

Elisa Facchetti

25/11

Working paper

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

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

Elisa Facchetti[†]

This Version: December 8, 2025

First Version: February 2021

Abstract

Leveraging unprecedented austerity cuts, this paper studies how a large-scale reorganization of police resources induced by the closure of over 70% of police stations – while keeping force-wide police strength constant – affects crime, police effectiveness, civilian cooperation, and residents' welfare. Combining geo-referenced police 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. 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, Magdalena Domínguez, Francesco Fasani, François Gerard, Randi Hjalmarsson, Tom Kirchmaier, Jens Ludwig, Rocco Macchiavello, Stephen Machin, Marco Manacorda, Giovanna Marcolongo, Giovanni Mastrobuoni, Brendon McConnell, 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 safety and social welfare, while keeping budgets under control.

To address this gap, I leverage a natural experiment generated by unprecedented austerity cuts. To comply with savings 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. These closures generated a large-scale territorial reorganization of police resources. As an immediate *direct* effect, the average distance to the nearest station doubled and aggregate response times slowed, reducing police deterrence. At the same time, the consolidation of resources into fewer stations generated *indirect* local gains, with police presence at the remaining stations increasing by 80%, as officers were redeployed from fewer bases. 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 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

¹Austria, Belgium, Denmark, France, Germany, Ireland, and the UK reduced funding to police departments (Fyfe et al., 2013). For the US, see also 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>.

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.

Estimating the causal effect of police station closures poses non-trivial challenges. First, 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. As a result, every census block experienced either a change in proximity to a station, a change in officer numbers, or both. To overcome these challenges, I adopt a difference-in-differences strategy that compares changes in local outcomes in census blocks that experience an increase in distance to the nearest station to blocks that do not, before and after the closures, while conditioning on contemporaneous officer strength at the nearest station. This approach identifies the effect of becoming farther from a police station, holding local police levels constant. I show that these two groups follow similar trends in outcomes prior to the closures. To ensure that the estimates are not driven by simultaneous cuts in local funding for other public services, I further control for time-varying neighborhood attributes. I also show that the results are robust to the exclusion of blocks near surviving stations. To capture the indirect effects of the reorganization around surviving stations, I compare outcomes in blocks close to these stations to those farther away, and report direct and indirect impacts systematically side by side across outcomes.

I present three sets of results. First, in blocks whose nearest station closes, violent crimes, measured as assaults and murders, increase by 9.4% in the most saturated specification. This increase is sudden and persistent, showing that greater distance to the police weakens deterrence. At the same time, police effectiveness declines: the probability that an incident is cleared falls by 2-3% relative to the baseline clearance rate. The

number of cleared incidents decreases 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 display a steep distance gradient around closed stations: effects are largest in blocks nearest to closed stations and gradually decay with distance. This pattern indicates that the impact is driven not only by reduced police visibility but also by longer response times. Conversely, the concentration of front-line officers at the remaining stations reduces violence only in their immediate vicinity. Despite these localized gains, I estimate a significant net increase in violent offenses when excluding blocks with surviving stations.

To integrate the direct and indirect effects in a unified framework and quantify their aggregate and distributional impacts, I develop a quantitative spatial model of crime and policing. Building on recent advances in economic geography ([Redding and Rossi-Hansberg, 2017](#)), the model incorporates individuals' endogenous decisions about 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 nearest police station. Leveraging variation induced by the closures, I identify the model's structural parameters, with elasticities aligning closely with the reduced-form evidence. The model predicts a 2% overall increase in violent crime, but this aggregate effect masks substantial spatial heterogeneity, with some areas experiencing sharp increases and others minimal changes. I then consider a counterfactual scenario in which closures are chosen to minimize local expected utility from crime. Under this optimal allocation, the police could have fully offset the increase in violent crime and achieved net gains by prioritizing closures that minimized changes in police proximity.

Second, police station closures discourage citizens from cooperating with law enforcement. Employing victimization survey data, I estimate a 2% 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), survey data indicate an increase in victimization. Beyond reporting, as distance from a police station increases, victims become

less satisfied with how investigations are handled and less confident in police effectiveness, eroding overall trust in law enforcement.

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, as well as in neighborhoods where police station buildings were left abandoned. Adopting a capitalization approach, I compute that for every £5 saved by the public authorities, £1.5 to £3 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 up to £4 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,

²This 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. Surprisingly, 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 \(1998\)](#), 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 significantly 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 in the UK on the Brexit vote (Fetzer, 2019), and on the spatial concentration of crime (Giulietti and McConnell, 2020), showing that austerity disproportionately hit already deprived areas. I complement previous research by estimating the aggregate cost of police station closures, quantifying the shadow price of public savings and informing the design of optimal policies for reallocating scarce resources in 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. It 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 typically divided into early, late, and night rotations, each lasting approximately eight hours. At

³During 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.

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 their shifts, officers routinely return to police stations. Each station has a canteen for meal breaks, and serves as operational base for briefings, paperwork, coordination with supervisors and specialist units, custody, victim and witness handling, as virtually all documentation and evidence-processing had to be completed at the station.⁴

For example, whenever an arrest is made, officers must return to the station with the detained individual to complete the required documentation and process the incident. This procedure, which includes fingerprinting, criminal record checks, and the necessary paperwork, takes about 3.5 hours on average, and longer when solicitors, interpreters, or CCTV reviews are involved. 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 Figure 1 shows the geographical distribution of police

⁴Until the introduction of tables in 2017, officers had to return to the station to complete paperwork.

⁵Following the cuts, more than 600 out of 900 police stations across police forces in England shut down (Pratt, 2019). Police forces nationwide faced similar reductions (see Appendix B1). Between 2012 and 2016, the MPS made £600 million in savings (MOPAC, 2013).

⁶A police station is defined as an operational building with a public front counter for face-to-face contact. Before the closures, all London stations had such counters, so closing a front counter effectively meant closing the entire building. Figure A1 shows examples of closed stations in pictures.

⁷According to the MPS, as only few people reported crimes by walking into the station, the reduction in stations would have minimally affected reporting. Based on a Freedom of Information Act (FOIA) request filed to the MPS, face-to-face crime reporting declined from 8% in 2011 to 6% by 2016, while phone reporting remained steady at around 90%. Conversely, local communities were concerned that closures might worsen police response times, reduce police visibility, and harm trust in public authorities.

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 features, the station closures generated a major reorganization of police resources with two distinct components affecting police deployment. The first is a direct impact, arising from reduced proximity to stations. The second is an indirect impact, due to the increased concentration of officers at the surviving stations.⁹

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

⁸Civilian staff manage back-office functions essential to organizational operations (e.g., finance, IT, HR, HMIC, 2011b). Reductions in back-office staff could plausibly create bottlenecks, diminishing administrative capacity. Because these functions are common to the entire division, and thus are not station-specific, any capacity loss would likely affect policing uniformly within the division. The empirical analysis accounts for this by including police division-by-time fixed effects.

⁹In addition, the MPS did not change their use of other inputs (e.g., capital or technology) nor their patrolling strategy, which aimed to minimize response times throughout London. 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., forthcoming), affects police performance.

MPS response time data. Following the closures, the median response time increased by 45%, from 13 minutes to 19 minutes.¹⁰ 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 80% after the closures, from 100 to more than 180 (Figure 3(b)). This concentration led to localized improvements at the remaining facilities, such as increased police visibility near active stations due to higher police traffic.

Because of this large-scale territorial reorganization, no part of London remained entirely unaffected: each block experienced either a *direct* change in proximity to a station, or an *indirect* change in local officer numbers, or both. I therefore proceed in three steps. First, I isolate the proximity effect by conditioning on contemporaneous changes in officer strength at the nearest station, identifying the impact of becoming farther from a station while holding local police levels constant (Section 3.A). Second, I estimate the localized gains from the increased concentration of officers in areas near surviving stations (Section 3.B). Third, I integrate these two channels in a spatial model that quantifies how the reorganization induced by the police station closures shifted police effectiveness across space (Section 5). Although fully untreated areas are unavailable, the framework exploits within-block variation in treatment intensity to credibly quantify the aggregate and distributional effects of the reorganization.

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.¹¹ 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

¹⁰Appendix Figure A2(a) shows that, using police division-level data, average response times and average distance to the nearest station are positively correlated within LAs (correlation ≈ 0.43).

¹¹These 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 about 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.

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 2011–2016.¹² 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.¹³ 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. To account for the prevalence of zeros in the crime and clearance outcomes, I follow Chen and Roth (2024) and replace zeros with a constant equal to the estimated effect on the extensive margin ($x = 0.025$) before applying the log transformation, so that the transformation assigns the same value to a 0–1 change as implied by the estimated extensive-margin effect.¹⁴

¹²I exclude periods after December 2016, as the MPS undertook a territorial division restructuring.

¹³The 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.

¹⁴The working paper version of this article estimated count models using the inverse hyperbolic sine (*asinh*) transformation (Bellemare and Wichman, 2020). Because subsequent work has highlighted the sensitivity of the *asinh* transformation, this version adopts the explicit calibration approach of the log transformation proposed by Chen and Roth (2024), that makes the value assigned to the extensive margin transparent. Appendix C describes this procedure in detail and shows that the results are robust to alternative transformations.

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.¹⁵ I use as a measure of police performance the police ability to solve (i.e. clear) an incident, conditional on an investigation taking place.¹⁶ At baseline, 16% of incidents are cleared at the end of an investigation. Clearance rates greatly vary 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.

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 E 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 E1 presents the descriptive statistics on respondents and incidents. The average incident-level reporting rate is 37% and it greatly varies across

¹⁵ASB 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.

¹⁶From 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.

crime types, ranging from 30% for thefts to 70% for serious assaults and burglaries.

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).

Sample selection and descriptive evidence. The final sample includes the universe of the census blocks in Greater London, excluding blocks located in 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. Table 1 shows the summary statistics for all the variables used in the analysis.

Station closures and their initial locations are non-random by nature. Panel A of Table A1 shows that stations were originally located in blocks with persistently higher crime levels and higher house values than those without stations. Panel B shows that the MPS subsequently closed stations in areas with relatively lower crime levels and higher house values compared with areas where stations remained open. Thus, blocks experiencing closures had more favorable pre-closure conditions than control blocks. Although station presence and closures were not random, the identification strategy does not require similarity in levels. It only assumes that, absent the closures, treated and control blocks would have followed parallel 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

This section outlines two empirical designs used to isolate the two channels generated by the reorganization induced by the police station closures. The first design exploits within-block changes in distance to the nearest station. Because distance and local officer numbers changed simultaneously, the reduced-form estimates capture both channels; conditioning on contemporaneous changes in officer strength isolates the distance effect. The second design estimates the localized effects in areas near surviving stations driven by increased officer concentration. Section 5 develops a spatial model that brings the two channels together and quantifies the policy’s aggregate impacts.

3.A Difference-in-differences

My identification strategy exploits the time and spatial variation in police station closures in London, which generate 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 the closures. Out of 4,701 census blocks, 2,039 experienced a closure of their nearest station during the sample period. Because some blocks were exposed to multiple closures, for example, after the closest station shut down, the next-closest station also closed (183 blocks experienced two closures and 30 experienced three), I adopt an Intention-to-Treat (ITT) approach focusing on first closures only. Control units are blocks whose nearest station never closed.

To identify the direct effects compares blocks with unchanged distance to those with increased distance from the nearest station, I estimate the following equation:

$$y_{it} = \beta_1 \text{Closed}_{it} + \beta_2 P_{it} + \phi_i + \phi_t + \varepsilon_{it} \quad (1)$$

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 block characteristics, and ϕ_t are calendar date dummies. Standard errors are clustered at the block level, allowing for serial correlation over time (Bertrand et al., 2004).

A challenge in interpreting β_1 as the effect of reduced station proximity is that surviving stations experienced an increase in officer concentration, which may partially offset the direct distance effect. To address this, I control for P_{it} , defined as the number of officers assigned to the nearest station.¹⁷ When P_{it} is omitted, it captures the net reduced-form response to the reorganization, combining changes in proximity with contemporaneous changes in local officer presence. When P_{it} is included, β_1 isolates the distance effect *conditional* on officer strength in the nearest station. To further isolate the impact of reduced station proximity net of police strength reallocation, I also employ alternative definitions of the control group that exclude areas near operating stations, thereby restricting to blocks unaffected by reallocation.

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, β_1 recovers the causal effect of becoming farther from a station. Finally, I replace the treatment indicator with (log) distance to estimate elasticities with respect to proximity. Identification then comes from within-block variation in treatment *intensity* induced by closures, consistent with the spatial model developed in Section 5.

A final concern is related to the presence of contemporaneous policies that may be correlated with the staggered closings of the stations. Suppose, for instance, that dur-

¹⁷As I do not directly observe the number of officers allocated to each station, I compute it as total officers in the division divided by the number of operating stations. This may introduce measurement error in P_{it} . Under classical measurement error, elasticities with respect to police strength would be attenuated. In the presence of non-classical measurement error - for instance, if officers from closed stations were disproportionately reassigned to areas most affected by increased distance - the resulting bias would partially offset the true proximity effect. In both cases, the estimated distance elasticities are lower bounds on the true effects.

ing 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 services provided by the LAs (e.g. welfare, housing, education). To address this, I augment to the specification s with an interaction term between LA-specific fixed effects and time dummies to non-parametrically control for any (observed and unobserved) time-varying local factors. Having point estimates virtually unchanged suggests that the impacts are not driven by the presence of contemporaneous confounding factors at the LA level.¹⁸

Dynamic specification. To test the validity of the common trend assumption and to estimate the dynamic impacts of the closures, I estimate a generalized difference-in-differences specification. Recent econometric literature has raised concerns regarding two-way fixed effects (TWFE) estimators when timing of treatment varies and treatment effects are heterogeneous.¹⁹ In my setting, most 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 potential pitfalls of TWFE estimators, I adopt a stacked-by-event design. The main advantage of a stacked design relative to a pure event study design is that it uses a never-treated group, which allows the removal of event-time trends that do not appear in calendar time. In my context, this is 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

¹⁸Because 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. However, there may be unobserved changes in officer behavior (e.g. shifts in motivation or informal preferences for particular patrol locations). While I cannot empirically test this, leveraging variation in the continuous distance mitigates this concern, as such unobservables would have to correlate with changes in the treatment intensity.

¹⁹The TWFE estimate is a weighted average of all the possible 2×2 comparisons, including comparisons among already-treated units and not-yet-treated units, where the already-treated units serve as controls (see, e.g., [De Chaisemartin and d’Haultfoeuille, 2020](#); [Goodman-Bacon, 2021](#); [Callaway and Sant’Anna, 2021](#); [Borusyak et al., 2024](#)). This can induce bias in the presence of heterogeneous treatment effects across blocks experiencing closures at different points in time.

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.²⁰ In each of these datasets, all blocks 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:

$$y_{it} = \sum_{k=-B}^T \delta_k \text{Closure}_i \times D_t^k + \sum_{k=-B}^T \beta_k D_t^k + \beta_2 P_{it} + \phi_i + \phi_t + \varepsilon_{it} \quad (2)$$

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. Consistent with the main specification, I condition on P_{it} , so the estimates capture changes due to proximity holding officer strength per station fixed, i.e., net of consolidation-driven reallocation. 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. Following the stacked difference-in-differences literature (e.g., Cengiz et al., 2019), all fixed effects in Equation 2 are fully interacted with stacked dataset-specific indicators. This specification is robust to heterogeneous treatment effects, under which traditional event studies perform poorly.

The coefficients δ_k s identify treatment effects k periods from the closure, with $k = -1, \dots, -B$ 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. Esti-

²⁰To estimate the event study specification, I collapse the dataset at the quarterly level.

mates of pre-treatment coefficients are close to zero and not statistically 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 is consistent with the view that station closures were not decided based on local crime trends.

3.B Indirect effects

To isolate the indirect impacts of the closures generated by the greater concentration of front-line officers at surviving stations, I restrict the sample to census blocks whose *baseline* nearest station never closes (2,514 blocks). 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:

$$y_{it} = \gamma Near_i \times Post_{lt} + Post_{lt} + \phi_i + \phi_t + \varepsilon_{it} \quad (3)$$

where y_{it} is the outcome of interest 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 $l(i)$ 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. I report specifications with and without controlling for police strength per station, P_{it} . When including P_{it} , I compare blocks that are equally exposed to the same police strength increase but differ only in their proximity to the surviving station. Because officer

relocation is highly localized, with officers repeatedly operating from and returning to the same station, controlling for P_{it} has little effect on the estimated coefficients.

To further exploit the variation in the intensity of the treatment, I employ as main regressor the continuous distance, expressed as the inverse of distance, 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 γ identifies the indirect effect of the station closures.

4 Effects of police station closures on violent crime and police effectiveness

In this section, I present the reduced-form estimates of the direct and indirect effects of the station closures on police deterrence and police performance. I use police-recorded violent offenses as measures of actual crime, as they are least prone to under-reporting, and clearances as a measure of police effectiveness. For each outcome, I report the direct and indirect effects side by side in the same table.

4.A Impact on violent crimes

Direct effects. Panel A and B of Table 2 report estimates of the direct effects for violent crime, defined using the MPS category of aggravated assaults and murders, using both discrete and continuous treatment. Column 1 includes monthly date fixed effects, column 2 includes block and date fixed effects, columns 3 controls for officer strength (in levels). In column 4 I further add $LA \times$ date fixed effects, which absorb all time-varying changes occurring within a police division, including all the potential impacts due to the reallocation of police resources within divisions (e.g. unobserved changes in organization, congestion effects).

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. When including the control for officer

strength, the coefficient on distance is stable, whereas the elasticity with respect to police strength is smaller in magnitude and of the opposite sign. This suggests that the primary channel through which closures impact crime is increased distance rather than changes in local police strength as controlling for officers per station does not attenuate the proximity effect. Estimates are barely affected by the inclusion of $LA \times \text{date}$ fixed effects. Columns 6 and 7 exclude blocks near surviving stations or those within the first quintile of baseline distance to the nearest surviving station. Following an increase in the distance to the nearest police station, the average number of recorded violent offenses increases by 7% to 9.4% per treated census block-month. This effect translates into three additional assaults and murders in each block per year. In Panel B, I estimate a positive elasticity of violent crime with respect to distance between 0.03 and 0.04, so that a 10% increase in distance increases violent crimes by roughly 0.4%. 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.²¹ Figure 6 also 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.

Indirect effects. Panels C and D of Table 2 report the indirect effects of the closures on violent crimes from Equation (3). Blocks in close proximity to an open station experience a reduction in violent crime following the first closure within their police division. Unsurprisingly, additional officers are associated with fewer violent crimes even where no closures occur; however, the effect is localized: the $\text{proximity} \times \text{post-closure}$ interaction is negative and sizable, indicating that the benefits from resource concentration accrue primarily to blocks immediately around surviving stations rather than being spread uniformly across areas. Controlling for officer numbers at the nearest surviving station does not significantly change the estimates. In the specification with LA -by-date fixed effects, the implied total effect is a reduction of about 4.5 violent

²¹Results 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 A3(a)).

offenses per year for blocks close to operating stations. Figure 7(c) 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. Figure A3(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. To quantify the aggregate effect, Section 5 develops a spatial model of police and crime that formally integrates direct and indirect effects within a unified framework.

4.B Impacts on clearance

The relationship between station closures and police clearance is *a priori* ambiguous. On the one hand, closures increase travel distance and 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 investigate crime, potentially improving clearance conditional on reporting. 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.

Direct and indirect effects. Columns 1–2 in Panel A of Table 3 show that the incident-level probability of clearance significantly drops by 0.3-0.5 percentage points, approximately 2-3% of the baseline clearance rate of 17%. Results are robust to the inclusion of crime type fixed effects, which absorb the unobserved complexities associated with investigating different crime types.²² As with violent crime, controlling for police strength at the nearest station leaves the distance coefficient largely unchanged, indicating that the deterioration in clearance is primarily driven by reduced proximity.

²²Figure A3(c) shows that before the closures, period-specific coefficients on clearance rate are not statistically different from zero, supporting the validity of the parallel trend assumption.

Columns 3 and 7 further show that higher distance significantly decreases the total volume of cleared and convicted offenses in a blocks by 6% and 3%, respectively. Panels C and D of Table 3 report the indirect effects. The incident-level probability of clearance does not change for blocks near surviving stations, although the volume of cleared offenses increases modestly. This result is consistent with the indirect channel operating through enhanced police deterrence, likely due by greater police visibility near surviving stations, rather than actual improvements in the police ability to clear offenses.

4.C Mechanisms

Why is distance critical for police effectiveness and deterrence? Station closures may reduce police visibility and perceptions of police presence in the surroundings of the stations. This is in line with prior evidence showing that police visibility – through police buildings or police on guard – affects deterrence (e.g. [Di Tella and Schargrodsky, 2004](#); [MacDonald et al., 2016](#); [Blesse and Diegmann, 2022](#)). Furthermore, an increase in distance prevents police from arriving very fast at those crime scenes, hurting their ability to collect evidence and clear crimes ([Blanes i Vidal and Kirchmaier, 2018](#)).²³

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 to identify whether 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). 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.²⁴

²³A third potential channel is patrolling intensity. [Blanes i Vidal and Mastrobuoni \(2018\)](#) suggests that small and temporary changes in patrolling intensity have little, if any, impact on crime. Such reduction in convictions may reinforce the adverse effects on crime through lower incapacitation of offenders ([Mastrobuoni, 2019](#)), and may further discourage the public from reporting such incidents.

²⁴Appendix Figure A2(b) shows that, using police division-level data, average response times and clearance rates are negatively correlated within LAs.

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 support the hypothesis that areas most affected by increased distance are those with higher opportunities for crime and greater potential returns from police presence.

4.D Robustness checks

The results are robust to alternative specifications and variable definitions. I conduct a series of checks addressing several potential concerns. Full details and tables reported in the Online Appendix C. First, the findings are robust to alternative transformations of crime and clearance outcomes (Table C1). Second, alternative measures of distance (commuting distance, driving time, and alternative ITT definitions) and alternative measures of police workforce produce nearly identical estimates (Tables C2–C3). Third, excluding blocks adjacent to treated areas rules out spatial criminal displacement from treated to nearby control areas (Table C4). Fourth, controlling for contemporaneous cuts to other local public services or for baseline potential exposure to police stations leaves the results unchanged (Tables C5). Fifth, using Conley standard errors to allow for spatial and serial correlation yields similar inference (Table C6). Sixth, exploiting closures of non-visible custody suites at otherwise open stations, I show that reductions in custody capacity are also associated with higher violent crime, reinforcing the role of operational capacity rather than salience alone (Table C7). Finally, the decline in clearance is not a mechanical consequence of lower reporting: examining violent and property crimes separately shows that clearance rates fall even for violent offenses, whose reporting remains stable (Table C8).

5 Spatial model of police and crime

In this section, I develop a quantitative spatial model that yields estimable specifications directly applicable to the data for identifying the direct and indirect effects of police station closures. 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 in-

corporate individuals' endogenous decisions regarding criminal activity and location choices, alongside a police production function where police effectiveness depends on station proximity and officer strength. Using the estimated parameters, I quantify the aggregate impacts of the closures, and the optimal allocation of police station closures. I relegate derivations to Online Appendix [D](#).

Individual preferences and supply of crime. Consider a city that consists of a set of discrete neighborhoods, indexed by i , 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 an individual o committing a crime in location i are defined over three components: the returns from committing a crime in a specific location (r_i), 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 ($0 < \pi_i < 1$); 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 i is

$$u_{oi} = \left(\frac{r_i}{\pi_i S} \right) \varepsilon_{oi}, \quad (4)$$

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

$$G(\varepsilon_{ois}) = \exp \left[- \sum_s \left(\sum_j \varepsilon_{ojs}^{-\theta} \right)^{\mu/\theta} \right],$$

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 crim-

inal 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

$$C = \frac{U_C^\mu}{U_C^\mu + \bar{U}_{NC}^\mu} \bar{L}, \quad (5)$$

where U_C is the expected utility to commit crime, and μ is the dispersion parameter that identifies the cross-sectoral elasticity. The fraction $U_C^\mu / (U_C^\mu + \bar{U}_{NC}^\mu)$ 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 i is

$$C_i = \frac{r_i^\theta \pi_i^{-\theta}}{\sum_j r_j^\theta \pi_j^{-\theta}} C, \quad (6)$$

where C is the total number of crimes committed in the city defined in Equation (5). Thus, crime in each block i 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 i depends on two local inputs: the distance to the nearest police station, d_i , and the number of officers per station, P_i . The police output, π_i , represents the clearance rate in area i :

$$\pi_i = A_i \left(\frac{1}{d_i} \right)^\alpha P_i^\beta. \quad (7)$$

Here, A_i is a location-specific exogenous productivity term that reflects local factors, such as availability of CCTV in the area. As P_i is not directly observed, I compute it

as $P_i = P_{l(i)}/k_{l(i)}$, where $P_{l(i)}$ is the total number of police officers in police division l of area i , and $k_{l(i)}$ is the number of stations in the same division.²⁵ α and β are the parameters that determine the elasticity of police output with respect to proximity and the local police workforce, respectively.

Estimation. This simple model has four parameters, $\{\alpha, \beta, \theta, \mu\}$. The police input elasticities α and β and the location-choice elasticity θ are identified from variation in distance to the nearest police station and police strength over time and across areas generated by the station closures, under the assumption that location-specific police productivity and local returns to crime are time-invariant. Once these parameters are estimated, I use the model structure to recover local fundamentals.

Combining crime location decisions with the policing technology delivers two estimating equations: the clearance equation,

$$\log \pi_{it} = -\alpha \log d_{it} + \beta \log P_{it} + \gamma_i + \gamma_t + \eta_{it}, \quad (8)$$

and the crime-flow equation,

$$\log C_{it} = \delta \log d_{it} - \nu \log P_{it} + \phi_i + \phi_t + \varepsilon_{it}, \quad (9)$$

The parameters $\delta = \alpha\theta$ and $\nu = \beta\theta$ combine the police-input elasticities with the substitution parameter θ . Identification relies on variation in treatment intensity in distance to the nearest station and police strength across blocks generated by the closures. The fixed effects γ_i absorb the location-specific productivity term A_i , while ϕ_i absorb r_i and A_i , under the assumption that the location-specific productivity and local returns from crime are time-invariant. Details on the derivation of these expressions and on the empirical implementation are provided in Appendix D.

For estimation, I use intention-to-treat (ITT) measures of distance and police strength that exclude the seven station openings. Police strength is measured using front-line of-

²⁵I implicitly assume that officers are uniformly distributed across patrolling areas under each station's command. In addition, as the MPS police did not adjust officer allocation across areas in response to changes in criminal activity after the reform, 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.

ficers. Table D1 reports the estimates. The crime-flow equation yields $\hat{\delta} = 0.046$ and $\hat{\nu} = 0.007$, and the clearance equation yields $\hat{\alpha} = 0.031$ and $\hat{\beta} = 0.001$. These imply values of the substitution elasticity $\hat{\theta}$ which range from 1.5 to 4.5.²⁶ The 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 $\log r_j$.

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 D.

While the model predicts an average city-wide increase in crime of approximately 2%, this hides substantial heterogeneity across census blocks and police divisions. Figure D1 plots the spatial distribution of crime changes across census blocks, where the average block-level increase is 3%, with the 25th percentile at -0.5% , and the 75th percentile at 5%. At the police division level, the average predicted increase is around 2%, but changes range from -0.3% to nearly 14% (Figure D2(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.²⁷ 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 D2 summarizes the results for violent crime across the different counterfactual scenarios. Random closures perform worse than the actual policy, increasing aggregate crime by

²⁶I take the mean value for $\hat{\theta} = 3$, as the two estimates have overlapping 95% confidence intervals $([0.58, 2.39]; [0.65, 8.36])$, computed using the delta method. This value is consistent with commuting elasticities estimated in other contexts (e.g., Khanna et al., 2023; Tsivanidis, forthcoming; Zárate, 2024).

²⁷This set-up reflects the institutional context, where police divisions operate independently.

approximately 4.3%. In contrast, optimal closures are predicted to reduce aggregate crime by 1.4% on average, with evenly distributed gains across LAs (Figure D2).

What explains these potential gains? Under this counterfactual, the optimal closures are those that minimize increases in distance. Figure D2(b) compares the distribution of distances to the nearest station before and after the closures under the actual and optimal scenarios. Under the optimal closures, the average distance increases to 1.9 km, which is approximately 40% (or 1.2 km) less than under the actual policy. Taken together, this evidence shows that proximity plays a decisive role, highlighting how effective spatial allocation can substantially limit, if not offset, the crime costs of closures.

6 Effects of police station closures 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.

6.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, 1998). An increase in the distance to the nearest 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 police effectiveness, further discouraging citizens from reporting.²⁸ If closures significantly reduce r (particularly for less severe

²⁸Lack 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’”.

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 4 presents estimates of the impact of distance to the nearest station on incident-level reporting. The estimates indicate that a 10% increase in distance leads to a 0.6 percentage point decline in reporting – a 1.6% 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. Panels C and D report the indirect effects generated by increased concentration near surviving stations. These estimates are small and statistically insignificant, consistent with reporting behavior being driven primarily by the loss of the local station rather than by changes in workforce at remaining stations.

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 5 document a 3% reduction in police recorded property crimes (excluding burglaries) and a 7% decline in thefts.²⁹ Yet, CSEW data indicate an increase in victimization over the same period (Figure E1). The two pieces of evidence combined imply that lower recorded property crimes reflect under-reporting rather than an actual drop in crime.³⁰ Estimates on

²⁹Figure A4 plots period-specific coefficients for all police records, property crimes, drugs, and ASB.

³⁰I 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 (excluding burglaries), using estimates from both police and CSEW data implies roughly an 1.5% rise in actual property crime. I approximate 95% confidence intervals using the delta method to be about $[-0.04, 0.29]$ and $[-0.57, 0.29]$ for violent and property crimes, respectively. Given the small CSEW

burglaries further support this interpretation. Since burglaries are typically reported for insurance purposes, they are less sensitive to changes in station proximity. Accordingly, the coefficients on recorded burglaries in columns 3–4 are positive and statistically significant.³¹ 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 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 A4 shows a significant decline in police-recorded ASB incidents, whereas CSEW data indicate an increase in self-reported ASB across multiple categories (Table E2). Together, these findings suggest that police station closures reduce reporting for incidents 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. Columns 1–2 of Table 6 show that, among victims who reported a crime, satisfaction with how the police handled the incident falls after a closure. 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, columns 5–8 examine confidence in other criminal justice institutions: increased distance to a station has no effect on confidence in the wider criminal justice system. Taken together, these findings confirm that residents correctly attribute the decline in effectiveness to the police, rather than to the criminal justice system in general.³² The reduced willingness to report reflects a general lack of trust in policing

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.

³¹98% 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 1.3% as a result of the closures (Table 5, 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.

³²Consistent with the absence of changes in reporting near surviving stations, the indirect effects are generally small and negligible. For police-recorded offenses, such as thefts or drugs that rely heavily on police detection, indirect impacts tend to operate through greater detection rather than changes in victim behavior. Indirect effects on ASB and confidence in the police are modest and statistically weak, which is perhaps unsurprising given that the higher officer concentration at surviving stations is an internal operational change, thus far less salient to residents than the closure of a local station.

resulting from reduced local police visibility.

6.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 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.³³

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 £8,800 for the closure of a local police station.³⁴ 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 A3, 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

³³Data are aggregated at the quarterly frequency. I restrict the sample to sales of residential properties only. Figure A5 estimates the dynamic specification and shows no pre-trends before the closures.

³⁴This magnitude is comparable to other studies linking house prices with crime in the UK (e.g. [Gibbons, 2004](#); [Adda et al., 2014](#)).

that cuts to police funding hit harder those areas that were already losing from austerity cuts (Fetzer, 2019; Giuliatti and McConnell, 2020).

The reduction in house prices is not driven by an expansion in local housing supply following the sale and regeneration of closed stations. Between 2011 and 2016, 45 of 80 closed stations were sold: 25 were converted into residential buildings, 7 into other public amenities (such as schools or community centers), and 3 into offices.³⁵ Table A4 examines whether the impact on house prices varies with the post-closure estate use of the station. If anything, the negative effects are concentrated in blocks where the nearest closed station was neither regenerated nor replaced with another public amenity, contrasting the hypothesis that price declines are driven by a housing supply shock. As a sanity check, column 5 shows that the rise in violent crime is also concentrated in areas with abandoned stations. These negative effects reflect not only changes in local crime, but also the loss of amenities represented by the stations themselves, which directly lowers residents' valuation of the neighborhood and is not offset by new amenities.

Cost-effectiveness. To quantify the total loss using a capitalization approach, I compare the house value loss with public savings from closures. The implied cost-benefit ratio is 0.3, and in high-crime blocks it rises to about 0.6 (see Table F1), so that for every £5 saved by the public authority, residents lose roughly £1.5–3 in housing value, effectively shifting most burden on to residents. The capitalization approach has some drawbacks, at it only includes the valuation of the 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 Hendren (2016) 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 F. I compute

³⁵This implies that 55% of the 2,039 treated blocks whose closest station closed saw that station sold, while 36% saw it regenerated into new residential estates.

MVPF estimates ranging from 1.4 to 4, indicating that each pound saved through this policy imposes up to £4 in additional costs on society.

7 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 with law enforcement, 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 trust 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 public institutions.

References

- Acemoglu, Daron and Matthew O Jackson**, “Social norms and the enforcement of laws,” *Journal of the European Economic Association*, 2017, 15 (2), 245–295.
- Adda, Jérôme, Brendon McConnell, and Imran Rasul**, “Crime and the depenalization of cannabis possession: Evidence from a policing experiment,” *Journal of Political Economy*, 2014, 122 (5), 1130–1202.
- Ahlfeldt, Gabriel M, Stephen J Redding, Daniel M Sturm, and Nikolaus Wolf**, “The economics of density: Evidence from the Berlin Wall,” *Econometrica*, 2015, 83 (6), 2127–2189.

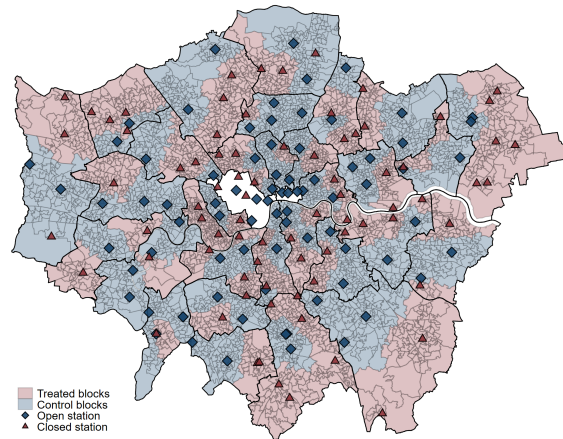
- Ang, Desmond, Panka Bencsik, Jesse M Bruhn, and Ellora Derenoncourt**, “Community Engagement with Law Enforcement after High-profile Acts of Police Violence,” Technical Report, National Bureau of Economic Research 2024.
- Barbosa, Daniel A. C., Thiemo Fetzer, Caterina Soto, and Pedro C. L. Souza**, “De-escalation Technology: The Impact of Body-Worn Cameras on Citizen-Police Interactions,” *Review of Economics and Statistics*, forthcoming.
- Becker, Gary S**, “Crime and punishment: An economic approach,” in “The Economic Dimensions of Crime,” Springer, 1968, pp. 13–68.
- Bellemare, Marc F and Casey J Wichman**, “Elasticities and the inverse hyperbolic sine transformation,” *Oxford Bulletin of Economics and Statistics*, 2020, 82 (1), 50–61.
- Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan**, “How much should we trust differences-in-differences estimates?,” *Quarterly Journal of Economics*, 2004, 119 (1), 249–275.
- Besley, Timothy and Hannes Mueller**, “Estimating the Peace Dividend: The impact of violence on house prices in Northern Ireland,” *American Economic Review*, 2012, 102 (2), 810–33.
- Blesse, Sebastian and André Diegmann**, “The place-based effects of police stations on crime: Evidence from station closures,” *Journal of Public Economics*, 2022, 207, 104605.
- Borusyak, Kirill, Xavier Jaravel, and Jann Spiess**, “Revisiting event-study designs: Robust and efficient estimation,” *Review of Economic Studies*, 2024, 91 (6), 3253–3285.
- Callaway, Brantly and Pedro HC Sant’Anna**, “Difference-in-differences with multiple time periods,” *Journal of Econometrics*, 2021, 225 (2), 200–230.
- Cengiz, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zipperer**, “The effect of minimum wages on low-wage jobs,” *Quarterly Journal of Economics*, 2019, 134 (3), 1405–1454.
- Chaisemartin, Clément De and Xavier d’Haultfoeuille**, “Two-way fixed effects estimators with heterogeneous treatment effects,” *American Economic Review*, 2020, 110 (9), 2964–96.
- Chalfin, Aaron and Justin McCrary**, “Criminal deterrence: A review of the literature,” *Journal of Economic Literature*, 2017, 55 (1), 5–48.
- Chen, Jiafeng and Jonathan Roth**, “Logs with zeros? Some problems and solutions,” *The Quarterly Journal of Economics*, 2024, 139 (2), 891–936.
- Cook, Philip J and Jens Ludwig**, “Economical crime control,” Technical Report w16513, National Bureau of Economic Research 2010.
- Deshpande, Manasi and Yue Li**, “Who is screened out? Application costs and the targeting of disability programs,” *American Economic Journal: Economic Policy*, 2019, 11 (4), 213–48.
- Draca, Mirko, Stephen Machin, and Robert Witt**, “Panic on the streets of London: Police, crime, and the July 2005 terror attacks,” *American Economic Review*, 2011, 101 (5), 2157–81.
- Durlauf, Steven N and Daniel S Nagin**, “Imprisonment and crime: Can both be reduced?,” *Criminology & Public Policy*, 2011, 10 (1), 13–54.
- Ehrlich, Isaac**, “Participation in illegitimate activities: A theoretical and empirical investigation,” *Journal of Political Economy*, 1973, 81 (3), 521–565.
- Evans, William N and Emily G Owens**, “COPS and Crime,” *Journal of Public Economics*, 2007, 91 (1–2), 181–201.
- Fetzer, Thiemo**, “Did austerity cause Brexit?,” *American Economic Review*, 2019, 109 (11), 3849–86.
- Fu, Chao and Kenneth I Wolpin**, “Structural estimation of a Becker–Ehrlich equilibrium model of crime: Allocating police across cities to reduce crime,” *Review of Economic Studies*, 2018, 85 (4), 2097–2138.
- Fyfe, Nicholas R, JB Terpstra, and Pieter Tops**, *Centralizing forces? Comparative perspectives on contemporary police reform in Northern and Western Europe*, Eleven International Publishing, 2013.

- Garicano, Luis and Paul Heaton**, “Information technology, organization, and productivity in the public sector: Evidence from police departments,” *Journal of Labor Economics*, 2010, 28 (1), 167–201.
- Gauthier, Germain**, “Measuring crime reporting and incidence: Method and application to# MeToo,” Technical Report SSRN 4242506, <http://dx.doi.org/10.2139/ssrn.4242506> 2022.
- Gibbons, Steve**, “The costs of urban property crime,” *The Economic Journal*, 2004, 114 (499), F441–F463.
- Giulietti, Corrado and Brendon McConnell**, “Kicking You When You’re Already Down: The Multi-pronged Impact of Austerity on Crime,” Technical Report preprint arXiv:2012.08133, arXiv 2020.
- Goodman-Bacon, Andrew**, “Difference-in-differences with variation in treatment timing,” *Journal of Econometrics*, 2021, 225 (2), 254–277.
- Heblich, Stephan, Stephen J Redding, and Daniel M Sturm**, “The making of the modern metropolis: Evidence from London,” *Quarterly Journal of Economics*, 2020, 135 (4), 2059–2133.
- Hendren, Nathaniel**, “The policy elasticity,” *Tax Policy and the Economy*, 2016, 30 (1), 51–89.
- **and Ben Sprung-Keyser**, “A unified welfare analysis of government policies,” *Quarterly Journal of Economics*, 2020, 135 (3), 1209–1318.
- HMIC**, “Adapting to Austerity: A Review of Police Force and Authority Preparedness for the 2011/12–14/15 CSR Period,” 2011.
- , “Demanding Times: The front line and police visibility,” 2011.
- Home Office**, *Crime outcomes in England and Wales: Year ending March 2016* 2016.
- i Vidal, Jordi Blanes and Giovanni Mastrobuoni**, “Police Patrols and Crime,” Technical Report IZA Discussion Paper 11393 2018.
- **and Tom Kirchmaier**, “The effect of police response time on crime clearance rates,” *Review of Economic Studies*, 2018, 85 (2), 855–891.
- Khanna, Gaurav, Anant Nyshadham, Daniel Ramos-Menchelli, Jorge Andrés Tamayo, and Audrey Tiew**, “Evaluating the impact of urban transit infrastructure: Evidence from Bogota’s transmilenio,” 2023.
- Klick, Jonathan and Alexander Tabarrok**, “Using terror alert levels to estimate the effect of police on crime,” *Journal of Law and Economics*, 2005, 48 (1), 267–279.
- Levitt, Steven D**, “The relationship between crime reporting and police: Implications for the use of uniform crime reports,” *Journal of Quantitative Criminology*, 1998, 14, 61.
- Linden, Leigh and Jonah E Rockoff**, “Estimates of the impact of crime risk on property values from Megan’s laws,” *American Economic Review*, 2008, 98 (3), 1103–27.
- MacDonald, John M, Jonathan Klick, and Ben Grunwald**, “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*, 2016, 179 (3), 831–846.
- Machin, Stephen and Olivier Marie**, “Crime and police resources: The street crime initiative,” *Journal of the European Economic Association*, 2011, 9 (4), 678–701.
- Mas, Alexandre**, “Pay, reference points, and police performance,” *Quarterly Journal of Economics*, 2006, 121 (3), 783–821.
- Mastrobuoni, Giovanni**, “Police disruption and performance: Evidence from recurrent redeployments within a city,” *Journal of Public Economics*, 2019, 176, 18–31.
- , “Crime is terribly revealing: Information technology and police productivity,” *Review of Economic Studies*, 2020, 87 (6), 2727–2753.
- Mello, Steven**, “More COPS, less crime,” *Journal of Public Economics*, 2019, 172, 174–200.

- MOPAC**, “Estate Strategy 2013–2016, MOPAC/MPS,” Technical Report 2013.
- , “Review of MPS Contact Points,” Technical Report 2015.
- , “Public Access Strategy,” Technical Report 2017.
- Owens, Emily**, *The economics of policing*, Berlin: Springer, 2020.
- and **Bocar Ba**, “The economics of policing and public safety,” *Journal of Economic Perspectives*, 2021, 35 (4), 3–28.
- Pinotti, Paolo**, “The credibility revolution in the empirical analysis of crime,” *Italian Economic Journal*, 2020, 6 (2), 207–220.
- Pratt, Alison**, “Police stations: are they a thing of the past?,” *UK Parliament, House of Commons Library*, May, 2019, 28.
- Redding, Stephen J and Esteban Rossi-Hansberg**, “Quantitative spatial economics,” *Annual Review of Economics*, 2017, 9 (1), 21–58.
- Rosen, Sherwin**, “Hedonic prices and implicit markets: Product differentiation in pure competition,” *Journal of Political Economy*, 1974, 82 (1), 34–55.
- Sun, Liyang and Sarah Abraham**, “Estimating dynamic treatment effects in event studies with heterogeneous treatment effects,” *Journal of Econometrics*, 2021, 225 (2), 175–199.
- Tella, Rafael Di and Ernesto Schargrodsky**, “Do police reduce crime? Estimates using the allocation of police forces after a terrorist attack,” *American Economic Review*, 2004, 94 (1), 115–133.
- Thaler, Richard**, “An econometric analysis of property crime: Interaction between police and criminals,” *Journal of Public Economics*, 1977, 8 (1), 37–51.
- , “A note on the value of crime control: Evidence from the property market,” *Journal of Urban Economics*, 1978, 5 (1), 137–145.
- Tsivanidis, Nick**, “Evaluating the Impact of Urban Transit Infrastructure: Evidence from Bogotá’s Trans-Milenio,” *American Economic Review*, forthcoming.
- Vollaard, Ben and Joseph Hamed**, “Why the police have an effect on violent crime after all: Evidence from the British Crime Survey,” *Journal of Law and Economics*, 2012, 55 (4), 901–924.
- Weisburd, Sarit**, “Police presence, rapid response rates, and crime prevention,” *Review of Economics and Statistics*, 2021, 103 (2), 280–293.
- Wilson, James Q and George L Kelling**, “Broken Windows: The police and neighborhood safety,” *Atlantic Monthly*, 1982, 249 (3), 29–38.
- Zárate, Román D**, “Spatial misallocation, informality, and transit improvements: Evidence from Mexico City,” 2024.

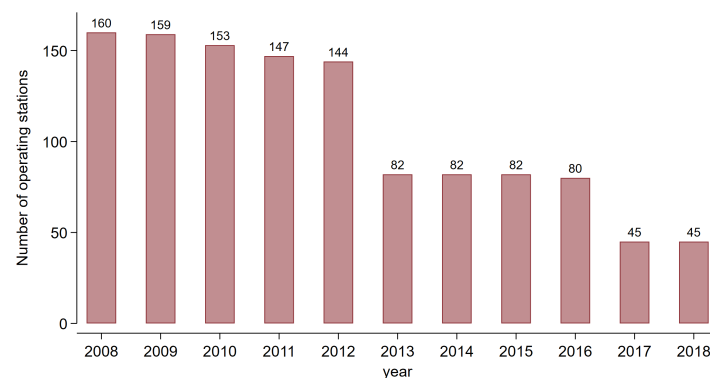
Figures

Figure 1: Police station closures and treated blocks in Greater London



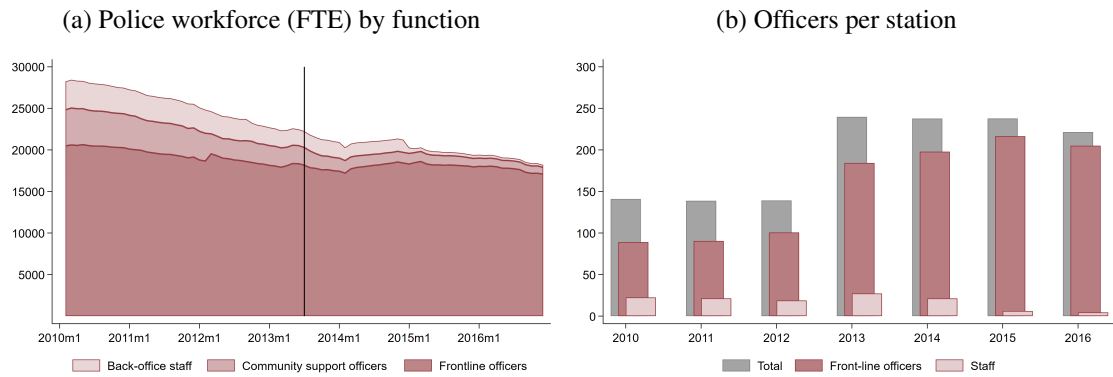
Note: This map plots police stations operating in Greater London at the end of 2016. Open stations are shown as blue diamonds; closed stations as red triangles. Blocks whose nearest station closed between 2011 and 2016 are shaded red, while blocks whose nearest station remained open are shaded blue. Thick black lines mark the 33 London borough boundaries, which coincide with police division boundaries. The final sample excludes City of London and Westminster.

Figure 2: Number of Police Stations in Greater London, 2008-2018



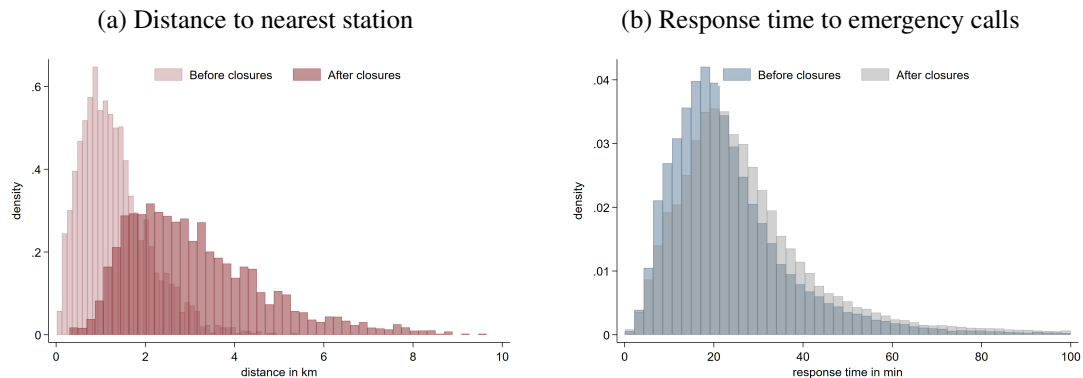
Note: This figure shows the number of police stations operating in Greater London from 2008 to 2018, with counts recorded at the end of each year. The empirical sample ends in 2016, when the MPS restructured local policing. Data on station openings and closures were obtained through FOIA requests to the MPS.

Figure 3: MPS Workforce and Officers per Station, 2010–2016



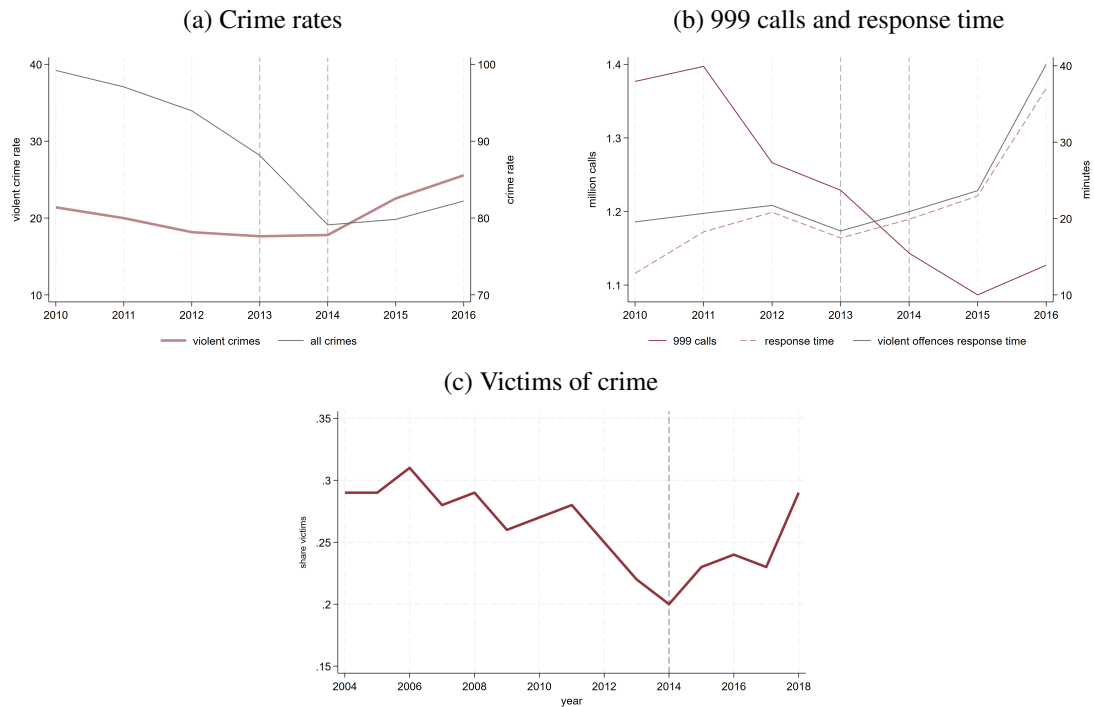
Note: This figure presents the MPS workforce in full-time equivalents (FTE) by policing function from 2010 to 2016. Panel (a) shows total FTEs; panel (b) normalizes them by the number of operating stations. Front-line officers include constables and detective constables engaged in patrolling and emergency response. Community support officers (PCSOs) are uniformed staff with community-focused duties (e.g. addressing ASB, handling minor offenses, crowd control, and directing traffic). Back-office roles comprise civilian administrative and support functions (e.g., HR, finance, control rooms). The vertical line marks July 2013, when most station closures occurred. Data are from the Mayor’s Office of Policing and Crime. The sample spans 2011-2016 and excludes City of London and Westminster.

Figure 4: Distance to Police Stations and Police Response Times



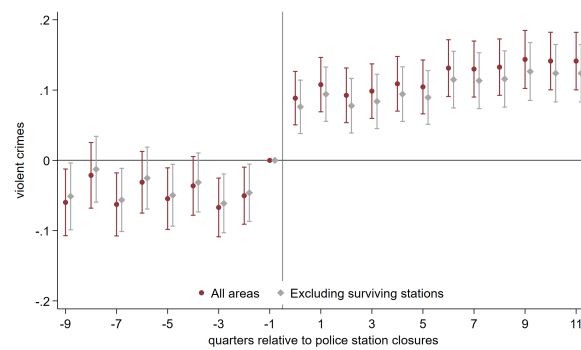
Note: Panel (a) shows the distribution of the distance to the nearest police station for census blocks whose closest station closed before and after the closures. Panel (b) shows the distribution of police response times before and after the closures, using MPS administrative data, which are only available at the police-division level. The sample spans 2011-2016 and excludes City of London and Westminster.

Figure 5: Aggregate Trends in Crime, Emergency Calls, and Response Times in Greater London



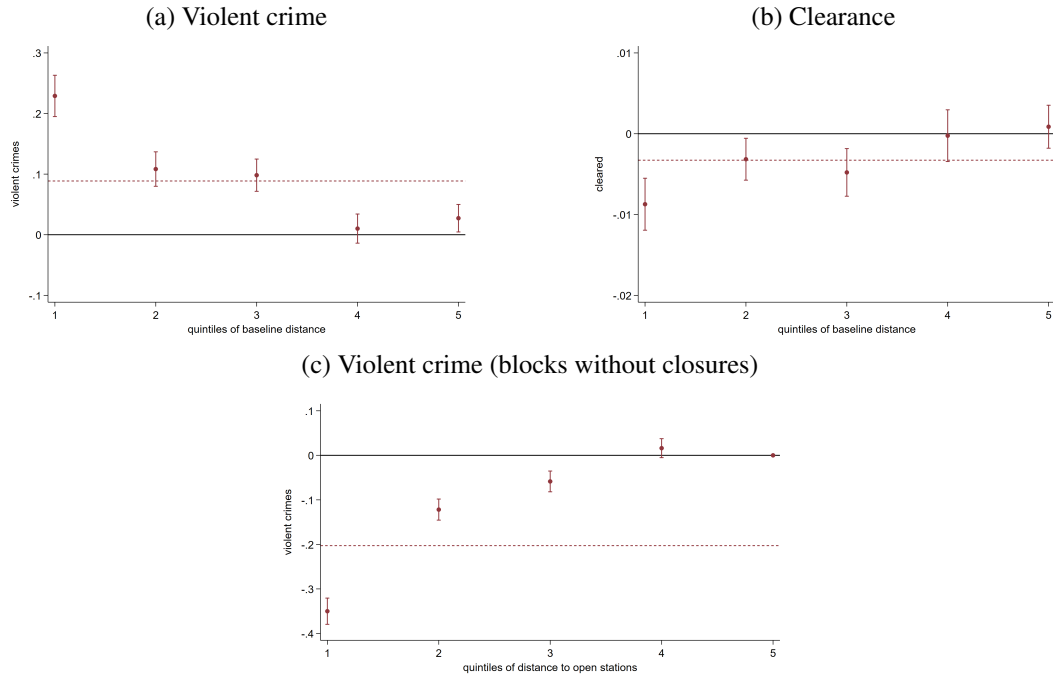
Note: Panel (a) shows crime rates per 1,000 individuals for violent crimes (left axis) and all recorded incidents (right axis) from 2010 to 2016. Panel (b) reports the number of 999 calls and average response times for calls classified as “I” (immediate) or “S” (significant). Response times include both call handling and travel to the scene and are sourced from aggregate MPS data. Average response times are shown for all offences and violent offences separately. Panel (c) plots the share of London respondents reporting victimization, based on CSEW aggregate data from 2004-05 to 2018-19.

Figure 6: Dynamic Effects of Police Station Closures on Violent Crime



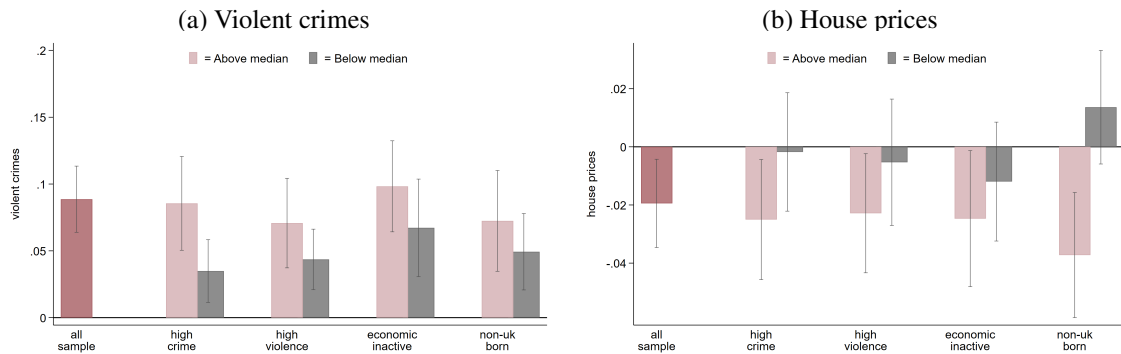
Note: The figure reports event-study estimates of the effect of police station closures on violent crime, using on police records. The dependent variable is the log transformation of violent crime counts, with zeros replaced by a constant (0.025) to account for the extensive margin. Estimates are obtained excluding distant relative periods (Sun and Abraham, 2021). The specification includes census block and date fixed effects and controls for a workforce per station, defined as the total number of officers in the LA divided by the number of operating stations and scaled so that one unit corresponds to 10 officers. All fixed effects are interacted with stacked-dataset indicators. The figure plots estimates using all control blocks (red) and excluding blocks near surviving stations (gray). Standard errors are clustered at the census block level, and error bars show 95% confidence intervals. The sample excludes City of London and Westminster.

Figure 7: Effects of Police Station Closures by Baseline Distance to Stations



Note: The panels show estimated effects of police station closures by baseline distance to the nearest station. Violent crime outcomes use the log transformation with zeros replaced by 0.025 (panel (a)), and clearance outcomes use an indicator for incidents with non-missing outcomes (panel (b)). All specifications include census block and date fixed effects and a workforce-per-station control, defined as total officers in the local authority divided by the number of operating stations and scaled so that one unit equals 10 officers. Panel (c) reports estimates for blocks that never experienced a closure, using a distance-bin specification in which the most distant quintile is the reference group. Standard errors are clustered at the census block level, and error bars show 95% confidence intervals. The sample excludes City of London and Westminster.

Figure 8: Heterogeneous Effects by Baseline Block Characteristics



Note: The panels report estimates of the effect of police station closures by baseline characteristics of the census block. Panel (a) uses police-recorded violent crime counts, transformed using the log specification with zeros replaced by a constant (0.025). Panel (b) uses average log house prices, with observations weighted by the number of sales in the block-quarter cell. All estimates include census block and date fixed effects and a workforce-per-station control. Baseline characteristics include 2008 violent crime rates (from historical MPS data) and 2011 Census measures of economic inactivity and foreign-born population shares. Standard errors are clustered at the census block level, and error bars show 95% confidence intervals. The sample excludes City of London and Westminster.

Tables

Table 1: Descriptive statistics

	Treated blocks		Blocks near surviving stations		Blocks far from surviving stations	
	Pre (1)	Post (2)	Pre (3)	Post (4)	Pre (5)	Post (6)
Panel A: LSOA-level dataset (2011-2016)						
Distance from nearest police station (in km)	1.38	2.99	0.78	0.78	2.07	2.08
<i>Police records</i>						
All crimes	17.22	14.44	25.38	21.07	16.54	14.00
Violent crimes	2.70	3.06	4.04	4.41	2.63	3.04
Property crimes	6.07	6.37	8.45	9.06	5.63	6.00
Drug-related crimes	0.49	0.52	0.93	0.97	0.45	0.48
Anti-social behaviour	5.56	3.72	8.13	5.40	5.58	3.68
Cleared crimes	1.03	1.71	1.73	2.90	0.93	1.61
Cleared property crimes	0.33	0.51	0.56	0.90	0.27	0.43
Cleared violent crimes	0.29	0.58	0.44	0.84	0.29	0.58
Convicted crimes	0.50	0.79	0.82	1.35	0.44	0.74
Convicted property crimes	0.21	0.30	0.34	0.52	0.18	0.26
Convicted violent crimes	0.13	0.26	0.21	0.39	0.13	0.26
<i>Police workforce per station per LA</i>						
Total police strength in FTE (in 10s)	13.26	24.29	13.91	23.28	12.93	21.95
Front-line officers in FTE (in 10s)	9.24	21.24	9.88	20.37	8.97	18.95
Observations	59,131	87,677	35,365	55,139	34,507	55,997
Panel B: incident-level dataset (2012-2016)						
<i>Investigation outcomes</i>						
Pr(cleared)	0.16	0.16	0.18	0.19	0.16	0.16
Pr(convicted)	0.08	0.08	0.09	0.09	0.07	0.07
Observations	387,936	923,394	338,303	847,544	206,425	569,946
Panel C: LSOA-level dataset from Land Registry (2011-2016)						
House prices	488,207	654,482	492,362	697,805	426,159	585,320
Number of transactions	10.39	18.42	9.96	18.84	10.37	16.43
Observations	10,769	18,560	7,547	12,917	4,251	7,932

Note: This table presents summary statistics for all variables used in the empirical analysis. Panel A reports averages from police records calculated at the census block-month level. Panel B provides statistics from the incident-level dataset on investigation outcomes. Panel C reports average house prices from Land Registry data, weighted by the number of transactions and aggregated at the census block level on a quarterly basis. Treated blocks are defined as census blocks that experienced an increase in distance to the nearest police station during the sample period. Control blocks are split into those located close to surviving stations (below the median distance) and those located farther away (above the median). “Pre” and “Post” periods refer to periods before and after the closures. Investigation outcome data are available from 2012 onward. The main sample spans 2011-2016, and excludes City of London and Westminster.

Table 2: Effects of Police Station Closures on Violent Crime

	Dep. var.: Assaults and murders					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Direct effects, binary treatment						
dummy distance	-0.009*** (0.002)	0.073*** (0.012)	0.089*** (0.013)	0.094*** (0.013)	0.073*** (0.012)	0.024 (0.012)
N. officers			-0.005*** (0.001)		-0.005*** (0.001)	-0.005*** (0.001)
Panel B: Direct effects, continuous distance						
log(distance)	-0.069*** (0.004)	0.030*** (0.010)	0.040*** (0.011)	0.036*** (0.011)	0.031*** (0.011)	0.004 (0.011)
N. officers			-0.004*** (0.001)		-0.005*** (0.001)	-0.004*** (0.001)
Observations	337,794	337,794	337,794	337,794	333,042	292,506
Mean Dep. Variable	2.48	2.48	2.48	2.48	2.38	2.16
Panel C: Indirect effects, binary treatment						
dummy near $\times$ Post	0.014*** (0.003)	-0.217*** (0.019)	-0.217*** (0.019)	-0.157*** (0.020)		
N. officers			-0.004** (0.001)			
Panel D: Indirect effects, continuous distance						
log(1/distance) $\times$ Post	0.015*** (0.003)	-0.211*** (0.017)	-0.212*** (0.016)	-0.180*** (0.018)		
N. officers			-0.004*** (0.001)			
Observations	181,008	181,008	181,008	181,008		
Mean Dep. Variable	2.44	2.44	2.44	2.44		
Date FE	✓	✓	✓		✓	✓
LSOA FE		✓	✓	✓	✓	✓
LAXDate FE				✓		

Note: This table reports estimates of the effect of police station closures on violent crime, using police-recorded assaults and murders. The dependent variable is the log transformation of crime counts, with zeros replaced by 0.025. Panel A reports estimates based on a binary treatment indicator; Panel B replaces the indicator with continuous log distance to the nearest station. Panel C restricts the sample to blocks that never experienced a closure, and Panel D replaces the binary indicator with the log of inverse distance to the nearest operating station. Column (1) includes date fixed effects; column (2) adds LSOA fixed effects; column (3) adds controls for the workforce-per-station measure (scaled so that one unit corresponds to 10 officers); column (4) includes LA $\times$ date fixed effects. Columns (5) and (6) progressively exclude blocks with a surviving station and those within the first quintile of distance to the nearest surviving station. Specifications in columns (1)-(3), Panels C-D, also include the $Post_{it}$ indicator from Equation 3. The table reports baseline means. Standard errors are clustered at the census block level. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table 3: Effects of Police Station Closures on Clearance

Dep. var.:	Pr(cleared)		Volume cleared crimes				Volume convictions		
			All crimes		Property crimes	Violent crimes	All crimes	Property crimes	Violent crimes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Direct effects, binary treatment									
dummy distance	-0.005*** (0.001)	-0.003* (0.001)	-0.061*** (0.007)	-0.058*** (0.007)	-0.016*** (0.004)	-0.020*** (0.003)	-0.030*** (0.005)	-0.010*** (0.003)	-0.008*** (0.002)
N. officers		-0.000*** (0.000)	0.001 (0.000)						
Panel B: Direct effects, continuous distance									
log(distance)	-0.004*** (0.001)	-0.003*** (0.001)	-0.028*** (0.006)	-0.023*** (0.006)	-0.003 (0.004)	-0.010*** (0.003)	-0.011*** (0.004)	-0.003 (0.002)	-0.004** (0.001)
N. officers		-0.000*** (0.000)	0.000 (0.001)						
Observations	3,346,332	3,346,332	337,794	337,794	337,794	337,794	337,794	337,794	337,794
Mean Dep. Variable	0.17	0.17	1.29	1.29	0.41	0.35	0.62	0.26	0.17
Panel C: Indirect effects, binary treatment									
dummy near × Post	0.002 (0.002)	0.002 (0.002)	0.125*** (0.012)	0.094*** (0.012)	0.046*** (0.008)	0.032*** (0.005)	0.060*** (0.008)	0.031*** (0.005)	0.014*** (0.003)
N. officers		-0.000 (0.000)	0.001 (0.001)						
Panel D: Indirect effects, continuous distance									
log(1/distance) × Post	0.001 (0.002)	0.001 (0.002)	0.123*** (0.011)	0.107*** (0.012)	0.055*** (0.009)	0.038*** (0.006)	0.073*** (0.010)	0.036*** (0.006)	0.020*** (0.003)
N. officers		-0.000 (0.000)	0.002** (0.001)						
Observations	1,962,218	1,962,218	181,008	181,008	181,008	181,008	181,008	181,008	181,008
Mean Dep. Variable	0.17	0.17	1.33	1.33	0.42	0.36	0.63	0.26	0.17
Date FE	✓	✓	✓						
LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
LAXDate FE				✓	✓	✓	✓	✓	✓
Crime type FE	✓	✓							

Note: This table reports estimates of the effect of police station closures on clearance outcomes for incidents with a valid crime identifier and non-missing investigation outcome. Columns (1)–(2) use the probability of clearance; columns (3)–(9) use log counts (with zeros replaced by a 0.025) of cleared crimes and convictions for total, property, and violent offenses, respectively. Panels A-B report direct effects using a binary indicator and continuous log distance, while Panels C-D report corresponding estimates for blocks that never experienced a closure. Specifications include date and LSOA fixed effects, with some columns adding LA×date and crime-type fixed effects. Number of officers is scaled so that one unit corresponds to 10 officers. Specifications in columns (1)–(3), Panels C-D, also include the $Post_{it}$ indicator from Equation 3. The table reports baseline means. Standard errors are clustered at the census block level. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table 4: Effects of Police Station Closures on Crime Reporting (CSEW)

Dep. var.:	=1 if incident reported									
Type of offence:	All incidents		Violent crimes		Assaults		Property crimes (excl. burglaries)		Thefts	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Direct effects, binary treatment										
dummy distance	-0.009 [0.045]	-0.028 [0.047]	0.007 [0.021]	0.005 [0.020]	0.009 [0.017]	0.004 [0.017]	-0.028 [0.036]	-0.052 [0.037]	-0.011 [0.034]	-0.028 [0.035]
Panel B: Direct effects, continuous distance										
log(distance)	-0.046 [0.037]	-0.061* [0.036]	0.000 [0.014]	0.000 [0.013]	0.001 [0.012]	-0.002 [0.011]	-0.023 [0.034]	-0.036 [0.032]	-0.014 [0.027]	-0.024 [0.027]
Observations	5,307	5,325	5,307	5,325	5,307	5,325	5,307	5,325	5,307	5,325
Mean dep. Variable	0.37	0.37	0.04	0.04	0.03	0.03	0.22	0.22	0.18	0.18
Panel C: Indirect effects, binary treatment										
dummy near $\times$ Post	-0.013 [0.066]	-0.036 [0.068]	-0.010 [0.029]	0.000 [0.029]	-0.022 [0.022]	-0.018 [0.023]	-0.023 [0.052]	-0.043 [0.055]	-0.037 [0.047]	-0.048 [0.050]
Panel D: Indirect effects, continuous distance										
log(1/distance) $\times$ Post	0.009 [0.043]	-0.006 [0.045]	0.020 [0.016]	0.029 [0.018]	0.011 [0.014]	0.018 [0.015]	-0.016 [0.035]	-0.031 [0.038]	-0.027 [0.032]	-0.033 [0.035]
Observations	3,055	3,057	3,055	3,057	3,055	3,057	3,055	3,057	3,055	3,057
Mean dep. variable	0.37	0.37	0.34	0.34	0.33	0.33	0.37	0.37	0.36	0.36
Control for workforce	✓		✓		✓		✓		✓	
Date, LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
LA-specific linear trends		✓		✓		✓		✓		✓

Note: This table reports estimates of the effect of police station closures on reporting using CSEW data. The sample includes incidents experienced by survey respondents residing in London, excluding the City of London and Westminster. The dependent variable is a binary indicator equal to one if the respondent reported the incident, for all incidents (columns 1-2) and by offense type (columns 3-10). Violent crimes include assaults and robberies; property crimes include thefts, shoplifting, and criminal damage, and exclude burglaries. Panels A-B report direct effects using a binary indicator and continuous log distance to the nearest station, while Panels C-D report corresponding estimates for blocks that never experienced a closure. Specifications include LSOA and quarterly date fixed effects, with odd-numbered columns controlling for workforce-per-station and even-numbered columns adding LA-specific linear time trends. Specifications in odd columns in Panels C-D also include the $Post_{it}$ indicator from Equation 3. The table reports baseline means. Standard errors are clustered at the census block level. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table 5: Effects of Police Station Closures on Crime Types

	Robberies		Burglaries		Theft		Property crimes (excl. burglaries)		Drugs-related offences	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Direct effects, binary treatment										
dummy distance	0.007** (0.003)	0.013*** (0.003)	0.010** (0.005)	0.005 (0.005)	-0.065*** (0.010)	-0.074*** (0.010)	-0.023*** (0.008)	-0.028*** (0.008)	-0.016*** (0.003)	-0.012*** (0.003)
N. officers	-0.002*** (0.000)		-0.002*** (0.000)		-0.002** (0.001)		-0.001* (0.001)		-0.000 (0.000)	
Panel B: Direct effects, continuous distance										
log(distance)	0.003 (0.003)	0.002 (0.002)	0.012*** (0.004)	0.009** (0.004)	-0.026*** (0.007)	-0.028*** (0.008)	-0.006 (0.006)	-0.012* (0.006)	-0.009*** (0.003)	-0.005** (0.003)
N. officers	-0.002*** (0.000)		-0.003*** (0.000)		-0.003*** (0.001)		-0.001** (0.001)		0.000 (0.000)	
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	0.65	0.65
Panel C: Indirect effects, binary treatment										
dummy near $\times$ Post	-0.040*** (0.005)	-0.030*** (0.005)	0.026*** (0.007)	0.005 (0.007)	0.201*** (0.016)	0.113*** (0.015)	0.069*** (0.012)	0.035*** (0.012)	0.016*** (0.006)	0.008 (0.005)
N. officers	-0.003*** (0.000)		-0.003*** (0.001)		-0.005*** (0.001)		-0.001 (0.001)		0.000 (0.000)	
Panel D: Indirect effects, continuous distance										
log(1/distance) $\times$ Post	-0.033*** (0.004)	-0.028*** (0.004)	0.021*** (0.005)	0.004 (0.005)	0.179*** (0.015)	0.115*** (0.015)	0.076*** (0.009)	0.055*** (0.010)	0.014** (0.006)	0.009 (0.006)
N. officers	-0.003*** (0.000)		-0.003*** (0.001)		-0.004*** (0.001)		-0.001 (0.001)		0.001 (0.000)	
Observations	181,008	181,008	181,008	181,008	181,008	181,008	181,008	181,008	181,008	181,008
Mean Dep. Variable	0.68	0.68	1.63	1.63	0.11	0.11	5.43	5.43	0.69	0.69
Date, LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
LAXdate FE		✓		✓		✓		✓		✓

Note: This table reports estimates of the effect of police station closures on crime types using police records. The dependent variables are the log transformation of crime counts, with zeros replaced by a constant (0.025). Property crimes include thefts, shoplifting, and criminal damage, and exclude burglaries. Panels A-B report direct effects using a binary indicator and continuous log distance to the nearest station, while Panels C-D report corresponding estimates for blocks that never experienced a closure. Odd-numbered columns include LSOA and date fixed effects, and even-numbered columns add LAXdate fixed effects. Specifications in odd columns in Panels C-D also include the $Post_{it}$ indicator from Equation 3. The table reports baseline means of the crime counts. 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 Confidence in the Police

Dep. var.:	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: Direct effects, binary treatment								
dummy distance	-0.017 [0.041]	-0.022 [0.043]	0.071*** [0.024]	0.064*** [0.025]	0.016 [0.026]	0.019 [0.026]	0.030 [0.026]	0.041 [0.026]
Panel B: Direct effects, continuous distance								
log(distance)	-0.062** [0.030]	-0.069** [0.031]	0.048** [0.019]	0.040** [0.019]	0.000 [0.020]	0.006 [0.020]	0.018 [0.019]	0.022 [0.019]
Observations	5,143	5,161	9,420	9,449	8,551	8,580	9,046	9,072
Mean dep. variable	0.251	0.251	0.365	0.365	0.464	0.464	0.505	0.505
Panel C: Indirect effects, binary treatment								
dummy near $\times$ Post	-0.024 [0.061]	-0.018 [0.064]	-0.046 [0.036]	-0.024 [0.038]	0.042 [0.039]	0.050 [0.042]	0.020 [0.038]	0.033 [0.040]
Panel D: Indirect effects, continuous distance								
log(1/distance) $\times$ Post	-0.005 [0.039]	0.003 [0.042]	-0.045* [0.027]	-0.027 [0.028]	0.009 [0.031]	0.014 [0.033]	0.002 [0.027]	0.011 [0.029]
Observations	2,975	2,977	5,325	5,331	4,831	4,836	5,125	5,131
Mean dep. variable	0.244	0.244	0.361	0.361	0.46	0.46	0.50	0.50
Control for workforce	✓		✓		✓		✓	
Date, LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓
LA-specific linear trends		✓		✓		✓		✓

Note: This table reports estimates of the effect of police station closures on satisfaction with police response and confidence in police effectiveness, using CSEW data. Variable definitions follow the survey questions listed in Appendix E. Panels A-B report direct effects using a binary treatment indicator and continuous log distance to the nearest station, while Panels C-D report corresponding estimates for blocks that never experienced a closure. All specifications include LSOA and quarterly date fixed effects; odd-numbered columns additionally control for the workforce-per-station, and even-numbered columns add LA-specific linear time trends. Specifications in odd columns in Panels C-D also include the $Post_{it}$ indicator from Equation 3. The table reports baseline means. Standard errors are clustered at the census block level. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table 7: Effects of Police Station Closures on House Prices

Dep. var.:	House prices							
	All properties		Flats		Terraced houses		Detached houses	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Direct effects, binary treatment								
dummy distance	-0.014 (0.007)	-0.019* (0.008)	-0.007 (0.007)	-0.007 (0.007)	0.007 (0.008)	0.001 (0.009)	-0.002 (0.013)	0.001 (0.016)
Panel B: Direct effects, continuous distance								
log(distance)	-0.010* (0.005)	-0.008 (0.006)	-0.005 (0.006)	0.001 (0.006)	0.005 (0.007)	0.005 (0.007)	-0.009 (0.011)	-0.003 (0.013)
Observations	62,674	62,664	52,670	52,647	38,878	38,872	18,037	18,029
Panel C: Indirect effects, binary treatment								
dummy near $\times$ Post	0.026* (0.011)	0.014 (0.011)	-0.006 (0.009)	-0.005 (0.008)	-0.001 (0.013)	0.008 (0.012)	-0.002 (0.020)	-0.003 (0.019)
Panel D: Indirect effects, continuous distance								
log(1/distance) $\times$ Post	0.039*** (0.011)	0.032*** (0.011)	0.000 (0.008)	0.002 (0.006)	0.001 (0.009)	0.012 (0.008)	-0.018 (0.020)	-0.022 (0.018)
Observations	32,643	32,636	27,497	27,475	19,943	19,937	8,425	8,408
Date, LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓
LAxDate FE		✓		✓		✓		✓

Note: This table reports estimates of the effect of police station closures on house prices using quarterly Land Registry data. Observations are weighted by the number of sales recorded in the quarter-census block cell. The dependent variables are log average house prices for all properties and by property type (flats, terraced houses, and detached/semi-detached houses). Panels A-B report direct effects using a binary indicator (A) and continuous log distance to the nearest station (B), while Panels C-D report corresponding estimates for blocks that never experienced a closure. Specifications include LSOA and quarterly date fixed effects, odd-numbered columns additionally control for the workforce-per-station, and even-numbered specifications add LA $\times$ date fixed effects. Standard errors are clustered at the census block level. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

ONLINE APPENDIX

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

Elisa Facchetti

Table of Contents

A	Additional figures and tables	A2
B	Additional background material	A8
C	Robustness checks	A11
D	Model derivations	A20
E	Crime Survey of England and Wales	A27
F	Cost-effectiveness of police station closures	A31

Appendix A Additional figures and tables

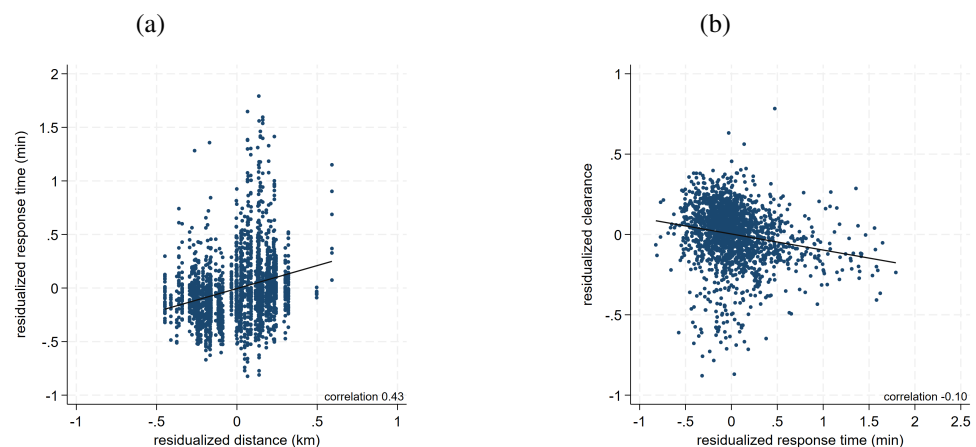
A1 Additional figures

Figure A1: Examples of Closed Police Stations in London



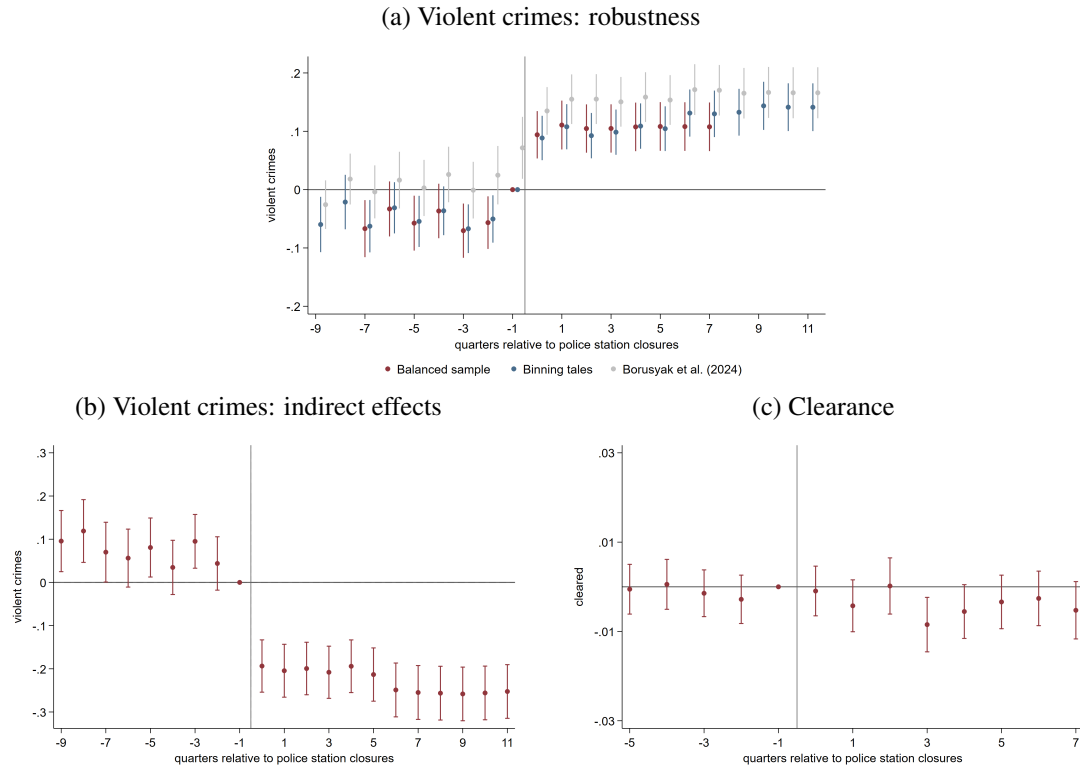
Note: These photographs show examples of closed police stations in London. The second image in the top row depicts a station that has been repurposed into a restaurant, while the last image in the bottom row shows an architectural rendering of a residential property built on the site of a former station.

Figure A2: Response Times, Distance to Police Stations, and Cleared Offenses



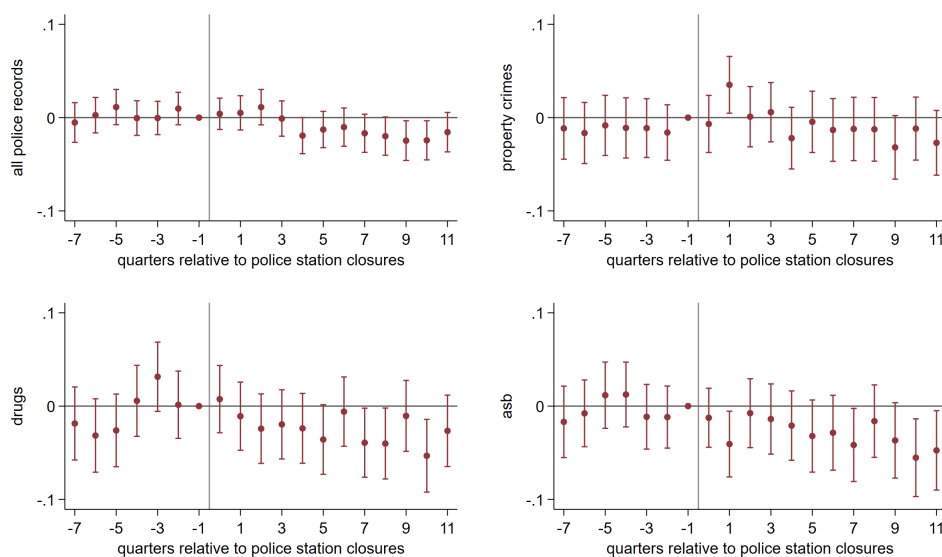
Note: Panel (a) shows the correlation between residualized distance to the nearest operating police station and average response time at the LA level. Panel (b) shows the correlation between residualized average response time and the (log) volume of detected offenses at the LA level. In both panels, variables are residualized after absorbing LA fixed effects. Response time data come from MPS administrative records, which are only available at the police-division level. The sample covers 2011–2016 and excludes the City of London and Westminster.

Figure A3: Dynamic Effects of Police Station Closures on Violent Crime and Clearance



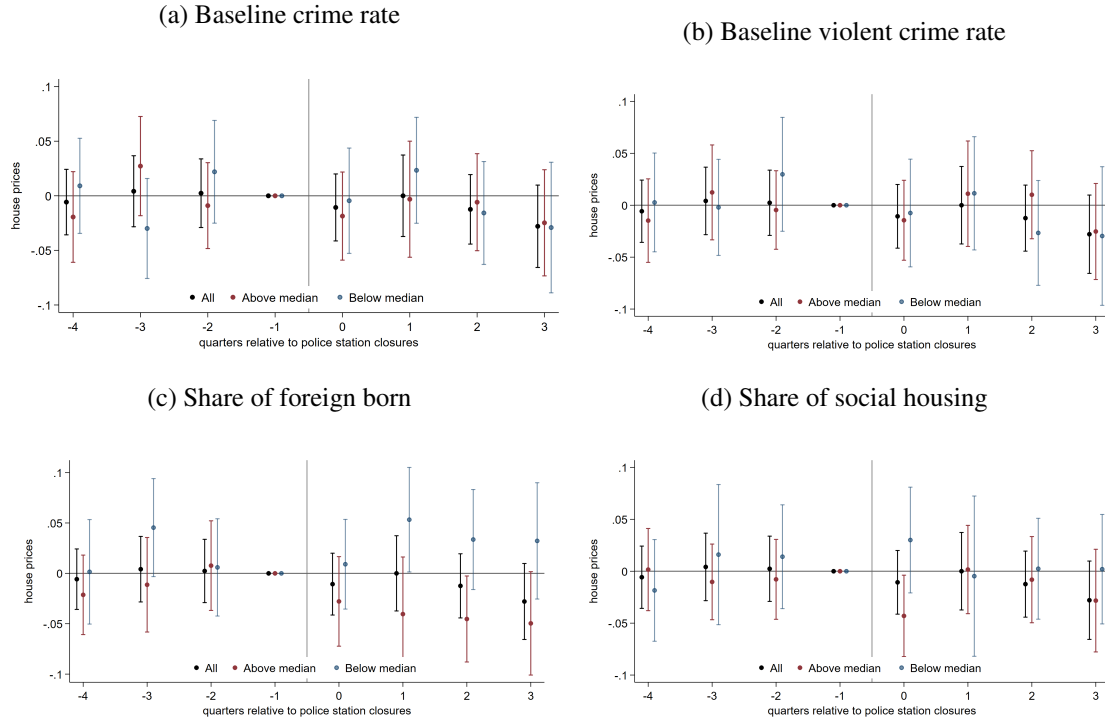
Note: The panels report robustness event-study estimates of the effect of police station closures on violent crime and clearance outcomes, based on police records. All estimates follow the baseline specification, including census block and date fixed effects, stacked dataset-specific fixed effects, and workforce-per-station, defined as the total number of officers in the LA divided by the number of operating stations and scaled so that one unit corresponds to 10 officers. Panel (a) presents alternative specifications: (i) a balanced sample of blocks spanning 7 periods before and after closures; (ii) binned relative periods before -9 and after $+11$; and (iii) estimates following [Borusyak et al. \(2024\)](#). Panel (b) restricts the sample to blocks that never experienced a closure to assess indirect effects; these estimates are not stacked over groups. Panel (c) uses the incident-level probability of clearing a crime as the dependent variable. Standard errors are clustered at the census block level, and error bars show 95% confidence intervals.

Figure A4: Dynamic Effects of Police Station Closures on Police Records



Note: The figure reports event-study estimates of the effect of police station closures on police records. The dependent variables are the log transformation of crime counts, with zeros replaced by a constant (0.025) to account for the extensive margin. Estimates are obtained excluding distant relative periods (Sun and Abraham, 2021). The specification includes census block and date fixed effects and controls for a workforce per station, defined as the total number of officers in the LA divided by the number of operating stations and scaled so that one unit corresponds to 10 officers. All fixed effects are interacted with stacked-dataset indicators. Standard errors are clustered at the census block level, and error bars show 95% confidence intervals.

Figure A5: Dynamic Effects of Police Station Closures on Local House Prices



Note: The panels report robustness event-study estimates of the effect of police station closures on house prices. The dependent variable is the average log house price at the census block level, with observations weighted by the number of sales in the block-quarter cell. All estimates follow the baseline specification, including census block and date fixed effects, stacked dataset-specific fixed effects, and a workforce-per-station control. Estimates exclude distant relative periods following [Sun and Abraham \(2021\)](#). Each panel reports coefficients for the full sample and separately for blocks with baseline characteristics above and below the London median. Standard errors are clustered at the census block level, and error bars show 95% confidence intervals.

A2 Additional tables

Table A1: Pre-Closure Characteristics and Police Station Closures

	All census blocks				Census blocks with police stations in 2010			
	Police station presence		Police station ever closed		Police station ever closed		Police station ever sold	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log all crimes	0.067*** (0.007)		0.026*** (0.006)		-0.243*** (0.058)		-0.133* (0.064)	
log violent crimes		0.046*** (0.008)		0.021** (0.007)		-0.322** (0.103)		-0.323* (0.123)
log property crimes		0.008 (0.008)		0.008 (0.006)		0.259* (0.095)		0.131 (0.093)
log drug-related offences		0.012** (0.004)		-0.004 (0.003)		-0.268*** (0.053)		-0.075 (0.054)
log house prices		0.046*** (0.011)		0.012 (0.008)		-0.479** (0.150)		0.053 (0.219)
Observations	13584	7718	13584	7718	414	251	414	251
N. treated blocks	4,701	4,701	4,701	4,701	147	147	147	147
FE	LAXYear	LA	LAXYear	LA	LAXYear	LA	LAXYear	LA

Note: Panel A uses the full sample of census blocks; Panel B restricts the sample to blocks with an operating station at the beginning of the period. The dependent variables are indicators for station presence and closure (Panel A), and for station presence and sale (Panel B). All explanatory variables are measured prior to any closures, using 2008-2010 crime data (from MPS Historical Crime Data collection) and 2006-2010 average house prices. Odd columns include LA fixed effects, and even columns LA×year fixed effects. Standard errors are clustered at the census block level. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table A2: Police Station Closures and Investigation Outcome Selection

	Sample: All incidents Pr(non-missing investigation outcome)			
	(1)	(2)	(3)	(4)
Panel A: binary treatment				
dummy distance	0.000 (0.001)	0.001 (0.001)	0.000 (0.001)	0.001* (0.000)
Panel B: continuous distance				
distance	0.000 (0.001)	-0.000 (0.000)	0.000 (0.001)	-0.000 (0.000)
Observations	3,346,332	3,346,332	3,346,332	3,346,332
Mean Dep. Variable	0.873	0.873	0.873	0.873
Date, LSOA FE	✓	✓	✓	✓
LAXDate FE		✓		✓
Crime type FE			✓	✓

Note: This table tests whether police station closures affect the likelihood that an incident has a recorded investigation outcome. The sample includes all incidents with a valid crime identifier. The dependent variable is an indicator for having a non-missing investigation outcome. All specifications include LSOA and date fixed effects and control for the workforce-per-station, with some models adding LA×date and crime-type fixed effects. Standard errors are clustered at the census block level. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table A3: House Price Effects by Property Characteristics

	Dep. var.: 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)
<i>Panel A: binary treatment</i>												
dummy distance	0.001 (0.010)	-0.028** (0.011)	-0.025** (0.010)	-0.011 (0.013)	-0.027*** (0.010)	0.001 (0.010)	0.000 (0.009)	-0.028*** (0.010)	-0.016 (0.011)	-0.026** (0.012)	-0.026** (0.011)	-0.009 (0.012)
<i>Panel B: continuous distance</i>												
distance	0.001 (0.008)	-0.010 (0.007)	-0.017** (0.007)	0.002 (0.010)	-0.010 (0.007)	-0.008 (0.010)	-0.007 (0.008)	-0.010 (0.007)	-0.009 (0.008)	-0.013 (0.009)	-0.010 (0.008)	-0.004 (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: This table reports heterogeneous effects of police station closures on house prices by property characteristics. The dependent variable is the log average house price, with observations weighted by the number of sales in the quarter–census block cell. Property characteristics are sourced from Energy Performance Certificate (EPC) data and measured at baseline (2008-2012). Each pair of columns splits the sample by whether the census block lies above or below the London median for the relevant characteristic. Characteristics include the number of habitable rooms, floor height, share of flats, share of homes, share of historical buildings (pre-1950), and share of renters in social housing. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table A4: Effects of Police Station Closures by Post-Closure Use

	House prices (weighted by the number of transactions)				Assaults and murders	Property crimes
	(1)	(2)	(3)	(4)	(5)	(6)
dummy distance	-0.009 (0.010)	-0.016 (0.011)	-0.009 (0.010)	-0.017 (0.011)	0.088*** (0.017)	-0.029*** (0.011)
dummy distance * I[Sale]	-0.008 (0.010)	-0.005 (0.011)	-0.032** (0.014)	-0.034** (0.015)	0.044 (0.028)	-0.022 (0.017)
dummy distance * I[Residential]			0.035*** (0.013)	0.041** (0.016)	-0.047 (0.029)	0.036** (0.018)
Observations	62,893	62,883	62,893	62,883	338,472	338,472
Date, LSOA FE	✓	✓	✓	✓	✓	✓
LAXDate FE		✓		✓	✓	✓

Note: This table reports heterogeneous effects of police station closures by the post-closure destination of the station building. Columns 1-4 use quarterly house price data, weighted by the number of sales in the quarter-census block cell; the dependent variable is log average house prices. Columns 5–6 use police records, where the dependent variables are the log transformation of crime counts, with zeros replaced by 0.025 (violent and property crimes, with property crimes excluding burglaries). Regressors are the closure indicator and its interaction with indicators for whether the closest police station was sold, or sold and subsequently converted into residential property. Specifications include LSOA and date fixed effects, with odd-numbered columns including the workforce-per-station control. Standard errors are clustered at the census block level. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Appendix B Additional background material

B1 Austerity in the UK

Following the 2010 general election, the newly formed Conservative-Liberal Democrat coalition government launched a comprehensive spending review. The scale of the planned adjustment was among the largest in the OECD, amounting to roughly 5% of GDP between 2010 and 2015, compared with an average of 4% in the euro area (Chandler and Johnson, 2015).

As documented by Fetzer (2019), the austerity program included three main components: (i) substantial cuts to local government funding, (ii) a nominal freeze on public-sector wages, and (iii) a major reform of the welfare system enacted through the *Welfare Reform Act 2012*, which reduced spending on welfare and social protection by around 16% in real terms. The first component directly affected the provision of local services, which in England are largely delivered by LAs responsible for education, waste collection, social housing, and welfare provision. Central government transfers to local councils fell by approximately 55% in real terms between 2010-11 and 2019-20. Cuts were unevenly distributed across space: LAs more dependent on central grants were typically the more deprived areas, and experienced the largest reductions. Because London has a high concentration of such deprived areas, it faced one of the steepest regional declines in funding (around 33%, Ogden and Phillips (2024a)). These reductions translated into widespread cuts to local services, with average spending across all local functions declining by 21%, and net spending per person falling most sharply in culture (-52%), housing (-52%), and transport (-42%, Ogden and Phillips (2024b)). This included for instance also closures of community facilities such as youth clubs (Villa, 2024), libraries, and Sure Start centers.

B2 Police Station closures across forces

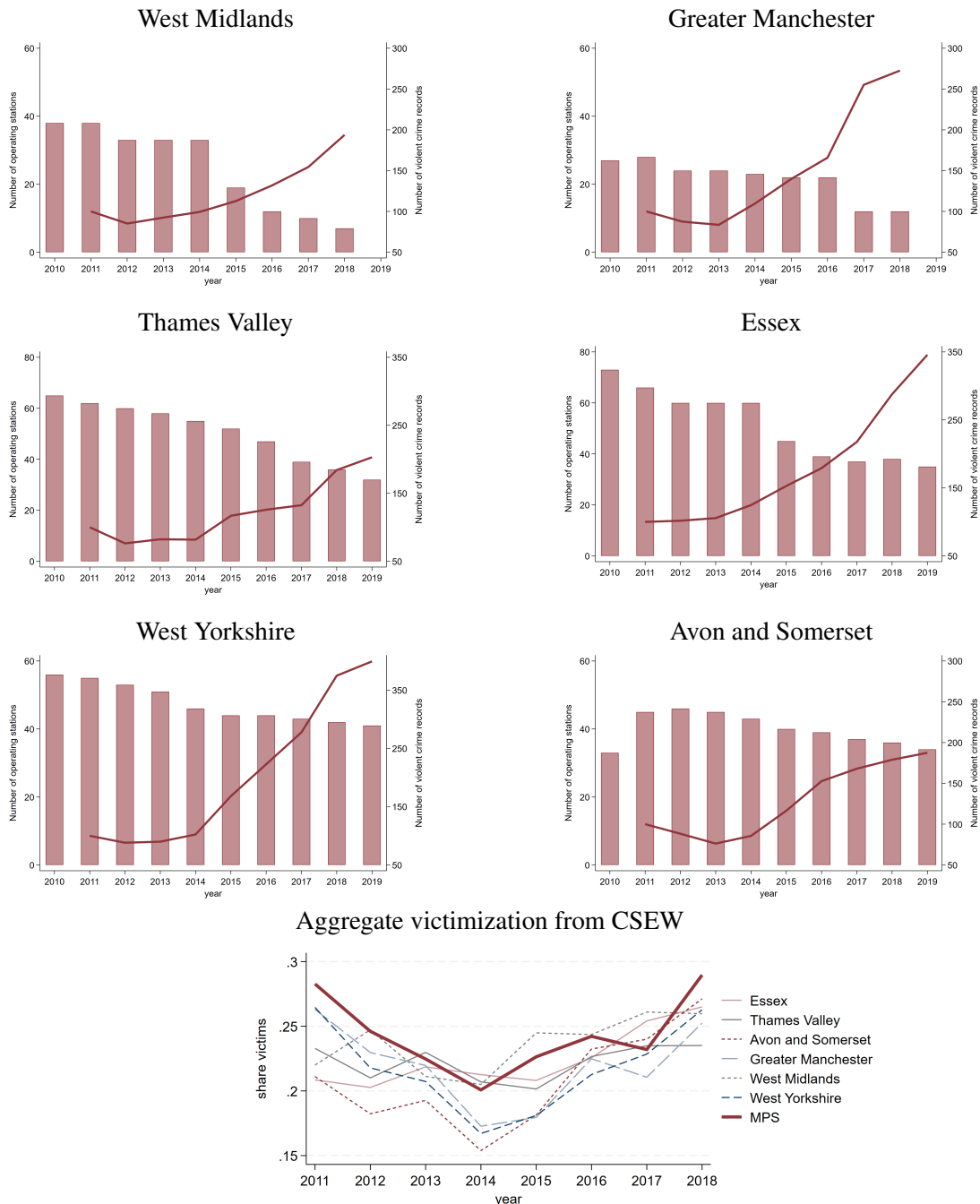
Police budgets were reduced by about 20% in real terms following the 2010 Spending Review (HMIC, 2011b,a). In London, the police had to save £600 million between 2012 and 2016 (MOPAC, 2013) and a further £400 million by 2022 (MOPAC, 2017).

These cuts were not confined to the MPS but all 39 independently administered police forces in England experienced similar budget reductions, triggering a wave of police station closures nationwide. In total, around 600 of approximately 900 stations closed between 2010 and 2018 (Pratt, 2019).

In light of this, I assembled information on police station closures for several of the largest forces in England in order to provide additional descriptive evidence. There are 39 independently administered police forces in England. I focus on Greater Manchester, West Midlands, West Yorkshire, Thames Valley, Avon and Somerset, and Essex. Each of these forces accounts for approximately 3–5% of recorded crime, and together they cover roughly 25% of all police records (while the MPS alone accounts for about 20% of recorded crime in 2010/11). As forces are independently administered and maintain their own records, I collected these data by filing separate Freedom of Information requests to each force.

All six forces closed substantial shares of their stations between 2010 and 2018: West Midlands 82%, Greater Manchester 56%, Essex 52%, Thames Valley 51%, West Yorkshire 27%, and Avon and Somerset 27%. Figure B1 plots aggregate violent-crime trends for these forces alongside the timing of their closures. In the bottom panel of the figure I also plot average victimization rate across these police forces using aggregate CSEW data. The patterns are consistent with increases in violent crime following closures, suggesting that the mechanisms identified in London are also present in other large police forces.

Figure B1: Number of Operating Stations and Aggregate Crime Trends by Police Force



Note: In the top panels, the left y-axis reports the number of operating stations at the end of each calendar year, the right y-axis reports the number of recorded violent crimes, normalized to 100 in 2011 (the first year with available data). Crime data come from each police force's police records. Data on station numbers come from Freedom of Information requests submitted to individual police forces. The bottom panel reports the average victimization rate by police force from aggregate CSEW data from 2011-12 to 2018-19.

Appendix C Robustness checks

I provide a range of robustness checks to address various concerns.

Alternative transformations of crime and clearance. Crime data at fine geographic levels contain many zeros. In the paper I add a constant $c = 0.025$ to crime observations equal to zero in order to apply the log-transformation in OLS regressions. This section justifies that choice and reports robustness tests.

Following [Chen and Roth \(2024\)](#) and [Mullahy and Norton \(2024\)](#), transformations such as the inverse hyperbolic sine (*asinh*) or $\log(y + 1)$ can yield scale-dependent estimates: rescaling the outcome changes the magnitude of coefficients, though not their statistical significance. As a result, these estimates cannot be interpreted as elasticities. Intuitively, such transformations implicitly mix the extensive and intensive margins, complicating interpretation especially when policies affect the probability of positive outcomes. [Chen and Roth \(2024\)](#) formalize this issue as a trilemma: when the outcome includes zeros, no transformation (other than the log applied to strictly positive y) can simultaneously satisfy (i) scale invariance, (ii) interpretation as the average of individual-level treatment effects, and (iii) point identification. Applied researchers must therefore decide which property to relax, depending on their object of interest.

My preferred approach follows [Chen and Roth \(2024\)](#), who propose a transparent calibration that assigns an explicit weight to the extensive margin. Intuitively, the treatment of zeros can be interpreted as how much “utility gain” is assigned to generating the first unit (extensive margin) versus increasing strictly positive outcomes (intensive margin). Formally, Chen and Roth propose the following transformation $m(y)$:

$$m(y) = \begin{cases} \log(y) & \text{if } y > 0 \\ -x & \text{if } y = 0 \end{cases}$$

where x determines the gain from moving from zero to the minimum positive value of y . For instance, setting $x = 0.1$ implies that the treatment effect assigns a value of 10 log points (approximately 10%) to a change between 0 and 1.³⁶

³⁶This transformation is only meaningful if the support of y excludes the interval $(0, e^{-x})$; otherwise

Table C1: Robustness to Alternative Transformations of Crime and Clearance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Dep. var: Assaults and murders										
Transformation: $\log(y)$, - x if $y > 0$										
	$\mathbb{1}(crime > 0)$		$x = 0.01$		$x = 0.02$		$x = 0.025$		$x = 0.03$	
dummy distance	0.027*** (0.005)	0.033*** (0.005)	0.088*** (0.013)	0.094*** (0.013)	0.088*** (0.013)	0.094*** (0.013)	0.089*** (0.013)	0.094*** (0.013)	0.089*** (0.013)	0.095*** (0.013)
t-stat	5.10	6.31	7.02	7.44	7.02	7.44	7.02	7.44	7.03	7.44
log(distance)	0.014*** (0.004)	0.015*** (0.004)	0.040*** (0.011)	0.036*** (0.011)	0.040*** (0.011)	0.036*** (0.011)	0.040*** (0.011)	0.036*** (0.011)	0.040*** (0.011)	0.036*** (0.011)
t-stat	3.30	3.66	3.69	3.40	3.70	3.41	3.70	3.41	3.70	3.41
Observations	337,794	337,794	337,794	337,794	337,794	337,794	337,794	337,794	337,794	337,794
Panel B. Dep. var: Clearances										
Transformation: $\log(y)$, - x if $y > 0$										
	$\mathbb{1}(crime > 0)$		$x = 0.01$		$x = 0.02$		$x = 0.025$		$x = 0.03$	
dummy distance	-0.025*** (0.004)	-0.021*** (0.004)	-0.060*** (0.007)	-0.057*** (0.007)	-0.060*** (0.007)	-0.058*** (0.007)	-0.061*** (0.007)	-0.058*** (0.007)	-0.061*** (0.007)	-0.058*** (0.007)
t-stat	-6.22	-5.32	-8.41	-7.87	-8.41	-7.87	-8.42	-7.87	-8.42	-7.87
log(distance)	-0.014*** (0.003)	-0.010*** (0.003)	-0.028*** (0.006)	-0.023*** (0.006)	-0.028*** (0.006)	-0.023*** (0.006)	-0.028*** (0.006)	-0.023*** (0.006)	-0.028*** (0.006)	-0.023*** (0.006)
t-stat	-4.27	-3.00	-4.61	-3.80	-4.62	-3.80	-4.62	-3.80	-4.63	-3.80
Observations	337,794	337,794	337,794	337,794	337,794	337,794	337,794	337,794	337,794	337,794
Panel C. Dep. var: Assaults and murders										
	Crime in levels		Crime per capita (per 1,000 ind.)		Power function $Crime^{0.5}$		$\mathbb{1}(crime > 75pct)$			
dummy distance	0.322*** (0.051)	0.412*** (0.058)	0.167*** (0.029)	0.217*** (0.032)	0.094*** (0.015)	0.120*** (0.016)	0.036*** (0.007)	0.047*** (0.007)		
t-stat	6.25	7.09	5.86	6.75	6.22	7.63	5.23	6.87		
log(distance)	0.131*** (0.042)	0.158*** (0.046)	0.063*** (0.024)	0.081*** (0.025)	0.041*** (0.013)	0.048*** (0.013)	0.018*** (0.006)	0.021*** (0.006)		
t-stat	3.09	3.46	2.67	3.20	3.20	3.69	3.23	3.76		
Observations	337,794	337,794	337,794	337,794	337,794	337,794	337,794	337,794		
Date, LSOA FE	✓		✓		✓		✓			
LAXDate FE		✓		✓		✓		✓		

Note: This table reports robustness checks using alternative transformations of crime and clearance outcomes. Panel A and Panel C present results for violent crime, while Panel B reports results for clearance. Odd-numbered columns include block and date fixed effects and control for police workforce; even-numbered columns additionally include LA×date fixed effects. For each specification, the table reports both coefficients and corresponding t-statistics. Standard errors are clustered at the census block level. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

I implement this approach by replacing zeros with a constant x corresponding to the estimated effect on the extensive margin. In my data, this effect is approximately 0.03 for the discrete treatment indicator and 0.02 for the continuous distance measure (Panel A, columns 1-2). I therefore set $x = 0.025$ and test alternative values; both coefficients and t -statistics remain stable. I perform the same exercise for clearances (Panel B) with similarly robust results.

In Panel C, I further compare results with outcomes in levels (columns 1-2) and $m(y)$ would be non-monotonic in y over its support (Chen and Roth, 2024).

normalized, such as crimes per capita (columns 8-9). Both approaches preserve point identification but sacrifice scale invariance. I also report results using power-function transformations (y^k , with $k = 0.5$), which are scale-equivariant [Thakral and Tô \(2023\)](#), as t-statistics (though not coefficients) remain stable under rescaling, thereby relaxing Chen and Roth's strict scale-invariance condition. These transformations include, as special cases, levels ($k = 1$) and the linear probability model ($k = 0$), the latter isolating the extensive margin. Finally, I also consider an alternative extensive-margin definition using a high-crime indicator (above the 75th percentile; columns 7-8). These specifications yield interpretable percentage-point effects but capture parameters distinct from elasticities.

Alternative definitions of distance. First, I vary the definition of distance to the nearest station. In columns 1–4 of Table [C2](#) 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 compute travel distances and times as the fastest road network commuting route using the OSMnx Python package, which interacts with OpenStreetMap APIs. The average travel speed for vehicles in London is 36 km per hour. 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.

Table C2: Robustness to Alternative Distance Measures

	Dep. var.: Assaults and murders									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
log(commuting distance)	0.056*** (0.013)	0.046*** (0.013)								
log(commuting time)			0.055*** (0.014)	0.047*** (0.013)						
dummy distance across LAs					0.086*** (0.012)	0.076*** (0.012)				
log(distance across LAs)							0.043*** (0.012)	0.037*** (0.011)		
log(ITT distance)									0.042*** (0.011)	0.035*** (0.011)
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: This table reports robustness checks using alternative measures of distance to the nearest police station. Columns 1-2 use commuting distance; columns 3-4 use commuting time, both constructed from OpenStreetMap data. Columns 5-8 use geodesic distance to any closest station, including stations in other police divisions. Columns 9-10 use baseline distance to stations that subsequently opened, estimated as an intention-to-treat measure. All specifications use the log transformation of crime counts, with zeros replaced by 0.025, and control for workforce-per-station. Standard errors are clustered at the census block level. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Alternative measures of police workforce. I distinguish officers by function, separating front-line officers, who are directly responsible for emergency response, from civilian staff employed by the MPS. Table C3 shows that the crime-reducing effect is driven entirely by front-line officers: staff numbers have no deterrent effect and, if anything, enter with the opposite sign. I also compare specifications that use levels versus logs of officer numbers (columns 1-2 and 4-5). The coefficients on the level of officers are statistically significant, whereas those on the log transformation are not. This pattern is consistent with diminishing marginal returns to scale: one additional officer has a larger marginal effect than a 1% increase in officer numbers.

Table C3: Robustness to Alternative Measures of Police Workforce

	Dep. var.: Assaults and murders					
	(1)	(2)	(3)	(4)	(5)	(6)
dummy distance	0.089*** (0.013)	0.072*** (0.013)	0.091*** (0.012)			
log(distance)				0.040*** (0.011)	0.027** (0.011)	0.040*** (0.011)
N. officers	-0.005*** (0.001)			-0.004*** (0.001)		
log(N. officer)		0.004 (0.020)			0.023 (0.021)	
N. frontline officers			-0.011*** (0.001)			-0.010*** (0.001)
N. staff			0.058*** (0.006)			0.060*** (0.006)
Observations	338,472	338,472	333,771	337,794	337,794	333,093
Mean Dep. Variable	2.48	2.48	2.48	2.48	2.48	2.48
Date, LSOA FE	✓	✓	✓	✓	✓	✓

Note: This table reports estimates of the robustness checks using alternative measures of police workforce. Front-line officers include constables and detective constables engaged in patrolling and emergency response. Staff roles include administrative and clerical jobs performed by civilians, encompassing support functions (e.g., finance, HR, training), middle-office tasks such as intelligence processing, and control-room operations. All regressions include block and date fixed effects. Standard errors are clustered at the census block level. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Local spillovers. 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, exclude them from the sample, and repeat the analysis using as a control group only “landlocked” blocks (i.e. blocks not bordering with any treated areas). Table C4 shows that the estimates are unchanged.

Table C4: Effects of Police Station Closures on Crime Types, Excluding Bordering Blocks

	Assaults and murders	Robberies	Burglaries	Theft	Property crimes (excl. burglaries)	Drugs-related offences
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: binary treatment						
dummy distance	0.094*** (0.014)	0.011*** (0.003)	0.001 (0.005)	-0.073*** (0.011)	-0.034*** (0.009)	-0.013*** (0.004)
Panel B: continuous distance						
log(distance)	0.028** (0.011)	0.000 (0.002)	0.009** (0.004)	-0.022*** (0.008)	-0.013* (0.007)	-0.004 (0.003)
Observations	277,818	277,818	277,818	277,818	277,818	277,818
Mean Dep. Variable	2.50	0.62	1.58	0.04	5.22	0.67
Date, LSOA FE	✓	✓	✓	✓	✓	✓
LAXDate FE	✓	✓	✓	✓	✓	✓

Note: Table notes follow from Tables 2 and 5. The sample includes treated blocks and “landlocked” blocks, and excludes 833 bordering blocks. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Local confounders. An additional concern is that the effects are confounded by other austerity cuts that lowered the provision of local public goods. Estimates in Table C5 show no differential effects on violent crime based on the incidence of austerity cuts to other welfare expenditures. The measure of welfare cuts is borrowed from Beatty and Fothergill (2013), and was used in Fetzer (2019) and is computed as the financial loss per working-age adult in an LA and year. 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 would not be enough to control for this if a census block’s treatment status is also determined by its pre-determined 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 C5 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 C6).

Table C5: Robustness to Local Confounders and Baseline Potential Exposure

	Dep. var.: Assaults and murders					
	LAs by incidence of austerity:		(3)	(4)	(5)	(6)
	Above median	Below median				
	(1)	(2)				
Panel A: binary treatment						
dummy distance	0.095*** (0.019)	0.093*** (0.017)	0.092*** (0.013)	0.087*** (0.013)	0.086*** (0.013)	0.088*** (0.013)
Panel B: continuous distance						
log(distance)	0.033** (0.016)	0.039*** (0.014)	0.037*** (0.011)	0.032*** (0.011)	0.029*** (0.011)	0.030*** (0.011)
Observations	165,744	172,050	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: This table reports robustness checks for potential local confounders. Columns 1–2 split the sample by above- and below-median exposure to austerity cuts, measured as the annual financial loss per working-age adult in each LA. Columns 3–6 address differential baseline exposure to treatment. The initial number of stations is computed as the number of police stations operating in 2008 within 2, 3, 4, and 5 km of each census block centroid. Columns 3, 5 interact this baseline exposure with a linear annual time trend; columns 4, 6 interact it with a linear monthly time trend. All regressions follow the specification and notes of Table 2. Standard errors are clustered at the census block level. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table C6: Conley standard errors

	Dep. var.: Assaults and murders											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: binary treatment												
dummy distance	0.089*** (0.005)	0.094*** (0.005)	0.089*** (0.005)	0.094*** (0.005)	0.089*** (0.005)	0.094*** (0.005)	0.089*** (0.006)	0.094*** (0.005)	0.089*** (0.006)	0.094*** (0.006)	0.089*** (0.006)	0.094*** (0.006)
N. officers	-0.005*** (0.000)		-0.005*** (0.000)		-0.005*** (0.000)		-0.005*** (0.000)		-0.005*** (0.000)		-0.005*** (0.000)	
Panel B: continuous distance												
log(distance)	0.040*** (0.004)	0.036*** (0.004)	0.040*** (0.004)	0.036*** (0.004)	0.040*** (0.004)	0.036*** (0.004)	0.040*** (0.005)	0.036*** (0.004)	0.040*** (0.005)	0.036*** (0.004)	0.040*** (0.005)	0.036*** (0.005)
N. officers	-0.004*** (0.000)		-0.004*** (0.000)		-0.004*** (0.000)		-0.004*** (0.000)		-0.004*** (0.000)		-0.004*** (0.001)	
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
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
Date FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
LAxDate FE		✓		✓		✓		✓		✓		✓
Cut-off (km)	0.5	0.5	0.75	0.75	1	1	1.5	1.5	2	2	3	3

Note: Table notes follow Table 2. Standard errors are computed using Conley standard errors. Census block coordinates are specified in latitude–longitude degrees, and the spatial kernel cut-off is expressed in kilometers, ranging from 0.5 km 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). *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Police station custody function. I examine changes in a non-visible police station input: custody capacity. In London, only some stations have custody cells, which are

locked rooms designed for short-term detention. When an arrest is made at a station without custody, the detainee must be taken to the nearest station with available cells. Filing FOI requests from the MPS and MOPAC, I construct a panel of custody suites and cell capacity over time. The number of stations with custody suites fell from 75 in 2008 to 23 by the end of 2018, and total cell capacity roughly halved (from approximately 1,100 to 550 cells). A custody suite can close even if the station itself remains open and publicly visible, so this captures a change in operational capacity without necessarily changing visible police infrastructure or distance to a station. Table C7 shows that the closure of custody suites is associated with additional increases in violent crime, for both direct and indirect effects, with magnitudes similar to those of station closures. This evidence is hard to reconcile with a story based only on perceived police presence. If salience were the only channel, we would expect only very visible changes, such as police building closures, to matter for crime. Instead custody suite closures, which are largely invisible to residents, still leads to more crime. This suggests that proximity and operational capacity play a central role, over and above salience alone.

Table C7: Effects of Police Station and Custody Suite Closures

	Dep. var.: Assaults and murders			
	(1) Direct effects	(2)	(3) Indirect effects	(4)
dummy distance	0.088*** (0.013)			
log(distance)		0.039*** (0.011)		
dummy near × Post			-0.218*** (0.019)	
log(1/distance) × Post				-0.213*** (0.016)
closed custody suite	0.041*** (0.010)	0.042*** (0.010)	0.049*** (0.016)	0.051*** (0.015)
Observations	338,472	337,794	181,008	181,008
Date, LSOA FE	✓	✓	✓	✓

Note: This table reports effects of police station closures and custody suite closures on violent crime. Columns 1-2 report direct effects, and Columns 3-4 report indirect effects. Data on custody capacity were obtained through FOIA requests to the MPS. All specifications include LSOA and date fixed effects, and control for workforce per station and a dummy for the station having a custody suite. Standard errors are clustered at the census block level.

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Clearance rates. The observed reduction in clearance may simply reflect the smaller pool of reports to investigate. A decrease in deterrence and reporting may mechani-

cally 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 8–12 of Table 3 indicate that the total volume of both cleared violent and property crimes falls by approximately 2%. The observed decrease in cleared violent offenses unambiguously indicates a decline in police effectiveness. Table C8 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. 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.

Table C8: 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*** (0.001)	-0.004*** (0.001)	-0.004*** (0.001)	-0.005*** (0.001)	-0.001 (0.002)	-0.002 (0.002)	-0.002*** (0.001)	-0.002*** (0.001)	-0.003*** (0.001)	-0.003*** (0.001)	0.000 (0.001)	-0.001 (0.001)
Panel B: continuous distance												
log(distance)	-0.003*** (0.001)	-0.002*** (0.001)	-0.002*** (0.001)	-0.002** (0.001)	-0.003** (0.002)	-0.002 (0.002)	-0.001** (0.000)	-0.001 (0.000)	-0.001** (0.001)	-0.001* (0.001)	-0.000 (0.001)	0.000 (0.001)
Observations	336,445	336,445	323,636	323,636	280,277	280,277	336,445	336,445	323,636	323,636	280,277	280,277
Mean Dep. Variable	0.06	0.06	0.04	0.04	0.12	0.12	0.03	0.03	0.03	0.03	0.05	0.05
Date, LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Date, LAxDate FE		✓		✓		✓		✓		✓		✓

Note: Table notes follow 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 (excluding acquittals and discharges). *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Appendix D Model derivations

D1 Derivations of supply of crime

This section provides detailed derivations of Section 5.

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 an individual committing crime in block j is also Fréchet-distributed with shape parameter θ :

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

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

$$\begin{aligned} \lambda_j &= Pr[u_j \geq \max(u_i) \forall i \neq j] \\ &= \int_0^\infty \left(\prod_{i \neq j} F_i(u) \right) f_j(u) du \\ &= \int_0^\infty \left(\prod_{i \neq j} e^{-\phi_i u^{-\theta}} \right) \phi_j \theta u^{-\theta-1} e^{-\phi_j u^{-\theta}} du \\ &= \int_0^\infty \exp\left\{ - \left(\sum_i \phi_i \right) u^{-\theta} \right\} \phi_j \theta u^{-\theta-1} du \end{aligned} \quad (11)$$

Let $y = (\sum_i \phi_i) u^{-\theta}$, so that $dy = -(\sum_i \phi_i) \theta u^{-\theta-1} du$. Substituting yields

$$\begin{aligned} &= \frac{\phi_j}{\sum_i \phi_i} \int_{y=0}^{\infty} e^{-y} dy \\ &= \frac{\phi_j}{\sum_i \phi_i} \\ &= \frac{r_j^\theta (\pi_j S)^{-\theta}}{\sum_i r_i^\theta (\pi_i S)^{-\theta}}. \end{aligned}$$

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

$$\lambda_j = \frac{r_j^\theta \pi_j^{-\theta}}{\sum_i r_i^\theta \pi_i^{-\theta}}. \quad (12)$$

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 equals 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

$$\mathbb{E}[U] = \int_0^\infty u f(u) du = \int_0^\infty u \theta \phi u^{-\theta-1} e^{-\phi u^{-\theta}} du. \quad (13)$$

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

$$= \int_0^\infty \phi^{\frac{1}{\theta}} y^{-\frac{1}{\theta}} e^{-y} dy = \Gamma\left(\frac{\theta-1}{\theta}\right) \phi^{1/\theta}, \quad (14)$$

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

$$\mathbb{E}[U] = \Gamma\left(\frac{\theta-1}{\theta}\right) \phi^{1/\theta} = \gamma \left[\sum_i \left(\frac{r_i}{\pi_i S} \right)^\theta \right]^{1/\theta} = \bar{U}. \quad (15)$$

D2 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 $\{\theta, \alpha, \beta\}$. I then use the structure of the model to recover the location fundamentals.

First, taking logs, Equation (7) becomes

$$\log \pi_j = \log A_j - \alpha \log d_j + \beta \log P_j. \quad (16)$$

Adding a random error component and the time dimension, Equation (16) becomes a log-linear regression that can be estimated to retrieve the police inputs elasticities:

$$\log \pi_{jt} = \gamma_j + \gamma_t - \alpha \log d_{jt} + \beta \log P_{jt} + v_{jt}. \quad (17)$$

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 Equa-

tion (6) in logs, I derive the following expression for the supply of crime:

$$\log C_j = \theta \log r_j - \theta \log \pi_j - \log\left(\sum_i r_i^\theta \pi_i^{-\theta}\right) + \log(C). \quad (18)$$

Substituting π_j from Equation (16) in Equation (18), adding a random error component and the time dimension, I obtain a log-linear regression to estimate

$$\log C_{jt} = \phi_j + \phi_t + \delta \log d_{jt} - \nu \log P_{jt} + \varepsilon_{jt}. \quad (19)$$

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 (18) are constant and absorbed by the fixed effects.

While the structural model implies a log–log relationship in police workforce, in the empirical implementation I replace the term $\log P_{jt}$ with its first-order linear approximation around the average level of workforce. This yields a log–linear specification in P_{jt} , which preserves the sign and local comparative statics implied by the model while providing a more stable empirical fit. I report estimates in Table D1.

Table D1: Estimation of Clearance and Crime Flow Equations

	(log) Assaults and murders		(log) Cleared crimes	
	(1)	(2)	(3)	(4)
log ITT Distance	0.046*** (0.011)	0.043*** (0.011)	-0.031*** (0.006)	-0.029*** (0.006)
ITT N. front-line officers	-0.007*** (0.001)		0.002** (0.001)	
ITT N. officers		-0.004*** (0.001)		0.000 (0.001)
Observations	333,241	337,794	333,241	337,794
Mean Dep. Variable	2.31	2.31	2.31	2.31
Date, LSOA FE	✓	✓	✓	✓

Note: Columns 1–2 report estimates from the crime flow equation (Equation 19), and columns 3–4 report estimates from the clearance equation (Equation 17). Workforce is measured using either front-line officers (columns 1 and 3) or total officers (columns 2 and 4). All regressions include block and year fixed effects. Standard errors are clustered at the block level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Columns 3–4 report estimates of α and β from the clearance equation, while columns 1–2 report estimates of δ and ν from the crime flow equation. From these estimates, I recover the model’s substitution parameter as $\hat{\theta} = \hat{\delta}/\hat{\alpha}$ and $\hat{\theta} = \hat{\nu}/\hat{\beta}$. I use ITT distance and front-line officer measures to avoid capturing the impacts of the few station

openings and to isolate the response of crime to effective police strength. Preferred estimates come from columns 1 and 3. 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 (17). 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: I compute a ratio $\frac{\text{Var}(\log(r))}{\text{Var}(\log(A))}$ of around 9.

D3 Aggregate impacts

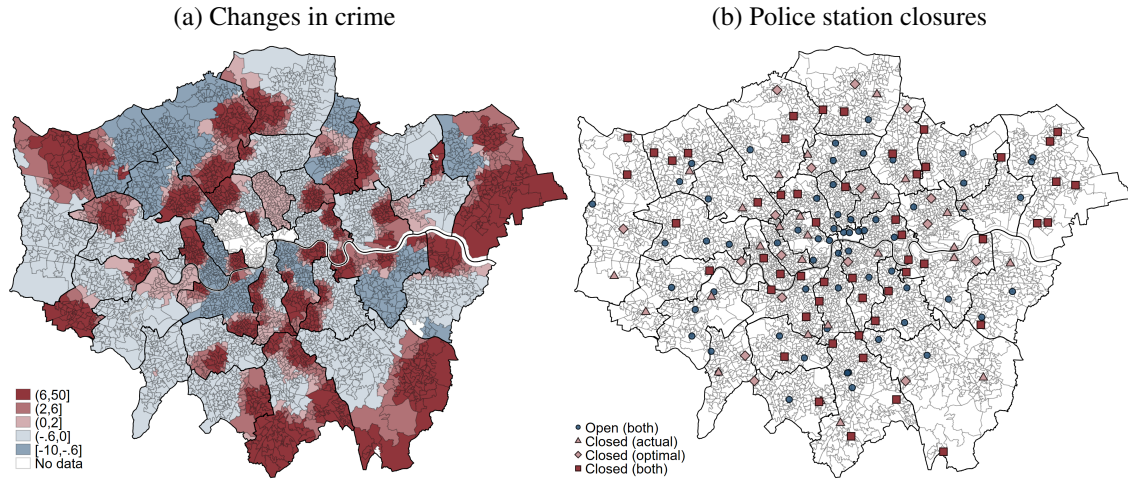
To quantify the aggregate effect on total violent crime, I compute each element in Equation (5) as follows. First, the pre-reform crime rate C_0 is computed using police records for 2011, and the baseline London population $\bar{L}$ from the 2011 Census. To measure the post-reform crime rate C_1 , I 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 (15), 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 D2: Aggregate effect on violent crime under different closure scenarios

Local authorities	Current policy	Optimal policy	Random policy
Average change in violent crime	1.79%	-1.43%	4.34%

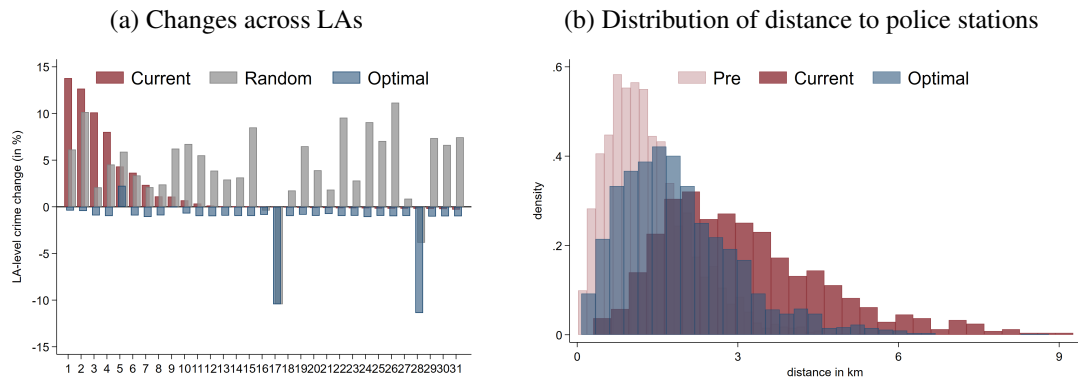
Note: The table summarizes the average aggregate change in crime predicted by the model under three scenarios: current (column 1), optimal (column 2), and a random policy scenario (column 3). It uses estimates for α , β , θ computed in Table D1. The current scenario reflects observed crime changes following 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 D2). Change in aggregate crime is expressed relative to the baseline scenario, representing the pre-closure configuration in 2012.

Figure D1: Maps of changes in crime and 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 D2: Changes in crime and distance under different policy scenarios



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 E Crime Survey of England and Wales

Table E1 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 2011-12 and 2016-17, 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. I use the restricted geo-referenced version of the CSEW that includes LSOA-level data 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 respondents, 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.

For a subsample of respondents, the survey includes additional questions on attitudes toward the police. The outcome variables are constructed from the following survey questions. First, victims are asked: (i) “Overall, were you satisfied or dissatisfied with the way the police handled the matter?”. I code the outcome as 1 if the respondent answers “Fairly satisfied” or “Very satisfied”. For the confidence in policing measures, outcomes are built from three additional 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?”. These outcomes are coded as 1 if the respondent answers “Not very confident” or “Not confident at all”. Due to the survey’s modular structure, not all questions are asked of all respondents. Questions regarding confidence in the police, the CPS, and the CJS are asked of 50% of London respondents (those completing Modules A and B of the CSEW), while the question on satisfaction with police handling of cases is asked to victims only. Questions related to antisocial behavior are asked of 25% of respondents (those completing Module A).

Table E1: Descriptive statistics from CSEW data

	mean	sd	N
Panel A: All respondents			
<i>Individual characteristics</i>			
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	0.830	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
<i>Victimization</i>			
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
<i>Local characteristics</i>			
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
<i>Outcomes</i>			
lack of confidence in police effectiveness	0.328	0.470	10,591
lack of confidence in CPS effectiveness	0.402	0.490	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	0.450	6,592
– theft	0.164	0.370	6,592
– violent crime	0.043	0.202	6,592
Satisfaction with police response	0.727	0.446	2,293

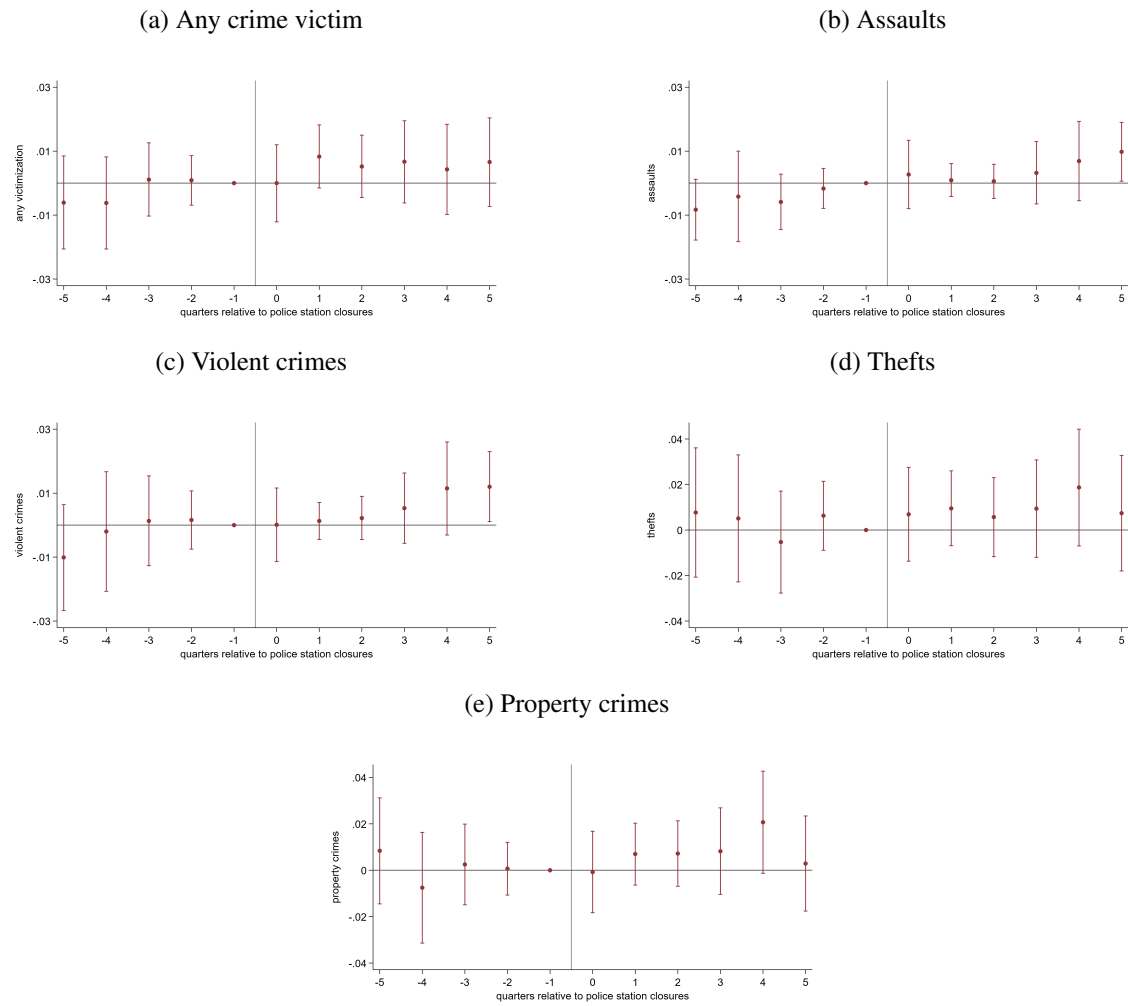
Panel A presents descriptive statistics for all respondents (victims and non-victims). Panel B provides descriptive statistics for incident-level outcomes. The sample is restricted to CSEW respondents residing in London (excluding City of London and Westminster) at the time of the survey interview, covering the period from 2011-12 to 2016-17.

Table E2: The Effects of Police Station Closures on ASB Experiences (CSEW)

Dep. var.:	Experiences of ASB:								
	Street drinking			Vandalism			Rowdy behavior		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Direct effects, binary treatment									
dummy distance	0.039 [0.036]	0.059 [0.036]	0.083** [0.038]	0.031 [0.026]	0.041 [0.027]	0.040 [0.028]	0.006 [0.031]	0.011 [0.031]	0.013 [0.033]
Panel B: Direct effects, continuous distance									
log(distance)	0.026 [0.026]	0.032 [0.027]	0.046* [0.027]	0.036* [0.021]	0.039* [0.020]	0.041** [0.021]	0.030 [0.024]	0.025 [0.023]	0.026 [0.024]
Observations	5,039	5,052	5,052	5,039	5,052	5,052	5,039	5,052	5,052
Mean dep. variable	0.35	0.35	0.35	0.14	0.14	0.14	0.22	0.22	0.22
Panel C: Indirect effects, binary treatment									
dummy near $\times$ Post	0.044 [0.052]	0.074 [0.053]	0.052 [0.055]	0.003 [0.032]	0.006 [0.032]	0.010 [0.034]	0.042 [0.043]	0.033 [0.045]	0.037 [0.047]
Panel D: Indirect effects, continuous distance									
log(1/distance) $\times$ Post	0.019 [0.040]	0.040 [0.041]	0.021 [0.042]	0.029 [0.024]	0.042* [0.024]	0.046* [0.026]	0.022 [0.033]	0.018 [0.032]	0.029 [0.035]
Observations	3,005	3,005	3,004	3,005	3,005	3,004	3,005	3,005	3,004
Mean dep. variable	0.37	0.37	0.37	0.15	0.15	0.15	0.24	0.24	0.24
Control for workforce	✓			✓			✓		
Date, LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
LA-specific trends		LA lin. trend	LA $\times$ Year FE		LA lin. trend	LA $\times$ Year FE		LA lin. trend	LA $\times$ Year FE

Note: This table reports estimates of the effect of police station closures on experiences of antisocial behavior (ASB), using CSEW data. The dependent variables are indicators for whether the respondent experienced or witnessed specific types of ASB, as defined in the CSEW questionnaire. Panels follow the structure of Table 4. All specifications include LSOA and quarterly date fixed effects. Columns 1, 4 and 7 additionally control for police workforce per station; columns 2, 5 and 8 instead add LA-specific linear time trends; columns 3, 6 and 9 replace these with LA-by-year fixed effects. Standard errors are clustered at the census block level. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Figure E1: Dynamic Effects of Police Station Closures on the Probability of Being a Crime Victim (CSEW)



Note: The figure reports event-study estimates of the effect of police station closures on the probability that a respondent is a victim of any crime, and by crime type, using CSEW data. Each panel plots coefficients from Equation (2) for the relevant outcome. All fixed effects are interacted with stacked-dataset indicators. Standard errors are clustered at the census block level, and error bars show 95% confidence intervals.

Appendix F Cost-effectiveness of police station closures

F1 Capitalization approach

I quantify the total net loss of value of house prices using a capitalization approach by combining the estimated losses in blocks near closed stations with the offsetting gains in blocks near surviving stations. I use point estimates reported in Table 7, column 2 Panel A for direct effects, Panel C for indirect effects. The change is £−9,143 per treated block-quarter in areas near closures and £5,775 near surviving stations, yielding a net capitalization loss of about £3,368 per treated block-quarter. 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 £12.3k saved per treated block-quarter. Taken together, the implied capitalization-to-savings ratio ranges from about 0.3 up to 0.6 in high-crime areas, meaning that for every £5 saved by the authority, roughly £1.5 to £3 is borne by homeowners through capitalization losses.

Table F1: Cost–benefit analysis using capitalization approach

	(1)	(2)
<i>Sample:</i>	All sample	High-crime areas
	Value (£)	
<i>Panel A: Police station closure costs</i>		
Direct effects:		
Average house prices in blocks near closed stations	454,488	448,888
Estimated decrease in price per treated block-quarter	-9,143	-11,382
Indirect effects:		
Average house prices in blocks near surviving stations	426,181	429,216
Estimated increase in price per treated block-quarter	5,775	4,624
Total net cost for private owners	-273,213,171	-175,737,214
<i>Panel B: Police station closure savings</i>		
Total savings	600 million	
Total savings per treated block	294,262	
Savings per treated block per treated block-quarter	12,261	
Cost/Benefit	-0.3	-0.6

Note: This table reports the cost–benefit calculations based on the capitalization approach. Column 1 uses the full sample; Column 2 restricts to blocks with baseline (2008) crime rate above the median. Panel A quantifies direct capitalization losses near closed stations and indirect gains near surviving stations, using the estimates from Table 7, column 2 (Panel A for direct effects; Panel C for indirect effects). Average house prices are computed in the pre-period (prior to 2013q2). Totals for direct effects are based on the number of blocks near closed stations (2,039 in the full sample and 957 in the high-crime subsample). Totals for indirect effects use the number of blocks near surviving stations (1,257 in the full sample and 772 in the high-crime subsample).

F2 Marginal value of public funds

I compute the marginal value of public funds (hereafter MVPF; [Hendren, 2016](#); [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 F2 summarizes the calculations of the MVPF of closing police stations.

Table F2: Calculation of the Marginal Value of Public Funds

	Direct effects			Net of indirect effects	
	Value (£)	Value (£)	Value (£)	Value (£)	Value (£)
	(1)	(2)	(3)	(4)	(5)
<i>Willingness to pay</i>					
Society's willingness to pay for additional crimes	226,534,878	226,534,878	226,534,878	226,534,878	91,048,648
Willingness to pay for worse labor market prospects by additional incarcerated individuals		277,792,707			
Loss in house capitalization			447,438,885	273,213,171	273,213,171
Aggregate WP (Social cost)	226,534,878	504,327,585	673,973,763	499,748,049	364,261,819
<i>Net cost to the government</i>					
Mechanical savings from the closures	-600,000,000	-600,000,000	-600,000,000	-600,000,000	-600,000,000
Fiscal externalities:					
- Fiscal costs of incarceration	787,768,389	787,768,389	787,768,389	787,768,389	277,061,337
- Foregone income tax revenues		29,305,884			
- Lower revenues from lower stamp duty land tax			13,423,167	8,196,395	8,196,395
- Fiscal savings of Police	-21,021,855	-21,021,855	-21,021,855	-21,021,855	-10,510,928
- Fiscal savings of Prison and Probation	-1,225,286	-1,225,286	-1,225,286	-1,225,286	-612,643
- Fiscal savings of CJS	-8,284,453	-8,284,453	-8,284,453	-8,284,453	-4,142,226
Net fiscal saving (if >0 means cost)	157,236,796	186,542,679	170,659,962	165,433,191	-330,008,065
Aggregate WP / Net Cost	1.44	2.70	3.95	3.02	1.10

Note: This table shows the calculations of the marginal value of public funds (MVPF). For a full derivation of these costs, please refer to Tables F1–F5.

I compute a MVPF ranging from 4 to 1.1. I detail all calculations below. One advantage of calculating this ratio is that it can be compared to the MVPF of other policy changes.³⁷ My estimate of the MVPF closely aligns with the MVPFs for policies targeting adults, such as food stamps, housing voucher, and cash welfare programs for low-income households (Hendren and Sprung-Keyser, 2020). 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 public resources, and potential long-term societal consequences due to a rise in criminal activity.

³⁷Note 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).

Willingness to pay. The numerator of the MVPF is the aggregate social willingness to pay (WTP). Column 1 of Table F2 first presents a conservative estimate that includes only the willingness to pay for additional crimes resulting from lower deterrence and incapacitation. First, a lower clearance rate encourages potential criminals to offend because of lower deterrence, therefore resulting in more crimes. I compute the resulting increase in non-deterred crimes in Table F3, column 2. In addition, I include the social costs generated by the increase in criminal activity itself, as well as the reduced costs that arise from lower reporting rates for certain crime types (as explained Section 4), computed in Table F4, column 2. To assign monetary values to these effects, I rely on the estimates for unit costs of crime computed in the Home Office Report (Heeks et al., 2018). For each crime category, they include 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). I combine the unit costs, shown in Table F3 column 1, with the estimates of the additional number of crimes committed (Table F4) and the additional number of non-deterred crimes due to lower clearance (Table F3). Note that the additional crimes from lower deterrence (Table F3) are conceptually distinct from the net change in recorded crimes in Table F4, which captures changes in underlying crime incidence and reporting behavior. Thus, these components do not overlap and can be added without double counting.

Table F3: Costs due to decreased clearance

Crime category	(1) Detected crimes in areas with closed stations (at baseline)	(2) Crimes non deterred	(3) Unit cost per crime (£)	(4) Cost of crime non- deterred (£)
Violent crimes	36,602	861	10,793	9,295,005
- <i>Violence against the person</i>	32,404	762	10,761	8,204,761
- <i>Robbery</i>	4,198	99	351	34,684
Property crimes	39,445	928	2,655	2,464,497
- <i>Domestic burglary</i>	8,740	206	5,930	1,219,487
- <i>Theft</i>	105	2	1,664	4,112
- <i>Criminal damage and arson</i>	7,329	172	1,505	259,584
Total	76,047	1,789	13,448	11,759,502

Note: The number of detected offenses by crime category are computed restricting the sample to the pre-period (i.e. before June 2013; columns 1). I omit other crime categories because there is no corresponding crime cost computed in the Home Office report (Heeks et al., 2018). Column 2 is computed multiplying (1) by 0.004 (the β increase in the clearance rate, from column 1 in Table 3) divided by 0.17 (the average clearance rate).

Table F4: Costs due to increased crime

Crime category	(1) Offences in areas with closed stations	(2) Extra crimes committed	(3) Cost extra crimes committed	(4) Probability of conviction	(5) Probability of incarceration	(6) Uk sentence length	(7) Extra years of incarceration	(8) Cost from extra incarceration
Violent crimes								
- <i>Violence against the person</i>	5,066.57	471.67	213,177,970	0.10	0.31	37	21,756	756,358,393
- <i>Robbery</i>	1,311.62	16.456	242,692	0.08	0.44	36	914	31,771,654
Property crimes								
- <i>Domestic burglary</i>	86.08	15.774	3,928,662	0.07	0.32	19	274	9,531,373
- <i>Theft</i>	1,620.55	-6.230	-435,456	0.02	0.30	4	-7	-251,161
- <i>Criminal damage and arson</i>	0.79	-33.825	-2,138,493	0.05	0.18	22	-277	-9,641,869
Total	442,140	0.027	214,775,376				22,659	787,768,389

Note: The number of offenses recorded by crime category are computed restricting the sample to the pre-period (i.e. before June 2013; columns 1). Column 2 uses estimates by crime type from Tables 2 and 5, and column 3 uses unit cost of crime from Heeks et al. (2018). The probabilities of conviction and incarceration (conditional on conviction) in columns 4 and 5 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).

Column 2 of Table F2 adds the willingness to pay for worsened labor market prospects for the individuals whose likelihood of incarceration increases following the increase

in violent crimes.³⁸ 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 (computed multiplying columns 1 × 2 × 4 × 5 in Table F4) × the employment rate × the annual median income × the average sentence served (Table F4 column 6). The total foregone income of affected individuals during incarceration is equal to £277 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 F1. Introducing this cost increases the aggregate willingness to pay to £670 million. Columns 4–5 then adjust these estimates for indirect gains from concentration, including increases in property values (Column 4) and lower crime rate (Column 5) near surviving stations.

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 F5 (columns 6–8). In column 2, I include 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 &

³⁸To be precise, 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.

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 sales tax imposed on 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. Finally, Columns 4 and 5 compute the net change in fiscal revenues once I account for the decrease in property sales is offset by higher prices in areas near surviving police stations, and the lower fiscal savings as deterrence improves, leading to fewer crimes in proximity of surviving stations.

Table F5: Criminal justice savings from decreasing clearance rate by β percentage points

Crime category	(1) Crimes non deterred	(2) Multiplier	(3) Police costs	(4) Prison & probation costs	(5) CJS costs	(6) Total police saved costs	(7) Tot. prison & prob. saved costs	(8) Total CJS saved costs
Violent crimes	861	7.08	2,976	100	949	18,154,354	609,981	5,790,658
- <i>Violence against the person</i>	762	7.25	3,094	314	861	17,105,493	1,737,626	4,758,632
- <i>Robbery</i>	99	4.30	1,010	1,260	2,420	428,986	535,171	1,027,868
Property crimes	928	3.11	993	213	863	2,867,502	615,306	2,493,794
- <i>Domestic burglary</i>	206	3.60	530	390	880	392,375	288,728	651,490
- <i>Theft</i>	2	3.86	5,244	487	3,238	50,068	4,652	30,921
- <i>Criminal damage and arson</i>	172	1.98	170	97	341	58,134	32,944	116,454
Total						21,021,855	1,225,286	8,284,453

Note: Column 1 corresponds to the number of crimes non-deterred computed in Table F3. The multiplier of each crime category (column 2) is obtained from the Home Office report (table 4 in [Heeks et al., 2018](#)). Columns 3-5 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).

Online Appendix References

- Beatty, Christina and Steve Fothergill**, “Hitting the poorest places hardest: The Local and Regional Impact of Welfare Reform,” Technical Report, Sheffield Hallam University 2013.
- Becker, Gary S**, “Crime and punishment: An economic approach,” in “The Economic Dimensions of Crime,” Springer, 1968, pp. 13–68.
- Blattman, Christopher, Donald P Green, Daniel Ortega, and Santiago Tobón**, “Place-based interventions at scale: The direct and spillover effects of policing and city services on crime,” *Journal of the European Economic Association*, 2021, 19 (4), 2022–2051.
- Borusyak, Kirill and Peter Hull**, “Nonrandom exposure to exogenous shocks,” *Econometrica*, 2023, 91 (6), 2155–2185.
- Chandler, David and Paul Johnson**, *The Coalition and the Economy*, Cambridge: Cambridge University Press, 2015.
- Chen, Jiafeng and Jonathan Roth**, “Logs with zeros? Some problems and solutions,” *The Quarterly Journal of Economics*, 2024, 139 (2), 891–936.
- Fetzer, Thiemo**, “Did austerity cause Brexit?,” *American Economic Review*, 2019, 109 (11), 3849–86.
- Heeks, Matthew, Sasha Reed, Mariam Tafsiri, and Stuart Prince**, *The Economic and Social Costs of Crime*, Home Office Research report, 2018.
- Hendren, Nathaniel**, “The policy elasticity,” *Tax Policy and the Economy*, 2016, 30 (1), 51–89.
- **and Ben Sprung-Keyser**, “A unified welfare analysis of government policies,” *The Quarterly journal of Economics*, 2020, 135 (3), 1209–1318.
- HMIC**, “Adapting to Austerity: A Review of Police Force and Authority Preparedness for the 2011/12–14/15 CSR Period,” 2011.
- , “Demanding Times: The front line and police visibility,” 2011.
- MOPAC**, “Estate Strategy 2013–2016, MOPAC/MPS,” Technical Report 2013.
- , “Public Access Strategy,” Technical Report 2017.
- Mullahy, John and Edward C Norton**, “Why transform Y? The pitfalls of transformed regressions with a mass at zero,” *Oxford Bulletin of Economics and Statistics*, 2024, 86 (2), 417–447.
- Ogden, Kate and David Phillips**, “How have English councils’ funding and spending changed? 2010 to 2024,” Technical Report, Institute for Fiscal Studies, London 2024.
- **and —** , “What is the Outlook for English Councils’ Funding?,” Technical Report, Institute for Fiscal Studies, London 2024.
- Pratt, Alison**, “Police Stations: Are They a Thing of the Past?,” Technical Report, House of Commons Library May 2019.
- Sun, Liyang and Sarah Abraham**, “Estimating dynamic treatment effects in event studies with heterogeneous treatment effects,” *Journal of Econometrics*, 2021, 225 (2), 175–199.
- Thakral, Neil and Linh T Tô**, *When are estimates independent of measurement units?*, Boston University-Department of Economics, 2023.
- Villa, Carmen**, “The Effects of Youth Clubs on Education and Crime,” Technical Report 24/51, Institute for Fiscal Studies, London 2024.