concern is that the effects are confounded by other austerity cuts that lowered the provision of local public goods. Estimates in Table A6 show no differential effects on violent crime based on the incidence of austerity cuts to other welfare expenditures. I also consider if the effects are driven by census blocks that are 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 (Borusyak and Hull, 2023). Census block fixed effects control are insufficient to control for this if a census block's treatment status is also determined by the initial potential exposure to treatment. To address this concern, I build a measure of baseline potential exposure, counting the number of stations located at baseline in the surroundings of each census block, and interact it with a linear time trend. Results in Table A7 are robust to the use alternative cut-offs of potential exposure (2, 3, 4, and 5 km). Finally, I compute the Conley standard error to account for both spatial and serial correlation (Table A8).

A final concern is that the observed reduction in clearance may simply reflect the smaller pool of reports to investigate. A decrease in deterrence and reporting may mechanically drive the clearance rate down by simply changing the denominator, even keeping constant police effectiveness. To address this, I examine the impact on clearance rates for violent and property crimes separately. As reporting of violent crimes remains unchanged, any changes in clearance rates for such crimes would primarily reflect changes in police effectiveness. Furthermore, if the closures only affected crime prevention through deterrence, we would expect to observe an increase in cleared violent offenses. Columns 5–6 and 8–9 of Table 3 indicate that the total volume of both cleared violent and property crimes falls by 3% and 4%, respectively. The observed decrease in cleared violent offenses unambiguously indicates a decline in police effectiveness.[21] Considering the reduction in the pool of reports, the estimated drop in clearance is likely to constitute a lower bound of the actual decline in police effectiveness.

[21]Table A9 draws similar conclusions using as the outcome the ratio between charges and convictions over the total reports for all investigated offenses, and for property and violent crimes separately. Although reporting falls, the ratio also decreases, and more so for property crimes, suggesting that the ability of the police to clear incidents and convict criminals worsened disproportionately.

4.D Spatial model of police and crime

To provide a formal framework that integrates both direct and indirect effects, I develop a quantitative spatial model. Drawing on the recent urban spatial structural literature (e.g. Ahlfeldt et al., 2015; Redding and Rossi-Hansberg, 2017; Heblich et al., 2020), the model incorporates individuals' endogenous decisions regarding criminal activity and location choices, alongside a police production function where police effectiveness depends on station proximity and officer allocation. The structural model delivers a set of estimating equations that allow identification of key elasticities governing the responsiveness of crime and clearance to policing inputs. I estimate these parameters by leveraging the quasi-experimental variation introduced by station closures. Equipped with these estimates, I quantify the aggregate impact of the closures and run a counterfactual to determine the optimal allocation of police station closures. I present the main results, relegating derivations to Online Appendix B.

Individual preferences and supply of crime. Consider a city that consists of a set of discrete neighborhoods, indexed by j, which, in the empirical setting, correspond to census blocks. Individuals first decide whether to engage in criminal activities, and then, if they choose to commit a crime, decide where to commit it.

In line with the Becker (1968) model of crime, the preferences of a criminal o committing a crime in location j are defined over three components: the returns from committing a crime in a specific location (r_j), which captures characteristics that make a block more or less attractive for criminal activity (e.g. demographic composition, public transportation); the probability of being caught ($0 < \pi_j < 1$); and the penalty ($S > 0$) for committing a crime, assumed to be constant across individuals and areas. The indirect utility function for individual o who commits crime in location j is

$$u_{oj} = \left(\frac{r_j}{\pi_j S} \right) \varepsilon_{oj}, \tag{4}$$

where ε_{oj} is an idiosyncratic preference shock for crime location j, capturing individual-specific reasons for committing crimes in different parts of the city. The unobserved component ε_{oj} is drawn from a nested Fréchet distribution,

$$G(\varepsilon_{ojs}) = \exp\left[-\sum_{s} \left(\sum_{j} \varepsilon_{ojs}^{-\theta} \right)^{\mu/\theta} \right],$$

where s indexes the sector (criminal $s = C$ or non-criminal $s = NC$). The parameters θ and μ (with $0 < \mu < \theta$) control the dispersion of idiosyncratic shocks across locations and sectors, respectively. μ captures the substitutability between the criminal and legal sectors, while θ governs the substitutability across crime locations within the city. The condition $\mu < \theta$ implies that substituting across locations is easier than between criminal and legal sectors. This idiosyncratic preference structure results in a nested logit demand system, where the upper nest represents the choice between the criminal and non-criminal sectors, while the lower nest corresponds to the choice of crime location.

In the upper nest, individuals decide whether to engage in crime. The outside option $\bar{U}_{NC}$ representing the utility from not committing a crime, is fixed and exogenous. The city has a fixed aggregate population $\bar{L}$, and given the preferences of individuals, the endogenous total number of crimes C in the city is determined by

$$C = \frac{U_C^{\mu}}{U_C^{\mu} + U_{NC}^{\mu}} \bar{L}, \tag{5}$$

where U_C is the expected utility to commit crime, and μ is the dispersion parameter that identifies the cross-sectoral elasticity. The fraction $U_C^{\mu}/(U_C^{\mu} + U_{NC}^{\mu})$ gives the probability of choosing sector $s = C$ that emerges from a simple logit choice model.

In the lower nest, individuals who choose to commit a crime observe idiosyncratic utility shocks for each possible crime location and choose the location that maximizes their utility. Given this preference structure, the market clearing condition for criminals requires that the total number of crimes in location j is

$$C_j = \frac{r_j^{\theta} \pi_j^{-\theta}}{\sum_i r_i^{\theta} \pi_i^{-\theta}} C, \tag{6}$$

where C is the total number of crimes committed in the city defined in Equation (5). Thus, crime in each block j is increasing in the local returns to crime and decreasing in the local probability of being caught, with elasticity θ.

Police production function. The police production function in area j depends on two local inputs: the distance to the nearest police station, d_j, and the number of officers per station, P_j.[22] The police output, π_j, represents the clearance rate in area j:

$$\pi_j = A_j \left(\frac{1}{d_j}\right)^{\alpha} P_j^{\beta}. \tag{7}$$

Here, A_j is a location-specific exogenous productivity term that reflects local factors, such as availability of CCTV in the area. As P_j is not directly observed, I compute it as $P_j = P_{l(j)}/k_j$, where $P_{l(j)}$ is the total number of police officers in police division l of area j, and k_j is the number of stations in the same division.[23] α and β are the parameters that determine the elasticity of police output with respect to proximity and the local police workforce, respectively.

Estimation The model has four parameters: $\{\alpha, \beta, \theta, \mu\}$. The police input elasticities α and β and the location choice elasticity θ can be identified by exploiting variation in distance and police strength over time and across areas generated by the natural experiment of police station closures, under the assumption that location-specific police productivity and local returns to crime are constant over time. Local fundamentals are then recovered using the model's structure.

From Equation (7), I first derive a log-linear relationship between police output and policing inputs. All algebraic derivations are reported in Online Appendix B. I include a random error component and the time dimension, and I derive the following equation that can be estimated to retrieve the police input elasticities:

$$\log \pi_{jt} = \gamma_j + \gamma_t - \alpha d_{jt} + \beta P_{jt} + v_{jt}. \tag{8}$$

[22] The MPS police did not adjust officer allocation across areas in response to changes in criminal activity after the reform. Because of this, unlike the existing theoretical literature of police and crime (e.g. Ehrlich, 1973; Fu and Wolpin, 2018), I do not model local levels of police as a function of crime.

[23] Given the lack of data on patrolling patterns, by doing so I implicitly assume that officers are uniformly distributed across patrolling areas under each station's command.

Here, the area fixed effects γ_j capture all area characteristics A_j, under the assumption that the location-specific productivity term is time-invariant.

Next, I derive a structural equation predicting local criminal flows. By transforming Equation (6) in logs, plugging in Equation (8), and adding a random error component and the time dimension, I obtain the following log-linear equation:

$$\log C_{jt} = \phi_j + \phi_t + \delta d_{jt} - \nu P_{jt} + \varepsilon_{jt}. \tag{9}$$

Here, the area fixed effects ϕ_j absorb location characteristics $\{r_j, A_j\}$, under the assumption that the location-specific productivity and local returns from crime are time-invariant. The parameters $\delta = \alpha \cdot \theta$ and $\nu = \beta \cdot \theta$ are a combination of the input elasticities α and β and the shape parameter θ. I leverage variation in both inputs generated by the police station closures to estimate α, β, δ, and ν from Equations (8) and (9). Based on these estimates, I recover θ.

Table 5 presents the results from the parameter estimations. Columns 1 and 5 present the crime supply and clearance elasticities with respect to distance alone, and are consistent with the reduced-form estimates in Section 4. Columns 2 and 3 further include police workforce size, measured as total number of officers (or number of front-line officers) per station. The distance elasticity indicates that a 1 km increase in distance increases violent crimes by 6%, consistent with the net effect reported in Table 2. The clearance elasticity shows a net reduction of around 3%. The elasticity with respect to workforce size is smaller and has the opposite sign. Importantly, the elasticity with respect to proximity remains stable even after controlling for the number of officers per station. This suggests that the primary channel through which closures impact crime and clearance is increased distance rather than changes in police workforce per station.

For the model estimation, I use columns 4 and 8 with ITT variables that exclude the seven police station openings. The estimated parameters are: $\hat{\delta} = 0.062$, $\hat{\nu} = 0.010$, $\hat{\alpha} = 0.032$, and $\hat{\beta} = 0.003$. From these estimates, I compute $\hat{\theta}$, which ranges between 1.9 and 3.3. For the counterfactual analysis, I use the mean value $\hat{\theta} = 2.6$.[24] The

[24] I take the mean value for $\hat{\theta}$ since the two estimates have overlapping 95% confidence intervals ($[1.40, 2.48]$; $[0.84, 5.83]$), computed using the delta method. The resulting value is consistent with commuting elasticities estimated in other contexts (e.g., Khanna et al., 2023; Tsivanidis, 2024; Zárate, 2024).

parameter μ, which governs the elasticity between criminal and non-criminal activities, is sourced from the existing literature. Given the nested preferences structure, where $\mu < \theta$, I take a value of $\mu = 2.4$, following Khanna et al. (2023). Finally, I use the structure of the model to recover the unobserved fundamentals $\{r_j, A_j\}$. Note that A_j can be retrieved from the fixed effects estimates $\hat{\gamma}_j$ in Equation (8). Next, using the fixed effects estimates $\hat{\phi}_j$ from Equation (9), I solve for $\log r_j$. Reassuringly, the variance of police productivity in space is smaller than the variance of returns to crime in space.

Aggregate impacts and optimal allocation. Equipped with the model's estimates, I evaluate the aggregate effects of the policy by estimating total crime levels, C_0 and C_1, as defined in Equation (5), before and after the policy change. Each component is computed following the procedure detailed in Online Appendix B.

While the model predicts an average city-wide increase in crime of approximately 2%, there is substantial heterogeneity across census blocks and police divisions. Figure B1 plots the spatial distribution of crime changes across census blocks, where the average block-level increase is 2.5%, with the 25th percentile at −0.7%, and the 75th percentile at 5%. At the police division level, the average predicted increase is around 2%, but changes vary significantly, ranging from −0.8% to nearly 15% (Figure B2(a)).

Finally, I evaluate optimal station closures and compare them to the actual allocation and a random closure scenario. For the optimal scenario, I run a counterfactual where each police division retains the stations that minimize the expected utility of committing a crime, keeping the number of closures per division fixed.[25] As a benchmark, I construct a scenario where station closures occur at random. To do so, I randomly draw police station closures 500 times, and compute the average across the draws. Table B1 summarizes the results for violent crime across the different counterfactual scenarios. Random closures perform worse than the actual policy, increasing aggregate crime by approximately 2.25%. In contrast, optimal closures are predicted to reduce aggregate crime by 3% on average. Notably, these gains are evenly distributed across LAs (Figure B2).

What explains these potential gains? Under the counterfactual, the optimal closures

[25] This set-up reflects the institutional context, where police divisions operate independently.

are those that minimize increases in distance. Figure B2(b) compares the distribution of distances to the nearest station before and after the closures under both the actual and optimal policies. Under the optimal closures, the average distance increases to 2.4 km, which is approximately 25% (or 700 meters) less than under the actual policy.

5 Effects on victims and residents

The previous section focused on police-recorded serious offenses to isolate the impact on crime while minimizing under-reporting. In this section, I study how station closures affect crime reporting and attitudes towards the police, drawing on both victimization survey data and police records. Finally, I examine the broader welfare consequences of station closures by estimating their impact on house prices.

5.A Impacts on victims' reporting and confidence in police

Let reported crimes be expressed as $C = r \cdot C^{*}$. Police station closures can affect both the actual level of crime, C^{*}, and the reporting probability, r (Levitt, 1998a). An increase in the distance to the nearest police station may influence r through two main channels. First, residents living near closed stations may report fewer crimes due to concerns about police responsiveness or the higher travel costs of visiting a station in person. Second, reduced police presence may erode trust in law enforcement's effectiveness, further discouraging citizens from reporting.[26] If station closures significantly reduce r (particularly for less severe crimes), the observed changes in reported crime can reflect shifts in both r and C^{*}.

Reporting using victimization survey. To isolate the causal impact of station closures on crime reporting, I leverage data from the CSEW and estimate Equation (1) on the sample of London victims. Table 6 presents estimates of the impact of distance to the nearest station on incident-level reporting. The estimates indicate that a 10% increase

[26] Lack of community cooperation can undermine law enforcement and exacerbate crime (e.g. Acemoglu and Jackson, 2017). This notion is related to the "broken windows" theory (Wilson and Kelling, 1982), which suggests that, because of fewer resources and diminished police effectiveness, "citizens may soon stop calling the police, because they know 'they can't do anything' ".

in distance leads to a 0.35 percentage point decline in reporting – a 9.5% reduction relative to the baseline reporting rate of 37%. While the coefficients are not statistically significant, the decline is concentrated in low-severity crimes, such as property crimes (excluding burglaries) and thefts (columns 7–10). By contrast, the impact on violent crime reporting is close to zero (columns 3 and 4). These findings imply that closures mainly reduce the reporting of less severe crimes. Since violent crime reporting appears relatively inelastic, the observed increase in recorded violent crime estimated in Section 4 likely reflects a genuine rise in actual crime C^* rather than a shift in reporting.

Police records by crime type. To complement the survey evidence, I examine a broader set of crime categories using police records. If station closures primarily decrease r for lower severity crimes, then police records for these crimes would drop even if actual victimization remains constant or rises. Columns 5–8 of Table A3 document a 3% reduction in police recorded property crimes (excluding burglaries) and an 11% decline in thefts.[27] Yet, CSEW data indicate an increase in property crime victimization over the same period (Figure C1). The two pieces of evidence combined underscore that lower recorded property crimes actually reflect under-reporting rather than an actual drop in crime.[28] Estimates on burglaries further support this interpretation. Since burglaries are typically reported for insurance purposes, they are less sensitive to changes in police station proximity. Accordingly, the coefficients on recorded burglaries in columns 3 and 4 are null or slightly positive, though statistically insignificant.[29]

A final test of citizen cooperation with law enforcement comes from ASB incidents (e.g., vandalism and street drinking), which rely entirely on citizen reporting and never

[27] Figure A3 plots period-specific coefficients for all police records, property crimes, thefts, and ASB.

[28] I can formally compute the actual increase in crime for violent and property offenses. If observed crime is $C = rC^*$, then $\frac{\partial C^*}{\partial d} = \frac{\partial C}{\partial d}\frac{1}{r} - \frac{\partial r}{\partial d}\frac{C^*}{r}$. For violent crimes, as survey evidence suggests, $\partial r/\partial d = 0$ and reporting rate is $r = 70\%$. This implies a 12% increase in actual violent crime. For property crimes, using estimates from both police and CSEW data implies roughly an 8% rise in actual property crime. I approximate 95% confidence intervals using the delta method to be about $[-0.04, 0.28]$ and $[-0.28, 0.43]$ for violent and property crimes, respectively. Given the small CSEW subsample, these estimates are very imprecise and should be taken with caution. Nevertheless, they suggest that property crimes also increase, once under-reporting is accounted for.

[29] 98% of CSEW respondents use some form of home security and 80% have home insurance. Robberies, an additional category of violent offenses in MPS data, corroborate the same result, rising by 1.6% as a result of the closures (Table A3, columns 1 and 2). In contrast, station closures reduce recorded drug offenses (columns 9 and 10), which rely heavily on police-initiated detection rather than victim reports.

involve police investigations. While police records include ASB incidents, the CSEW also captures self-reported experiences of ASB from a subset of respondents. Any discrepancy between the two sources would therefore reflect differences in reporting behavior. Figure A3 shows a significant decline in police-recorded ASB incidents, whereas CSEW data indicate an increase in self-reported ASB across multiple categories (Table C2). Together, these findings suggest that police station closures reduce reporting for crimes prone to under-reporting (e.g., minor property crimes, ASB), while the observed rise in violent offenses reflects a genuine increase in crime.

Confidence and satisfaction with police. Police station closures are associated with a decline in confidence in police effectiveness. For a subsample of respondents, the survey includes additional questions on attitudes toward the police. Columns 1 and 2 of Table C3 show a decrease in satisfaction with how the police handled incidents among those who reported a crime. Furthermore, columns 3 and 4 confirm that a decrease in visible police presence causes a decline in trust towards police, among all respondents. As a falsification test, I look at the impacts on confidence in other criminal justice institutions to ensure that the closure of the stations influenced local residents' views specifically through its impact on perceptions of the police. Columns 5–8 of the same table indicate no statistically significant effect of increased distance on confidence in the overall criminal justice system. These findings confirm that residents have a clear understanding of the decline in police effectiveness due to the station closures and do not attribute it to other criminal justice institutions. The reduced willingness to report reflects a general lack of trust in policing resulting from reduced local police visibility. This pattern is further supported by local welfare reductions, as shown in the next subsection.

5.B Impacts on local house prices

The welfare implications of the police station closures ultimately depend on how much value citizens place on having a police station near where they live. As residents care about their exposure to crime risk, an increase in violence influences their perceptions of safety, and their valuation of living in close proximity to the crime scene (e.g. Thaler, 1978). Furthermore, house prices therefore not only reflect the direct costs related to increased criminality or to changes in crime composition, but also changes in local amenities and residents' perceptions of the neighborhood (Rosen, 1974).

To estimate the overall impact of the closures through both crime and non-crime channels, I employ the universe of house transactions from the Land Registry recorded in London from 2011 to 2016. I estimate a specification analogous to Equation (1) on the mean (log) house prices in the census block, where observations are weighted by the number of transactions recorded in the census block during the quarter.[30]

Table 7 presents the estimated effects of police station closures on house prices. The results indicate that an increase in the distance to the nearest station leads to a roughly 2% decline in house prices in treated blocks. Interpreting this coefficient as an implicit price in a hedonic function gives a mean price of around £8,000 for the closure of a local police station.[31] Strikingly, Figure 8(b) highlights how police station closures exacerbate inequality in house values between high- and low-crime blocks. These heterogeneous effects mirror the patterns of increased violence previously discussed. Closures significantly reduce the willingness to pay for housing in high-crime blocks and areas with greater socio-economic disadvantages. Furthermore, as evidenced in Tables 7 and A10, the decline in house values is driven predominantly by flats, and by areas characterized by a more-deprived housing stock, such as those with a high proportion of social rents, fewer detached houses, and smaller units. Overall, these findings suggest that cuts to police funding hit harder those areas that were already losing from austerity cuts (Fetzer, 2019; Giulietti and McConnell, 2020).

The reduction in house prices is not driven by the expansion in the local housing supply following the sale and regeneration of the closed police stations. Between 2011 and 2016, out of 80 closed stations, 45 were sold. 25 stations were transformed into new residential buildings, seven into other public amenities, such as education or community

[30] Data are aggregated at the quarterly frequency. I restrict the sample to sales of residential properties only. Figure A4 estimates the dynamic specification and shows no pre-trends before the closures.

[31] This magnitude is comparable to other studies linking house prices with crime in the UK (e.g. Gibbons, 2004; Adda et al., 2014).

centers, and three into offices.[32] In Table A11 I explore whether the impacts on house prices depend on the estate use of the closed stations. If anything, the negative effects on prices originate from blocks where the nearest closed station was either not regenerated and abandoned, or replaced with another public amenity, contrasting the hypothesis that the price reduction was driven by a supply shock to the local estate market. As a sanity check, column 5 of the same table shows also that the rise in violent crimes is concentrated in areas with abandoned stations. These negative effects reflect not only the changes in local criminality, but also the loss of local amenities represented by the closure of police stations, that directly affects residents' valuation of the neighborhood and has not been offset by new local amenities.

Cost-effectiveness To quantify the total loss using a capitalization approach, I compare the house value loss with public savings from closures. The cost–benefit ratio is 0.7 (details in Table D1), meaning that for every £5 saved by the public authority, residents incur £3 in losses. These costs are disproportionately borne by high-crime blocks, where the ratio rises to 0.9, effectively shifting the entire burden on to residents.

The capitalization approach has some drawbacks, at it only includes the valuation of marginal movers induced by the policy. Therefore, by ignoring any reduction in property values experienced by residents who chose not to sell, it likely produces a lower bound on welfare losses. To provide a broader measure of the policy's social welfare effects, I apply the marginal value of public funds (MVPF) framework developed by Finkelstein and Hendren (2020) and Hendren and Sprung-Keyser (2020). The MVPF represents the ratio of society's willingness to pay for a policy to its net fiscal cost. In the context of spending cuts, I compute the marginal loss of public funds, assuming symmetry between public spending expansions and reductions. Full details on calculations can be found in Online Appendix D. Table D7 reports MVPF estimates ranging from 2.6 to more than 10, indicating that each pound saved through this policy imposes an additional £3 to £10 on society.

[32] This translates into 55% out of the 2,039 treated blocks having their closest station closed and sold, while 36% of them had the nearest stations regenerated and transformed into new residential estates.

6 Conclusion

The relationship between police presence and crime is well established in the literature, yet the mechanisms through which policing affects crime remain a black box. This paper sheds light on this issue by examining how a major reorganization of police resources through station closures affected crime, police effectiveness, victim cooperation, and overall community welfare. Findings reveal that reduced proximity to police stations has detrimental consequences, weakening the mechanisms that help curb future crime, as citizens become less likely to cooperate with law enforcement.

These results carry several compelling implications, particularly in light of ongoing debates about police funding and public safety. First, while policymakers may view station closures as a cost-saving measure to enhance efficiency, this strategy is far from cost-effective. The resulting increase in crime disproportionately affects lower-income communities and ultimately requires costly corrective measures. To offset the crime surge, the police would need to recruit an additional 17,000 officers, wiping off any fiscal savings from the closures. Second, police infrastructure plays a crucial role in the optimal provision of public safety. Police proximity is not just about emergency response times; it also fosters stronger police-community interactions, and deterrence. As law enforcement represents the state, any erosion in its perceived legitimacy can weaken public trust and engagement with law enforcement and other public institutions.

References

Acemoglu, D. and Jackson, M. O. (2017). Social norms and the enforcement of laws. *Journal of the European Economic Association*, 15(2):245–295.

Adda, J., McConnell, B., and Rasul, I. (2014). Crime and the depenalization of cannabis possession: Evidence from a policing experiment. *Journal of Political Economy*, 122(5):1130–1202.

Ahlfeldt, G. M., Redding, S. J., Sturm, D. M., and Wolf, N. (2015). The economics of density: Evidence from the Berlin Wall. *Econometrica*, 83(6):2127–2189.

Ang, D., Bencsik, P., Bruhn, J. M., and Derenoncourt, E. (2024). Community engagement with law enforcement after high-profile acts of police violence. Technical report, National Bureau of Economic Research.

Ba, B. A. (2020). Going the extra mile: The cost of complaint filing, accountability, and law enforcement outcomes in Chicago.

Barbosa, D., Fetzer, T., Souza, P. C., and Vieira, C. (2021). De-escalation technology: The impact of body-worn cameras on citizen–police interactions. Technical report.

Beatty, C. and Fothergill, S. (2013). Hitting the poorest places hardest: The local and regional impact of welfare reform. Technical report, Sheffield Hallam University.

Becker, G. S. (1968). Crime and punishment: An economic approach. In *The Economic Dimensions of Crime*, pages 13–68. Springer.

Bellemare, M. F. and Wichman, C. J. (2020). Elasticities and the inverse hyperbolic sine transformation. *Oxford Bulletin of Economics and Statistics*, 82(1):50–61.

Bertrand, M., Duflo, E., and Mullainathan, S. (2004). How much should we trust differences-in-differences estimates? *Quarterly Journal of Economics*, 119(1):249–275.

Besley, T. and Mueller, H. (2012). Estimating the Peace Dividend: The impact of violence on house prices in Northern Ireland. *American Economic Review*, 102(2):810–33.

Blanes i Vidal, J. and Kirchmaier, T. (2018). The effect of police response time on crime clearance rates. *Review of Economic Studies*, 85(2):855–891.

Blanes i Vidal, J. and Mastrobuoni, G. (2018). Police patrols and crime. Technical Report IZA Discussion Paper 11393.

Blattman, C., Green, D. P., Ortega, D., and Tobón, S. (2021). Place-based interventions at scale: The direct and spillover effects of policing and city services on crime. *Journal of the European Economic Association*, 19(4):2022–2051.

Blesse, S. and Diegmann, A. (2022). The place-based effects of police stations on crime: Evidence from station closures. *Journal of Public Economics*, 207:104605.

Borusyak, K. and Hull, P. (2023). Nonrandom exposure to exogenous shocks. *Econometrica*, 91(6):2155–2185.

Borusyak, K., Jaravel, X., and Spiess, J. (2024). Revisiting event-study designs: Robust and efficient estimation. *Review of Economic Studies*, 91(6):3253–3285.

Callaway, B. and Sant'Anna, P. H. (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics*, 225(2):200–230.

Chalfin, A. and McCrary, J. (2017). Criminal deterrence: A review of the literature. *Journal of Economic Literature*, 55(1):5–48.

Cook, P. J. and Ludwig, J. (2010). Economical crime control. Technical Report w16513, National Bureau of Economic Research.

De Chaisemartin, C. and d'Haultfoeuille, X. (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review*, 110(9):2964–96.

Deshpande, M. and Li, Y. (2019). Who is screened out? Application costs and the targeting of disability programs. *American Economic Journal: Economic Policy*, 11(4):213–48.

Di Tella, R. and Schargrodsky, E. (2004). Do police reduce crime? Estimates using the allocation of police forces after a terrorist attack. *American Economic Review*, 94(1):115–133.

Draca, M., Machin, S., and Witt, R. (2011). Panic on the streets of London: Police, crime, and the July 2005 terror attacks. *American Economic Review*, 101(5):2157–81.

Durlauf, S. N. and Nagin, D. S. (2011). Imprisonment and crime: Can both be reduced? *Criminology & Public Policy*, 10(1):13–54.

Ehrlich, I. (1973). Participation in illegitimate activities: A theoretical and empirical investigation. *Journal of Political Economy*, 81(3):521–565.

Evans, W. N. and Owens, E. G. (2007). COPS and crime. *Journal of Public Economics*, 91(1–2):181–201.

Fetzer, T. (2019). Did austerity cause Brexit? *American Economic Review*, 109(11):3849–86.

Finkelstein, A. and Hendren, N. (2020). Welfare analysis meets causal inference. *Journal of Economic Perspectives*, 34(4):146–67.

Fu, C. and Wolpin, K. I. (2018). Structural estimation of a Becker–Ehrlich equilibrium model of crime: Allocating police across cities to reduce crime. *Review of Economic Studies*, 85(4):2097–2138.

Fyfe, N. R., Terpstra, J., and Tops, P. (2013). *Centralizing forces? Comparative perspectives on contemporary police reform in Northern and Western Europe*. Eleven International Publishing.

Garicano, L. and Heaton, P. (2010). Information technology, organization, and productivity in the public sector: Evidence from police departments. *Journal of Labor Economics*, 28(1):167–201.

Gauthier, G. (2022). Measuring crime reporting and incidence: Method and application to# MeToo. Technical Report SSRN 4242506, http://dx.doi.org/10.2139/ssrn.4242506.

Gibbons, S. (2004). The costs of urban property crime. *The Economic Journal*, 114(499):F441–F463.

Giulietti, C. and McConnell, B. (2020). Kicking you when you're already down: The multipronged impact of austerity on crime. Technical Report preprint arXiv:2012.08133, arXiv.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, 225(2):254–277.

Heblich, S., Redding, S. J., and Sturm, D. M. (2020). The making of the modern metropolis: Evidence from London. *Quarterly Journal of Economics*, 135(4):2059–2133.

Heeks, M., Reed, S., Tafsiri, M., and Prince, S. (2018). *The Economic and Social Costs of Crime*. Home Office Research report.

Hendren, N. and Sprung-Keyser, B. (2020). A unified welfare analysis of government policies. *Quarterly Journal of Economics*, 135(3):1209–1318.

HMIC (2011a). Adapting to austerity: A review of police force and authority preparedness for the 2011/12–14/15 CSR period.

HMIC (2011b). Demanding times: The front line and police visibility.

Home Office (2016). *Crime outcomes in England and Wales: Year ending March 2016*.

Khanna, G., Nyshadham, A., Ramos-Menchelli, D., Tamayo, J. A., and Tiew, A. (2023). Evaluating the impact of urban transit infrastructure: Evidence from Bogota's transmilenio.

Klick, J. and Tabarrok, A. (2005). Using terror alert levels to estimate the effect of police on crime. *Journal of Law and Economics*, 48(1):267–279.

Levitt, S. D. (1998a). The relationship between crime reporting and police: Implications for the use of uniform crime reports. *Journal of Quantitative Criminology*, 14:61.

Levitt, S. D. (1998b). Why do increased arrest rates appear to reduce crime: Deterrence, incapacitation, or measurement error? *Economic Inquiry*, 36(3):353–372.

Linden, L. and Rockoff, J. E. (2008). Estimates of the impact of crime risk on property values from Megan's laws. *American Economic Review*, 98(3):1103–27.

MacDonald, J. M., Klick, J., and Grunwald, B. (2016). The effect of private police on crime: Evidence from a geographic regression discontinuity design. *Journal of the Royal Statistical Society Series A: Statistics in Society*, 179(3):831–846.

Machin, S. and Marie, O. (2011). Crime and police resources: The street crime initiative. *Journal of the European Economic Association*, 9(4):678–701.

Mas, A. (2006). Pay, reference points, and police performance. *Quarterly Journal of Economics*, 121(3):783–821.

Mastrobuoni, G. (2019). Police disruption and performance: Evidence from recurrent redeployments within a city. *Journal of Public Economics*, 176:18–31.

Mastrobuoni, G. (2020). Crime is terribly revealing: Information technology and police productivity. *Review of Economic Studies*, 87(6):2727–2753.

Mello, S. (2019). More COPS, less crime. *Journal of Public Economics*, 172:174–200.

MOPAC (2013). Estate Strategy 2013–2016, MOPAC/MPS. Technical report.

MOPAC (2015). Review of MPS Contact Points. Technical report.

MOPAC (2017). Public Access Strategy. Technical report.

Owens, E. (2020). *The economics of policing*, pages 1–30. Springer, Berlin.

Owens, E. and Ba, B. (2021). The economics of policing and public safety. *Journal of Economic Perspectives*, 35(4):3–28.

Pinotti, P. (2020). The credibility revolution in the empirical analysis of crime. *Italian Economic Journal*, 6(2):207–220.

Pratt, A. (2019). Police stations: are they a thing of the past? *UK Parliament, House of Commons Library, May*, 28.

Redding, S. J. and Rossi-Hansberg, E. (2017). Quantitative spatial economics. *Annual Review of Economics*, 9(1):21–58.

Rosen, S. (1974). Hedonic prices and implicit markets: Product differentiation in pure competition. *Journal of Political Economy*, 82(1):34–55.

Sun, L. and Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2):175–199.

Thaler, R. (1977). An econometric analysis of property crime: Interaction between police and criminals. *Journal of Public Economics*, 8(1):37–51.

Thaler, R. (1978). A note on the value of crime control: Evidence from the property market. *Journal of Urban Economics*, 5(1):137–145.

Tsivanidis, N. (2024). Evaluating the impact of urban transit infrastructure: Evidence from Bogota's transmilenio.

Vollaard, B. and Hamed, J. (2012). Why the police have an effect on violent crime after all: Evidence from the British Crime Survey. *Journal of Law and Economics*, 55(4):901–924.

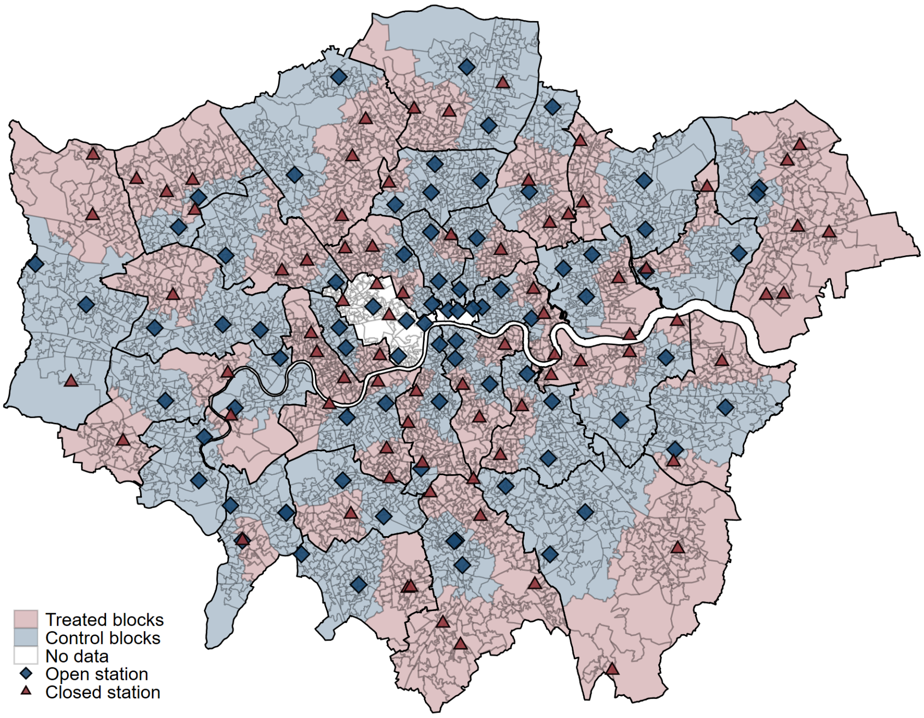
Weisburd, S. (2021). Police presence, rapid response rates, and crime prevention. *Review of Economics and Statistics*, 103(2):280–293.

Wilson, J. Q. and Kelling, G. L. (1982). Broken windows: The police and neighborhood safety. *Atlantic Monthly*, 249(3):29–38.

Zárate, R. D. (2024). Spatial misallocation, informality, and transit improvements: Evidence from Mexico City.

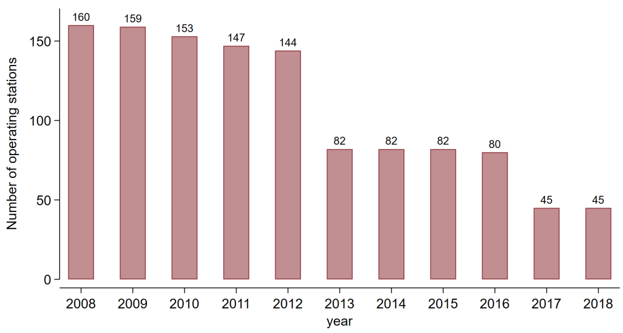
Figures

Figure 1: Map of police station closures and treated blocks in Greater London



Note: The map shows the locations of police stations operating in Greater London as of the end of 2016. Open stations are marked with blue diamonds, and closed stations with red triangles. Treated census blocks – whose nearest station was closed – are shaded in red, while blocks whose nearest station remained open are shaded in blue. Thick black borders indicate the boundaries of the 33 London boroughs, which align with the police division boundaries. The final sample excludes the City of London and Westminster.

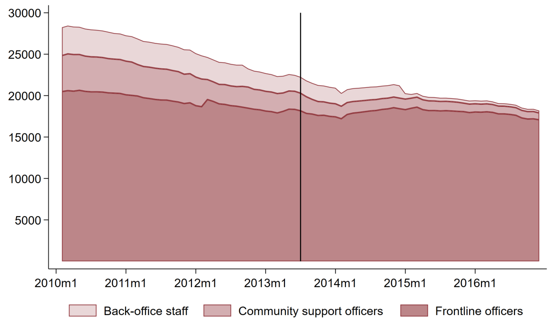
Figure 2: Number of police stations in Greater London



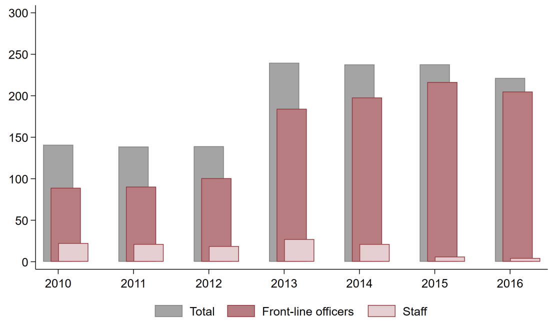
Note: The figure shows the total number of police stations operating in Greater London from 2008 to 2018, with counts recorded at the end of each year. The sample period for the empirical analysis ends in 2016 due to changes in the local policing structure thereafter. Data on police stations were obtained by the author through FOIA requests filed to the MPS.

Figure 3: Police workforce

(a) Police workforce (FTE) by function



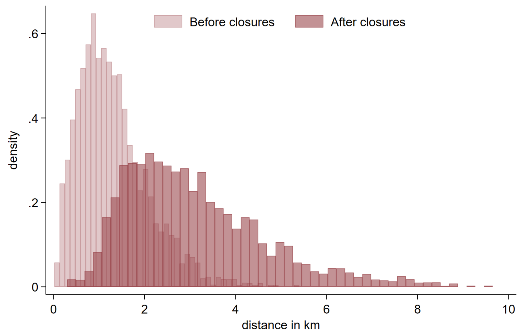
(b) Officers per station



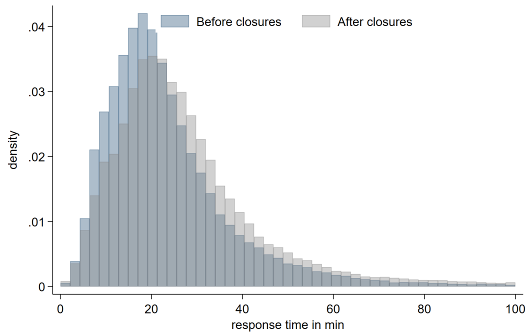
Note: This figure presents the breakdown of the MPS police workforce in full-time equivalents (FTE) by policing function from 2010 to 2016. The data, sourced from the Mayor's Office of Policing and Crime, provide LA-level information on the MPS workforce. Panel (a) depicts total numbers, while panel (b) normalizes them by the number of operating stations. Front-line officers include police constables and detective constables employed in patrolling and emergency response. Community support officers (PCSOs) are uniformed members of police staff whose main duties include tackling ASB, dealing with minor offenses, crowd control, and directing traffic. Back-office roles include administrative or clerical jobs carried out by civilians such as support functions, training, finance and HR, middle office roles such as processing intelligence, working in control rooms, and preparing files for court. The vertical line corresponds to July 2013, when the majority of closures occurred.

Figure 4: Distance to police station and response time before and after the closures

(a) Distance to nearest station



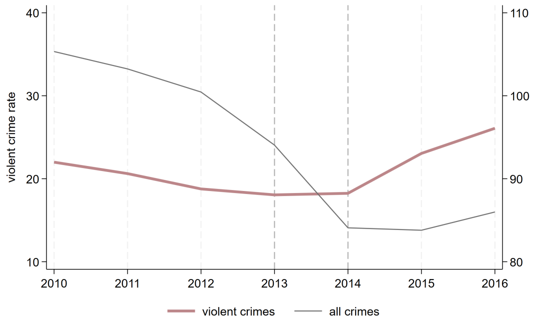
(b) Response time to emergency calls



Note: Panel (a) plots 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, as calculated by the author. Panel (b) plots the distribution of the response time before and after the closures using aggregate response time data, sourced from MPS. The sample excludes the City of London and Westminster, and spans the years 2011–2016.

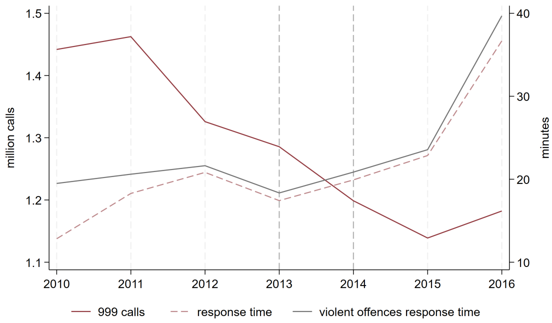
Figure 5: Crime, emergency calls, and response time in Greater London

(a) Crime rates



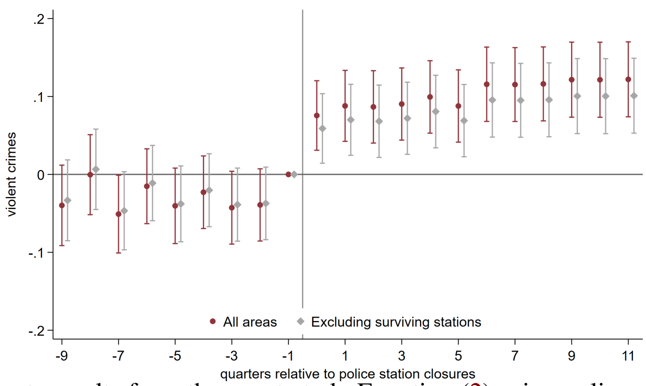
(b) Victims of crime

(c) 999 calls and response time



Note: Panel (a) shows crime rates per 1,000 individuals for all violent crimes (left axis) and all incidents (right axis) from 2010 to 2016. Panel (b) plots the total share of respondents who are victims of crime and reside in London, based on CSEW aggregate data from 2004 to 2019. Panel (c) reports the number of emergency calls and average response times over the same period, calculated for 999 calls classified as "I" (immediate) or "S" (significant). Response times include both the call duration with the operator and the travel time to the scene, and are sourced from aggregate MPS data. Average response times are presented for all offenses and violent offenses separately.

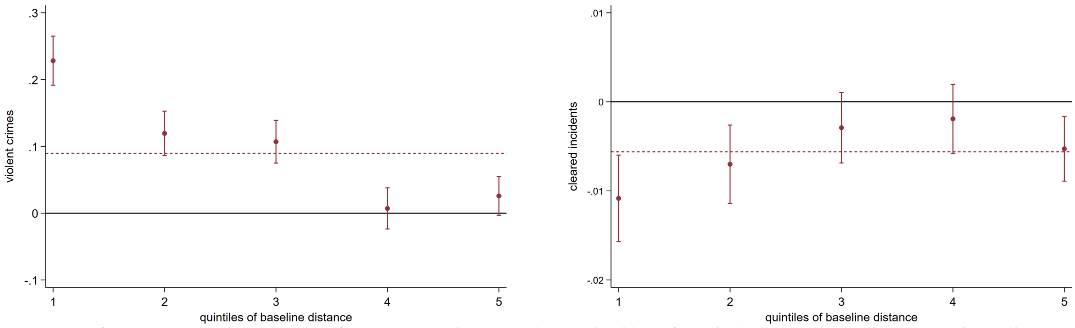
Figure 6: Dynamic effects of police station closures on violent crimes



Note: The figure presents results from the event-study Equation (2) using police records. See Section 3 for details about the regression specification and sample definition. The dependent variable is the asinh-transformed violent crimes from police records. I omit the dummy for the period before the closures and exclude distant relative periods following Sun and Abraham (2021). The figure plots estimates from including all control blocks (red dots) and excluding blocks near to surviving stations (gray dots). Standard errors are clustered at the census block level. Error bars represent 95% confidence intervals.

Figure 7: Effects on violent crime and clearance by baseline distance

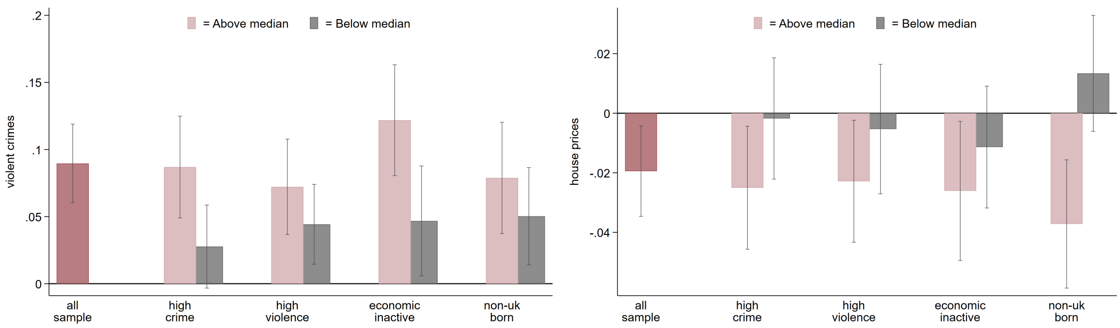
(a) Violent crime (b) Clearance



Note: The figures present results from Equation (1) by quintile of (asinh) baseline distance (i.e. distance to the nearest police station measured before any change occurs). The dashed horizontal lines report the average effects. See Section 3 for details about the regression specification and sample definition. Panel (a) employs census block data and uses the asinh-transformed number of violent crimes as the dependent variable. In Panel (b), incident-level data are used, the sample is restricted to incidents with non-missing outcomes, and the dependent variable is a dummy for whether the incident was cleared. Standard errors are clustered at the census block level. Error bars represent 95% confidence intervals.

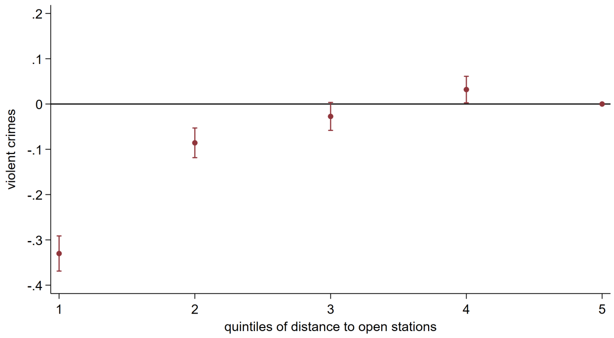
Figure 8: Heterogeneous effects by baseline characteristics of the census blocks

(a) Violent crimes (b) House prices



Note: Panel (a) presents results from Equation (1) on police-recorded violent crimes. See Section 3 for details about the regression specification and sample definition. The dependent variable is the asinh-transformed violent crimes from police records. Panel (b) plots estimates on (log) house prices. The observations are weighted by the number of sales in the census block during the quarter. Baseline crime rates are computed using data from 2008 and come from LSOA-level MPS historical data. Baseline local characteristics come from the 2011 Census and include the population share of economically inactive and the share of non-UK born. Standard errors are clustered at the census block level. Error bars represent 95% confidence intervals.

Figure 9: Effects of police station closures on violent crime by distance to operating stations



Note: The figure plots δ_q coefficients and confidence intervals from the regression, $y_{i,t} = \sum_{q=1}^{4} \delta_q I\{Bin_i = q\} * Post_{l,t} + \phi_i + \phi_{t,l} + \varepsilon_{it}$, where the $I\{Bin_i = q\}$ dummies indicate whether each block is in the qth quintile of the baseline distance distribution, with $q = 1, \ldots, 5$. The omitted category, which serves as the control group, consists of areas at the highest quintile of baseline distance, $q = 5$ (i.e. furthest away from the nearest open station). The rest of the notation follows from Equation (3). Regressions are run on the sample of blocks which never experienced a closure (2,514, among which 1,257 are below the median). The dependent variable is the asinh-transformed violent crimes from police records. Standard errors are clustered at the census block level. Error bars represent 95% confidence intervals.

Tables

Table 1: Descriptive statistics

	Treated blocks		Control blocks	
	Pre	Post	Pre	Post
	(1)	(2)	(3)	(4)
Panel A: LSOA-level dataset from police records				
Distance				
Distance from closest police station (in km)	1.38	2.82	1.46	1.44
Crime				
All crimes	17.74	14.47	20.97	17.37
Violent crimes	2.75	2.99	3.28	3.63
Property crimes	5.92	6.42	6.71	7.50
Drug-related crimes	2.86	0.68	3.57	0.92
Anti-social behaviour	5.89	3.75	7.00	4.54
Cleared crimes	0.86	1.72	1.07	2.26
Cleared property crimes	0.27	0.51	0.34	0.68
Cleared violent crimes	0.25	0.57	0.30	0.70
Convicted crimes	0.46	0.86	0.58	1.13
Convicted property crimes	0.20	0.33	0.24	0.43
Convicted violent crimes	0.12	0.27	0.15	0.34
Observations	48,936	97,872	63,888	127,776
Panel B: incident-level dataset on investigation outcomes				
Pr(cleared)	0.18	0.16	0.19	0.18
Pr(convicted)	0.10	0.08	0.10	0.09
Observations	232,550	1,029,749	356,991	1,612,388
Panel C: LSOA-level dataset on house prices				
House prices	484,744.93	648,554.40	465,604.32	655,271.21
Number of transactions	31.61	54.03	31.32	52.06
Observations	9,562	19,767	10,882	22,692

Note: This table presents summary statistics for all variables used in the empirical analysis. Panels A and B use police records, and Panel C relies on Land Registry data. Panel A reports averages calculated at the census block-month level. Panel B provides statistics from the incident-level dataset of investigation outcomes. Panel C computes average house prices weighted by the number of transactions aggregated at the census block level with a quarterly frequency. Treated blocks are defined as census blocks that experienced an increase in distance to the nearest police station during the sample period. "Pre" and "Post" periods are defined before and after the closures.

Table 2: The effects of police station closures on violent crimes

	Assaults and murders				
	(1)	(2)	(3)	(4)	(5)
Panel A: Binary treatment					
dummy distance	-0.011***	0.090***	0.115***	0.096***	0.049***
	(0.003)	(0.015)	(0.015)	(0.015)	(0.015)
Panel B: Continuous treatment					
distance	-0.093***	0.086***	0.087***	0.073***	0.035**
	(0.006)	(0.016)	(0.016)	(0.016)	(0.016)
Observations	337,794	337,794	337,794	333,042	292,506
Mean Dep. Variable	2.48	2.48	2.48	2.485	2.485
LSOA FE		✓	✓	✓	✓
Date FE	✓	✓			
LAxDate FE			✓	✓	✓

Note: This table presents estimates from Equation (1) on asinh-transformed violent crimes using police records. The explanatory variables are: the binary treatment as defined in Section 3 in Panel A; the continuous geodesic distance between the centroid of each census block and the closest police station, measured in km and asinh-transformed, in Panel B. Column 2 includes LSOA and date fixed effects. Columns 3–5 add LA $\times$ date fixed effects. Columns 1–3 use the full sample. Column 4 excludes census blocks with a surviving police station (66 census blocks), whereas Column 5 further excludes areas within the first quintile of distance to the nearest surviving police station (629 control blocks). "LA" refers to the 31 London LAs (excluding Westminster and City of London). The table displays the baseline mean of the number of assaults and murders (in absolute terms). Standard errors are clustered at the census block level. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table 3: The effects of police station closures on clearance

	Pr(cleared)			Volume cleared crimes			Volume convictions		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				All crimes	Property crimes	Violent crimes	All crimes	Property crimes	Violent crimes
Panel A: Binary treatment									
dummy distance	-0.006***	-0.003**	-0.003**	-0.073***	-0.027***	-0.039***	-0.046***	-0.019***	-0.020***
	(0.002)	(0.002)	(0.001)	(0.009)	(0.006)	(0.005)	(0.007)	(0.005)	(0.004)
Panel B: Continuous treatment									
distance	-0.007***	-0.003	-0.005***	-0.063***	-0.018***	-0.037***	-0.036***	-0.012**	-0.017***
	(0.002)	(0.002)	(0.001)	(0.009)	(0.007)	(0.005)	(0.007)	(0.005)	(0.004)
Observations	3,221,504	3,221,504	3,221,504	337,794	337,794	337,794	337,794	337,794	337,794
Mean Dep. Variable	0.19	0.19	0.19	1.28	0.41	0.35	0.68	0.28	0.18
Date, LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
LAxDate FE		✓							
Crime type FE			✓						

Note: This table presents estimates from Equation (1) on police effectiveness using police records. The sample is restricted to all incidents with a non-missing investigation outcome. The dependent variable in columns 1–3 is the incident-level probability of an incident being cleared. The dependent variables in columns 4–9 are defined as: the total number of cleared crimes by type of offense (columns 4–6) and the total number of convictions by type of offense (columns 7–9). Convictions refer to incidents where an individual is declared guilty of a criminal offense by the verdict of a court (thus exclude acquittals and discharges). The explanatory variables are defined as in Table 2. Column 1 includes LSOA and date fixed effects, column 2 adds LA $\times$ date fixed effects, and column 3 adds crime-type fixed effects. "LA" refers to the 31 London LAs (excluding Westminster and City of London). The table displays the baseline mean of the outcomes (in absolute terms). Standard errors are clustered at the census block level. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table 4: The indirect effects of police station closures on violent crimes on sub-sample of operating stations

	Assaults and murders			
	(1)	(2)	(3)	(4)
Post	0.062***		0.018	
	(0.019)		(0.027)	
dummy near × Post	-0.253***	-0.180***		
	(0.023)	(0.024)		
$1/distance\times$ Post			-0.071***	-0.059***
			(0.020)	(0.018)
Observations	181,008	181,008	181,008	181,008
Mean Dep. Variable	2.44	2.44	2.44	2.44
Date, LSOA FE	✓	✓	✓	✓
LAxDate FE		✓		✓

Note: This table presents estimates from Equation (3) on asinh-transformed violent crimes using police records. The explanatory variables *Post* and *Near* are defined in Section 3. $1/distance$ is the inverse of the distance to the nearest open station in km. Columns 1 and 3 include LSOA and date fixed effects. Columns 2 and 4 add LA × date fixed effects. Regressions are run on the sample of blocks which never experienced a closure (2,514, among which 1,257 are below the median). "LA" refers to the 31 London LAs (excluding Westminster and City of London). The table displays the baseline mean of the number of assaults and murders (in absolute terms). Standard errors are clustered at the census block level. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table 5: Model's parameters: elasticities with respect to distance and police workforce per station

	Assaults and murders				Volume cleared crimes			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Distance (in km)	0.049***	0.057***	0.058***		-0.028***	-0.029***	-0.030***	
	(0.006)	(0.006)	(0.006)		(0.003)	(0.003)	(0.003)	
ITT Distance (in km)				0.062***				-0.032***
				(0.006)				(0.003)
N. officers		-0.007***				0.001*		
		(0.001)				(0.001)		
N. front-line officers			-0.010***				0.003***	
			(0.001)				(0.001)	
ITT N. front-line officers				-0.010***				0.003***
				(0.001)				(0.001)
Observations	337,794	337,794	337,794	337,794	337,794	337,794	337,794	337,794
Mean Dep. Variable	2.31	2.31	2.31	2.31	2.31	2.31	2.31	2.31
Date FE	✓	✓	✓	✓	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓

Note: This table presents estimates from Equations (8) and (9) using police records. The explanatory variables are distance from the nearest police station (measured in km), total number of police officers per station, and number of front-line officers per station. The police workforce measures are computed as total number of officers per LA over the number of stations, and standardized such that a one-unit increase corresponds to an addition of 10 officers. The dependent variables are number of assaults and murders (columns 1–4) and number of cleared incidents (columns 5–8) asinh-transformed. The table displays the baseline mean of the outcomes (in absolute terms). Standard errors are clustered at the census block level. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table 6: The effects of police station closures on reporting using CSEW

	=1 if incident reported									
Type of offence	All incidents		violent crimes		assaults		property crimes		thefts	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Binary treatment										
dummy distance	-0.028	-0.028	0.008	0.005	0.01	0.004	-0.05	-0.054	-0.022	-0.028
	[0.045]	[0.047]	[0.020]	[0.020]	[0.016]	[0.017]	[0.040]	[0.043]	[0.033]	[0.035]
Panel B: Continuous distance										
distance	-0.032*	-0.035*	0.001	-0.000	0.002	-0.002	-0.049	-0.052	-0.027	-0.024
	[0.018]	[0.018]	[0.013]	[0.013]	[0.012]	[0.011]	[0.035]	[0.034]	[0.026]	[0.027]
Observations	5,325	5,325	5,325	5,325	5,325	5,325	5,325	5,325	5,325	5,325
Mean dep. Variable	0.37	0.37	0.043	0.043	0.026	0.026	0.289	0.289	0.177	0.177
Date, LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
LA-specific linear trends		✓		✓		✓		✓		✓

Note: The table presents estimates from Equation (1) on reporting using CSEW data. The sample is restricted to incidents experienced by respondents residing in London (excluding Westminster and City of London), as described in Online Appendix C. The outcome variable is defined as a binary indicator equal to 1 if the respondent reported the incident for all incidents (columns 1 and 2) and by type of incidents (columns 3–10). Violent crimes include assaults and robberies. Property crimes include thefts, shoplifting, and criminal damage, and exclude burglaries. The explanatory variables are defined as in Table 2. Odd columns include LSOA and quarterly date fixed effects, and even columns add LA-specific linear time trends. "LA" refers to the 31 London LAs (excluding Westminster and City of London). The table displays the baseline mean of the outcome. Standard errors are clustered at the census block level. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table 7: The effects of police station closures on house prices

	House prices							
	All properties		Flats		Terraced houses		Detached houses	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Binary treatment								
dummy distance	-0.023***	-0.019**	-0.013*	-0.007	0.000	0.001	-0.008	0.001
	(0.007)	(0.008)	(0.006)	(0.007)	(0.008)	(0.009)	(0.013)	(0.016)
Panel B: Continuous treatment								
distance	-0.015***	-0.008	-0.009	0.001	0.003	0.005	-0.011	-0.003
	(0.005)	(0.006)	(0.005)	(0.006)	(0.007)	(0.007)	(0.011)	(0.013)
Observations	62,674	62,664	52,670	52,647	38,878	38,872	18,037	18,029
Date, LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓
LAxDate FE		✓		✓		✓		✓

Note: This table shows regression results using the quarterly house price dataset. Observations are weighted by the number of sales recorded in the quarter–census block cell. Dependent variables are the (log) average house prices for all properties (columns 1 and 2), and by property type, including flats (62%, columns 3 and 4), terraced houses (27%, columns 5 and 6), and detached/semi-detached houses (10%, columns 7 and 8). The explanatory variables are defined as in Table 2. "LA" refers to the 31 London Local Authorities, excluding City of London and Westminster. Standard errors are clustered at the census block level. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Appendix - For Online Publication

Appendix A Appendix Figures and tables

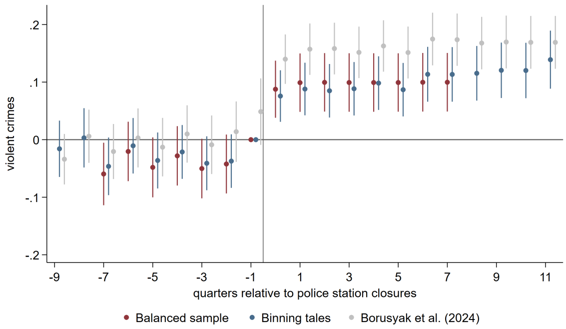
A1 Additional figures

Figure A1: Examples of closed police stations in London

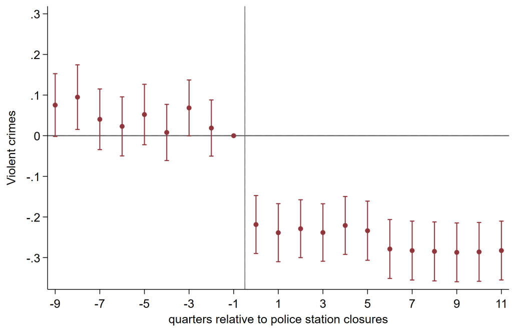
Note: These photographs show examples of closed police stations in London. The second image in the top row depicts a station that has been repurposed into a restaurant, while the last image in the bottom row shows an architectural rendering of a residential property built on the site of a former station.

Figure A2: Dynamic effects of police station closures on violent crimes and clearance

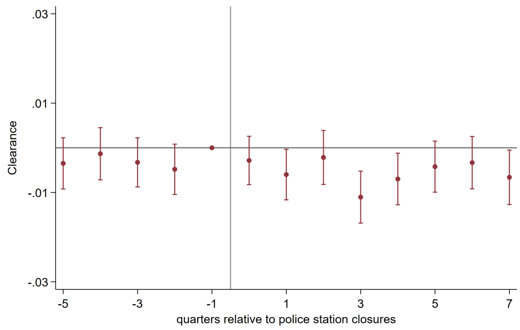
(a) Violent crimes: robustness



(b) Violent crimes: indirect effects

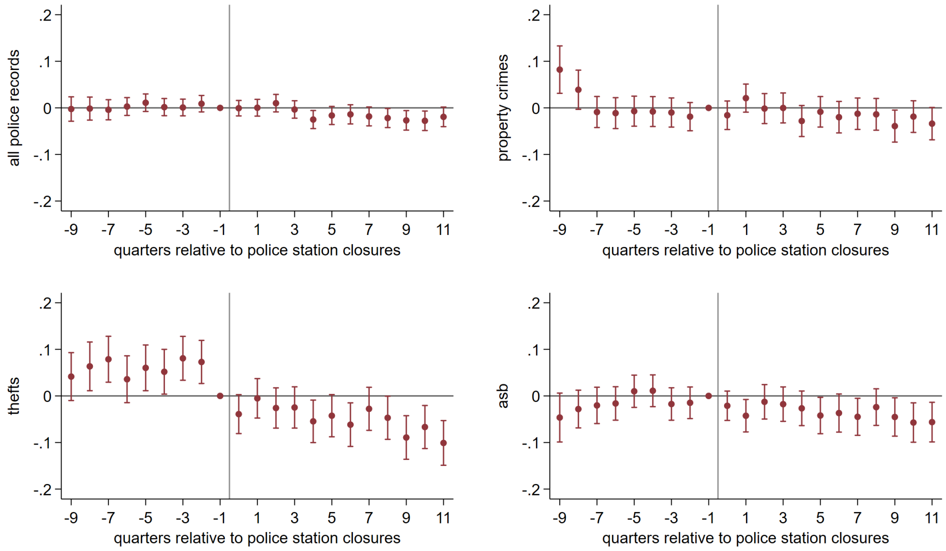


(c) Clearance



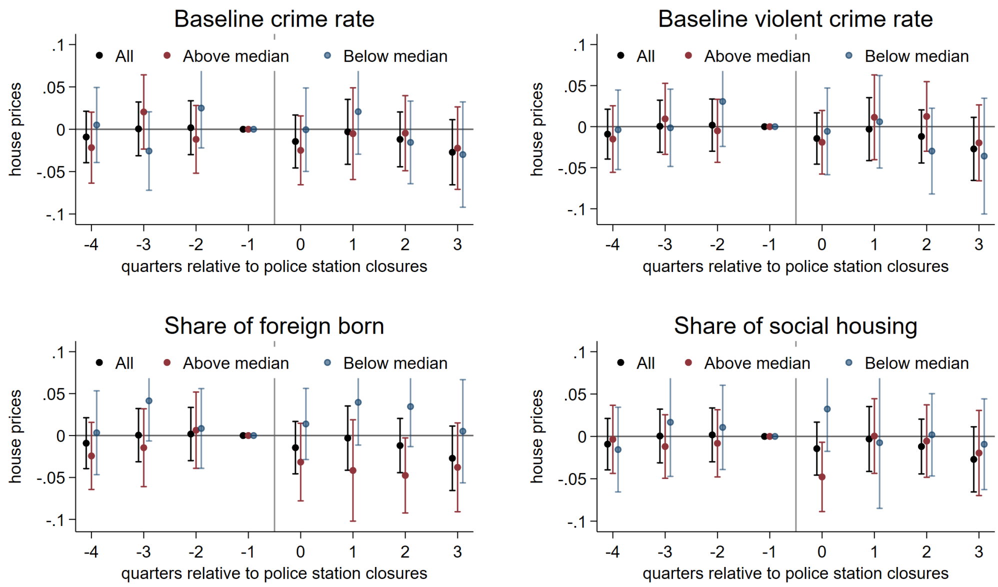
Note: The panels present results from the event-study Equation (2) See Section 3 for details about the regression specification. I omit the dummy for the period before the closures and exclude distant relative periods following Sun and Abraham (2021). In Panel (a) and Panel (b) the dependent variable is the asinh-transformed violent crimes from police records. Panel (a) runs alternative event-study specifications: (i) using a balanced sample of census blocks, spanning $-7/+7$ periods around closures; (ii) binning periods before -9 and after $+11$, assuming constant treatment effects within bins as in Sun and Abraham (2021); (iii) following the Borusyak et al. (2024) method. Panel (b) restricts the sample to blocks that never experienced a closures. Definition of treatment is the same as in Equation (3). Panel (c) uses as a dependent variable the one in column 1 of Table 3. Standard errors are clustered at the census block level. Error bars represent 95% confidence intervals.

Figure A3: Dynamic effects of police station closures on police records



Note: The panels present results from the event-study Equation (2) using police records. See Section 3 for details about the regression specification. I omit the dummy for the period before the closures and exclude the distant relative periods following Sun and Abraham (2021). Property crimes exclude burglaries. Standard errors are clustered at the census block level. Error bars represent 95% confidence intervals.

Figure A4: Dynamic effects of police station closures on local house prices



Note: The panels present results from the event-study Equation (2). See Section 3 for details about the regression specification and sample definition. The dependent variable is the average (log) house prices computed in the census block. Observations are weighted by the number of sales recorded in the quarter–census block cell. I omit the dummy for the period before the closures and exclude the distant relative periods following Sun and Abraham (2021). Each panel shows the coefficients using the whole sample, and splitting the sample by baseline characteristics (above versus below the London median). Baseline characteristics are defined in Figure 8. Standard errors are clustered at the census block level. Error bars represent 95% confidence intervals.

A2 Additional tables

Table A1: Local characteristics, police station presence and closures

	Police station presence			Police station ever closed		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All census blocks						
log all crimes	0.068***			0.027***		
	(0.007)			(0.006)		
log violent crimes		0.021***	0.047***		0.010**	0.021***
		(0.004)	(0.008)		(0.004)	(0.007)
log property crimes		0.035***	0.008		0.020***	0.008
		(0.008)	(0.008)		(0.006)	(0.006)
log drug-related offences		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)
Observations	13584	12993	7718	13584	12993	7718
N. treated blocks	4,712	4,712	4,712	4,712	4,712	4,712
FE	LAxYear	LAxYear	LA	LAxYear	LAxYear	LA
Panel B: Census blocks with any police station in 2010						
	Police station ever closed			Police station ever sold		
log all crimes	-0.241***			-0.134**		
	(0.057)			(0.064)		
log violent crimes		-0.080	-0.326***		-0.123	-0.337**
		(0.108)	(0.101)		(0.096)	(0.120)
log property crimes		0.159	0.259**		0.129	0.145
		(0.100)	(0.092)		(0.088)	(0.089)
log drug-related offences		-0.246***	-0.267***		-0.109*	-0.076
		(0.056)	(0.053)		(0.056)	(0.054)
log house prices			-0.484***			0.024
			(0.139)			(0.210)
Observations	417	415	251	417	415	251
N. treated blocks	148	148	148	148	148	148
FE	LAxYear	LAxYear	LA	LAxYear	LAxYear	LA

Note: Panel A uses the entire sample of census blocks, and Panel B restricts the sample to blocks with an operating police station at the beginning of the sample. The dependent variables are indicators for police station presence and station closures (Panel A), and indicators for station presence and sale of a police station (Panel B). Explanatory variables are computed prior to any closures: crime variables are based on 2008–2010 MPS Historical Crime Data collection, and house prices are computed as census block averages over the five years prior the sample period (2006–2010). Columns 1 and 2 add LA $\times$ year FE, and column 3 includes LA FE. "LA" refers to the 31 London LAs (excluding City of London and Westminster). Standard errors are clustered at the census block level. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. * p<0.1.

Table A2: Police station closures and sample selection of investigations

	Sample: All incidents Pr(non-missing investigation outcome)			
	(1)	(2)	(3)	(4)
Panel A: Binary treatment				
dummy distance	-0.001	0.001	-0.001	0.001*
	(0.001)	(0.001)	(0.001)	(0.000)
Panel B: Continuous treatment				
distance	-0.001	-0.000	-0.001	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	3,346,332	3,346,332	3,346,332	3,346,332
Mean Dep. Variable	0.87	0.87	0.87	0.87
Date FE	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓
LAxDate FE		✓		✓
Crime type FE			✓	✓

Note: The table presents estimates based on a sample of all incidents. The dependent variable is an indicator for whether an incident has a non-missing investigation outcome. The explanatory variables are defined as in Table 2. Column 1 includes LSOA and date fixed effects, column 2 adds LA $\times$ date fixed effects, and columns 3 and 4 add crime-type FE. "LA" refers to the 31 London LAs (excluding Westminster and City of London). The table displays the baseline mean of the outcomes (in absolute terms). Standard errors are clustered at the census block level. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table A3: The effects of police station closures on crime types

	Robberies		Burglaries		Theft		Property crimes (excluding burglaries)		Drugs-related offences	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Binary treatment										
dummy distance	-0.006	0.016***	0.002	0.003	-0.109***	-0.110***	-0.027***	-0.030***	-0.031***	-0.024***
	(0.005)	(0.005)	(0.007)	(0.007)	(0.013)	(0.013)	(0.008)	(0.008)	(0.005)	(0.005)
Panel B: Continuous treatment										
distance	-0.002	0.008	0.007	0.016**	-0.093***	-0.061***	-0.023***	-0.024***	-0.023***	-0.012**
	(0.005)	(0.005)	(0.007)	(0.007)	(0.013)	(0.013)	(0.008)	(0.009)	(0.005)	(0.005)
Observations	337,794	337,794	337,794	337,794	337,794	337,794	337,794	337,794	337,794	337,794
Mean Dep. Variable	0.64	0.64	1.62	1.62	0.04	0.04	5.30	5.30	0.66	0.66
Date, LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
LAxDate FE		✓		✓		✓		✓		✓

Note: This table presents estimates from Equation (1) on asinh-transformed crime variables from police records. The explanatory variables are defined as in Table 2. Property crimes in columns 7 and 8 exclude burglaries. Odd columns include LSOA and date fixed effects, and even columns add LA $\times$ date fixed effects. "LA" refers to the 31 London LAs (excluding Westminster and City of London). The table displays the baseline mean of the number of crimes (in absolute terms). Standard errors are clustered at the census block level. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table A4: Alternative definitions of distance

	Assaults and murders									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
commuting distance	0.099***	0.092***								
	(0.018)	(0.018)								
commuting time			0.085***	0.084***						
			(0.018)	(0.018)						
dummy distance across LAs					0.085***	0.105***				
					(0.015)	(0.015)				
distance across LAs							0.105***	0.099***		
							(0.018)	(0.018)		
ITT distance									0.093***	0.088***
									(0.016)	(0.017)
Observations	336,930	336,930	336,930	336,930	338,472	338,472	338,472	338,472	337,794	337,794
Mean Dep. Variable	2.48	2.48	2.48	2.48	2.48	2.48	2.48	2.48	2.48	2.48
Date, LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
LAxDate FE		✓		✓		✓		✓		✓

Note: Table notes follow from Table 2. As a measure of distance, Columns 1 and 2 use the commuting distance computed using OpenStreetMap data. Columns 3 and 4 employ the commuting time as the regressor, using street-level speed limits from OpenStreetMap data. I compute travel times by car using the OSMnx Python package, which interacts with OpenStreetMap APIs. This allows me to calculate the fastest route by car between each census block and each police station. The average travel speed for vehicles in London is 36 km per hour. Columns 5–8 use the geodesic distance between the centroid of each census block and any closest police station, including stations located in different police divisions. Columns 9 and 10 use baseline distance to blocks that experienced an opening estimating as an ITT regressor. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table A5: The effects of police station closures on crime types excluding bordering blocks

	Assaults and murders		Robberies		Burglaries		Theft		Property crimes (excluding burglaries)		Drugs-related offences	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Binary treatment												
dummy distance	0.086***	0.113***	-0.014***	0.014***	-0.001	0.000	-0.110***	-0.109***	-0.035***	-0.037***	-0.034***	-0.027***
	(0.016)	(0.017)	(0.005)	(0.005)	(0.007)	(0.007)	(0.014)	(0.014)	(0.008)	(0.009)	(0.006)	(0.006)
Panel B: Continuous treatment												
distance	0.082***	0.078***	-0.007	0.005	0.005	0.015**	-0.093***	-0.055***	-0.027***	-0.029***	-0.024***	-0.011**
	(0.017)	(0.017)	(0.005)	(0.005)	(0.007)	(0.007)	(0.013)	(0.014)	(0.009)	(0.009)	(0.005)	(0.006)
Observations	277,818	277,818	277,818	277,818	277,818	277,818	277,818	277,818	277,818	277,818	277,818	277,818
Mean Dep. Variable	2.50	2.50	0.62	0.62	1.58	1.58	0.04	0.04	5.22	5.22	0.67	0.67
Date, LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
LAxDate FE		✓		✓		✓		✓		✓		✓

Note: Table notes follow from Tables 2 and A3. The sample includes treated blocks and "landlocked" blocks, and excludes 833 bordering blocks. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table A6: The effect of police station closures on violent crime by incidence of austerity cuts

	Assaults and murders LAs by incidence of austerity	
	(1) Above median	(2) Below median
Panel A: binary treatment		
dummy distance	0.112***	0.117***
	(0.022)	(0.021)
Panel B: continuous treatment		
distance	0.076***	0.095***
	(0.025)	(0.021)
Observations	165,744	172,050
Mean Dep. Variable	2.485	2.485
LSOA, LAxDate FE	✓	✓

Note: Table notes are the same as for Table 2. The measure of welfare cuts is borrowed from Beatty and Fothergill (2013), and is computed as the financial loss per working-age adult in an LA and year. Column 1 restricts the sample to census blocks in LAs with above-median austerity incidence, while column 2 includes those below the median. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table A7: The effects of police station closures on violent crime conditional on initial exposure to stations

	Assaults and murders			
	(1)	(2)	(3)	(4)
Panel A: Binary treatment				
dummy distance	0.112***	0.107***	0.105***	0.108***
	(0.015)	(0.015)	(0.015)	(0.015)
Panel B: Continuous treatment				
distance	0.087***	0.080***	0.077***	0.079***
	(0.016)	(0.016)	(0.016)	(0.016)
Observations	337,794	337,794	337,794	337,794
Mean Dep. Variable	2.48	2.48	2.48	2.48
LSOA, LAxDate FE	✓	✓	✓	✓
# stations * Linear time trend	Monthly	Monthly	Monthly	Monthly
Cut-off (km)	2	3	4	5

*Note:*Table notes are the same as for Table 2. The initial number of stations is computed as the number of police stations operating in 2008 and located within 2, 3, 4, and 5 km from the centroids of the census blocks. Columns 1, 3, 5, and 7 interact this with a linear annual time trend; columns 2, 4, 6, and 8 interact it with a linear monthly time trend. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table A8: Conley standard errors

	Assaults and murders													
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Panel A: Binary treatment dummy distance	0.090***	0.115***	0.090***	0.115***	0.090***	0.115***	0.090***	0.115***	0.090***	0.115***	0.090***	0.115***	0.090***	0.115***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Panel B: Continuous treatment distance	0.086***	0.087***	0.086***	0.087***	0.086***	0.087***	0.086***	0.087***	0.086***	0.087***	0.086***	0.087***	0.086***	0.087***
	(0.006)	(0.007)	(0.006)	(0.007)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Observations	337,794	337,794	337,794	337,794	337,794	337,794	337,794	337,794	337,794	337,794	337,794	337,794	337,794	337,794
Mean Dep. Variable	2.48	2.48	2.48	2.48	2.48	2.48	2.48	2.48	2.48	2.48	2.48	2.48	2.48	2.48
Date FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
LAxDate FE		✓		✓		✓		✓		✓		✓		✓
Cut-off (km)	0.25	0.25	0.5	0.5	0.75	0.75	1	1	1.5	1.5	2	2	3	3

Note: Table notes are the same as for Table 2. Standard errors are calculated using Conley standard errors. Census block locations are specified in latitude–longitude degrees, and the kernel cut-off is measured in kilometers, ranging from 250 m to 3 km. A conical kernel is employed, which decays linearly in all directions to account for spatial correlations. Serial correlation is allowed across six time periods (months). $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table A9: The effects of police station closures on the share of cleared offenses over total reports

	Cleared crimes / reports						Convictions / reports					
	All crimes		Property crimes		Violent crimes		All crimes		Property crimes		Violent crimes	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Binary treatment dummy distance	-0.005***	-0.005***	-0.003	-0.002***	-0.003***	-0.000	-0.004***	-0.005***	-0.002	-0.002***	-0.003***	-0.001
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Panel B: Continuous treatment distance	-0.005***	-0.004***	-0.006***	-0.002**	-0.002***	-0.000	-0.004***	-0.004***	-0.003	-0.001*	-0.002**	0.000
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Observations	336,445	323,636	280,279	336,445	323,636	280,279	336,445	323,636	280,279	336,445	323,636	280,279
Mean Dep. Variable	0.06	0.04	0.12	0.03	0.03	0.05	0.06	0.04	0.12	0.03	0.03	0.05
Date, LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Date, LAxDate FE							✓	✓	✓	✓	✓	✓

Note: Table notes are the same as for Table 3. The dependent variables are the ratios of cleared offenses (columns 1–6) or convictions (columns 7–12) over total reports, for all offenses, property, and violent crimes, respectively. Convictions refer to incidents where an individual is declared guilty of a criminal offense by the verdict of a court (thus excluding acquittals and discharges). $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table A10: The effects of police station closures on house prices by property characteristics

	House prices											
	N. habitable rooms		Floor height		Share of flats		Share of houses		Share of historical buildings		Share of social rent	
	Above median	Below median	Above median	Below median	Above median	Below median	Above median	Below median	Above median	Below median	Above median	Below median
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Binary treatment												
dummy distance	0.001	-0.028**	-0.025**	-0.011	-0.027***	0.001	0.000	-0.028***	-0.016	-0.026**	-0.026**	-0.009
	(0.010)	(0.011)	(0.010)	(0.013)	(0.010)	(0.010)	(0.009)	(0.010)	(0.011)	(0.012)	(0.011)	(0.012)
Panel B: Continuous treatment												
distance	0.001	-0.010	-0.017**	0.002	-0.010	-0.008	-0.007	-0.010	-0.009	-0.013	-0.010	-0.004
	(0.008)	(0.007)	(0.007)	(0.010)	(0.007)	(0.010)	(0.008)	(0.007)	(0.008)	(0.009)	(0.008)	(0.009)
Observations	23,480	36,753	38,658	21,579	40,484	19,750	20,364	39,890	28,268	31,953	34,631	25,626
Date, LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
LAxDate FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Note: Table notes are the same as for Table 7. The dependent variable is the (log) average house price. Property characteristics are sourced from Energy Performance Certificate (EPC) data and computed as average census block shares at baseline (over the period 2008–2012). Each column is split by whether the local property characteristic is above or below the London median. Characteristics include the number of habitable rooms (columns 1 and 2), floor height (columns 3 and 4), share of flats (columns 5 and 6), share of homes (columns 7 and 8), share of historical buildings (pre-1950) (columns 9 and 10), and share of renters in social housing (columns 11 and 12). $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table A11: Effect of police station closures on house prices by destination of the closed station

	Log house prices (weighted by the number of transactions)				Assaults and murders	Property crimes
	(1)	(2)	(3)	(4)	(5)	(6)
dummy distance	-0.018*	-0.016	-0.018*	-0.017	0.111***	-0.030***
	(0.010)	(0.011)	(0.010)	(0.011)	(0.020)	(0.011)
dummy distance * I[Sale]	-0.007	-0.005	-0.034**	-0.034**	0.042	-0.024
	(0.010)	(0.011)	(0.014)	(0.015)	(0.035)	(0.018)
dummy distance * I[Residential]			0.038***	0.041**	-0.049	0.037*
			(0.013)	(0.016)	(0.035)	(0.019)
Observations	62,893	62,883	62,893	62,883	338,472	338,472
Date, LSOA FE	✓	✓	✓	✓	✓	✓
LAxDate FE		✓		✓	✓	✓

Note: Columns 1–4 show regression results using the quarterly house price dataset. Observations are weighted by the number of sales recorded in the quarter–census block cell. The dependent variable is the average (log) house prices computed in the census block. Columns 5 and 6 use the police records, where the dependent variable is assaults and murders and property crimes transformed in asinh. The explanatory variables are the treatment dummy as defined in Section 3, and its interaction with an indicator for whether the closest police station was sold, or sold and then transformed in a new residential building. "LA" refers to the 31 London Local Authorities, excluding City of London and Westminster. Standard errors are clustered at the census block level. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Appendix B Detailed derivations of the model

B1 Derivations of supply of crime

This section provides detailed derivations of Section 4.D.

Location choice. I model the individual's decision about where to commit a crime, conditional on choosing to operate in the criminal sector. The implicit assumption is that each individual chooses to commit only one crime.

Using the idiosyncratic component of utility ε from the indirect utility specified in Equation (4), the distribution of the utility of a individual committing crime in block j is also Fréchet-distributed with shape parameter θ:

$$F_j(u) = Pr[U \leq u] = Pr\left(\varepsilon \leq \frac{u}{r_j}\pi_j S\right) = e^{-(r_j/\pi_j S)^{\theta} u^{-\theta}} = e^{\phi_j u^{-\theta}}, \tag{10}$$

where $\phi_j = (r_j/\pi_j S)^{\theta}$. The corresponding density function is: $f(u) = \phi_j \theta u^{(-\theta-1)} e^{\phi_j u^{-\theta}}$.

Individuals who choose to commit a crime sort across locations depending on their idiosyncratic preferences and the characteristics of these locations. Using the fact that the maximum of a sequence of Fréchet-distributed random variables is itself Fréchet-distributed, and that ε is independently distributed, the probability that an individual chooses area j where to commit crime out of all areas within the city can be derived as

$$\begin{aligned}
\lambda_j &= Pr[u_j \geq \max(u_i) \forall i \neq j] \\
&= \int_{-\infty}^{\infty} \prod_{i \neq j} F(u_i) f(u_j) du_j \\
&= \int_{-\infty}^{\infty} \prod_{i \neq j} e^{\phi_j u^{-\theta}} \phi_j \theta u^{(-\theta-1)} e^{\phi_j u^{-\theta}} du_j = \\
&= \int_{-\infty}^{\infty} \prod_{i} e^{\phi_j u^{-\theta}} \phi_j \theta u^{(-\theta-1)} \\
&= \int_{-\infty}^{\infty} \exp\{-\sum_i \phi_j u^{-\theta}\} \phi_j \theta u^{(-\theta-1)} du_j.
\end{aligned}$$

Substituting $u_j = s^{-(1/\theta)}$ and $du = -(1/\theta)s^{-[(\theta+1)/\theta]}ds$ and simplifying, it becomes

$$\begin{aligned}
&= -\int_{0}^{\infty} e^{-\sum_i \phi_j s} \phi_j ds == -\phi_j \frac{1}{\sum_i \phi_i} e^{-\sum_i \phi_i s}\Big|_{0}^{-\infty} = \frac{\phi_j}{\sum_i \phi_i} \\
&= \frac{r_j^{\theta} (\pi_j S)^{-\theta}}{\sum_i r_i^{\theta} (\pi_i S)^{-\theta}}.
\end{aligned}$$

As S is constant across j's, this finally gives

$$\lambda_j = \frac{r_j^{\theta} \pi_j^{-\theta}}{\sum_i r_i^{\theta} \pi_i^{-\theta}}. \tag{11}$$

where $\theta > 0$ is the shape parameter of the Fréchet distribution. The probability that an individual commits a crime in location j depends on the local returns from crime and the apprehension risk in that block, while being inversely related to the returns and risks across all other locations.

Given the preference structure, the market-clearing condition for criminals requires that the total number of crimes in location j is defined as $C_j = \lambda_{j|s=C} \cdot C$. This is the expression reported in Equation (6).

Ex-ante expected utility. Population mobility implies that the expected utility is the same for all locations and equal to the reservation level of utility in the wider economy. Criminals are mobile between the city and the larger economy, which delivers a constant utility $\bar{U}$. Given this Fréchet distribution for utility, I can take the expectation over the distribution for idiosyncratic utility and define the (*ex ante*) expected utility from moving to the city as

$$\mathbb{E}[U] = \int_0^{\infty} u f(u) du = \int_0^{\infty} u \theta \phi u^{-\theta-1} e^{-\phi u^{-\theta}} du. \tag{12}$$

Define $y = \phi u^{-\theta}$ and $dy = -\theta \phi u^{-\theta-1} du$ and substitute

$$= \int_0^{\infty} \phi^{\frac{1}{\theta}} y^{-\frac{1}{\theta}} e^{-y} dy = \Gamma\left(\frac{\theta-1}{\theta}\right) \phi^{1/\theta}, \tag{13}$$

where $\gamma = \Gamma[(\theta-1)/\theta]$ and $\Gamma(\cdot)$ is the Gamma function. This is the usual constant that arises after integrating the pdf from the Fréchet distribution. Individuals are mobile between the city and the larger economy, which delivers a constant utility $\bar{U}$.

Spatial equilibrium requires that the expected utility from moving to the city equals the reservation level of utility in the wider economy. Therefore, the *ex-ante* expected utility from moving to the city is

$$\mathbb{E}[U] = \Gamma\left(\frac{\theta-1}{\theta}\right)\phi^{1/\theta} = \gamma\left[\sum_i \left(\frac{r_j}{\pi_j S}\right)^{\theta}\right]^{1/\theta} = \bar{U}. \tag{14}$$

B2 Estimation

The model includes two exogenous variables, $\{P_j, d_j\}$, two endogenous variables, $\{\pi_j, C_j\}$, four parameters, $\alpha, \beta, \theta, \mu$, and two location-specific fundamentals, $\{r_j, A_j\}$. The natural experiment of the police station closures allows me to causally identify the model's structural parameters. In this section, I explain in detail how I estimate the model parameters $\{\theta, \alpha, \beta\}$. I then use the structure of the model to recover the location fundamentals.

First, taking logs, Equation (7) becomes

$$log\pi_j = logA_j - \alpha d_j + \beta P_j. \tag{15}$$

Adding a random error component and the time dimension, Equation (15) becomes a log-linear regression that can be estimated to retrieve the police inputs elasticities:

$$log\pi_{jt} = \gamma_j + \gamma_t - \alpha d_{jt} + \beta P_{jt} + v_{jt}. \tag{16}$$

Under the assumption that the location-specific productivity term is exogenous and time-invariant, A_j is absorbed by the area fixed effects γ_j. Second, transforming Equation (6) in logs, I derive the following expression for the supply of crime:

$$\log C_j = \theta \log r_j - \theta \log \pi_j - \log(\sum_i r_i^{\theta} \pi_i^{-\theta}) + \log(C). \tag{17}$$

Substituting π_j from Equation (15) in Equation (17), adding a random error component and the time dimension, I obtain a log-linear regression to estimate

$$\log C_{jt} = \phi_j + \phi_t + \delta d_{jt} - \nu P_{jt} + \varepsilon_{jt}. \tag{18}$$

Under the assumption that the location-specific productivity and returns to crime term are exogenous and time-invariant, $\{r_j, A_j\}$ are absorbed by the area fixed effects ϕ_j. The third and fourth terms in Equation (17) are constant and thus absorbed by the fixed effects.

Aggregate impacts. To quantify the aggregate effect on total violent crime, I compute each element in Equation (5) as follows. First, the pre-reform crime rate C_0 is computed using police records for 2011, and the baseline London population $\bar{L}$ from the 2011 Census. To measure the post-reform crime rate C_1, I compute several components from the model. First, using Equation (7), I quantify the probability of apprehension, represented by π_0 and π_1. Then, using Equation (14), I derive the expected utility values $\bar{U}_{C_0}$ and $\bar{U}_{C_1}$. While it is difficult to precisely estimate S (the penalty for committing a crime) from the data, this parameter does not affect the aggregate impact calculations, as it is time-invariant and it cancels out. Therefore, no specific value for S is required for the model estimation. Subsequently, I use $\bar{U}_{C_0}$ to calculate U_{NC}, the utility associated with non-criminal activities. This value is obtained by inverting Equation (5) as $U_{NC} = (U_C^{\mu}(\bar{L}/C_0) - U_C^{\mu})^{1/\mu}$. I assume that U_{NC} remains unaffected by the policy, as it primarily depends on factors unrelated to the reform, such as conditions in the legal labor market, in line with the Becker model.

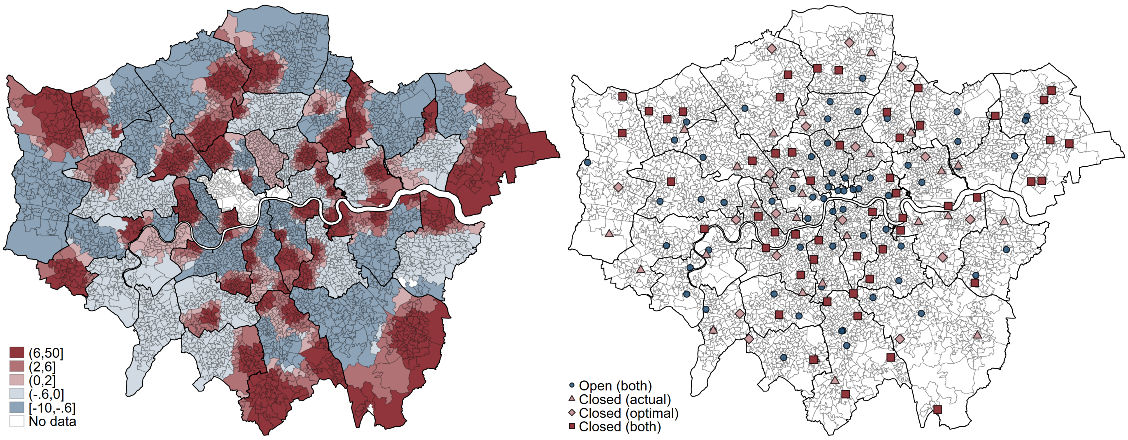
Table B1: Aggregate effect on violent crime under different closure scenarios

Local authorities	Current policy	Optimal policy	Random policy
Average change in violent crime	1.68%	–3.16%	2.25%

Note: The table summarizes the average aggregate change in crime predicted by the model under three scenarios: current (column 1), optimal (column 2), and a random policy scenario (column 3). It uses estimates for α, β, θ computed in Table 5. The current scenario reflects observed crime changes following all actual station closures up to 2016. The optimal scenario models station closures designed to minimize expected utility from crime. Crime changes are computed based on the optimal combination of stations per LA. The random scenario averages the outcomes of 500 simulations where station closures are selected randomly. The problem is solved independently for each LA (LA-specific results are shown in Figure B2). Change in aggregate crime is expressed relative to the baseline scenario, representing the pre-closure configuration in 2012.

Figure B1: Maps of changes in crime and police station closures

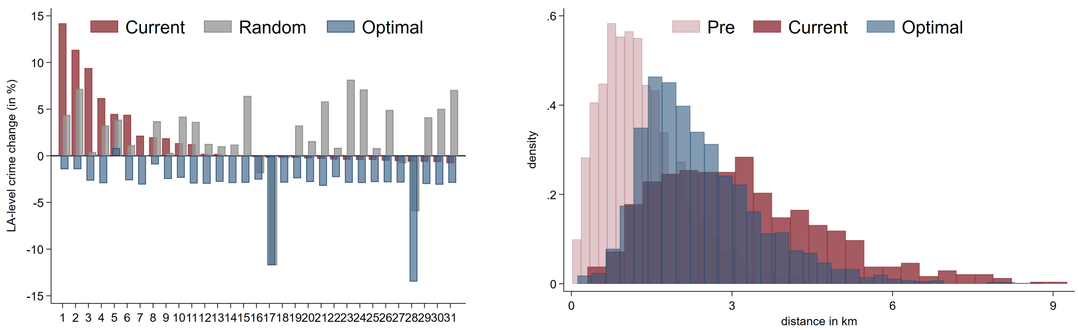
(a) Changes in crime (b) Police station closures



Note: The left panel plots changes in local crime C_j from Equation (6) of the model before and after the closures. The right panel maps the locations of (i) stations that remained open, (ii) those closed exclusively under the current closure policy, (iii) those closed only under the optimal scenario, and (iv) those closed under both scenarios.

Figure B2: Changes in crime and distance under different policy scenarios

(a) Changes across LAs (b) Distribution of distance to police stations



Note: Panel (a) shows average LA changes in violent crime under three scenarios: current policy (red bars), optimal policy (blue bars), and random policy (gray bars). Each bar represents a different LA (31 total on the x-axis). Panel (b) shows the distribution of distances to the nearest police station across census blocks affected by station closures under three scenarios: before any closures (2011), after all closures under the actual policy (2016), and after closures under an optimal closure policy.

Appendix C Crime Survey of England and Wales

Table C1 provides descriptive statistics for the sample of Crime Survey for England and Wales (CSEW) respondents residing in London (excluding the City of London and Westminster) between 2011 and 2016, which is the focus of this analysis. The sample consists of 21,409 respondents who reported living in London, with approximately

9,342 respondents living in treated areas. The restricted geo-referenced version of the CSEW includes LSOA-level data only from 2010–2011 onward. The sample spans 4,678 Lower-layer Super Output Areas (LSOAs), while the subset of victims spans across 3,093 LSOAs. Panel A reports statistics for respondent, while Panel B is for incident-level outcomes. A total of 5,087 individuals reported being victimized, with an average of 1.4 incidents per victim and an average incident-level reporting rate of 37%. The census-block panel data are unbalanced, with an average of two periods per LSOA. Due to the survey's modular structure, not all questions are asked of all respondents. Questions regarding confidence in the police, the Crown Prosecution Service (CPS), and the Criminal Justice System (CJS) are asked of 50% of London respondents (those completing Modules A and B of the CSEW), while questions related to anti-social behavior are asked of 25% of respondents (those completing Module A). Finally, the question on satisfaction with police handling of cases is asked to victims only.

Table C1: Descriptive statistics from CSEW data

	mean	sd	N
Panel A: All respondents			
Individual characteristics			
Male	0.455	0.489	21,409
Age	47.152	17.838	21,162
Student	0.050	0.219	21,406
No qualification	0.182	0.386	21,409
Higher education	0.662	0.485	21,284
In employment	0.83	0.375	19,617
Self-employed	0.169	0.375	19,617
British	0.780	0.414	21,372
White	0.649	0.477	21,336
Asian	0.196	0.397	21,336
Black	0.130	0.337	21,336
Household-income: below 10k£	0.188	0.390	17,227
Household-income: above 50k£	0.196	0.397	17,227
Household size	2.500	1.425	21,409
Number of children per household	0.548	0.954	21,409
Number of adults per household	1.952	0.984	21,409
Victimization			
Victim	0.237	0.425	21,409
Victim of:			
- assaults	0.018	0.122	21,409
- burglaries	0.035	0.184	21,409
- thefts	0.128	0.335	21,409
- property crimes	0.182	0.386	21,409
- violent crimes	0.025	0.155	21,409
ASB: drunk	0.344	0.475	6,700
ASB: vandalism	0.124	0.397	6,700
ASB: rowdy behavior	0.203	0.403	6,700
Local characteristics			
Local deprivation index	4.500	2.508	18,420
Local crime rate (police records)	0.030	0.028	21,409
Violent crime rate (police records)	0.006	0.005	21,409
Property crime rate (police records)	0.012	0.014	21,409
Outcomes			
lack of confidence in police effectiveness	0.328	0.47	10,591
lack of confidence in CPS effectiveness	0.402	0.49	9,799
lack of confidence in CJS effectiveness	0.441	0.497	10,266
Panel B: All incidents			
=1 if incident reported	0.372	0.483	6,592
=1 if incident reported by type of crime:			
- assault	0.029	0.167	6,592
- burglaries	0.081	0.272	6,592
- property crimes	0.282	0.45	6,592
- theft	0.164	0.37	6,592
- violent crime	0.043	0.202	6,592
Satisfaction with police response	0.727	0.446	2,293

Note: Panel A presents descriptive statistics for all respondents (victims and non-victims). Panel B provides descriptive statistics for incident-level outcomes (victims only). The sample is restricted to CSEW respondents residing in London (excluding City of London and Westminster) at the time of the survey interview, covering the period from 2011 and 2016.

Table C2: The effects of police station closures on ASB experiences using the CSEW data

	Experiences of ASB episodes:								
	Street drinking			Vandalism			Rowdy behavior		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Binary treatment dummy distance									
	0.036	0.059	0.083**	0.022	0.041	0.04	0.015	0.011	0.013
	[0.035]	[0.036]	[0.038]	[0.025]	[0.027]	[0.028]	[0.030]	[0.031]	[0.033]
Panel B: Continuous distance									
distance	0.025	0.032	0.046*	0.026	0.039*	0.041**	0.036	0.025	0.026
	[0.025]	[0.027]	[0.027]	[0.020]	[0.020]	[0.021]	[0.022]	[0.023]	[0.024]
Observations	5,052	5,052	5,052	5,052	5,052	5,052	5,052	5,052	5,052
Mean dep. Variable	0.347	0.347	0.347	0.14	0.14	0.14	0.223	0.223	0.223
Date, LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
LA-specific trends		LA lin. trend	LAxYear FE		LA lin. trend	LAxYear FE		LA lin. trend	LAxYear FE

Note: The table presents estimation results on experiences of ASB from Equation (1) using CSEW data. The sample includes all respondents residing in London (excluding Westminster and the City of London). The outcome variables are indicators for types of ASB experienced or witnessed by respondents. The explanatory variables are defined as in Table 2. Column 1 includes LSOA and quarterly date FE, column 2 adds LA-specific linear time trends, while column 3 adds LA $\times$ date fixed effects. Standard errors are clustered at the census block level. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

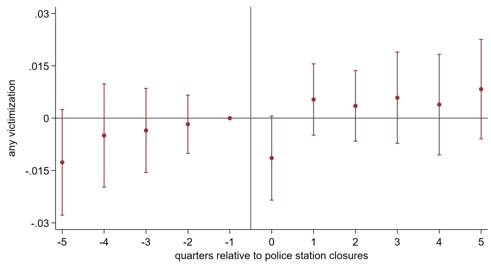
Table C3: The effects of police station closures on confidence in police

	Satisfaction with police response		Lack of confidence: in police effectiveness		in CPS effectiveness		in CJS effectiveness	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Binary treatment								
dummy distance	-0.028	-0.022	0.059**	0.066***	0.014	0.021	0.014	0.038
	[0.040]	[0.043]	[0.024]	[0.024]	[0.025]	[0.026]	[0.025]	[0.026]
Panel B: Continuous distance								
distance	-0.067**	-0.069**	0.035**	0.039**	0.000	0.006	0.003	0.009
	[0.029]	[0.031]	[0.018]	[0.019]	[0.019]	[0.020]	[0.009]	[0.010]
Observations	5,161	5,161	9,647	9,647	8,757	8,757	9,262	9,262
Mean dep. Variable	0.251	0.251	0.365	0.365	0.462	0.462	0.504	0.504
Date FE	✓	✓	✓	✓	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓
LA-specific linear trends		✓		✓		✓		✓

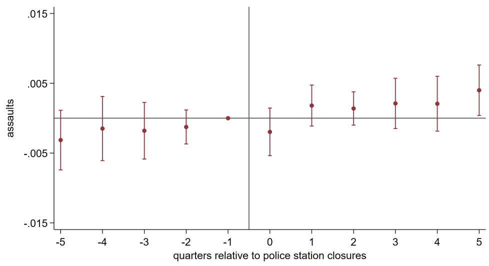
Note: The table presents estimates from Equation (1) using CSEW data. The sample includes respondents residing in London (excluding Westminster and the City of London). Column 1 is further restricted to victims only. The outcomes are built from the following survey questions. (i) "Overall, were you satisfied or dissatisfied with the way the police handled the matter?" (columns 1 and 2). Outcome is coded as 1 if respondents answer "Fairly satisfied" or "Very satisfied". For columns 3–8, outcomes are built from the following questions. (ii) "How confident are you that the police are effective at catching criminals?" (iii) "How confident are you that the Crown Prosecution Service is effective at prosecuting people accused of committing a crime?" (iv) "How confident are you that the Criminal Justice System as a whole is effective?" Outcomes are coded as 1 if respondents answer "Not very confident" or "Not confident at all". Explanatory variables are defined as in Table 6. "LA" refers to the 31 London LAs. The table displays the baseline mean of the outcome. Standard errors are clustered at the census block level. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Figure C1: Effects of police station closures on the probability of being a victim of crime, using CSEW data

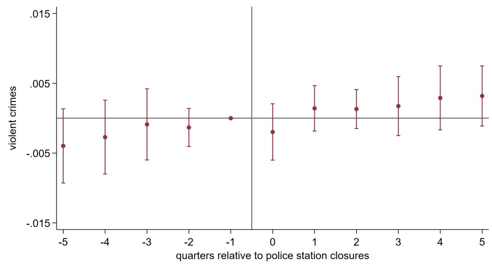
(a) Any crime victim



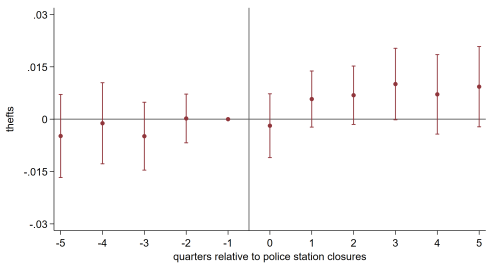
(b) Assaults



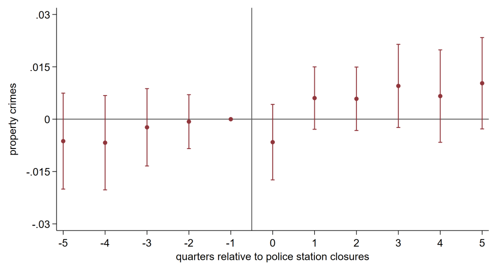
(c) Violent crimes



(d) Thefts



(e) Property crimes



Note: The figure displays the probabilities of being a victim of crime by type of crime, estimated running the event-study Equation (2) on CSEW data. Details of the regression specification are provided in Section 3. Standard errors are clustered at the census block level. Error bars represent 95% confidence intervals. I omit the dummy for the period before the closures and exclude the distant relative periods following Sun and Abraham (2021).

Appendix D Cost-effectiveness of police station closures

D1 Capitalization approach

I quantify the total loss of value of house prices using a capitalization approach. The average cost to a treated block from the closures equal to £8,557. This numbers comes from the house price estimates reported in Table 7, column 2. I quantify the benefits in terms of 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 £12,000 saved per treated block–quarter.

Table D1: Cost–benefit analysis using capitalization 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	8,557	11,133
Total cost for private owners	418,745,890	255,700,447
Panel B: Police station closure benefits		
Total savings	600 million	
Total savings per treated block	294,262	
Savings per treated block per treated block-quarter	12,261	
Cost/Benefit	0.70	0.91

Note: This table shows calculations of cost–benefit analysis using the capitalization approach. Column 1 includes the entire sample, while column 2 is restricted to blocks with baseline (2008) crime rates above the median. Panel A quantifies the costs using the house price estimate from column 2 of Table 7, as well as the estimate for a subsample of high-crime areas. Average house prices are calculated for the pre-period (prior to January 2013). Panel B uses the total number of treated blocks between 2012 and 2016 , the period during which the MPS implemented its planned estate strategy (MOPAC, 2017). 2,039 blocks were treated in total, and 957 high-crime blocks.

D2 Costs generated by the police station closures

I compute the crime-related costs associated with the police station closures as the sum of the (i) deterrence, (ii) incapacitation costs, and (iii) social cost of crime. First, I compute the costs associated with the additional crimes generated when clearance 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 (Table D2). First, I quantify the deterrence costs generated by the lower clearance rate resulting from the closures in Table D3. Specifically, I use the 0.6 percentage point coefficient on clearance estimated in Table 3. I assume a conservative estimate of elasticity of crime on clearance rate equal to -0.1. This comes from Levitt (1998b), is the same as that used by Blanes i Vidal and Kirchmaier (2018), and is at the lowest bound of estimates.I then use the estimates of the unit cost of crime from Table D2. Overall, I estimate a cost of £13 million resulting from a decrease in the clearance rate equal to 0.006. Next, I compute the incapacitation costs that stem from the impacts on crime types reported in Section 4.[33] Table D4 calculates the additional years of incarceration following the increase in crime. I adopt a conservative approach and take into account the reductions in reported property crimes, which reduces the cost burden of the police. Having MPS data on investigation outcomes, I can account for the fact that only a fraction of criminals are convicted, and only a small fraction of convicted offenders are incarcerated. Because the UK criminal justice system is relatively lenient, 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 the opposite is true for criminal damage crimes. I calculate that the changes in crime would lead to an extra 17,000 years of incarceration and to a total cost of £560 million. The social costs of crime combine estimates of the additional number of crimes committed due to the police station closures (Table D4) and the additional number of non-deterred crimes due to lower clearance (Table D5). I compute a total social cost of the closures of approximately £180 million.

[33] I include the fiscal costs of higher incarceration, but I do not consider 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 behavior. 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 with a different individual.

Table D2: Costs 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: 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 offenses in column 2 is obtained from table 4 of the same report. Column 3 is the weighted average cost of crime, computed using the frequencies of each subcomponent as weights. The reports use 2015/2016 prices.

D3 Savings generated by the police station closures

Detecting and clearing fewer crimes results in greater savings due to a lower CJS expenditure. Table D5 calculates the CJS savings resulting from decreasing the clearance rate by 0.6 percentage points. The calculations include costs associated with prosecution, courts' functioning, and legal aid as per the Home Office Report (Heeks et al., 2018). I include in the calculations the savings from lower probation and prison expenditures, equal to £1.8 million (column 7 of Table D5). Overall, I estimate a total saving resulting from the station closures for the CJS and the police of £10.1 million and £21 million, respectively. 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.

Table D3: Deterrence costs due to decreased clearance

Crime category	(1) Probability of crime	(2) Number offences	(3) Crimes non deterred	(4) Unit cost per crime (£)	(5) Cost of crime non-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: The proportion of crimes and the number of offenses by crime category are computed restricting the sample to the pre-period (i.e. before June 2013; columns 1 and 2). I omit other crime categories because there is no corresponding crime cost computed in the Home Office report (Heeks et al., 2018). Columns 3 and 5 are calculated as indicated in the bottom row. We assume an elasticity of crime on the clearance rate of -0.1 as in Blanes i Vidal and Kirchmaier (2018). To compute the additional number of crimes non-deterred, I multiply 0.1 (the assumed crime-clearance elasticity) by 0.006 (the β increase in the clearance rate) divided by 0.19 (the average clearance rate) and by the total number of incidents in the pre-period.

Table D4: Incarceration costs of crime due to increased crime

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) Uk sentence lenght	(8) Extra years of incarceration	(9) Total cost from extra incarceration
Violent crimes									
- *Violence against the person*	183,170	16,485	0.097	1,606	0.293	471	37	17,414	605,410,076
- *Robbery*	45,372	-272	0.083	-22	0.426	-10	36	-345	-11,977,370
Property crimes									
- *Domestic burglary*	126,870	254	0.070	18	0.314	6	19	106	3,681,426
- *Theft*	4,926	-537	0.017	-9	0.153	-1	4	-6	-194,856
- *Criminal damage and arson*	81,802	-2,372	0.059	-140	0.319	-45	22	-982	-34,139,463
Total	442,140	13,558	0.027	1,452	0.027	421	15	17,069	562,779,812
Calculations		(1) × β		(2) × (3)		(4) × (5)		(6) × (7)	(8) × £34,766

Note: This table shows the calculations of the incarceration costs resulting from changes in recorded crime. Column 2 uses estimates by crime type from Tables 2 and A3. The probabilities of conviction and incarceration (conditional on conviction) are computed restricting the sample to the pre-period (i.e. before June 2013). The average custodial sentence length comes from the Criminal Justice Statistics Quarterly Update (Ministry of Justice, December 2012). 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 D5: Criminal justice savings from decreasing clearance rate by β percentage points

Crime category	(1) Crimes non deterred	(2) Multiplier	(3) Prison & probation costs	(4) CJS costs	(5) Tot. prison & prob. saved costs	(6) Total CJS saved costs	(7) Police costs	(8) Total police saved costs
Violent crimes	722	7.08	100	949	511,168	4,852,614	2,976	15,213,482
- *Violence against the person*	578	7.25	314	861	1,318,253	3,610,143	3,094	12,977,110
- *Robbery*	143	4.30	1,260	2,420	776,291	1,490,972	1,010	622,265
Property crimes	1,953	3.11	213	863	1,294,517	5,246,596	993	6,032,824
- *Domestic burglary*	401	3.60	390	880	562,502	1,269,235	530	764,425
- *Theft*	16	3.86	487	3,238	29,289	194,693	5,244	315,248
- *Criminal damage and arson*	258	1.98	97	341	49,349	174,445	170	87,084
Total	4,554				1,805,686	10,099,210		21,246,306
Calculations	From Table D3				(1) × (2) × (3)	(1) × (2) × (4)		(1) × (2) × (7)

Note: Column 1 corresponds to the number of crimes non-deterred computed in Table D3. The multiplier of each crime category (column 2) is obtained from the Home Office report (table 4 in Heeks et al., 2018). Columns 3 and 4 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 defense, youth justice board. Police costs are estimates of the opportunity-cost of police time and resources taken up by investigating crime rather than engaging in other activities (e.g. responding to non-crime activities).

D4 Marginal value of public funds

I compute the marginal value of public funds (hereafter MVPF; Finkelstein and Hendren, 2020; Hendren and Sprung-Keyser, 2020) as the ratio of society's willingness to pay for a policy to the net cost of the policy to the government. Table D7 summarizes the calculations of the MVPF of closing police stations. I compute a MPVF ranging from 2.6 to 7.1. I detail all calculations below. This approach likely yields conservative estimates as it only quantifies direct costs associated with increased crime and decreased deterrence. It does not incorporate indirect costs, such as the impacts on community safety and well-being, loss of public trust in law enforcement, increased strain on other law enforcement resources, potential long-term societal consequences due to a rise in criminal activity.[34] One advantage of calculating this ratio is that it can be compared to the MVPF of other policy changes. My estimate of the MVPF closely aligns with the

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

MVPFs for policies targeting adults, such as food stamps, housing voucher, and cash welfare programs for low-income households (Hendren and Sprung-Keyser, 2020).

Table D6: Total social cost of crime

	(1)	(2)	(3)	(4)	(5)
Crime category	Average cost of crime	Non deterred crimes	Extra crimes committed	Total cost of non deterred crimes	Total cost of extra crimes committed
Violent crimes					
- *Violence against the person*	10,761	578	16,485	6,224,555	177,396,556
- *Robbery*	351	143	-272	50,311	-95,510
Property crimes					
- *Domestic burglary*	5,930	401	254	2,375,809	1,506,220
- *Theft*	1,664	16	-537	25,889	-893,715
- *Criminal damage*	1,505	258	-2,372	388,852	-3,570,568
All	7,106	1,396	13,558	9,065,416	174,342,983
Calculations	From Table D2	From Table D3	From Table D4	(1) x (2)	(1) x (3)

Note: This table computes the total social cost of crime. Column 1 is derived from Table D2, column 2 from D3, and column 3 from Table D4.

Willingness to pay. To calculate the numerator of the MVPF, I compute the aggregate social willingness to pay. Initially, I present a conservative estimate of the total willingness to pay, specifically focusing on the willingness to pay for additional crimes (column 1 of Table D7). This calculation includes two cost components: the deterrence and the incapacitation costs. First, a lower clearance rate encourages potential criminals to offend because of lower deterrence, therefore resulting in more crimes. Furthermore, *conditional on clearance*, more crime induces higher incarceration. I also account for reduced incapacitation costs stemming from a decrease in reporting for certain crime types, as outlined in Section 4. Table D6 presents the total social cost of crime. I use the estimates of the total economic and social costs of crime from the Home Office Report (Heeks et al., 2018), which includes 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 of crime is computed in Table D2. I combine it with the estimates of the additional number of crimes committed (Table D4) and the additional number of non-deterred crimes due to lower clearance

(Table D5).

Column 2 of Table D7) adds the willingness to pay for worsened labor market prospects by the individuals whose likelihood of incarceration increases following the increase in violent crimes.[35] I compute the total loss in wages they experience from this policy change. I consider youth aged 19–25 as the population at risk of incarceration. To quantify their foregone income, I use the employment rate from the Annual Population Survey in 2012, the year before the closures, of individuals aged 16–24 (40.2%), and the median annual income of employed individuals aged 20–29 in 2012 (£16,550) from HM Revenue & Customs. I calculate the total foregone income of affected individuals during incarceration by multiplying the number of individuals who were incarcerated because of the higher crime rate from Table D4 × the employment rate × the annual median income × the average sentence served from Table D4. The total foregone income of affected individuals during incarceration is equal to £19 million. Column 3 adds to the baseline calculation the total loss in house prices following the lower sales. It uses estimates of the total cost for private homeowners from Table D1. Introducing this cost increases the aggregate willingness to pay to £450 million.

Net cost of government. The denominator of the MVPF captures the net savings to the government of the policy. These include the direct savings from the closures, as well as positive and negative fiscal externalities. The mechanical costs are the public savings from the police station closures equal to £600 million (MOPAC, 2017). I add the fiscal savings for the CJS computed in Table D5 (columns 7, 8, and 10). In column 2, I add as fiscal externality the foregone income tax revenues driven by lower employment due to higher incarceration. To quantify this, I multiply the foregone income computed for the numerator by the average median tax rate for individuals aged 20–29 in the baseline year 2012 (10.55%) from the Survey of Personal Incomes (SPI) by HM Revenue & Customs. Column 3 considers the lost tax revenues resulting from a decrease in house sales, specifically related to the stamp duty land tax, which is a sale tax imposed on

[35] Applying the envelope theorem would entail excluding this component from the willingness to pay. In the context of a Becker (1968) model of crime, the envelope theorem would recommend considering solely the direct impact of the closures on the policy parameter of interest, which is the likelihood of apprehension, rather than the indirect effects on incarceration as those would instead work through the outside option to commit crimes or the other policy parameter, which is the size of the punishment.

those property transactions. I use a conservative stamp duty rate of 3%, which applies to property between £250,000 and £500,000. I do not account for fiscal externalities related to council tax (analogous to a property tax) for rents.

Table D7: Calculation of the marginal value of public funds

	Value (£) (1)	Value (£) (2)	Value (£) (3)
Willingness to pay			
Society's willingness to pay for additional crimes	-183,408,399	-183,408,399	-183,408,399
Willingness to pay for worse labor market prospects by additional incarcerated individuals		-19,027,866	
Total loss in house prices for sales			-418,745,890
Aggregate WP	-183,408,399	-202,436,265	-602,154,289
Net cost to the government			
Mechanical savings from the closures	-600,000,000	-600,000,000	-600,000,000
Fiscal externalities:			
- Fiscal costs of incarceration	562,779,812	562,779,812	562,779,812
- Foregone income tax revenues		2,007,354	
- Lower revenues from lower stamp duty land tax			12,562,377
- Fiscal savings of Police	-21,246,306	-21,246,306	-21,246,306
- Fiscal savings of Prison and Probation	-1,805,686	-1,805,686	-1,805,686
- Fiscal savings of CJS	-10,099,210	-10,099,210	-10,099,210
Net cost	-70,371,390	-68,364,036	-57,809,013
Aggregate WP / Net Cost	2.61	2.96	10.42

Note: This table shows the calculations of the marginal value of public funds (MVFP). For a full derivation of these costs, please refer to Tables D1–D5.