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

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

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Abstract

Utilizing newly compiled granular data on 7 million criminal incidents within a large police force, I examine the impact of police deployment on crime occurrence, reporting, police investigations, and overall citizens' welfare. Focusing on a wave of austerity cuts which resulted in the closure of 70% of the police stations (while preserving total police strength), I show that the closures persistently increased violent crimes in census blocks near the defunct stations. Consistent with lower deterrence and police effectiveness, I document reduced clearance rates, reduced reporting of non-violent offences, and lower local house prices in the most deprived areas. The policy appears not to be cost-effective.

JEL Classification: D29, K42, R53, H72.

Keywords: Austerity; Clearance; Crime; Police; Reporting.

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1 Introduction

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

The literature on crime prevention strategies underscores the critical importance of how police resources are allocated (Owens, 2020). To evaluate how well these allocations work, one would need to determine the extra benefits gained from investing an additional dollar in police resources. However, achieving this goal crucially depends on the availability of granular data to uncover the *black box* of the police production function, and to draw lessons on the optimal deployment of scarce resources (Cook and Ludwig, 2010).

In this paper, I show that police spending reductions have first-order impacts on crime prevention, and ultimately crime reporting and citizens' welfare. To do so, I study a natural experiment generated by a massive wave of police station closures in London. To comply with unprecedented centrally-imposed austerity cuts, the London police closed 70% of the stations, while they centralized police workforce from closed to surviving stations. As an immediate direct effect, the closures significantly increase the distance to the stations, affecting police deployment and, by means of slowing response time and reducing deterrence, hampering police effectiveness. Yet, the consolidation of police resources has the potential to yield efficiency gains. Additionally, changes in the perceived likelihood of solving crimes can influence victims' willingness to report. In this paper, I shed light on the ex-ante ambiguous net effects of such cuts on the police output and on the welfare consequences for the local communities.

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

Analyzing the impacts of police station closures on crime requires access to information on the exact location of offenses, victims and stations over time. I combine four extremely granular datasets. First, I geo-code all London police stations and collect their dates of closure. Second, I complement them with geo-referenced information on 7 million criminal incidents recorded by the London Metropolitan Police Service (MPS), with their occurrence 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 report exact residential locations and crime reporting attitudes of respondents.

One challenge to measuring the causal effect of police station closures is that such policy changes are not randomly assigned, but typically reflect deliberate policy choices. In the context of this study, police stations are originally located in persistently high-crime areas, and the closures affected stations located in relatively less deprived, low-crime blocks. To overcome these sources of endogeneity, I adopt a difference-in-differences strategy. I compare the number and composition of crimes in census blocks which experience a closure of the nearest police station (treatment) to blocks which do not (control), before and after the closure. In order to ensure that the impacts are not influenced by simultaneous reductions in local funding for other public services, I further control for time-varying attributes at the neighborhood level. I show that treated and control areas follow similar trends in outcomes prior to the closures. Because of the closures, the average distance to the nearest police station doubled, with the highest rise being experienced by areas located at baseline closer to the stations.

I present four classes of results. First, police station closures lower police deterrence and increase violence nearby closed stations. I estimate that in treated blocks violent crimes, measured as assaults and murders, increase by 11%. This impact is sudden and persists overtime, and shows that higher distance lowers police deterrence. However, this effect is non-linear in distance: the reduction in police deterrence is concentrated in blocks surrounding closed stations, and it gradually decays as distance increases. This evidence indicates that the impact is driven not only by reduced police visibility around closed stations but, more importantly, by longer response times. I rule out that the results are driven by criminals' displacement. I use a simple production function framework to discuss the various margins through which the policy affects police output. The relocation of front-line officers to surviving stations reduces violence near those stations, as more officers begin their shifts from there. Despite these positive indirect effects in the proximity of surviving stations, I estimate a significant overall net increase in violent offenses, highlighting the presence of dis-economies of scale in the police production function, where incremental losses at lower levels of policing dominates the gains at higher levels.

Second, the decline in police effectiveness is exclusively attributed to a deterioration in police ability to investigate and collect evidence necessary to clear crimes, rather than changes in the pool of reports. I estimate that following the closures the police likelihood to clear crime falls by 0.7 percentage points (p.p), equivalent to a 3.7% drop with respect to the baseline clearance rate. The effects are concentrated in blocks located closer to the stations at baseline, suggesting that distance affects clearance rate through slower response time. I document a decrease in the number of cleared violent offenses, indicating that the observed drop in clearance is rooted in a decline in police effectiveness, as opposed to a change in citizens' reporting.

Third, police station closures discourages citizens' cooperation with law enforcement. Using victimization survey data, I estimate a 0.6 p.p. drop in reported incidents, which accounts for 17% of the baseline reporting rate. This decline in reporting is associated with a reduction in respondents' confidence in police effectiveness. I also detect the reporting effect on police records by focusing on low-severity incidents, such as property offenses, which typically have lower marginal returns to report. I find that in treated areas reported property crimes decline by 3%. In line with this interpretation, I further document an increase in burglaries, a property offense which is not affected by reporting, given that a police report is required to claim insurance coverage.

Fourth, shutting down police stations reduces the social welfare of local residents.

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, entirely driven by high-crime and deprived blocks. Such uneven impacts generate substantial distributional consequences, further intensifying pre-existing inequalities. Adopting a capitalization approach, I compute that for every £5 saved by the public authorities, up to £3 are paid back by the local residents in terms of foregone house valuations. While supporters of the closures point at the reduced spending for the public finances, I show that the accrued savings for the criminal justice system do not outweigh the fiscal and social costs induced by the closures. Furthermore, following Hendren and Sprung-Keyser (2020) I calculate the marginal value of public funds (MVPF). I estimate that for each pound saved by the public administration, £3 to 7 of additional costs are borne by the society.

This paper contributes to the vast literature that investigates 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 the policy shocks, these papers struggle to isolate the mechanisms behind police deterrence. A related set of papers leverage 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 highlight the preventative role of visible police presence. Likewise, Blesse and Diegmann (2022) examined the deterrence effects of a police consolidation reform in German municipalities, estimating increases in crime attributed to lower police visibility. More recently a few papers have examined police effectiveness more closely (e.g., Adda et al., 2014; Blanes i Vidal and Mastrobuoni, 2018). Mastrobuoni (2019), by exploiting temporary disruption in police patrolling, shows that the likelihood that a robbery is cleared is lower during patrol shift changes conditional on a crime being committed. Blanes i

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

Vidal and Kirchmaier (2018) exploit discontinuities in the distance to response stations to show that increased response time lowers the clearance rate. Both papers find that criminals do not alter their behavior in response to localised or short-lived changes in policing patterns. Finally, Weisburd (2021) shows that in contexts of rapid-response strategies, police presence reduces crime through deterrence.

My paper advances the current literature in several ways. First, I leverage a natural experiment which led to localized variations in police proximity within a large police force. By doing so, I study a different and relatively neglected input to the police production function - proximity - which crucially determines response time (Kelling and Moore, 1989). I estimate a substantial elasticity of crime with respect to proximity, and I show that this effect arises not only from lower police visibility near stations, but also from longer response times. Furthermore, I highlight the presence of decreasing marginal returns to scale in the police production function, offering novel insights on the optimal resource allocation within a police force. Second, unlike previous studies, my analysis includes all actors involved in law enforcement. Thanks to the rich and highly granular data, I identify the impacts of increased distance from police stations on two equilibrium outcomes - police effectiveness and crime occurrence. Conversely to existing evidence, I find both an increase in violent crimes and a decrease in clearance. Furthermore, I consider at the same time the victims and citizens' viewpoint, estimating the broader social impacts of reduced proximity to the police.

In addition, this paper emphasizes the critical role of civilian crime reporting. As first highlighted by Levitt (1998a), if policy interventions affect *both* crime occurrence *and* reporting, ignoring changes in underlying reporting may lead to biased estimates on recorded crime. With the exceptions of Vollaard and Hamed (2012), identifying a positive correlation between reporting and police workforce size, and Ang et al. (2021), who discuss changes in civilian reporting after police violence events, the existing literature largely overlooks the influence of private attitudes on law enforcement.[3] My focus on reporting is motivated by its central role in crime detection, as law enforcement ulti-

[3] Few papers study under-reporting in the context of illegal migration (Comino et al., 2020; Jacome, 2022), or of gender-based violence (Miller and Segal, 2019). Because the reduction in reporting partially reflects an increase in the cost to access policing services, this paper also resonates with the literature on the impacts of changes in the cost to access public services (Deshpande and Li, 2019).

mately relies on victims' willingness to report incidents (Acemoglu and Jackson, 2017; Owens and Ba, 2021). Using police records and victimization survey, I study both changes in crime incidence and victims' reporting in response to police station closures. My findings reveal that shifts in police organization significantly impact police-civilian interactions and communities' trust in law enforcement.

Finally, this paper expands our knowledge on the drawbacks of austerity policies. Two recent studies focus on the welfare reforms targeting individual benefits in the UK and estimate the impacts on the Brexit vote (Fetzer, 2019), and on the the spatial concentration of crime (Giulietti and McConnell, 2020), showing that austerity cuts disproportionately hit already deprived areas. I complement previous research by evaluating a austerity-driven place-based policy that reduced local police resources. Quantifying the total costs of cuts to police forces may guide policy makers to appraise the shadow price of public savings and to better design compensation schemes for the losers.

2 Institutional context and data

2.1 Policing and budget cuts in London

The Metropolitan Police Service (henceforth, MPS) is the police force responsible for law enforcement in the metropolitan area of Greater London, serving around 8.9 million people. The MPS is organized into 32 territorial divisions, called Borough Operational Command Units (BOCUs), which correspond to the 32 London Local Authorities (LAs).[4] Each territorial division is responsible for the neighborhood patrolling and the incident response functions. Response officers start their shift from police stations, with officers heading out on patrol and responding to incidents.[5] When not on a call, emergency response teams are deployed on patrol. When on patrol, they start their shifts at

[4]Local Authorities (hereafter, LAs) are the local government units in England which are responsible for the provision of local public services (e.g. education, waste collection, social housing). The boundaries of each police division exactly overlap with those of London LAs. City of London is not policed by the MPS, but by the smaller City of London Police.

[5]Emergency calls are received at multiple central locations by police staff and call handlers (First Contact operators). Once the incident is classified, the call is passed to the Dispatch operators of the relevant police division. These operators determine which police response units to deploy.

a police station before traveling to the areas that they police.[6] They return to their bases to complete reports and carry out administrative work.

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%.[7] Consequently, the Mayor of London approved a plan aimed to curb expenses for policing. As a result, the MPS begun to drastically reduce the number of stations, closing several front counters and selling police buildings.[8] 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, 2017). They argued that, as only few people reported crimes by directly walking into the station, the reduction in stations would have only marginally affected residents' reporting behavior.[9]

Between 2008 and 2018, the number of operating stations dropped from 160 to 45, with 80% of all closures taking place in 2013 (Figure 1). As a result, the ratio of residents served by each station increased from 1 station per 50,000 people to 1 station per 200,000 people by 2018. Figure 2 shows the borders of the 32 police divisions, and the geographical distribution of the police stations in London. The closures were evenly distributed throughout the city. All police divisions was equally affected, each losing at least one station in 2013. The average number of stations per division declined from around 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, who are responsible for patrolling and incident response, at the expenses of the back-office staff

[6]The 'bobbies on the beat' patrolling activities are divided into three shifts (early, late and night), each lasting about eight hours. In each shift, officers are dispatched to patrol a specific patrolling zone.

[7]The Home Office and the Justice departments experienced a substantial budget cut of 26%, compared to the average departmental cut of 12% (Crawford et al., 2011). Following the cuts, more than 600 out of 900 police stations across police forces in England shut down. Between 2012 and 2016, the MPS made £600 million savings and needed to save additional £400 million by 2022 (MOPAC, 2013, 2017).

[8]A police station is defined as an operational building with a front counter where the public can have face-to-face contact with the police. Prior to the closures, all police stations in London had a front counter. In this context, closing a front counter is equivalent to releasing the whole building.

[9]According to a FOIA filed to the MPS, in 2011 around 8% of criminal incidents were reported via face-to-face contact. This share dropped to 6% by the end of 2016. Throughout the sample period. the share of incidents reported by phone was roughly stable at 90%. Local communities, however, were worried that the closures would have deteriorated police response time and perceptions of police presence, resulting in increased crime and lower trust in public authorities (Pratt, 2019).

and, to a larger extent, of the infrastructure. They maintained the distribution of officers across patrol zones and shifts unchanged (MOPAC, 2017), reflecting their core belief that active police presence on the streets is crucial in fighting crime over preserving stations. Between 2010 and 2016, they kept the number of front-line officers constant (with only a 1% reduction), while 60% of police staff and police support officers were let go, as shown in Figure 3.[10] This fact, paired with a reduction of police stations, mechanically increased the number of officers per surviving station, from 154 to 260. In Section 2.2, I outline a conceptual framework that clarifies how closing stations while maintaining a constant number of front-line officers affects police deployment.

Figure 4 (Panel 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. By the end of 2016 the average response time doubled for violent crimes, and tripled for all incidents (Panel B). This is in spite of a lower demand for police services, indicated by fewer reported incidents and a 20% reduction in emergency calls. In addition, I build as measure of civilian crime reporting the ratio of emergency calls for violent incidents to the number of violent offenses, borrowed from Ang et al. (2021), which indicates, for a given violent offense, how likely a community is to call the police. Panel C shows a 40% decrease in crime reporting after 2013. These stylized facts motivate the subsequent empirical analysis.

2.2 Interpreting the effects of police station closures

The police station closures involved two key policy components: an increase in the distance to the nearest police station and a constant number of front-line officers within each police division, increasing then number of officers per open station.[11] These two

[10] Police support officers' duties include tackling anti-social behavior, dealing with minor offenses, crowd controlling, directing traffic. Civilian staff cover all back-office roles, that include all activities necessary to the running of the organization, such as finance, information technology and human resources (HMIC, 2011b). It is plausible to think that cutting back-office staff could create bottlenecks, reducing the police administrative capacity. Since these tasks are shared within the entire police division, any reduction in capacity would uniformly affect the division. The empirical analysis will address this by incorporating division-specific time-fixed effects.

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

features influence police deployment in both *direct* and *indirect* ways. To structure the empirical analysis and provide guidance for interpreting the results, I outline a stylized conceptual framework of a police production function, where I consider two policing inputs: proximity (d), which determines the police response time, and police presence, defined as number of deployed officers (P). The police output, denoted as $Y(d,P)$, depends on both inputs.[12] In the empirical analysis, I will measure police output as number of violent crimes, and number of cleared incidents.

There are two ways through which police station closures affect police output. First, the closures increase the distance (d) between stations and crime scenes. Furthermore, the MPS did not alter the allocation of officers to patrolling areas. As front-line officers need to travel back and forth from the stations to start and end their shifts, to respond to calls, and to report the evidence collected during on-site investigations, their response times increase. This extended travel time constitutes the *direct* effect of the closures. Second, although the total number of front-line officers per police division remains constant ($\bar{P}$), the consolidation of police stations mechanically leads to an increase in the number of officers based at fewer locations. This may result in gains from concentration at the remaining facilities, potentially enhancing police output in the presence of complementaries between proximity and police size. For instance, it could increase police visibility in the vicinity of the active stations and strengthen community relations thanks to the higher police concentration. These complementarities between proximity and police levels constitute the *indirect* effect. Consequently, police output may see localized improvements around the active stations.

In the empirical analysis, I will first estimate the immediate, *direct* impacts on crime and police effectiveness of reduced proximity. The difference-in-differences strategy outlined in Section 3.1 washes out the differences in the levels of police presence, as the allocation of officers to patrolling areas remains unchanged. However, not considering the indirect impacts would lead to overestimate the overall impact of the closures. Therefore, in the second part of the empirical analysis, I will separately identify the

[12] In this framework, I intentionally abstract from criminal strategic response. The policy could impact criminals' decisions by affecting the probability of apprehension, reducing police deterrence. In principle, this might further lead to displacement of criminals. However, in practice, I demonstrate in Appendix B1 that criminals' displacement does not explain the empirical findings.

complementaries between the two inputs by estimating the *indirect* impacts on the subsample of surviving stations. This will be detailed in Section 3.3. Ultimately, I will estimate the *net* impact of the closures by comparing these two effects. This final step is important not only to quantify the total effect of the closures but also to understand the nature of the police production function - specifically, whether it exhibits decreasing or increasing marginal returns to scale. A positive net impact would indicate decreasing marginal returns to scale in the police production function.

2.3 Data

Police station closures. To study the impacts of police station closures, I construct a novel database including all the existing police stations between 2009 and 2018 in Greater London. I gather information from Freedom of Information Act (FOIA) requests lodged to the MPS on the universe of police stations, with their exact location and their dates of opening and closure. I then geo-locate all police stations and map them to their census blocks, that is, small-level geographies with a target population of about 700 households and an average size of just above 0.25 square miles.[13] For each station I also collect information on whether the building was sold and, if sold, the destination of the regenerated building. I therefore obtain a list of 168 police stations operating between 2009 and 2018. In the empirical analysis, I focus on the period 2011-2016.[14] During this period, the number of police stations in London dropped by 50%. I compute the geodesic distance between the centroid of a census block and each police station's exact geographical location. I measure such distance conditional on the police station being in the same LA as the census block, since law enforcement in London was managed at the police division, i.e. the LA, level.[15] Figure 5 shows the distribution of the distance to the nearest station before and after the closures: between 2011 and 2016 the median distance to the closest police stations more than doubled from 1.3 km to 3

[13] The census blocks considered in the analysis are the Lower Layer Super Output Areas (LSOAs), homogeneous geographical layers developed by the Office for National Statistics (ONS) for statistical purposes. There are 4,835 LSOAs in London, designed to fit the boundaries of the local authorities.

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

[15] The patrolling and emergency response functions are entirely managed within the division boundaries, with only 1% of police deployments being cross-border. Results are robust to computing distance across LAs (Appendix Table B3).

km. Figure 2 displays the location of all operating and closed stations, which drives the variation that I exploit to identify the causal effects of the police station closures.

Station closures and their initial locations are non-random by nature. Panel A of Table A1 shows that police stations were initially located in blocks with significantly and persistently higher levels of crime and house values than blocks without police stations. The MPS effectively chose to close stations in areas with relatively lower crime levels and higher house values than areas where stations remained opened (Panel B). The identification strategy exploits ex-ante differences in the distance to police stations across areas, but it does not require the initial presence of stations (nor their closure) to be random. It only requires that outcomes of treated and control blocks would have evolved similarly absent the closures. I will demonstrate in Section 3 that treated and control areas exhibit parallel trends in the periods before the closures.

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 7 million police records. All criminal incidents are geo-located, and then mapped to their census block. To account for the prevalence of zeros in the types of crimes, I transform the variables using the inverse hyperbolic sine transformation (*asinh*).[16] 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 reported. The dataset contains around 3.5 million incidents with the final outcome of their prosecution process.[17] Following Blanes i Vidal and Kirchmaier (2018), I define police performance as the probability of a criminal incident to be investigated, prosecuted and solved, i.e. *cleared*, conditional on a police investigation taking place. At baseline, 19% of inci-

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

[17] Appendix Table A2 shows the definitions of all crime and outcome types respectively. 60% of criminal incidents report a valid investigation outcome. 95% of criminal incidents without a valid outcome are anti-social-behavior incidents, which are never investigated by the police.

dents are cleared, i.e. charged by the police or the court at the end of the investigation. Out of all the charged cases, 65% are charged with a court sentence, while the remaining are resolved with an informal sanction, that applies in cases of minor offenses, or offenses which do not meet the public interest criterion. Only the police can assign informal sanctions in case of less severe offenses. The clearance rate greatly varies across crime types, reflecting the severity of the offenses, the difficulty in identifying a suspect and the amount of required evidence (Home Office, 2016).[18] I compute two additional indicators that I will use in the empirical analysis. First, I measure if the criminal investigation was solved with a court or a police decision. Second, I use convictions as a measure of court punishment, which constitute 77% of all court sentences and include all crimes sanctioned to imprisonment, fines, or other sentences by the court, excluding acquittals and discharges.

Victimization Survey. I use the Crime Survey for England and Wales (hereafter CSEW) for the period 2010/11-2016/17 to directly measure citizens' crime reporting. The CSEW is a victimization survey conducted on a nationally representative sample of approximately 35,000 to 45,000 respondents each year. It directly asks respondents about their experiences with crime, including whether they have been victimized in the previous year and whether they have reported a crime. Additionally, for a subset of respondents (around 50%) it also asks questions about their attitudes towards the police and the criminal justice system. I use the restricted-access geo-coded version of the CSEW, that include information on the census block of residence of the respondents. I restrict the sample to all respondents living in London (21,873), among whom 5,182 individuals reported being victims of crime. These victims experienced a total of 7,594 incidents during the study period. Appendix C reports the descriptive statistics on respondents and incidents. The average incident-level reporting rate is 37.2%.

House prices. I use administrative records from the UK Land Registry on the universe of house transactions from 2011 to 2016. Every transaction records the date and price

[18]For instance, for crimes directly detected by the police, such as drugs and weapon possession, the offender is usually identified when the crime comes to the attention of the police. Indeed, while around 95% of thefts remain unsolved, more than 65% of drugs and possession of weapons offenses are cleared.

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).

Summary statistics Table 1 shows the summary statistics for all the variables used in the analysis. The sample includes the universe of the census blocks located in Greater London. I exclude from the sample all blocks located in the boroughs of City of London and Westminster due to their very distinctive administrative features and to the fact that crime records without a physical locations are conventionally attributed by the MPS to these LAs. The resulting dataset consists of a monthly panel of 4,701 census blocks, observed between January 2011 and December 2016. Treated areas have lower crime rates and higher house prices than control areas, reflecting the fact that the police stations were shut down in areas with more favorable local conditions than those where police stations were left open. While the difference-in-differences design does not require treated and control units to be similar in *levels* prior to the closures, I will show evidence of pre-closure parallel trends for the key outcomes of the empirical analysis.

3 Empirical strategy

3.1 Difference-in-differences specification

My identification strategy exploits the time and spatial variation in police station closures in Greater London, which give rise to changes in the distance between each census block and their nearest police station. I define treated units as areas which experience an increase in the distance to the nearest police station solely induced by station closures. Out of 4,701 census blocks in London, 2,039 experienced a closure of their nearest station during the sample period. Control units are areas whose nearest station never closed. A caveat in the treatment definition arises from the fact that in principle blocks might be treated more than once, if, for instance, after the closest station shuts down, also the second closest is removed, and so on. Only 8% of treated blocks are treated more than once: 183 blocks were treated twice and 30 three times. Still, this might complicate the identification strategy. To address this issue, in the empirical analysis I adopt an Intention-to-Treat approach focusing on first closures only, and I define blocks as treated if their *baseline* nearest police station closed.

This design compares blocks with unchanged distance to those with increased distance from the nearest station. I estimate the following equation:

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

where $y_{i,t}$ is the outcome of 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 has ever shut down, and $Post_{i,t}$, equal to 1 in the period t when the nearest police station to census block i was closed. ϕ_i are block fixed effects and capture all time-invariant characteristics of the blocks, while ϕ_t are calendar date dummies. Standard errors are clustered at the census block level, allowing for serial correlation over time (Bertrand et al., 2004).[19] Under the assumption that, in the absence of the closures, the number and composition of criminal incidents would have evolved similarly in treated and control blocks, the parameter β provides the causal effect of police station closures on three sets of outcomes, namely crime, clearance and house prices. As in standard difference-in-differences models, the main threat to identification is the presence of differential pre-trends in the outcomes of treated and control units. I discuss the validity of the assumption in Section 3.2.

I further exploit the variation in the intensity of the treatment. To do it, I estimate Equation 1 employing as main regressor the continuous (log) distance from the closest police station. The resulting coefficient provides an estimate of the elasticity between distance and the outcomes. Furthermore, although the binary treatment predicts on average more than 90% of the changes in distance, the treatment intensity largely varies depending on the pre-determined location of blocks. After the closures, initially nearer

[19]In Appendix B1, I show results computing Conley standard errors (Conley, 1999) to account for both spatial and serial correlation of the errors.

census blocks display greater changes in distance than blocks initially farther away (Figure A1). For this reason, I will assess the existence of non-linearities in treatment effects along the baseline distance distribution.

A threat to the empirical strategy may arise from the presence of contemporaneous local confounding policies. Although the closures were enforced by the MPS, there may be other local time-varying factors correlated with the staggered closings of the stations. Suppose for instance that during the same period each police division decided to adopt different policing tactics as a response to the budget cuts, or that the lay-off of back-office staff increased congestion and administrative burdens within each division. Alternatively, suppose the central government cut funding to other welfare items, affecting the provision of other public services provided by the LAs (e.g. welfare, housing, education). To address this, I interact LA-specific indicators with time dummies to non-parametrically control for any (observed and unobserved) time-varying local change. The point estimates are virtually unchanged, suggesting that the impacts are not driven by the presence of contemporaneous confounding factors at the LA level.[20]

3.2 Dynamic specification

I employ a generalized difference-in-differences specification to test the validity of the common trend assumption and to estimate the dynamic impacts of closing a police station. Recent econometric literature has raised concerns regarding TWFE estimators in the presence of variation in the treatment timing and heterogeneous treatment effects.[21] In this context, the vast majority of treated blocks gets treated in the same period (78% in the third quarter of 2013 and 85% throughout 2013), which mitigates the concern of

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

[21]A burgeoning literature has emphasized that difference-in-differences designs with staggered treatment timing are likely to be biased in the presence of heterogeneous treatment effects (among others, De Chaisemartin and d'Haultfoeuille (2020). Goodman-Bacon (2021), Callaway and Sant'Anna (2021) and Borusyak et al. (2024)). As the TWFE coefficient is a weighted average of all the possible 2x2 comparisons in my sample, it is also estimated using comparisons among already-treated units and not-yet-treated units, where the already-treated units serve as control. In the presence of heterogeneous treatment effects across blocks experiencing closures at different points in time, this would induce a bias.

heterogeneous treatment effects across different treatment waves. Nevertheless, to deal with pitfalls in the TWFE estimation, I adopt a 'stacked-by-event' design. The main advantage of a stacked design relative to a pure event study design is that the former uses a never-treated, control group which allows to remove event-time trends that do not appear in calendar time. In my context, this becomes crucial if, for instance, the MPS chooses which station to close first based on crime trends in previous years. In that case, calendar date fixed effects alone would not eliminate the pre-trends .

Given that in my setting there is no straightforward definition of 'event' for control blocks, I adopt the approach outlined by Deshpande and Li (2019) and I build 'placebo' events for control areas. First, I create a separate dataset for each *treatment wave*, i.e. for each group of census blocks that experience the first closure in the same period. There are 9 treatment waves in total.[22] In each of these datasets, all blocks experiencing a closure in the considered time period form the treatment group, while blocks that never experienced the treatment serve as control. Second, in each dataset I define event-time dummies relative to the period of treatment, i.e. of closure. Finally, I stack all datasets into one. In this procedure, the same never-treated block serves as control multiple times, i.e., for each treatment wave. I restrict my main sample to event quarters -9 to 11, for a total of 450,901 block-quarter observations. I estimate the following equation:

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

where y_{it} is the outcome of interest for block i in calendar month t. D_t^k's are a set of relative event-time dummies, each taking 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 one if block i has ever experienced an increase in distance. Event-time dummies are assigned to both the treatment and the control group as explained above. I omit the period before the treatment and include $B = 9$ preceding and $T = 11$ subsequent periods. The stacked design allows to separately identify calendar-time (ϕ_t's) and event-time (ϕ_k's) fixed effects, eliminating event time trends that do not appear in calendar time. Standard

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

errors are clustered at the census block level, allowing for serial correlation over time (Bertrand et al., 2004). This specification is robust to heterogeneous treatment effects, under which traditional event studies perform poorly.[23]

The coefficients δ_k's identify treatment effects k periods from the closure, with $k = -1, \ldots, -B$ indicating pre-treatment, placebo estimates. Figure 6 plots the point estimates of δ_k's for violent crimes obtained using the stacked-by-event design. Estimates of pre-treatment coefficients are close to zero and statistically not significant for all main outcomes. We cannot reject that pre-treatment coefficients are jointly equal to zero, supporting the validity of the identifying assumption. This aligns with the observation that station closures were decided on the basis of by local crime trends.

3.3 Indirect effects

I estimate the indirect impacts of the closures close to the surviving stations. As discussed in Section 2.2, because a higher police presence deters crime, areas around surviving stations may benefit from a greater number of front-line officers starting and ending their shifts there. These indirect effects would arise from the complementarities between proximity to surviving stations and the greater concentration of officers there.

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

[23] In Appendix B1 I further assess the robustness of this approach either estimating alternative specifications of the stacked-by-event design or implementing the estimator proposed by Borusyak et al. (2024).

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

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

where y_{it} is the number of assaults and murders recorded in block i at monthly date t. $Post_{lt}$ is equal to 1 in the period when the first police station in the same police division l shuts down. ϕ_i and ϕ_t are block FE and date FE, respectively. The dummy $Near_i$ is equal to one for blocks located closer than the median distance to the nearest open station.[24] I further exploit the variation in the intensity of the treatment. I employ the continuous distance, expressed as the inverse of distance, as main regressor, and I examine whether there are non-linear treatment effects along the baseline distance distribution.

To quantify the net effects, I compare the direct and the indirect impacts of the police station closures. In the same spirit as Blattman et al. (2021), I compute the total number of non-deterred and deterred crimes as the product of (i) the coefficients of the direct and indirect effects from respectively Equation 1 and 3, and (ii) the number of treated and control blocks in London. Furthermore, I estimate Equation 1 going back to my main definition of treated units, but using as control blocks those areas unaffected by any changes, i.e. those whose proximity to stations remains unchanged and that are located far from operating stations. This exercise will yield an estimate of the *net* impacts of the closures, excluding all the indirect impacts.

4 Main results

4.1 Violent crimes

Violence increases as a result of police station closures. Table 2 shows regression results for violent crimes, defined using the MPS category of assaults and murders.[25] Column 1

[24] The validity of this design rests on the parallel trend assumption between treated (*near*) and control (*far*) areas. I provide evidence in favor of this assumption estimating a dynamic difference-in-differences specification using a stacked-by-event design. Coefficients are shown in Appendix Figure A2.

[25] Although ideally one would prefer to observe only aggravated assaults and murders, this is the most disaggregated category of violent crime provided by the MPS. I will directly address reporting as an

includes monthly date FE, column 2 presents estimates from Equation 1 with block and date fixed effects, while column 3 further includes LA-by-date FE. This last set of FEs accounts for all the unobserved impacts on crime of a re-allocation of policing resources within police 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 FE, the coefficient becomes positive. Estimates are barely affected by the inclusion of LA-by-date FE.[26] This last specification shows that following an increase in the distance to the nearest police station the average number of recorded violent offenses increases by 11% per treated census block-month. In Panel B, I estimate a positive elasticity of violence with respect to distance of approximately 0.09, so that a 10% increase in distance increases violent crimes by roughly 1%. This effect translates in 3 additional assaults and murders in each block per year, for a total of 5,500 additional violent offenses city-wise.[27] Violence starts diverging across treated and control blocks right after the wave of police station closures starts. Figure 6 plots the δs coefficients from Equation 2. The increase in violent offenses is significant and persistent up to four years after the intervention, indicating no adaptation or reorganization of the police workforce in response to the closure of the stations.[28]

The next step is to understand the mechanisms by which police station closures lead to a reduction in police deterrence against violent crimes. First, closures reduce police visibility and perceptions of police presence in the surroundings of the stations. This is in line with previous findings from Di Tella and Schargrodsky (2004), MacDonald et al.

outcome in the next subsection.

[26]In Appendix Table B4 I document that there are no differential effects on violent crime based on the incidence of austerity cuts to other welfare expenditures. This analysis reinforces that the findings are not influenced by other LA-specific austerity policies.

[27]Using robberies as an additional category of violent offenses that is included in the MPS data corroborates the same result. Robberies increase by 1.6% as a result of the station closures (Table B1, columns 1-2). Furthermore, changes in local police presence may generate significant spillover effects on neighboring streets (Blattman et al., 2021). For this reason, in Appendix Section B1 I examine whether the presence of spillovers from control areas contributes to the estimated increase in violence.

[28]Results are robust to binning distant periods together or using balanced sample of areas $-7/+7$ period from/since the closure of nearest police station (Appendix Figure B4). Furthermore, I test the robustness of the estimates obtained with the stacked-by-event design implementing the estimator proposed by Borusyak et al. (2024).

(2016) and Blesse and Diegmann (2022), who show that police visibility - through police buildings or police on guards - affects deterrence. Second, the station closures result in longer response times, that in turns damage police effectiveness and deterrence.[29]

To investigate the mechanisms, I assess whether the effect is driven by blocks initially located closer to or farther from the police stations. I exploit the intensive margin of the treatment and I estimate Equation 1 on each sub-sample of quintile of baseline distance. Figure 7 shows a significant gradient in violent crime relative to distance from closed stations. Blocks closer to the stations experience the most substantial increase in crime, consistent with the fact that nearest areas experience the greatest intensity of the treatment, that is, the largest increase in distance. This effect diminishes and eventually becomes negligible beyond the fourth quintile (beyond 1.65 km). Furthermore, in Figure 8 I show that the effects are driven by crime hot spots, i.e. areas with higher than median baseline crime rates.[30] These findings together corroborate the evidence in support of the deterrence channel. The *marginal* areas, which are more likely to suffer from an increase in the distance to the police station, are blocks that at baseline had higher opportunities for crime and higher returns from police presence. The impact is strongest in the nearest distance quintile and gradually lessens, underscoring the importance of not just visibility but also police response time. Section 4.3 will further assess the impacts on police effectiveness, isolating response time as a key channel.

4.2 Indirect impacts

Nearby operating police stations, violent crimes decrease. Table 3 shows regression results from estimating Equation 3. Column 1 adds date and block fixed effects, while column 2 augments the specification with LA-by-date FE, which absorb all time varying changes occurring within a police division. Blocks located in close proximity to an open station experience a reduction in violent crimes after the closure of the first station

[29] A third potential channel is patrolling intensity. Although I do not have data on patrolling to directly test this channel, based on previous evidence in similar contexts (Mastrobuoni, 2019; Blanes i Vidal and Mastrobuoni, 2018), we know that small and temporary changes in patrolling intensity have little if any impacts on crime.

[30] It is a well-established fact in the fields of criminology and economics of crime that risk perceptions and decisions regarding crime location are influenced by localized factors, including the local economic conditions and the likelihood of apprehension (Apel, 2013; Kirchmaier et al., 2024).

within their police division. The specification from the last column quantifies a total decrease of 5 violent offenses per year for each block close to the operating stations. The remaining columns in Table 3 shows that the indirect effect holds when interacting the *post* dummy with the inverse of the continuous distance. Overall, these findings highlight the existence of complementarities between proximity and the police workforce strength as they prove that police is most effective in deterring crime when it is in close proximity to the stations.

What channel explains these indirect impacts? Figure 9 flexibly accounts for spatial impacts and 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.

To quantify the net effects, I contrast the direct and the indirect impacts of the police station closures following Blattman et al. (2021). I compare the coefficients of the direct and indirect effects from Table 2 and 3, multiplying them by the baseline mean of the outcome and the number of treated and control blocks. These crude aggregates suggest that 614 more assaults and murders are committed city-wide because of police station closures in treated blocks, or 14% relative to the total number of violent crimes in London. Instead, 442 violent crimes are deterred near to open police stations, equal to 10% of the total violent crimes. Furthermore, in Appendix Table B7, I conduct two additional checks. First, I exclude areas with operating stations from the control group, and second, I exclude areas located close to operating stations. Both exercises lead to a net increase of similar magnitudes of 5% to 10%. This quantification exercise, which is not meant to generate a general equilibrium estimate, produces a policy-relevant magnitude of the net crime impacts in London. It also provides novel evidence of diminishing marginal returns to scale of policing. The fact that the direct effects outweigh the indirect ones implicitly suggests the existence of dis-economies of scale. Incremental gains achieved through higher levels of policing are smaller than the incremental losses experienced at lower levels. Furthermore, the two impacts speak to the two different channels: police visiblity and response time. However, the increased concentration of police in open stations does not sufficiently offset the adverse consequences of longer response times resulting from station closures.

4.3 Police effectiveness

I estimate the impact of the closures on police effectiveness, defined as the ability to effectively investigate, prosecute and solve, i.e. *clear*, crimes.[31] The relationship between police station closures and police effectiveness is a-priori ambiguous. On the one hand, the police station closures increase the distance and the response time to attend the crime scene. The later the police is brought in, the lower the chances are to gather the evidence and to successfully identify a suspect. Indeed, 70% of non-cleared robberies and 80% of non-cleared assaults are attributed to police failure to identify the suspect. On the other hand, a lower number of reports reduces congestion, freeing up resources to clear fewer crimes, thus improving police effectiveness, *conditional* on reporting.

To assess whether police closures have any effect on clearance, I estimate Equation 1 on the incident-level dataset, restricting the sample to all incidents with a non-missing investigation outcome. Police effectiveness worsens as a result of the closures. Table 5 shows that the incident-level probability of clearance significantly drops by 0.7 pp, equivalent to 3.7% with respect to the baseline clearance rate of 19%. Results are robust to the inclusion of crime type FEs (column 2), that absorb the heterogeneous unobserved complexities associated with investigating different categories of offenses.[32] Columns 4 and 7 of Table 5 indicate that higher distance to the stations significantly decreases the total volume of offenses cleared and brought to justice, i.e. convicted, in the census blocks by 7% and 5% respectively.[33] Combining these estimates with the ones from the

[31] Starting with Thaler (1977), many studies used clearance to measure of police performance (e.g. Mas, 2006; Garicano and Heaton, 2010; Blanes i Vidal and Mastrobuoni, 2018; Mastrobuoni, 2020).

[32] Figure A3 shows that before the closures, period-specific coefficients on clearance are not statistically different from zero, supporting the validity of the parallel trend assumption. Furthermore, in Appendix B2 I conduct a few robustness checks. Table B8 shows that the closures do not impact the likelihood of an investigation outcome being recorded upon report, and thus do not affect the sample selection of the investigations. Table B9 shows that the decline in clearance is solely due to poorer police performance, specifically in applying informal sanctions, rather than changes in court processes.

[33] Mastrobuoni (2019) underlines the incapacitation effects of police clearance. In light of this, such reduction in convictions may reinforce the adverse effects on crime through lower incapacitation of offenders, and may further discourage the public from reporting such incidents.

previous section, I can compute the implicit elasticity of violent crime with respect to clearance, defined as $\varepsilon = \frac{\partial C}{\partial Y}\frac{Y}{C} = \frac{\partial C}{\partial d}\frac{d}{C} \times \frac{\partial d}{\partial Y}\frac{Y}{d}$, where C denotes the number of crimes, and Y of cleared offences. I compute an elasticity of -0.38, implying that 1% decrease in the likelihood of being caught increases crime by 0.4%.[34]

Why is distance critical for police effectiveness? The main consequence of an increase in distance is to prevent the police from arriving very fast at those crime scenes, hurting its ability to collect evidence and clear crimes.[35] Figure 10 shows the heterogeneity analysis by quintile of baseline distance. Distance matters most for incidents located in areas in close proximity to a police station. In these cases, response time has a higher impact on the clearance rate relative to the average incident (Blanes i Vidal and Kirchmaier, 2018), confirming the importance of swift police responses in effectively addressing crimes.

A reasonable concern is that the observed reduction in clearance may simply reflect the smaller pool of reports to investigate. A decrease in deterrence and in reporting may mechanically drive the clearance rate down by simply changing the denominator, even keeping constant police effectiveness. To address this, I examine the impact on clearance rates for violent and property crimes separately. As reporting of violent crimes remained consistent, any changes in clearance rates for such crimes would primarily reflect changes in police effectiveness. Furthermore, if police station closures solely affected crime prevention through deterrence, we would expect to observe an increase in cleared violent offenses. Columns 5-6 and 8-9 indicate that the total volume of both cleared violent and property crimes falls by 3 and 4%, respectively. The observed decrease in cleared violent offenses unambiguously indicates a decline in police effectiveness.[36] Considering the reduction in the reports' pool, the estimated drop in clearance is likely to constitute a lower bound of the actual decline in police effectiveness.

[34]To my knowledge, only Anker et al. (2021) computed this key policy parameter while studying DNA registrations, and reported a 2.7 elasticity of crime in response to detection. Two factors may explain the difference in magnitudes: first, I focus on violent crimes, which, as Becker (1968) noted, are known to have lower elasticity; second, they study a potentially productivity-boosting technology.

[35]Ideally, one would want to empirically validate this argument. As I don't have access to incident-level response time data. Nonetheless, leveraging LA-level data on average response time for 999 emergency calls, I compute a positive correlation of 27% between response time and distance.

[36]Appendix Table B10 draws similar conclusions using as outcome the ratio between the number of charges and convictions over the number of reports.

4.4 Reporting

Police station closures may affect reporting of crime.[37] Residents living in proximity to closed police stations may report less because they anticipate the police may not respond promptly. Diminished police presence may also lead to erosion of trust in police effectiveness, further discouraging residents from reporting.[38] Additionally, longer travel times to the nearest station increases residents' material costs to report crimes. All these factors combined contribute to a reduction in crime reporting.

To provide direct evidence of reporting, I employ the Crime Survey of England and Wales (CSEW). The CSEW collects information on individuals' experiences as crime victims in the 12-month period preceding the survey interview, and includes questions on whether they reported crime (Appendix C describes the survey in details). I estimate Equation 1 on the sub-sample of victim respondents residing in London. The outcome variable is an incident-level indicator for reporting. The results are presented in Table 4.[39] Columns 1 and 2 show that a 10% increase in distance to the nearest station leads to a 0.6 p.p reduction in reporting, conditional on victimization. This corresponds to a reduction of 17%, from a baseline reporting rate of 37%.

The closure of the stations is associated to a decline in the confidence in police effectiveness. The survey additionally includes questions about respondents' attitudes towards the police for a sub-sample of participants. Results from Table C4 confirm that a decrease in visible police presence signals a reduced sense of order for citizens (and criminals alike). As a falsification test, I look at the impacts on confidence in other criminal justice institutions to ensure that the closure of the stations influenced local

[37] The number of reported crimes can be expressed as $C = r \cdot C^*$, where C^* is the actual crime, and r is the reporting probability. The policy potentially affects both r and C^* (Levitt, 1998a). Given that my findings show that reporting only affects property offences, the increase in recorded violent crimes can be interpreted as an increase in the actual crime occurrence.

[38] Lack of community cooperation with local police can not only undermine law enforcement but also fuel further law breaking. Few theoretical papers have stressed the critical role of private cooperation for effective law enforcement (e.g. Benabou and Tirole, 2011; Acemoglu and Jackson, 2017). This argument is also related to the "broken windows" theory by social scientists Wilson and Kelling (1982). In a setting where police is short on resources, and police effectiveness is decreasing, "*citizens may soon stop calling the police, because they know 'they can't do anything'*".

[39] In line with the evidence from police records, the decline in respondents' reporting is driven by low-severity, property crimes offenses (Appendix Table C3). Although no coefficient is statistically significant, the estimates on reporting of violent offenses are all close to zero, while those for reporting property crimes are consistently negative, larger in magnitude, and closer to the average impact.

residents' views specifically through its impact on police perceptions. Columns 3 and 4 of the same table indicate that there is no statistically significant effect of increased distance on confidence in the overall criminal justice system. These findings confirm that residents have a clear understanding of the decline in police effectiveness due to the station closures and do not attribute it to other criminal justice institutions. The reduced inclination to report reflects a general lack of trust in policing resulting from substantial and indiscriminate cuts to police resources.

A similar decline in reporting is observed when examining police-recorded property offenses, which I use as a measure of low-severity crimes. Low-severity offenses are more susceptible to variations in reporting than high-severity offenses, primarily because the costs associated with reporting are more likely to outweigh the benefits (e.g. MacDonald, 2001; Soares, 2004). Appendix Table B1 estimates the effect for thefts and for all property offenses (columns 5-8). Following the closure of the closest police station, property crimes drop by 3%, while thefts by 11%.[40] This observed decline in recorded property crime mirrors a reduction in the underlying reporting, rather than in the actual crime occurrence.[41] As a falsification test, I examine burglaries, which are less prone to reporting bias as insurance claims typically require police reports.[42] Consistent with this notion and in line with the findings on violent crimes, burglaries display a positive, although for the discrete variable insignificant, increase (columns 3-4).

4.5 House prices

The welfare implications of the police station closures ultimately depends on how much value citizens place on having a police station nearby their community. The loss of police stations reduces police visibility in the neighborhood. Furthermore, as residents care about their exposure to crime risk, an increase in violence influences their per-

[40]The police station closures also damages police crime *detection*. Higher distances cause a reduction in drugs-related offenses, which are only directed detected by the police (columns 9-10 of Table B1).

[41]To support this claim, I derive the impact on *reported crime* ($\frac{\partial C}{\partial d} \simeq -2\%$) by multiplying the change in crime-reporting ($\frac{\partial r}{\partial d} \simeq -17\%$), estimated using the CSEW, by the change in *actual crime* ($\frac{\partial C^*}{\partial d} \simeq 11\%$), proxied using the estimates from police records on violent crimes. I obtain a figure that is indeed very close to the estimates obtained using police property records.

[42]Under-reporting of burglaries in England is relatively minor. Data from CSEW indicate that 98% of respondents take some form of home security measures, and 80% have home insurance.

ceptions of safety, and their valuation of living in close proximity to the crime scene (e.g. Thaler, 1978). House prices therefore not only reflect the direct costs related to increased criminality or to changes in crime composition, but also changes in local amenities and residents' perceptions of the neighborhood (Rosen, 1974).

To estimate the overall impact of the closures through both crime and non-crime channels, I employ the universe of house transactions from the Land Registry recorded in London from 2011 to 2016. I estimate a specification analogous to Equation 1 on the mean (log) house prices in the census block, where observations are weighted by the number of transactions recorded in the census block during the quarter.[43]

Figure 11 plots the estimates on house prices with 95% confidence intervals. An increase in the distance to the closest station reduces house prices, although the average coefficient is not statistically significant.[44] Most importantly, police station closures increases the inequality in house values between high– and low–crime blocks. These heterogeneous impacts closely aligns with the ones on violence outlined in the previous section. Police station closures appear to significantly reduce the willingness to pay to reside in high-crime neighborhoods, in more deprived blocks and blocks with a higher socio-economic disadvantage. Overall, cuts to police funding hit harder those areas that were already losing from austerity cuts (Fetzer, 2019; Giulietti and McConnell, 2020).

The reduction in house prices is not driven by the expansion in the local housing supply following the sale and regeneration of the closed police stations. Between 2011 and 2016, out of 80 closed stations, 45 were sold. For 35 of them, I observe the estate destination of the sales: 25 stations were transformed into new residential buildings, 7 in other public amenities, such as education or community centers, 3 in offices.[45] In Appendix Table B11 I explore whether house prices vary differentially depending on the estate use of the closed stations. If anything, the negative effects on prices originate

[43] Data are aggregated at the quarterly frequency. I restrict the sample to sales of residential properties only. Figure B5 estimates the dynamic specification and show no pre-trends before the closures.

[44] The average effect is non negligible: house prices fall by 0.8% more per quarter in blocks were stations closed relative to blocks with still operating stations. Interpreting this coefficient as an implicit price in a hedonic function gives a mean price of around £3,400 for the closure of a local police station. This magnitude is comparable to other studies linking house prices with crime in the UK (e.g. Gibbons, 2004; Adda et al., 2014).

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

from blocks where the nearest closed station was either not regenerated and abandoned, or replaced with another public amenity, contrasting the hypothesis that the price reduction was driven by a supply shock to the local estate market. As a sanity check, column 5 of the same Table shows that also the rise in violent crimes is concentrated in areas with abandoned stations. These negative effects reflect not only the changes in local criminality, but also the loss of local amenities represented by the closure of police stations, that directly affects residents' valuation of the neighborhood and has not been offset by new local amenities.

I quantify the total loss of value of house prices using a capitalization approach. The average cost to a treated block from the closures equal to £5,404. I quantify the benefits in terms of the public savings from the closures. According to the official estimates, the MPS made savings equal to £600 million between 2012 and 2016, which translates into around £12,000 saved per treated block-quarter. The ratio between costs and benefits yields a value of 0.44 (all details are reported in Appendix Table D1). Costs are disproportionately borne by high-crime blocks: for them I estimate a cost-benefit ratio of 0.54. Overall, it appears that for every £5 saved by the public authority, 2 to 3 are paid back by the local residents.

5 Cost-effectiveness

In the final part of the paper, I evaluate the cost-effectiveness of the station closures. The capitalization approach adopted in the previous section has a significant drawback. It only includes the valuation of marginal movers induced by the policy, not of all affected households. Therefore, these effects are likely to be a lower bound on welfare losses as they ignore any reduction in property values experienced by residents who chose not to sell. Using the estimates from the previous sections, I quantify the overall social welfare effect of the policy. Full details on calculations can be found in Appendix D.

Marginal Value of Public Funds As an alternative framework to conduct welfare analysis, I follow the approach developed by Finkelstein and Hendren (2020) and Hendren and Sprung-Keyser (2020) and compute the marginal value of public funds (hereafter MVPF). The MVPF is the ratio of society's willingness to pay (WTP) for a policy to the cost of the policy net of any fiscal externality. In the context of public spending cuts, I compute the marginal *loss* of public funds, under the assumption that the MVPF is symmetric for expansions and reductions to public spending.

To calculate the numerator of the MVPF, I compute the aggregate social willingness to pay. Initially, I present a conservative estimate of the total WTP, specifically focusing on the willingness to pay for additional crimes (column 1 of Table D7). This calculation includes two cost components: the deterrence and the incapacitation costs. First, a lower clearance rate encourages potential criminals to offend because of lower deterrence, therefore resulting in more crimes. Furthermore, *conditional on clearance*, more crime induces higher incarceration. I also account for reduced incapacitation costs stemming from a decrease in reporting for certain crime types, as outlined in Section 4. Finally, I use the estimates of the total economic and social costs of crime from the Home Office Report (Heeks et al., 2018), which includes calculations of the costs in anticipation of crime (e.g. defensive expenditure and insurance), costs as a consequence of crime (e.g. physical and emotional harm, lost output, victims' services) and costs in response to crime (e.g. police and criminal justice costs). Additionally, I incorporate in an alternative specification the willingness to pay for the worsened labor market prospects of individuals who are incarcerated due to the rise in violent crimes (column 2 of Table D7), as well as the total loss in house sales (column 3).

The denominator of the MVPF captures the net savings to the government. This includes the direct savings from the closures, as well as positive and negative fiscal externalities, such as the foregone tax revenues from reduced house sales subject to the stamp duty land tax, which is a tax imposed on house sales, and the fiscal savings for the CJS. Table D7 summarizes the calculations for the MVPF, yielding an estimate ranging from 2.6 to more than 7.[46] One advantage of calculating this ratio is that it can be com-

[46] A traditional cost-benefit analysis, comparing the savings of police station closures with the potential costs generated by greater criminal activity (thus excluding fiscal externalities), would also conclude that closing stations is not a cost-effective way to implement public spending cuts. This approach likely yields conservative estimates as it only quantifies direct costs associated with increased crime and decreased deterrence. It does not incorporate indirect costs, such as the impacts on community safety and well-being, loss of public trust in law enforcement, increased strain on other law enforcement resources, potential long-term societal consequences due to a rise in criminal activity.

pared to the MVPF of other policy changes. 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). Overall, the findings from this exercise suggest that £1 of savings accrued on this policy delivers an additional cost of £3 to 7 borne by the society.

6 Conclusion

This paper shows that reductions to police spending contribute to a significant rise in violent crimes, to a reduction in reporting and a deterioration of police effectiveness and citizens' welfare. These impacts damage the mechanisms for curbing future crimes, as citizens become less likely to provide assistance or information to law enforcement.

Given the recent heated debate on the role of police funding for crime prevention and social welfare, this paper carries several compelling implications. First, policy makers may be inclined to cut funding and reorganize resources to promote public sector efficiency. In spite of the fact that the decision was based on the need for fiscal discipline, closing police stations is not cost-effective, and produce distributional consequences that disproportionately hit the poor. Second, I shed light on the strategic role of police infrastructure for the optimal provision of public safety. Police stations are critical for an effective police deployment, especially at lower levels of policing. To compensate the increase in crime, the police would need to recruit an additional 14 to 17 thousand officers, entailing costs that completely wipe off all savings from the closures.[47] Most importantly, achieving the socially optimal policing hinges on public-law enforcement cooperation. When police effectiveness declines, it carries hidden costs, like weakened trust and cooperation with the police. As police officers represent the state, any loss in their legitimacy can erode public trust and engagement with law enforcement and other public institutions.

[47] Detailed calculations supporting these figures can be found in Appendix D5.

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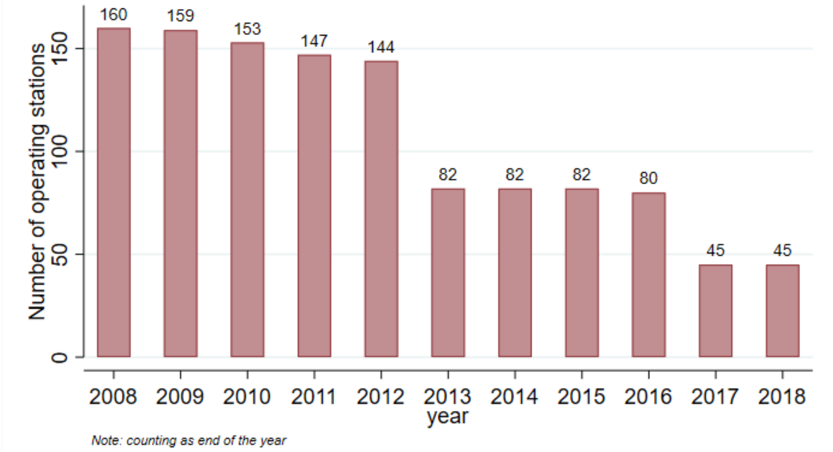
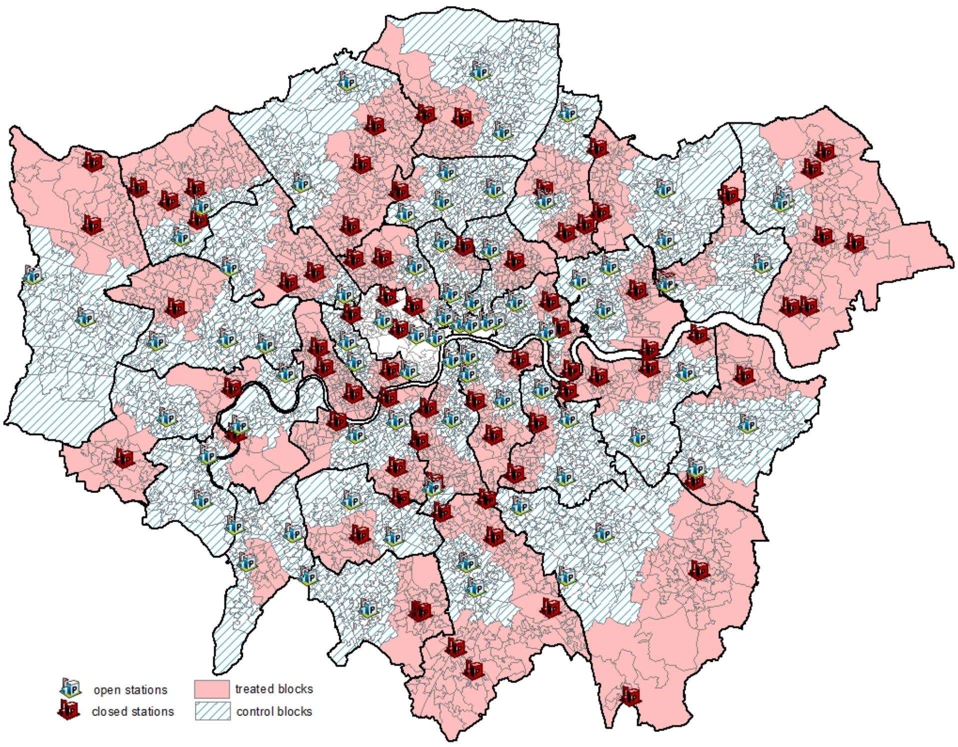
Figures

Figure 1: Number of police stations in Greater London



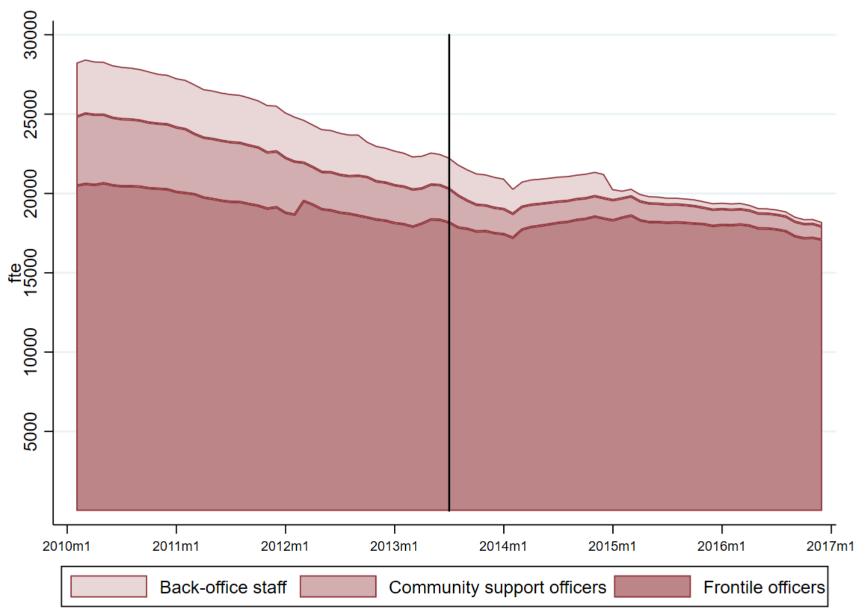
Note: The figure displays the total number of police stations operating in Greater London between 2008 and 2018. The sample period of the empirical analysis stops at the end of 2016 because of subsequent changes in the local policing structure.

Figure 2: Map of police station closures and treated blocks



Notes: The map plots the locations of police stations, including both open and closed stations, as of the end of 2016 (end of the sample period). In addition, the map codes treated and control census blocks: blocks where the nearest station was closed (in red) and blocks whose nearest station remained open (in blue and white). Black thick borders correspond to boundaries to the 31 boroughs of London, which overlap with the borders of the police divisions. The map excludes City of London and Westminster.

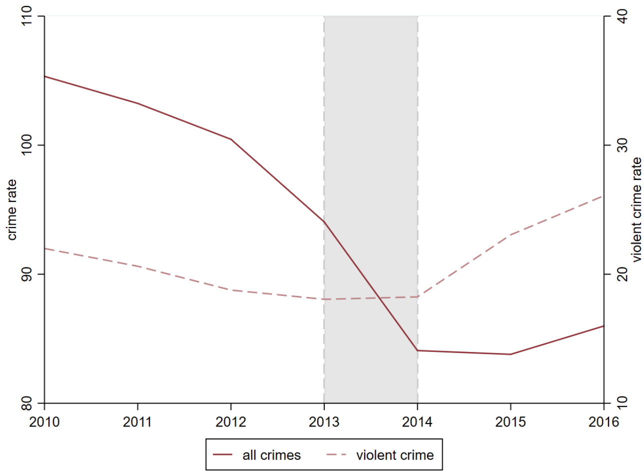
Figure 3: Police workforce



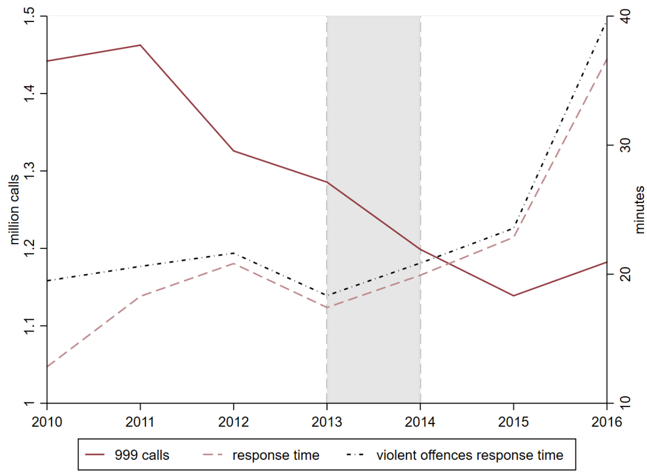
Note: This figure shows the MPS police workforce (in full-time equivalent) breakdown from January 2010 and December 2016. Front-line officers include police workforce employed in patrolling and emergency response, such as police constables and detective constables. Community support officers (PCSOs)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

Figure 4: Crime and 999 calls in Greater London

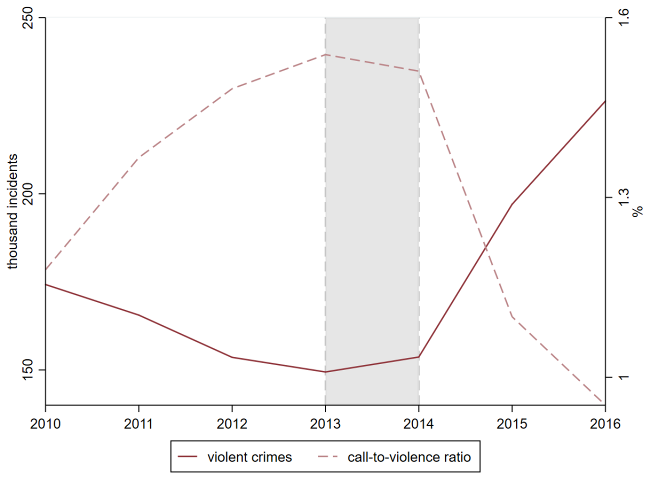
(a) Crime rates



(b) 999 calls and response time

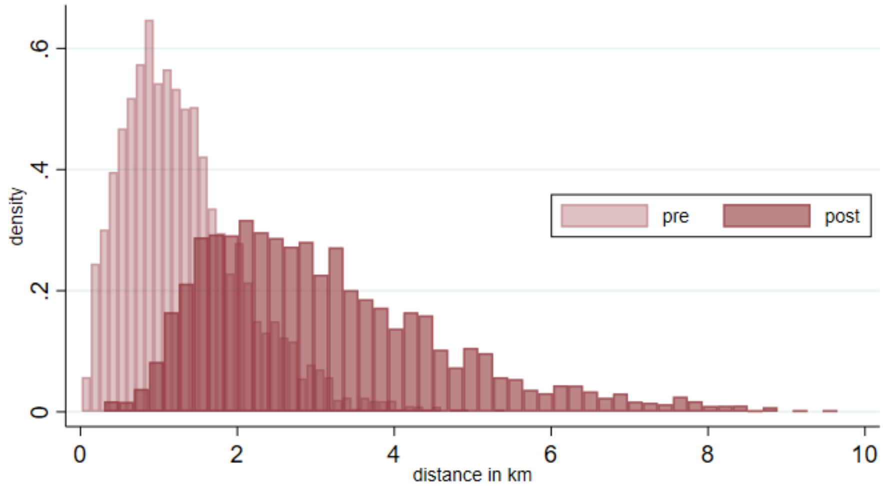


(c) Violence to emergency calls ratio



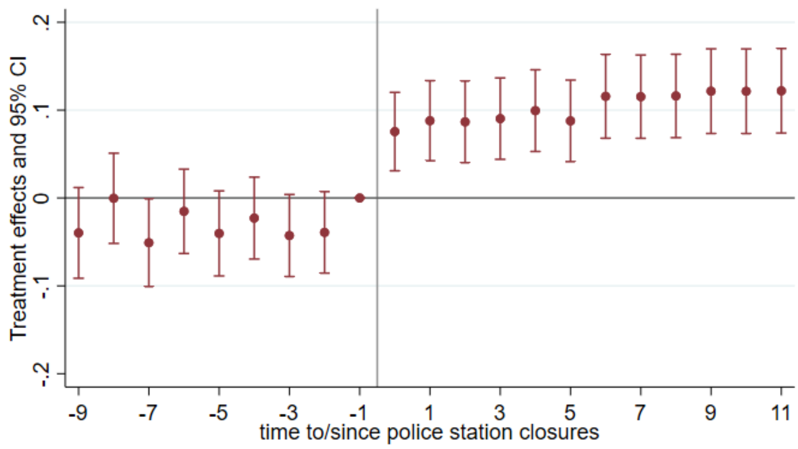
Note: Panel A displays the crime rate per 1,000 individuals. Panel B shows the number of emergency calls and the related average response time between January 2010 and December 2016 by the MET police. Average response time is for calls graded as "I" (Immediate calls) or "S" (Significant calls). Response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

Figure 5: Identifying variation in distance



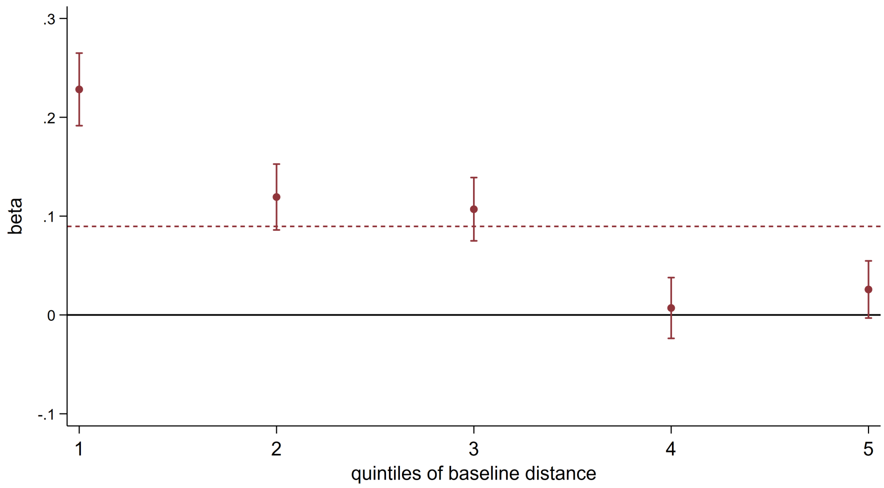
Note: This graph displays the distribution of the distance to the closest police station across all census blocks whose nearest police station closed, i.e. treated blocks, before and after the closures. The sample excludes blocks those located in City of London and Westminster and spans over the years 2011-2016.

Figure 6: Event study for violent crimes



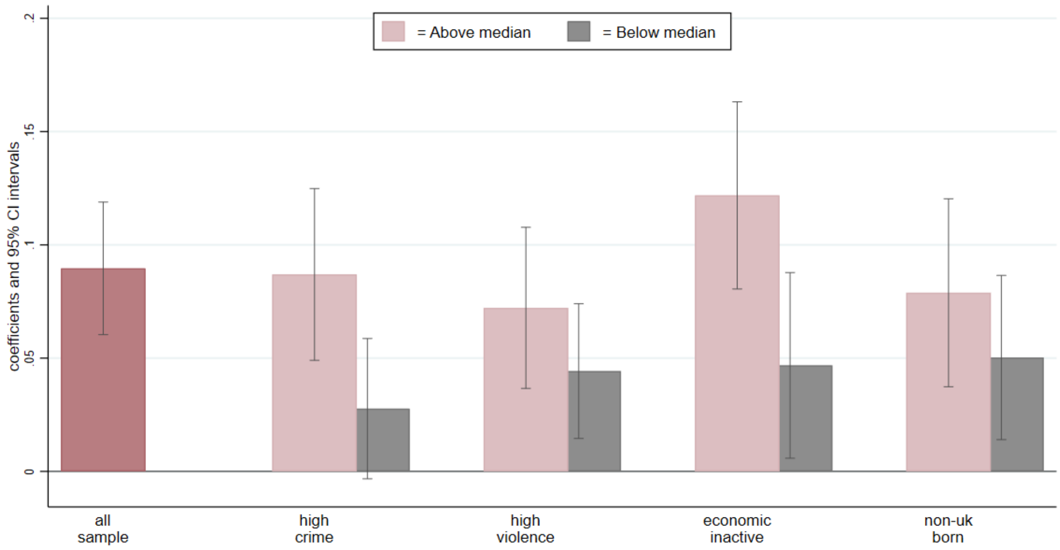
Note: The graph reports coefficients and 95% confidence intervals estimated according to Equation 2 and is produced using the stacked-by-event dataset at the quarterly frequency. Time on the horizontal axis is computed by subtracting the date when a block's nearest police station closes from the quarterly date of the observation. The dependent variable is the total number of violent crimes, defined as assaults and murders, recorded in a census block and transformed using the hyperbolic sine transformation. I omit the dummy for the period before the closures and, as suggested in Sun and Abraham (2021), I exclude the distant relative periods, keeping areas with 9 leads and 11 lags. All regressions include census block, calendar time (quarterly date), and relative time fixed effects. Standard errors are clustered at the census block level.

Figure 7: Effects on violent crimes by baseline distance



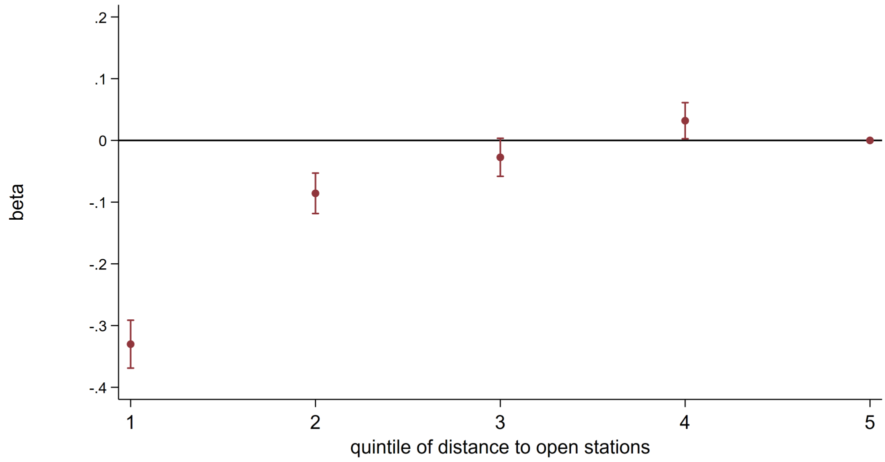
Note: The graph reports coefficients and 95% confidence intervals estimated based on Equation 1 for each quintile of (*asinh*) baseline distance, i.e. distance to the nearest police station measured before any change occurs. The dashed horizontal line reports the average effect. The dependent variable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

Figure 8: Effects on violent crime by baseline characteristics of the census blocks



Note: The figure plots estimates from Equation 1 splitting the sample by baseline characteristics (above versus below the London median). The dependent variable is the total number of violent crimes, defined as assaults and murders, recorded in a census block expressed using the hyperbolic sine transformation (*asinh*). Baseline crime rates are computed using data from 2008 and come from LSOA-level MPS historical data. Baseline characteristics of the census blocks come from the Census (2011) and include population share of economic inactive and share of non-UK born. Standard errors are clustered at the census block level.

Figure 9: Indirect effects on control group

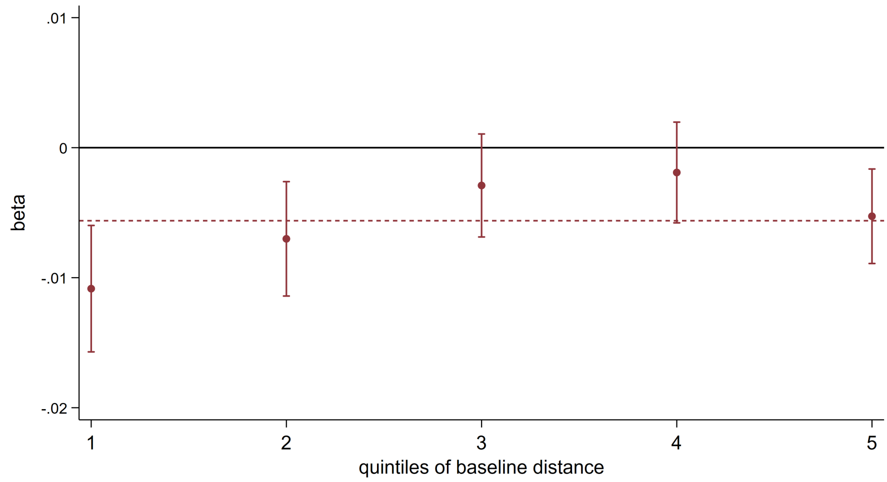


Note: The figure reports the δ_q's coefficients and confidence intervals from estimating the following regression:

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

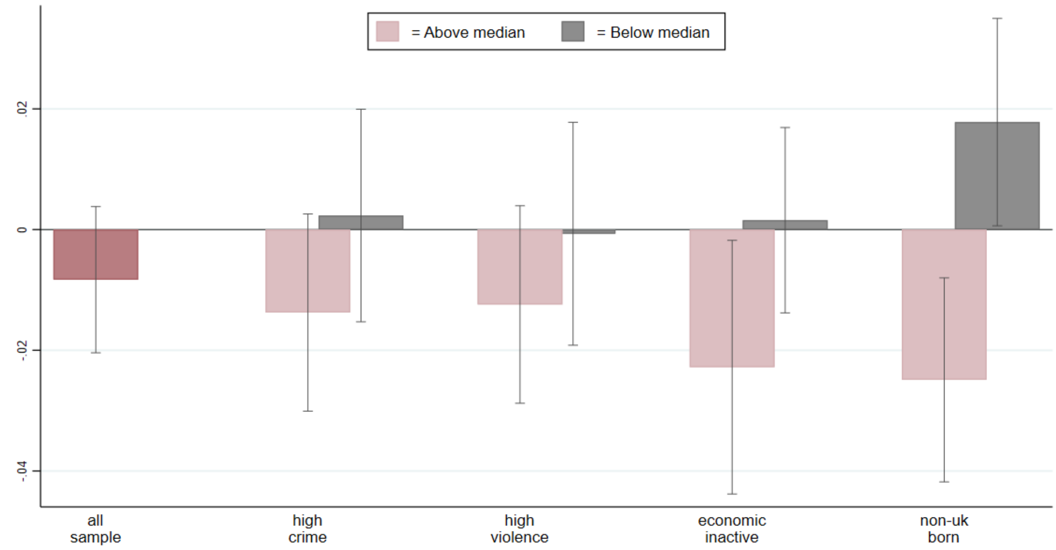
where the $I\{Bin_i = q\}$ dummies stand for whether each block is in the $q-th$ quintile of the baseline distance distribution, where $q = 1, \ldots, 5$. The omitted category, which serves as control group, consists of areas at the highest quintile of baseline distance ($q = 5$), i.e. furthest away from the nearest open station. The rest of the notation follows from Equation 3. The dependent variable is the number of assaults and murders, transformed using the *asinh* transformation. Standard errors are clustered at the census block level.

Figure 10: Effects on clearance by baseline distance



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

Figure 11: Effects on house prices by baseline characteristics of the census blocks



Note: The figure plots estimates from Equation 1, with 95% confidence intervals, splitting the sample by baseline characteristics (above versus below the London median). The dependent variable is the average (log) house prices computed in the census block. Each regression includes LSOA, and date-by-LA fixed effects. The dataset is collapsed at the quarterly level. Baseline crime rates are computed using data from 2008 and come from LSOA-level MPS historical data. Baseline characteristics of the census blocks come from the Census (2011) and include population share of economic inactive and share of non-UK born. The observations are weighted by the number of sales in the census block during the quarter. Standard errors are clustered at the census block level.

Tables

Table 1: Descriptive statistics

	Treated blocks (1)	Control blocks (2)
Panel A: crime dataset		
Distance		
Distance from closest police station (in km)	1.38	1.46
Crime		
All crimes	17.74	20.97
Violent crimes	2.75	3.28
Robberies	0.56	0.66
Assaults and murders	2.19	2.62
Property crimes	5.92	6.71
Burglaries	1.61	1.63
Criminal damage	0.68	0.75
Shoplifting	0.33	0.43
Vehicle crimes	1.70	1.67
Drugs	0.46	0.61
Public order offences	0.32	0.41
Anti-social behaviour	5.89	7.00
Observations	48,936	63,888
Panel B: house price dataset		
House prices	484,744.93	465,604.32
Number of transactions	31.61	31.32
Observations	9,562	10,882
Panel C: investigation outcomes dataset		
Cleared	0.18	0.19
Not cleared	0.82	0.81
Out-of-court sanction	0.06	0.07
Court sentence	0.12	0.12
Convicted	0.10	0.10
No sufficient evidence	0.01	0.01
No suspect identified	0.42	0.42
Observations	232,550	356,991

Note: This table shows summary statistics for the baseline years 2011-2012, before any closure occurs. Panel A displays averages computed at the census block- month level. Panel B displays the average house prices (weighted by the number of transactions) and number of house sales at the census block level with a quarterly frequency. Panel C reports statistics from the incident-level dataset of investigation outcomes. Treated blocks are census blocks which experienced an increase in distance from the closest police station throughout the sample period (2011-2016).

Table 2: Effects of police station closures on violent crimes

	Assaults and murders		
	(1)	(2)	(3)
Panel A: Binary treatment			
dummy distance	-0.011***	0.090***	0.115***
	(0.003)	(0.015)	(0.015)
Panel B: Continuous treatment			
distance	-0.093***	0.086***	0.087***
	(0.006)	(0.016)	(0.016)
Observations	337,794	337,794	337,794
Mean Dep. Variable	2.48	2.48	2.48
Date FE	✓	✓	✓
LSOA FE		✓	✓
LAxDate FE			✓

Note: Note: In Panel A, the explanatory variable is the dummy treatment as defined in Section 3; in Panel B the explanatory variable is the continuous geodesic distance between the centroid of the census block and the closest police station, measured in km and transformed using the *asinh* transformation. The dependent variable is transformed using the *asinh*. *LA* refers to the 31 London LAs (excluding Westminster and City of London). The table displays the baseline mean of the number of assaults and murders (in absolute terms). Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1. * p<0.1.

Table 3: Indirect effects on violent crimes on sub-sample of operating stations

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

Note: The table displays the estimates from Equation 3. *Dummy near* is equal to 1 if the census block is below the median distance to the nearest open police station. $1/distance$ is the inverse of the distance to the nearest open station in km. The *Post* dummy is equal to 1 in the period when the first police station within the same LA shuts down, and remains equal to 1 afterwards. Regressions are run on the sample of blocks which never experienced a closure (2,514, among which 1,257 are below the median). The dependent variable is transformed using the *asinh* transformation. The table displays the baseline mean of the outcome (in absolute numbers). *LA* refers to the 31 London LAs (excluding Westminster and City of London). Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Effects of closures on reporting using victimization survey

	=1 if incident reported		
	(1)	(2)	(3)
Panel A: binary treatment			
dummy distance	-0.024	-0.024	-0.038
	[0.044]	[0.047]	[0.049]
Panel B: Continuous distance			
distance	-0.062*	-0.062*	-0.057
	[0.036]	[0.036]	[0.038]
Observations	5,405	5,405	5,405
Mean dep. Variable	0.369	0.369	0.369
Date, LSOA FE	✓	✓	✓
LA-specific trends		LA lin. trend	LAxYear FE

Note: The table displays estimates from the CSEW restricting the sample to all incidents experienced by respondents in the 12 months prior the date of the interview. I include only respondents living in London. The outcome variable is an indicator equal to 1 if the incident was reported by the respondent. In Panel A, the explanatory variable is the dummy treatment as defined in Table 2; in Panel B the continuous distance is measured in km and transformed in logs. *LA* refers to the 33 London LAs. The table displays the baseline mean of the outcome. Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Effects of police station closures on clearance

	Pr(cleared)			Volume cleared crimes			Volume convictions		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				All crimes	Property crimes	Violent crimes	All crimes	Property crimes	Violent crimes
Panel A: Binary treatment									
dummy distance	-0.006***	-0.003**	-0.003**	-0.070***	-0.029***	-0.038***	-0.049***	-0.021***	-0.022***
	(0.002)	(0.002)	(0.001)	(0.009)	(0.007)	(0.006)	(0.007)	(0.005)	(0.004)
Panel B: Continuous treatment									
distance	-0.007***	-0.003	-0.005***	-0.050***	-0.018**	-0.032***	-0.031***	-0.012**	-0.014***
	(0.002)	(0.002)	(0.001)	(0.010)	(0.007)	(0.006)	(0.008)	(0.006)	(0.004)
Observations	3,221,504	3,221,504	3,221,504	337,794	337,794	337,794	337,794	337,794	337,794
Mean Dep. Variable	0.19	0.19	0.19	1.28	0.41	0.35	0.68	0.28	0.18
Date, LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
LAxDate FE		✓		✓	✓	✓	✓	✓	✓
Crime type FE			✓						

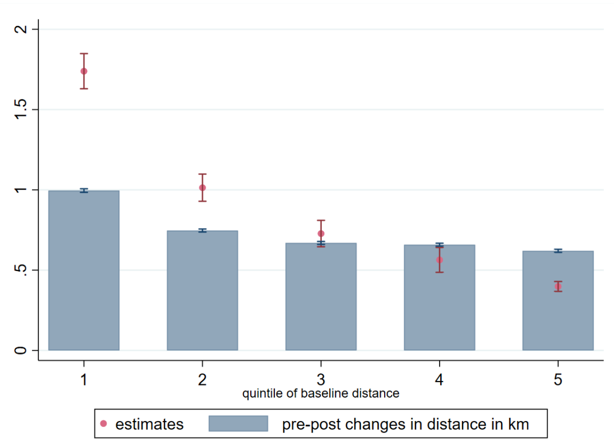
Note: This tables show the regression output on clearance. Columns 1-3 use the incident-level dataset, keeping all incidents with a non-missing investigation outcome, and define the outcome variable as equal to 1 if the incident has been cleared. Columns 4-9 use the dataset collapsed at the census block-level and define the outcomes as the total number of cleared crimes by type of offense (columns 4-6) and the total number of convictions by type of offense (columns 7-9). Convictions refer to incidents declared guilty of a criminal offense by the verdict of a court (thus exclude acquittals and discharges). Explanatory variables are defined as in Table 2. *LA* refers to the 31 London LAs (excluding Westminster and City of London). The table displays the baseline mean of the outcomes (in absolute terms). Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Appendix - For Online Publication

Appendix A Appendix figures and tables

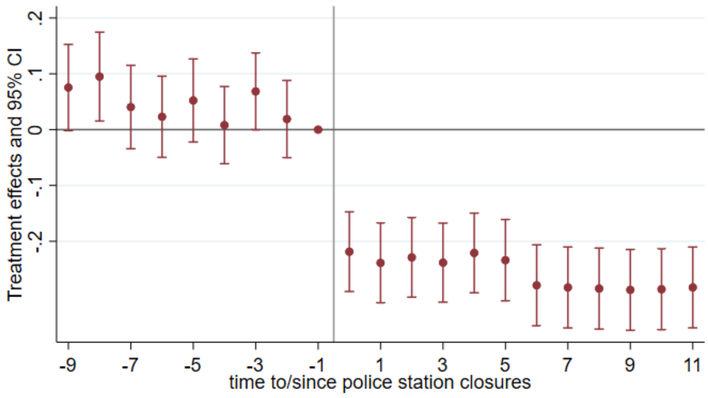
A1 Figures

Figure A1: Effect of police station closure on log distance by baseline distance



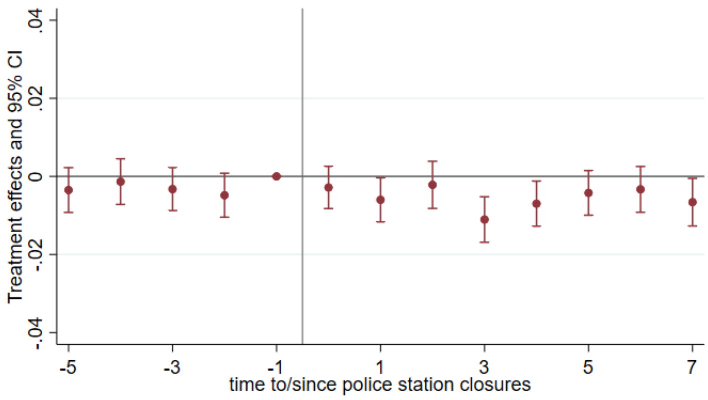
Note: The graph reports coefficients and 95% confidence intervals from the following regression: $logdist_{i,t} = \beta_1 Closed_{i,t} + \sum_{q=1}^{4} \gamma_q I\{Bin_i = q\} * Closed_{i,t} + \phi_i + \phi_{l,t} + \varepsilon_{i,t}$, where $Closed_{i,t}$ is defined as in Equation 1. For each bin $q = 1, \ldots, 4$, I plot the coefficients $\beta_1 + \gamma_q$, using as reference category the highest quintile of baseline distance. Standard errors are clustered at the census block level. The dependent variable is the log distance from the closest station. All regressions include census block and LA-by-monthly date fixed effects. The gray bars plot the distance in the block-specific post-closure period minus the distance in the pre-period (in km).

Figure A2: Event study for indirect effects among blocks with open police station



Note: The graph reports coefficients and 95% confidence intervals estimated according to Equation 2 and is produced using the stacked-by-event dataset at the quarterly frequency. Time on the horizontal axis is computed by subtracting the date when the first closure within the same LA occured from the quarterly date of the observation. Regressions are run on the sample of blocks which never experienced a closure (2,514). Among the 2,514 blocks, treated blocks are those with below-median distance to the nearest open station (1,257). The dependent variable is the total number of violent crimes, defined as assaults and murders, recorded in a census block and transformed using the *asinh* transformation. I omit the dummy for the period before the closures and, as suggested in Sun and Abraham (2021), I exclude the distant relative periods, keeping areas with 9 leads and 11 lags. All regressions include census block, calendar time (quarterly date), and relative time fixed effects. Standard errors are clustered at the census block level.

Figure A3: Event study for clearance



Note: The graph reports coefficients and 95% confidence intervals estimated according to Equation 2 and is produced using the stacked-by-event dataset at the quarterly frequency. Time on the horizontal axis is computed by subtracting the date when a block's nearest police station closes from the quarterly date of the observation. The dependent variable is an indicator for whether a criminal incident was cleared. I omit the dummy for the period before the closures. I omit the dummy for the period before the closures and, as suggested in Sun and Abraham (2021), I exclude the distant relative periods, keeping areas with 5 leads and 7 lags. The time window is shorter than for violent crimes as data on clearance are available only from 2012 to 2016. All regressions include census block, calendar time (quarterly date), and relative time fixed effects. Standard errors are clustered at the census block level.

A2 Tables

Table A1: Local characteristics predicting police station presence and closure

	Police station presence			Police station ever closed		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All census blocks						
log all crimes	0.068***			0.027***		
	(0.007)			(0.006)		
log violent crimes		0.021***	0.047***		0.010**	0.021***
		(0.004)	(0.008)		(0.004)	(0.007)
log property crimes		0.035***	0.008		0.020***	0.008
		(0.008)	(0.008)		(0.006)	(0.006)
log drug-related offences		0.010***	0.012***		-0.002	-0.004
		(0.003)	(0.004)		(0.002)	(0.003)
log house prices			0.047***			0.012
			(0.011)			(0.008)
Observations	13584	12993	7718	13584	12993	7718
N. treated blocks	4,712	4,712	4,712	4,712	4,712	4,712
FE	LAxYear	LAxYear	LA	LAxYear	LAxYear	LA
Panel B: Census blocks with any police station in 2010						
	Police station ever closed			Police station ever sold		
log all crimes	-0.241***			-0.134**		
	(0.057)			(0.064)		
log violent crimes		-0.080	-0.326***		-0.123	-0.337**
		(0.108)	(0.101)		(0.096)	(0.120)
log property crimes		0.159	0.259**		0.129	0.145
		(0.100)	(0.092)		(0.088)	(0.089)
log drug-related offences		-0.246***	-0.267***		-0.109*	-0.076
		(0.056)	(0.053)		(0.056)	(0.054)
log house prices			-0.484***			0.024
			(0.139)			(0.210)
Observations	417	415	251	417	415	251
N. treated blocks	148	148	148	148	148	148
FE	LAxYear	LAxYear	LA	LAxYear	LAxYear	LA

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

Table A2: MPS definition of types of incidents and investigation outcomes

Type of crimes	Definition	Frequency
Anti-social behaviour	Personal, environmental and nuisance anti-social behaviour.	0.288
Drugs	Offences related to possession, supply and production.	0.095
Property crimes	Bicycle thefts, burglaries, criminal damage and arso, shoplifting, thefts (including both thefts from person and bycicle thefts)	0.394
Public order and weapons	Possession of a weapon, such as a firearm or knife, and offences which cause fear, alarm or distress.	0.035
Violent crimes	Assaults and murders, robberies	0.189
Total number of incidents		5,843,654
Type of investigation outcome		
Non cleared:		
Evidencial difficulties	Court case unable to proceed; Unable to prosecute suspect	0.005
Suspect non identified	Investigation complete, no suspect identified	0.473
Other	Action to be taken by another body/agency; Offender otherwise dealt with; Under investigation	0.346
Cleared:		
Informal sanction	Offender given a caution	0.033
	Offender given penalty notice	0.011
	Local resolution	0.006
	Offender given a drugs possession warning	0.02
Court sanction	Offender fined	0.011
	Offender given suspended prison; Offender sent to prison	0.023
	Offender given absolute discharge; Offender given conditional discharge	0.007
	Offender given community sentence	0.014
	Defendant found not guilty	0.016
	Offender deprived of property; Offender ordered to pay compensation	0.001
	Awaiting court outcome; Court result unavailable; Defendant sent to Crown Court; Suspect charged as part of another case	0.033
No public interest	Formal action is not in the public interest; Further investigation is not in the public interest	0.000
Total number of investigation		3,231,678

Source: Frequencies are computed from the row MPS incident-level crime dataset. See https://www.met.police.uk/sd/stats-and-data/met/crime-type-definitions/ for all definitions of crime types used by the MPS.

Appendix B Robustness checks

B1 Robustness checks on crime

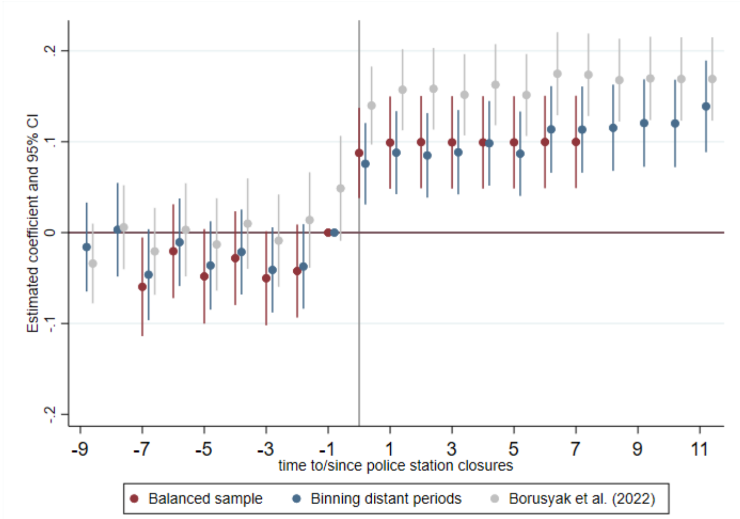
Table B1: Effects of police station closures on types of crimes

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

Note: Explanatory variables are defined as in Table 2. Property crimes in columns 7-8 include all thefts, criminal damage and arson, shoplifting and vehicle crime offenses. The dependent variables are crime types transformed using the *asinh*. *LA* refers to the 31 London LAs (excluding Westminster and City of London). The table displays the baseline mean of the number of offenses (in absolute terms). Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1. * p<0.1.

Dynamic specification I employ various alternative approaches to estimate the dynamic specification. First, I maintain the stacked-by-event design but utilize a balanced sample of census blocks. This ensures that the estimates are not driven by changes in the composition of census blocks observed for shorter or longer periods. Second, as an alternative to excluding temporally distant periods, I group distant periods together, as recommended by Sun and Abraham (2021). Third, I implement the estimator introduced by Borusyak et al. (2024). Notably, the point estimates are robust to these alternative approaches.

Figure B4: Alternative approaches to dynamic difference-in-differences



Note: The figure shows the event graph of violent crimes around the time of the closure of the nearest police station. The figure plots the time-specific treatment effects along with 95% confidence intervals. Variables are defined as in Figure 6. Red bars show coefficients from estimating stacked-by-event design on a balanced sample of blocks $-7/+7$ periods from/since the closures. Blue bars show coefficients from estimating stacked-by-event design binning together relative period before -9 and after +11, assuming constant treatment effects within the bin as suggested by Sun and Abraham (2021). The coefficients in gray use the estimator developed by Borusyak et al. (2024). All regressions include census block, calendar time, and relative time fixed effects. Standard errors are clustered at the census block level.

Spatial Correlation Given the high spatial resolution of the data, I estimate Conley (1999) standard errors with a spatial heteroskedasticity and autocorrelation consistent correction (HAC), allowing for both cross-sectional spatial correlation (across census blocks) and location-specific (within blocks across time) serial correlation, which decays as the distance from the block increases. Census blocks locations are specified in latitude-longitude degrees and the kernel cut-off is specified in km. For the spatial kernel, I retain a radius varying from 250 meters to 3 km and I use a conical kernel that decays linearly in all directions to weight spatial correlations. I allow for serial correlation across 6 time periods (months). Appendix Table B2 reports estimates robust to these adjustments.

Table B2: Conley standard errors

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

Note: Variables are defined as in Table 2. *LA* refers to the 31 London LAs (excluding Westminster and City of London). The table displays the baseline mean of the number of assaults and murders (in absolute terms). Standard errors are clustered using Conley standard errors varying the cut-off for the variance covariance matrix from 250 meters to 3 kilometres. *** p<0.01, ** p<0.05, * p<0.1.

Alternative definitions of distance As alternative explanatory variable I use the geodesic distance between the centroid of each census block and any closest police station, considering also stations located in different police divisions. This specification exploits also the variation in distance to census blocks whose nearest station is located in another LA. These blocks, likely to lie at the border across LAs, would not be considered treated according to the main specification because they are not affected by the closures within their division. Results shown in Table B3 (columns 1-4) do not support the hypothesis that police readjusts across different divisions as a result of the closures. In addition, in columns 5-6 I attribute the baseline distance to blocks that experienced an opening and I estimate an Intention-to-Treat regression. During the sample period 7 police stations opened, and 148 census blocks out of 4,701 blocks experienced a decrease in the distance. Results show that the estimates are virtually unchanged.

Table B3: Alternative definitions of distance

	Assaults and murders					
	(1)	(2)	(3)	(4)	(5)	(6)
dummy distance across LA	0.085***	0.105***				
	(0.015)	(0.015)				
distance across LA			0.105***	0.099***		
			(0.018)	(0.018)		
ITT distance					0.093***	0.088***
					(0.016)	(0.017)
Observations	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
Date, LSOA FE	✓	✓	✓	✓	✓	✓
LAxDate FE		✓		✓		✓

Note: The dependent variable is transformed using the *asinh*. *LA* refers to the 31 London Local Authorities, excluding City of London and Westminster. The table displays the baseline mean of the number of assaults and murders (in absolute terms). Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Heterogeneity by exposure to austerity cuts The implementation of the Welfare Reform Act 2012 led to a lower provision of local public services provided at the LA level (e.g., tax credits, changes to child benefit, capping of council tax benefits, the bedroom tax, changes to disability allowance). I investigate if higher exposure to LA-level welfare cuts magnifies the impacts of closures on violent crime. Table B4 indicates no differential effects on violent crime based on the incidence of austerity measures related to welfare.

Table B4: Effect on violent crime by incidence of austerity cuts

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

Note: Variables are defined as in Table 2. The dependent variables are transformed using the *asinh*. I draw on the same measure of incidence of welfare cuts that Fetzer (2019) and Giulietti and McConnell (2020) used and that is computed by Beatty and Fothergill (2013) as the financial loss per working age adult in a LA and year. *LA* refers to the 31 London LAs (excluding Westminster and City of London). Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Displacement of criminals This scenario could arise if criminals residing in control blocks choose to relocate to nearby treated areas where police presence is reduced, and where they face a lower risk of being apprehended. The potential presence of spillovers from control blocks to neighbouring treated blocks may pose an identification threat, even if the latter have not experienced the direct closure of their nearest police station. For this reason, I identify 833 neighbouring control blocks based on whether they share a border with treated blocks. I assess whether the main estimated effect picks up adjustments coming from bordering areas by excluding them from the estimation sample and repeating the analysis using as control group only "landlocked" blocks, i.e. blocks not bordering with any treated areas. Table B5 shows that the estimates do not change when I exclude bordering blocks from the estimation sample.

Table B5: Effect on reported crime types excluding bordering blocks from control group

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

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

Potential exposure to station closures Census blocks might be differentially exposed to the treatment because of their location: for instance, central blocks are potentially closer to more police stations than peripheral blocks just because of their geographical position. This concern is related to a form of non-random exposure to an exogenous shock (Borusyak and Hull, 2023), that gives rise to a peculiar type of omitted variable bias. Census block FEs control for all time-invariant characteristics of the area, including its geography. However, this might be insufficient if a census block's treatment status, and thus if changes in the distance to the police station, are also determined by the initial potential exposure to the treatment. I address this concern by flexibly controlling for the initial potential exposure to police stations, which is a function of the number of stations located in the surroundings of the area. To do it, I first count the initial number of stations operating within a certain radius from the centroid of each census block. I then augment the baseline specification by controlling for the baseline potential exposure interacted with a linear time trend. I use alternative cut-offs of potential exposure (2, 3, 4, 5 km) and interact them with a linear monthly trend. Table B6 shows that results remain virtually unchanged.

Table B6: Effects of violent crime conditional on initial exposure to police stations

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

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

Alternative definition of control group The analysis on the indirect effects shows that there is a localised reduction in violent crimes in close proxity to open stations, and then the effects fades away. To make sure that the estimated average treatment effect is actually driven by an increase in crime in treated areas, rather than a decrease in control areas, I conduct two tests. In column 1 of Table B7, I remove areas with existing operating stations from the control group (66 out of 2,662 control census blocks). In column 2, I exclude areas within the first quintile on the distribution of baseline distance from operating stations (629 control blocks). Results are robust to both sub-sample definitions, and suggest that the net effect of the police station closure is a net increase in violent crimes.

Table B7: Effect on violent crime removing areas with operating stations

	Assaults and murders	
	(1)	(2)
Panel A: Binary treatment		
dummy distance	0.096***	0.049***
	(0.015)	(0.015)
Panel B: Continuous treatment		
distance	0.073***	0.035**
	(0.016)	(0.016)
Observations	333,042	292,506
Mean Dep. Variable	2.485	2.485
LSOA FE	✓	✓
LAxDate FE	✓	✓

Note: Variables are defined as in Table 2. *LA* refers to the 31 London LAs (excluding Westminster and City of London). Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

B2 Robustness checks on clearance

Table B8: Sample selection of investigation outcomes

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

Note: The outcome is an indicator for whether an incident display a non-missing investigation outcome. In Panel A, the explanatory variable is the dummy treatment as defined in Table 2; in Panel B the continuous distance is measured in km and transformed in logs. LA refers to the 31 London LAs (excluding Westminster and City of London). Standard errors are clustered at the census block level *** p<0.01, ** p<0.05, * p<0.1. * p<0.1.

Table B9: Effects of closures on incident-level investigation outcomes

	Sample: Incidents with non-missing investigation outcomes								
	Pr(informal sanction)			Pr(going to court)			Pr(convicted)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Binary treatment									
dummy distance	-0.005***	-0.003***	-0.003***	-0.002*	-0.002	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Panel B: Continuous treatment									
log distance	-0.003***	-0.002***	-0.003***	-0.002*	-0.000	-0.001	-0.001	0.000	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	3,346,332	3,346,332	3,346,332	3,346,332	3,346,332	3,346,332	3,346,332	3,346,332	3,346,332
Mean Dep. Variable	0.06	0.06	0.06	0.10	0.10	0.10	0.09	0.09	0.09
Date FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
LAxDate FE		✓			✓			✓	
Crime type FE			✓			✓			✓

Note: The outcome variables are indicators equal to 1 if the incident was assigned an informal sanction (columns 1-3), went to Court (columns 4-6), or was assigned any conviction (columns 7-9). Informal sanctions correspond to "out-of-court" resolution and include: local resolutions, cautions, drugs possession warnings, penalty notice. Court outcomes include all incidents dealt by the Court. Convictions refer to incidents declared guilty of a criminal offense by the verdict of a court (thus exclude acquittals and discharges). In Panel A, the explanatory variable is the dummy treatment as defined in Table 2; in Panel B the continuous distance is measured in km and transformed in logs. *LA* refers to the 31 London LAs (excluding Westminster and City of London). The table displays the baseline mean of the outcomes. Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Reporting and police performance As the pool of reported incidents decrease, a valid concern arises regarding whether the reduction in reporting drives the overall effect on clearance. To address this concern, I construct the ratio between the number of charges and convictions vis-a-vis the number of reports for all investigated offenses, as well as separately for property and violent crimes. Table B10 shows a decrease in the ratio, suggesting that, although reporting falls, the police ability to clear incidents and convict criminals disproportionally worsened. The results for different types of crime provide two-sides of the same coin. From the one hand, out of the smaller pool of property offenses that are reported, the police demonstrate an even lower ability to clear them. On the other hand, despite the actual increase in violent offenses, police clearance marginally deteriorates, or at most remains unchanged. Furthermore, the negative coefficient for the property offenses ratio indicates that the decrease in reporting for this type of offense is less than the percentage change in clearance.

Table B10: Effects of closures on ratio of cleared and convicted offenses

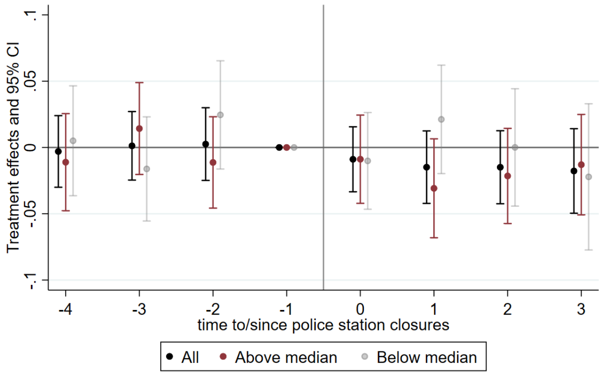
	Cleared crimes / reports			Convictions / reports		
	(1)	(2)	(3)	(4)	(5)	(6)
	All crimes	Property crimes	Violent crimes	All crimes	Property crimes	Violent crimes
Panel A: Binary treatment						
dummy distance	-0.004***	-0.005***	-0.002	-0.002***	-0.003***	-0.001
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)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

Note: This tables show the regression output the ratio between the total number of cleared crimes (columns 1-3) or of convicted crimes (columns 4-6) over the total number of reported crime. Convictions refer to incidents declared guilty of a criminal offense by the verdict of a court (thus exclude acquittals and discharges). Explanatory variables are defined as in Table 2. *LA* refers to the 31 London LAs (excluding Westminster and City of London). The table displays the baseline mean of the outcomes. Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

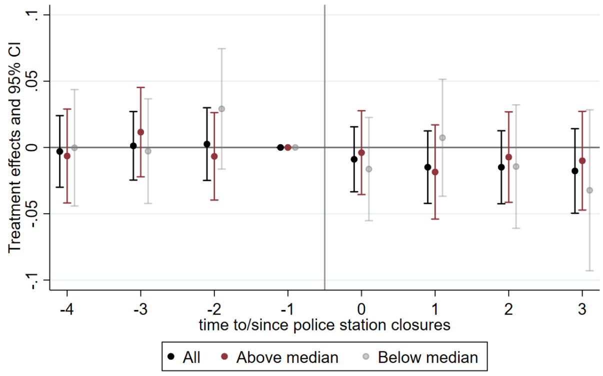
B3 Robustness checks on house prices

Figure B5: Event study for house prices

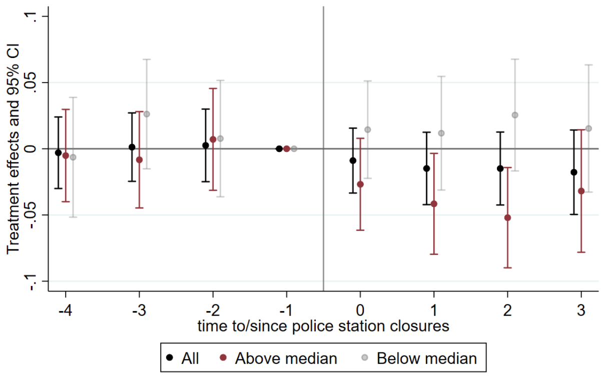
(a) Baseline crime rate



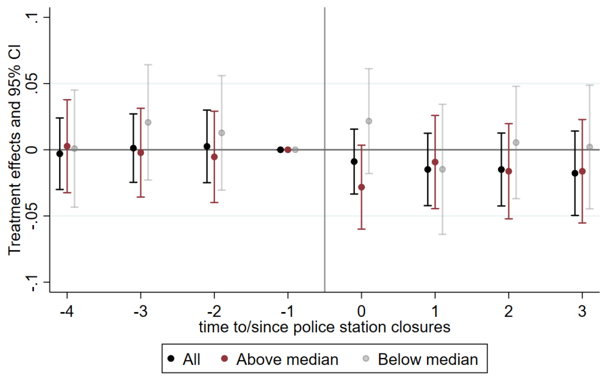
(b) Baseline violent crime rate



(c) Share of non-UK born



(d) Share of social housing



Note: The figures plot estimates from Equation 2, with 95% confidence intervals. The dependent variable is the average (log) house prices computed in the census block. Each panel shows the coefficients using all sample, and splitting the sample by baseline characteristics (above versus below the London median), respectively. Baseline crime rates used in panel A and B are computed using data from 2008 and come from LSOA-level MPS historical data. Baseline characteristics on the population share of non-UK born and social housing in panel C and D come from the Census (2011). Deprivation indices come from Indices of Multiple Deprivation in 2010, computed by the Ministry of Housing, Communities and Local Government. Time on the horizontal axis is computed by subtracting the date when a block's nearest police station closes from the quarterly date of the observation. I omit the dummy for the period before the closures. I omit the dummy for the period before the closures and, as suggested in Sun and Abraham (2021), I keeping a balanced sample of areas with 4 leads and 3 lags. All regressions include census block, calendar time (quarterly date) interacted with LA-specific dummies, and relative time fixed effects. The observations are weighted by the number of sales in the census block during the quarter. Standard errors are clustered at the census block level.

Sales of stations I test whether the effects on house prices are driven by the sales of the police stations, which expanded the local housing supply by placing on the estate market new regenerated housing units, and not by the removal of stations themselves. I therefore exploit the fact that around 50% of the closed police stations were sold, and 28% were then transformed into new residential buildings and explore whether house prices vary differentially between non-sold and sold stations, and depending on their intended estate use. 55% out of the 2,039 treated blocks had their closest station closed and sold, while for 36% of them, the closest stations were regenerated and transformed into new residential estates. The remaining sold stations were targeted to become public facilities, such as community and education centers. Table B11 shows the results of this triple-difference exercise. As a sanity check, Column 5 conducts the same heterogeneity analysis on violent crimes.

Table B11: Effect on house prices by destination of the closed police station

	Log house prices (weighted by the number of transactions)				Assaults and murders	Thefts
	(1)	(2)	(3)	(4)	(5)	(6)
dummy distance	-0.018**	-0.009	-0.018**	-0.010	0.111***	-0.109***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.020)	(0.017)
dummy distance * I[Sale]	0.009	0.001	-0.025*	-0.027*	0.042	-0.051*
	(0.010)	(0.010)	(0.014)	(0.014)	(0.035)	(0.028)
dummy distance * I[Residential]			0.048***	0.040***	-0.049	0.072**
			(0.013)	(0.015)	(0.035)	(0.029)
Observations	62,893	62,883	62,893	62,883	338,472	338,472
Date, LSOA FE	✓	✓	✓	✓	✓	✓
LAxDate FE		✓		✓	✓	✓

Note: Columns 1-4 show results of regressions on the quarterly-frequency dataset, where observations are weighted by the number of sales recorded in the census block during the quarter, and the dependent variable is the average (log) house prices computed in the census block. Column 5 and 6 use the same dataset as in Table 2. The dependent variable is assaults and murders (or thefts) transformed using the *asinh*. The explanatory variables are the dummy treatment as defined in Section 3, and the interaction of the treatment dummy with a dummy equal to 1 if the closest police station was sold, and if the closest police station was sold and then transformed into a new residential building. *LA* refers to the 31 London Local Authorities, excluding City of London and Westminster. Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Appendix C Crime Survey of England and Wales

Table C1 shows the descriptive statistics for the sample of the CSEW respondents living in London, that is the focus of my analysis. The sample is limited to 21,873 respondents who report residing in a London census block, with approximately 9,539 of them in treated areas, covering the years 2011-2016. The respondents cover 4,802 Lower-layer Super Output Areas (LSOAs), while the victim sample spans 3,169 LSOAs. Descriptive statistics for incident-level outcomes used in the regression analysis are presented in Table C2. A total of 5,182 individuals have been victimized, averaging 1.3 incidents per victim, and the overall incident-level reporting rate is 37.2%. The census-block date panel is unbalanced, with an average of 2 periods per LSOA.

Table C1: Descriptive statistics at respondent-level from CSEW

	mean	sd	N
All respondents			
Individual characteristics			
Male	0.456	0.498	21,873
Age	47.189	17.858	21,620
Student	0.051	0.221	21,867
No qualification	0.181	0.385	21,873
Higher education	0.624	0.484	21,747
In employment	0.83	0.376	20,034
Self-employed	0..170	9.375	20,034
British	0.777	0.416	21,833
Foreign-born	0.404	0.491	21,822
White	0.649	0.477	21,798
Asian	0.197	0.397	21,798
Black	0.129	0.336	21,798
Household-income: below 10k£	0.189	0.391	17,592
Household-income: [10-20k£]	0.204	0.403	17,592
Household-income: [20-30k£]	0.147	0.354	17,592
Household-income: [30-50k£]	0.196	0.397	17,592
Household-income: above 50k£	0.196	0.397	17,592
Household size	2.489	1.423	21,873
Number of children	0.543	0.951	21,873
Number of adults	1.946	0.982	21,873
Victim	0.236	0.425	21,873
Local characteristics			
Local deprivation index	4.492	2.5	18,823
Local crime rate (police records)	0.032	0.041	21,873
Violent crime rate (police records)	0.006	0.006	21,873
Property crime rate (police records)	0.013	0.022	21,873
Trust			
lack of confidence in police effectiveness	0.329	0.47	10,824
lack of confidence in CPS effectiveness	0.401	0.49	10,011
lack of confidence in CJS effectiveness	0.441	0.497	10,489

Note: This table provides descriptive statistics for the characteristics of the respondents (victims and non-victims) to the CSEW. The sample used for analysis is limited to respondents who were residing in London at the time of the survey interview, and who participated in the survey between 2011 and 2016. Questions regarding confidence in the police, Crown Prosecution Service (CPS), and Criminal Justice System (CJS) are asked to 50% of the London respondents (i.e. those who completed Module A and B of the CSEW).

Table C2: Descriptive statistics at incident-level from CSEW

	mean	sd	N
All victims (incident-level dataset)			
=1 if incident reported	0.372	0.483	6,706
=1 if incident reported by type of crime:			
violent crime	0.042	0.201	6,706
any assaults	0.029	0.167	6,706
common assaults	0.019	0.137	6,706
serious assaults	0.01	0.099	6,706
sexual offences	0.002	0.044	6,706
robberies	0.012	0.107	6,706
property crimes	0.282	0.45	6,706
theft	0.164	0.371	6,706
burglaries	0.081	0.272	6,706
criminal damage	0.037	0.188	6,706
threats	0.025	0.157	6,706

Note: This table provides descriptive statistics for the incident-level outcomes built for the subsequent regression analysis. The sample used for analysis is limited to respondents who were residing in London at the time of the survey interview, and who participated in the survey between 2011 and 2016. The table restricts to victims only and uses incident-level data.

Table C3: Effects of closures on reporting by type of offenses

	=1 if incident reported									
Type of offence	common assaults		serious assaults		robberies		property crimes		thefts	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: binary treatment										
dummy distance	0.006	-0.001	0.004	0.004	-0.003	0.000	-0.049	-0.053	-0.021	-0.027
	[0.014]	[0.014]	[0.008]	[0.008]	[0.010]	[0.010]	[0.040]	[0.043]	[0.032]	[0.035]
Panel B: Continuous distance										
distance	0.000	-0.004	0.002	0.001	-0.002	0.001	-0.050	-0.054	-0.028	-0.026
	[0.011]	[0.010]	[0.004]	[0.004]	[0.006]	[0.006]	[0.035]	[0.034]	[0.026]	[0.027]
Observations	5,405	5,405	5,405	5,405	5,405	5,405	5,405	5,405	5,405	5,405
Mean dep. Variable	0.016	0.016	0.011	0.011	0.014	0.014	0.288	0.288	0.177	0.177
Date FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
LSOA FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
LA-specific linear trends		✓		✓		✓		✓		✓

Note: The table displays estimates from the CSEW restricting the sample to all incidents occurred in the 12 months prior the date of the interview experienced by respondents living in London. The outcome variables are type of incident-specific indicators for reporting an offense. Explanatory variables are defined as in Table 4. *LA* refers to the 33 London LAs. The table displays the baseline mean of the outcome. Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Table C4: Effects of closures on confidence in police and Criminal Justice System

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

Note: The table displays estimates from the CSEW restricting the sample to all respondents (victims and non-victims) living in London. The outcome variables are built from the following questions: (i) *How confident are you that the police are effective at catching criminals?* for columns 1-2; (ii) *How confident are you that the Crown Prosecution Service is effective at prosecuting people accused of committing a crime?* for columns 3-4; (iii) *How confident are you that the Criminal Justice System as a whole is effective?* for columns 4-6. The outcomes are defined as equal to 1 for respondents who answer either *"Not very confident"* or *"Not confident at all"*. Explanatory variables are defined as in Table 4. *LA* refers to the 33 London LAs. The table displays the baseline mean of the outcome. Standard errors are clustered at the census block level. *** p<0.01, ** p<0.05, * p<0.1.

Appendix D Cost-effectiveness of police station closures

D1 Capitalization approach

Table D1: Cost-benefit analysis using capitalization approach

	(1)	(2)
Sample	All sample	High-crime areas
Program component	Value (£)	
Panel A: Police station closure cost		
Average house prices in treated blocks	450,369	445,316
Estimated decrease in price per treated block-quarter	5,404	6,680
Total cost for private owners	264,471,089	153,420,268
Panel B: Police station closure benefits		
Total savings	600 million	
Total savings per treated block	294,262	
Savings per treated block per treated block-quarter	12,261	
Cost/Benefit	0.44	0.54

Note: This table shows the cost-benefit comparison using house price estimates. In Panel A, I quantify the costs using house prices estimates outlined in Section 4.5. I compute the average house prices for the pre-period, i.e. before January 2013. Column 1 keeps all the sample, column 2 restricts the sample to blocks with higher than median baseline (2008) crime rate. In Panel B, I compute the total number of treated blocks between 2012 and 2016, the period where the MPS planned to make the savings. The total planned savings come from the MPS estate strategy (MOPAC, 2017). I count 2,039 treated blocks (957 high-crime treated blocks between 2011 and 2016. Total costs are computed *cost per treated block-quarter* × *number of treated blocks* × *number of quarters*.

D2 Costs generated by the police station closures

I compute the crime-related costs associated to the police station closures as the sum of the i) deterrence ii)and incapacitation costs, and iii) social cost of crime. First, I compute the costs associated to the additional crimes generated when the clearance rate decreases because of the police station closures. A lower clearance rate encourages potential criminals to offend because of lower deterrence, and therefore results in more crimes. I use the estimates of the total economic and social costs of crime from the Home Office Report (Heeks et al., 2018), which includes UK-based calculations of the costs in anticipation of crime (e.g. defensive expenditure and insurance), costs as a consequence of crime (e.g. physical and emotional harm, lost output, victims' services) and costs in response to crime (e.g. police and criminal justice costs). The average cost per crime is £7,106 (Appendix Table D2). In Appendix Table D3, I quantify the deterrence costs generated by the lower clearance rate resulting from the closures. Specifically, I measure the total costs following a lower likelihood of clearing crime equal to 0.6 pp, the coefficient estimated in Table 5. To do that, I first assume a conservative estimate of elasticity of crime on clearance rate equal to -0.1. This comes from Levitt (1998b), is the same used by Blanes i Vidal and Kirchmaier (2018) and is at the lowest bound of estimates.[48] I then use the estimates of the unit cost of crime from Appendix Table D2. Overall, I estimate a cost of £13 million resulting from a decrease in the clearance rate equal to 0.006. Second, I compute the incapacitation costs that stem from the impacts on crime types reported in Section 4.[49] Appendix Table D4 calculates the additional years of incarceration following the increase in crime. I adopt a conservative approach and take into account the reductions in reported property crimes, which reduces the cost burden of the police. Using MPS data on investigation outcomes, I account for the fact that only a fraction of crimes is convicted, and only a small fraction of convicted offenders are incarcerated. Because the U.K. criminal justice system is relatively lenient, conditional on incarceration, average custodial sentences are relatively short. Furthermore, I account for the fact that the transitions between different stages of the criminal justice system vary greatly across crime types. To illustrate, robberies comprise only a small proportion of crimes but lead to a high number of additional years of incarceration, while the opposite is true for criminal damage crimes. I calculate that the changes in crime would lead to an extra 17,000 years of incarceration and to a total cost of £560 million. The social costs of crime combines estimates of the additional number of crimes committed due to the police station closures (Table D4) and the additional number of non-deterred crimes due to lower clearance (Table D5). I compute a total social cost of the closures of approximately £180 million.

[48]Using the violent crime-clearance elasticity computed in Section 4.3 I would derive a deterrence cost four times larger.

[49]In this section, I compute the fiscal costs of higher incarceration. I do not include all the social costs of higher incarceration, in terms of economic impact (i.e., reduced employment, greater reliance on public assistance) and post-release criminal behavior. I therefore likely underestimate the actual costs of incarceration. Furthermore, given that I do not observe individuals, I assume that each criminal incident is associated to a different individual.

Table D2: Cost of crime

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

Note: The unit cost of crime for each of the crime categories in column 1 is obtained from Table 1 of the Home Office report (Heeks et al., 2018) and the number of offenses in column 2 is obtained from Table 4 of the same report. Column 3 is the weighted average cost of crime, computed using the frequencies of each sub-component as weights. The reports use 2015/2016 prices.

Table D3: Deterrence costs due to decreased clearance

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

Note: The proportion of crimes and the number of offenses by crime category are computed restricting the sample to the pre-period, i.e. before June 2013 (columns 1-2). I omit other crime categories because there is no corresponding crime cost computed in the Home Office report (Heeks et al., 2018). Columns 3 and 5 are calculated as indicated in the bottom row. We assume an elasticity of crime on the clearance rate of -.1 as in Blanes i Vidal and Kirchmaier (2018). To compute the additional number of crimes non deterred, I multiply .1 (the assumed crime-clearance elasticity) by .006 (the β increase in the clearance rate) divided by .19 (the average clearance rate) and by the total number of incidents in the pre-period.

Table D4: Incarceration costs of crime due to increased crime

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

Note: This table shows the calculations of the incarceration costs resulting from changes in recorded crime. Column 2 uses estimates by crime type from Tables 2 and B1. The probabilities of conviction and incarceration (conditional on conviction) are computed restricting the sample to the pre-period (i.e. before June 2013). The average custodial sentence length come from the Criminal Justice Statistics Quarterly Update (Ministry of Justice, Dec. 2012). The cost per incarceration year is £34,766 and is computed by the Ministry of Justice (*Costs per place and costs per prisoner*, Ministry of Justice, 2014).

Table D5: Criminal justice savings from decreasing clearance rate by β pp

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

Note: Column 1 corresponds to the number of crimes non-deterred computed in Table D3. The multiplier of each crime categories (column 2) is obtained from the Home Office report (Heeks et al., 2018, Table 4 in). Columns 3-4 are derived from Table 23 of Heeks et al. (2018). Prison and probation costs include costs related to: probation service, prison service and the National Offender Management Service headquarters. Criminal justice system costs include: costs in terms of prosecution, magistrates' court, crown court, jury service, legal aid, non legal-aid defense, youth justice board. Police costs are estimates of the opportunity-cost of police time and resources taken up by investigating crime rather than engaging in other activities (e.g. responding to non-crime activities) (Dubourg et al., 2005).

D3 Savings generated by the police station closures

Detecting and clearing less crimes results in greater savings due to a lower CJS expenditure. Appendix Table D5 calculates the CJS savings resulting from decreasing the clearance rate by 0.6 pp. The calculations include costs associated to prosecution, courts' functioning and legal aid as per the Home Office Report (Heeks et al., 2018). I include in the calculations the savings from lower probation and prison expenditures, equal to £1.8 million (column 7 of Table D5). Overall, I estimate a total saving resulting from the station closures for the CJS and the police of £10.1 million and £21 million, respectively. After comparing the savings of police station closures with the potential costs generated by greater criminal activity, I conclude that closing stations is not a cost-effective way to implement public spending cuts.

D4 Marginal Value of Public Funds

I compute the marginal value of public funds (hereafter MVPF, Finkelstein and Hendren, 2020; Hendren and Sprung-Keyser, 2020) as the ratio of society's willingness to pay for a policy to the net cost of the policy to the government. Table D7 summarizes the calculations of the MVPF of closing police stations. I compute a MPVF ranging from 2.6 to 7.1.[50]

Willingness to pay I start by computing the numerator of the ratio, which measures the aggregate social willingness to pay for the policy change. The primary component is society's willingness to pay for additional crimes, which quantifies of the total social cost of crime (Table D6) The average cost of crime is computed in Table D2. I combine it with the estimates of the additional number of crimes committed (Table D4) and the additional number of non-deterred crimes due to lower clearance (Table D5). I compute a total social cost of the closures of approximately £180 million (column 1).

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

Table D6: Total social costs of crime

Crime category	(1) Average cost of crime	(2) Non deterred crimes	(3) Extra crimes committed	(4) Total cost of non deterred crimes	(5) Total cost of extra crimes committed
Violent crimes					
- *Violence against the person*	10,761	578	16,485	6,224,555	177,396,556
- *Robbery*	351	143	-272	50,311	-95,510
Property crimes					
- *Domestic burglary*	5,930	401	254	2,375,809	1,506,220
- *Theft*	1,664	16	-537	25,889	-893,715
- *Criminal damage*	1,505	258	-2,372	388,852	-3,570,568
All	7,106	1,396	13,558	9,065,416	174,342,983
Calculations	From Table D1	From Table D3	From Table D4	(1) x (2)	(1) x (3)

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

Column 2 adds the willingness to pay for worsened labor market prospects by the individuals whose likelihood of incarceration increases following the increase in violent crimes.[51] I compute the total loss in wages they experience from this policy change. I consider as population at risk of incarceration youth aged 19-25. To 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 the HM Revenue & Customs. I calculate the total foregone income of affected individuals during incarceration by multiplying the number of individuals who were incarcerated because of the higher crime rate from Table D4 $\times$ the employment rate $\times$ the annual median income $\times$ the average sentence served from Table D4. The total foregone income of affected individuals during incarceration is equal £19 million. Column 3 adds to the baseline calculation the total loss in house prices following the lower sales. It uses estimates of the total cost for private home owners from Table D1. Introducing this cost increases the aggregate willingness to pay to £450 million.

Net cost of government The denominator of the MVPF captures the net cost to the government of the policy, and includes both mechanical costs and fiscal externalities. The mechanical costs are the public savings from the police station closures equal to

[51] Applying the envelope theorem would entail excluding this component from the WTP. 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.

£600 million (MOPAC, 2017). I add the fiscal savings for the CJS computed in Table D5 (column 7, 8, 10). In column 2 I add as fiscal externality the foregone income tax revenues driven by lower employment due to higher incarceration. To quantify it, 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 and Customs. Column 3 considers the lost tax revenues resulting from a decrease in house sales, specifically related to the stamp duty land tax, which is a sale tax imposed on those property transactions. I use a conservative stamp duty rate of 3% which applies to property between £250,000 to £500,000. I do not account for fiscal externalities related to council tax (analogous to a property tax) for rents.

Table D7: Calculation of the marginal value of public funds

	Value (£) (1)	Value (£) (2)	Value (£) (3)
Willingness to pay			
Society's willingness to pay for additional crimes	-183,408,399	-183,408,399	-183,408,399
Willingness to pay for worse labor market prospects by additional incarcerated individuals		-19,027,866	
Total loss in house prices for sales			-264,471,089
Aggregate WP	-183,408,399	-202,436,265	-447,879,487
Net cost to the government			
Mechanical savings from the closures	-600,000,000	-600,000,000	-600,000,000
Fiscal externalities:			
- Fiscal costs of incarceration	562,779,812	562,779,812	562,779,812
- Foregone income tax revenues		2,160,020	
- Lower revenues from lower stamp duty land tax			7,934,133
- Fiscal savings of Police	-21,246,306	-21,246,306	-21,246,306
- Fiscal savings of Prison and Probation	-1,805,686	-1,805,686	-1,805,686
- Fiscal savings of CJS	-10,099,210	-10,099,210	-10,099,210
Net cost	-70,371,390	-68,211,371	-62,437,258
Aggregate WP / Net Cost	2.61	2.97	7.17

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

D5 Alternative policy option

The most immediate policy response to increases in violence is to increase the recruitment of officers. To compute the financial implications of this, I use the UK Labour Force Survey (LFS), which contains information on occupations, hourly pay, and number of hours worked per week (I consider 52 weeks in the UK). In the period prior to the closures, the median annual pay for police officers was £35.5k, and the average £40k. As discussed in Section 4, the closure of police stations resulted in a spike in the number of violent crime per year equal to 5,500. I borrowing the police-crime elasticities from Draca et al. (2011) (.32 for all crimes and .38 for susceptible crimes), I calculate that compensating such increase in crime would require hiring an additional 14,000 to 17,000 officers. The associated costs would range from £51k to £68k. This additional costs would completely offset any cost savings stemming from the closure of police stations, estimated to be around £600 million.