The Unintended Political Consequences of Expanding Police Authority: Evidence From London[*]

Victoria Biagi[†] Enrico Cavallotti[‡] Abhinav Khemka[§]

Abstract

Policies expanding police discretionary authority are widespread and politically contested, yet their effects on police behaviour, institutional trust, and political preferences remain poorly understood. We study Britain's 2014 Anti-Social Behaviour, Crime and Policing Act, which authorised local councils to implement Public Space Protection Orders (PSPOs), prohibiting broadly defined anti-social activities and expanding officer discretion in designated areas. Using the first comprehensive dataset on PSPO implementation across London census tracts (2016–2024), linked to crime records, stop-and-search data, and electoral outcomes, we exploit PSPOs' staggered adoption to identify causal effects. These policies increase unproductive police stop-and-search activity, disproportionately targeting non-white residents. Reported anti-social behaviour and other crimes remain unaffected. These findings suggest that vaguely defined discretionary enforcement tools can generate racially unequal policing without delivering the public order benefits that justify their adoption.

Keywords: Police; Anti-social behaviour; Stop-and-search; Political backlash; Local governance
JEL Codes: K42, D72, J15

[*]We thank seminar audiences at the CLEAN Unit Winter Workshop, NWSSDTP Doctoral Conference, EUI internal seminar, and University of Liverpool internal seminar for invaluable comments and suggestions. The usual disclaimer applies.

[†]University of Liverpool, Economics Group
[‡]European University Institute, Department of Economics.
[§]Universitat Autonoma de Barcelona, Department of Economics.

1 Introduction

In recent decades, many governments have expanded the discretionary powers of their police forces as a comparatively low-cost strategy for managing public order and safety. Instead of increasing police numbers—a process that is typically slow, administratively complex, and politically fraught—authorities have opted to grant police broader autonomy in identifying, addressing, and sanctioning behaviours deemed disruptive. This shift is visible across multiple jurisdictions, reflected in trends such as the militarisation of local police units, the introduction of suspicion-less stop-and-search powers, and the criminalisation of formerly tolerated forms of passive resistance (White House, 2025; UK Home Office, 2023; Italian Council of Ministers, 2025). Although such measures promise institutional flexibility and administrative efficiency, they also recalibrate the balance between state authority and individual rights. Discretion has always been central to policing (Finnane, 1990), but its expansion raises distinct concerns when powers are broadly framed or lack clear procedural constraints (Yao, 2023). Under these conditions, traditional mechanisms of oversight and accountability may not scale proportionally to the authority delegated to frontline officers, creating uncertainty about how these powers are applied in practice and what outcomes they generate. Despite renewed policy interest and ongoing debates over whether enhanced discretion improves public safety without infringing civil liberties (Owens, 2020), empirical evidence on the broader societal and economic consequences of these reforms remains limited.

A central rationale behind these expansions draws on the "broken windows hypothesis" (Kelling and Wilson, 1982), which posits that visible signs of disorder left unaddressed signal the absence of social control and invite escalation to more serious offending (Burney, 2009). Under this logic, criminalising pre-criminal and sub-criminal conduct—such as loitering, public gatherings, or street drinking—serves a dual purpose: it addresses neighbourhood nuisance directly, and it functions as an investigative gateway through which officer-citizen encounters initiated for minor infractions can surface evidence of more serious criminal activity (Archer, 2023; Laniyonu, 2019). Anti-social behaviour legislation has accordingly been shaped by this dual ambition, treating the enforcement of low-level disorder not merely as an end in itself but as a proactive strategy for detecting and deterring more serious crime. Whether this rationale translates into measurable crime-reduction benefits, and who bears the costs of its unintended consequences, remains empirically contested.

To address this gap, in this paper we investigate how expanding police discretionary power affects police conduct. We then study the subsequent effects on individuals' political engagement. Our empirical setting exploits the introduction of Public Space Protection Orders (PSPOs) in London beginning in 2016. Created under the 2014 legislation in England and Wales, PSPOs empower local councils to prohibit specific behaviours—sometimes defined in very broad terms, such as "using foul and abusive language",[1] "reckless be-

[1]Source: ITV News, 2nd March 2016.

haviour", or "gathering in a group"[2]—classified as antisocial but not previously deemed criminal. Crucially, PSPOs do not expand police stop-and-search powers and do not mandate additional officer deployment, but simply increase the pool of anti-social behaviour crimes. By expanding the scope of police discretion in affected areas, we show that these Orders lowered the threshold for officer suspicion and lead to unintended significant changes in policing practices unrelated to mere anti-social behaviours detection. We then examine how residents' electoral behaviour shifted following PSPO adoption, focusing on party-related political preferences.

To do so, we assemble a novel dataset that draws on multiple sources. First, we hand-collect the universe of PSPOs issued in the Greater London Area from 2016 to 2024, compiling detailed information on their timing, content, and geographic coverage. We retrieve the full text and associated map of each implemented Order and code their boundaries at the census-tract level.[3] By 2024, 87% of London had been subject to at least one PSPO, with the majority implemented between 2017 and 2018. Notably, around 20% of treated areas were covered by Orders with broad or vaguely specified targets, often addressing activities such as assembling, loitering, or simply gathering in groups. Next, we construct a census-tract-by-semester panel from 2016 to 2024 by merging the PSPO dataset with administrative data on criminal incidents and police stop-and-searches obtained from the UK Police repositories. Crime records report the type, location, and date of each incident, while stop-and-search data include the demographic characteristics of stopped individuals and the ethnicity of the officer involved. To measure changes in political responses to PSPOs, we analyse party-level vote shares in council elections from 2014, 2018, and 2022. We exploit the staggered introduction of PSPOs at the census-tract level to causally identify the policy's effect, estimating event-study models using recent advances in the difference-in-differences literature that allow for variation in treatment timing across units (Sun and Abraham, 2021).

Following the implementation of the PSPO, we find no change in reported incidents of anti-social behaviour, the primary target of the policy. This null effect may reflect either the limited effectiveness of the policy or a deterrence mechanism: as the set of behaviours classified as anti-social increased, local residents may have become more aware of the risk of sanction and refrained from engaging in such behaviour. On police behaviour unrelated to anti-social behaviour crime detection, we document a sharp increase in stop-and-search activity in treated areas, amounting to approximately 7 additional stops per census tract per semester, roughly one year after PSPO adoption. Critically, the composition of search outcomes shifts toward unproductive stops: treated areas exhibit a rise in the number of stops and searches ending with no further action, while officer-detected crimes remain unchanged. Moreover, this increase is concentrated almost entirely among non-white residents: a triple-difference design reveals a differential effect of 1.363

[2]Source: The Guardian, 6th December 2024.

[3]Census tracts correspond to Lower Super Output Areas (LSOAs) under the 2011 UK Census classification; London contains approximately 4,800 LSOAs.

additional searches per 1,000 inhabitants per semester for non-white relative to white individuals, consistent with minority status acting as a proxy for suspicion when the *de facto* costs of initiating stops are reduced by PSPO designations (Knowles, Persico and Todd, 2001*a*). Taken together, these patterns are consistent with pretextual enforcement: PSPOs are likely to lower the threshold for officer-citizen encounters without generating the investigative or deterrent benefits used to justify them. Finally, we provide suggestive evidence about the policy's political costs. Treated electoral wards experience a shift in political support away from the main architect parties of the legislation at the national level, towards UKIP and the Labour Party, broadly suggesting that the policy generated a broad-based backlash among affected communities.

This paper speaks to three strands of the literature. First, it contributes to the growing body of research studying the effects of policing policies on crime (Draca, Machin and Witt, 2011; Facchetti, 2024; Mastrobuoni, 2019). While this literature has widely discussed the effects of changes in the presence of police officers on crime, it has paid limited attention to the quality and nature of policing practices. We shift the focus onto the expansion of discretionary police power, providing causal evidence on how granting officers broader enforcement authority shapes crime outcomes and police behaviour beyond what deployment alone can explain.

Second, this paper adds to the literature on the drivers of police behaviour and discrimination, expanding the analysis on how individual officer characteristics, implicit bias, and local institutional contexts shape enforcement patterns (Barilari and Zambiasi, 2025; Hoekstra and Sloan, 2022; Knowles, Persico and Todd, 2001*a*; LeRoy, 2024; Mastrorocco and Ornaghi, 2025). We complement these contributions by showing that the criminalisation of minor and generically defined behaviours creates structural conditions for discriminatory enforcement, expanding police authority in ways that leave substantial room for discretion. Crucially, we show that this discretion is systematically exercised along racial lines, with non-white residents bearing a disproportionate share of increased enforcement activity, consistent with statistical and taste-based discrimination mechanisms documented in this literature.

Third, our paper contributes to the literature investigating the relationship between policing, political trust, and electoral outcomes. Prior research has predominantly focused on the political consequences of violent police behaviour, documenting how the use of force and the occurrence of high-profile incidents of police violence erode institutional trust and reshape civic engagement (Morris and Shoub, 2024; Tyler, Fagan and Geller, 2014; Weaver and Lerman, 2010). We broaden the scope of this literature by suggesting that the political costs of policing are not confined to extreme incidents, but are also produced by the everyday, structural misuse of discretionary power, offering new preliminary insights into how routine enforcement practices shape political behaviour in affected communities. Even non-violent expansions of police discretionary authority, operating through the systematic over-policing of minor and vaguely defined offences, might be sufficient to erode institutional trust and generate measurable electoral backlash.

The remainder of the paper is structured as follows: Section 2 discusses the institutional background, Section 3 describes the conceptual framework, while Section 4 introduces the data. Section 5 illustrates the empirical strategy, Sections 6 and 7 discuss the main results and robustness checks, and Section 8 highlights our main conclusions.

2 Institutional Background

In this Section, we outline the institutional framework governing the regulation of antisocial behaviour in London, with a focus on the roles of local authorities and police forces. We then discuss the political economy of Public Space Protection Orders (PSPOs) and explain how their introduction may lead to a substantial expansion of discretionary policing authority.

2.1 Legal Framework of Anti-Social Behaviour

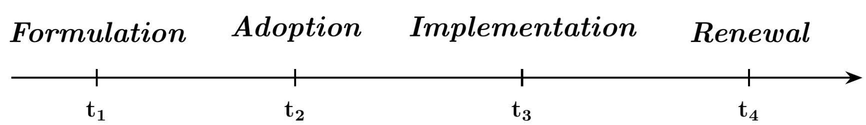
The concept of antisocial behaviour (ASB) emerged in the United Kingdom during the early 1990s, predating its formal legal definition defined by the Crime and Disorder Act 1998. These concerns were far from uniquely British: low-level disorder, persistent nuisance, and community-level distress were felt across Western societies (Skogan, 1992; Kelling and Wilson, 1982). In the United Kingdom, however, the question of how to address sub-criminal behaviour acquired a particular political salience. Across the ideological spectrum, official rhetoric converged on the twin imperatives of community safety and local-level enforcement. It was in this context that Tony Blair, in 1993, repositioned the Labour Party around the formulation *tough on crime, tough on the causes of crime*, a rhetorical and programmatic shift that embraced a new approach to low-level local enforcement. This approach recognised the responsibility of local authorities and communities to maintain public order, operating on the premise that doing so would, in turn, address deeper problems of social deprivation and localised criminality. The agenda drew heavily on Broken Windows theory (Kelling and Wilson, 1982), which held that visible signs of disorder, left unaddressed, signal an absence of social control and create the conditions for escalation to more serious crime.

It was within this political context that the Crime and Disorder Act 1998 introduced the first formal legal definition of anti-social behaviour in England and Wales. The Act introduced Anti-Social Behaviour Orders (ASBOs), civil injunctions targeting individuals whose conduct ""*caused or was likely to cause harassment, alarm or distress to one or more persons not of the same household"* (Home Office, 1998). As a result, the Act established statutory partnerships between local authorities and police forces, enabling the restriction of specific persons' behaviour, with breaches constituting a criminal offence.

2.2 Public Spaces Protection Orders (PSPOs)

This historical trend gave rise to a new wave of institutional localism. The Conservative Party's Big Society Agenda (Cameron, 2010) led to the devolution of dispersal powers from central to local government. Public Spaces Protection Orders (PSPOs) were introduced in England and Wales under the Anti-Social Behaviour, Crime and Policing Act 2014, passed by the Conservative-Liberal Democrats' coalition government with little political opposition (Brown, 2017). In this respect, PSPOs reflected a broad consensus in favour of stricter local regulation of public spaces. In some boroughs, PSPOs replaced earlier instruments, most notably Designated Public Place Orders (DPPOs), and Dog Control Orders (DCOs), which had focused solely on alcohol restrictions and dog control, respectively. While these Orders followed a similar legislative process, their scope was considerably narrower and more clearly defined than that of PSPOs. Policy-making powers over PSPOs are fully decentralised to local authorities,[4] with the policy process unfolding in four main steps as shown in Figure 1. First, local councils are responsible for identifying a specific behaviour conducted in public spaces that negatively affects the quality of life of residents on a continuous basis. These provisions are designed to capture broadly defined *anti-social* conduct that would not otherwise constitute a criminal offence. In practice, PSPOs may regulate dog control, begging, and littering, as well as more ambiguous activities such as street noise, public gatherings, or speaking in public spaces. Councils are required to define a specific geographical area that the Order will cover and to attach a corresponding map (Brown, 2017; Archer, 2023). Second, local authorities must consult with local policing bodies and community representatives before adoption, and may provide further publicity within the affected area through local signage.[5] Once adopted, PSPOs are enforced by police officers or private security agencies operating within the designated area. Orders last three years and may be renewed for additional three-year periods without limit. Notably, no evidence of an Order's effectiveness is required for renewal, though consultations may be conducted again before each renewal.

Figure 1: PSPOs policy process



When an officer encounters an individual engaging in conduct prohibited by a PSPO, the Act prescribes a graduated enforcement response. The officer may issue a verbal warning, requiring the individual to cease the prohibited behaviour. Where the individual fails to comply, the officer may issue a Fixed Penalty Notice of £100. Persistent or more serious non-compliance may result in arrest under Section 59 of the Anti-Social Behaviour, Crime and Policing Act 2014. This enforcement structure grants officers considerable discretion in determining both whether a prohibited behaviour is occurring and which response is proportionate, without

[4] Archer (2023) explains how neither central government nor the judiciary exercises oversight over the reasons behind the formulation of PSPOs.

[5] What constitutes the affected population and what evidence is required to justify an Order is entirely at the discretion of local authorities. In 2018, the Home Office (2018) released a Guidance Tool for local councils to formulate balanced and equitable PSPOs, which are not intended to specifically target vulnerable populations.

any requirement to document the grounds for escalation (Archer, 2023).

2.3 Police and Stop-and-Search Practices In London

Law enforcement in the Greater London Area (GLA) is directed by the Metropolitan Police Service (MPS), which organises and deploys officers across 12 local Basic Command Units (BCUs). Police officers in England and Wales exercise powers spanning a wide range of functions, including public reassurance, crime reduction, emergency services, and road traffic control (Bowling, Reiner and Sheptycki, 2019). As part of their patrolling routine, officers may stop and question individuals to account for their presence or behaviour, and may exercise statutory powers to stop and search persons or vehicles in public places. The primary legislative framework governing these powers is the Police and Criminal Evidence Act (PACE) 1984 (Home Office, 1984), which authorises officers to stop and search a person, a vehicle, or both in a public space where there is reasonable suspicion of possession of weapons, objects capable of causing criminal damage, drugs, or firearms (Bridges, 2015). These powers were subsequently expanded by Section 60 of the Criminal Justice and Public Order Act 1994 (Home Office, 1994), which dispenses with the reasonable suspicion requirement in circumstances involving a credible terrorist threat or an area known for intensive weapon carrying, permitting a senior officer to authorise mass stop and search for a period of 24 to 48 hours.[6] The exercise of these powers is formally constrained by obligations of fairness and proportionality. The MPS policy explicitly commits officers to ensuring that stop and search *"is used fairly and effectively"*, requiring that searches be justified, lawful, and necessary, and that individuals are treated with respect regardless of their background, in compliance with the Equality Act 2010 and Article 14 of the Human Rights Act 1998 (Metropolitan Police Service, 2020).

2.4 PSPOs and Searching Powers

This decentralised framework has generated substantial inter-jurisdictional heterogeneity in both the conduct targeted and the frequency with which sanctions are applied. A key implication is that PSPOs expand discretion along two dimensions. First, institutional discretion allows local councils to set the geographic scope of Orders, define prohibited behaviours, and decide whether enforcement is partly outsourced to private security agencies. Second, frontline discretion allows officers to determine when an informal warning escalates into conduct which informs the reasonable suspicion required to conduct a stop and search. Crucially, the 2014 Act grants no new stop-and-search powers and does not amend PACE 1984 or Section 60 of the Criminal Justice and Public Order Act 1994, nor does it mandate increased police presence within designated areas (UK Home Office, 2014). Enforcement proceeds exclusively through Fixed Penalty Notices of up to £100 (s. 68) and summary prosecution for non-compliance (s. 67), leaving the legal basis for search entirely unchanged. Any increase in stop-and-search activity following PSPO introduction, therefore, reflects

[6] Stop and searches registered under PACE account for 97.2% of the total number of stop and searches in our sample, while those conducted under Section 60 represent only 2.8%.

discretionary officer behaviour rather than a legal mandate. Two institutional features of MPS operational practice reinforce this mechanism. Internal MPS training materials confirm that responses to ASB follow a formal tiered escalation protocol in which stop and search plays no prescribed role (Police, 2026*b*). Officers are instead trained to analyse ASB spatially using the SARA/VOLT framework, in which the geographic designation of a problem area functions as a primary analytical input (Police, 2026*a*). PSPO designations thus directly activate the location-based reasoning officers are institutionally trained to apply, providing a plausible and precise mechanism through which spatial labelling shapes discretionary enforcement. Together, these sources of discretion have created structural conditions for arbitrary and potentially discriminatory enforcement, particularly where Orders are vaguely formulated, and the space for individual officer judgement is correspondingly wide (Livingston, 1997). As *The Guardian* reported in late 2024, fixed penalty notices issued for offences including swearing, shouting, loitering, and begging rose by 42% in 2023, with commentators noting that almost anyone could fall foul of broad prohibitions on activities such as excessive noise, nuisance, or gathering in a group.[7] Existing evidence further suggests that these two margins of discretion interact to produce unequal exposure to sanctions, particularly among visible and vulnerable groups (Brown, 2017; Archer, 2023; Heap and Dickinson, 2018).

3 Conceptual Framework

The effects of PSPOs on anti-social behaviour and police conduct are *ex ante* uncertain, depending on how the expansion of police authority affects localised criminal conduct and whether it triggers unintended consequences through changes in police discretion.

As mentioned in Section 2, PSPOs are not intended to expand police powers of any type, and do not mandate additional police deployment in the treated area. Instead, PSPO legislation criminalises specific anti-social behaviour in a designated area, where individuals weigh the expected benefits of committing ASB against the probability of detection and the severity of sanctions. A PSPO may raise the perceived probability of sanctions being detected for ASBs within the designated area, thereby reducing the net benefit of committing anti-social behaviour. Under a deterrence mechanism, reported ASB incidents may rise in the short run, as officers actively enforce the new rules and incidents that would previously go unrecorded are now formally logged, before falling in the long run as individuals internalise the new constraints, while other policing-related activities such as stop-and-search and officer-detected crimes remain unaffected. Given that the geographic scope of PSPOs is limited, enforcement within treated areas may simply push anti-social behaviour into neighbouring areas rather than eliminating it. The spatial equilibrium literature suggests that offenders re-optimise over locations when the cost of offending in one area rises, making the net social effect of place-based interventions ambiguous (Chalfin and McCrary, 2018; Weisburd et al., 2006). This would imply a reduction of ASB in the treated area and a recorded increase of ASB in neighbouring ones,

[7] The Guardian, 6th December 2024.

with other policing activities and officer-detected crimes unchanged.

It is also possible that the introduction of PSPOs creates the conditions for additional encounters between police officers and citizens, potentially lowering the legal threshold required to initiate other powers, including stop and search. If officers use this expanded authority and lower their suspicion threshold productively, we should both observe an increase in the number of stop and search which led to arrests, warnings, or sanctions, and higher detection rates: weapons, drugs, and public order offences, collectively officer-detected crimes (ODCs), which should rise alongside search volumes. A growing empirical literature finds that targeted, intelligence-led policing of this kind can reduce crime (Ratcliffe et al., 2011; Di Tella and Schargrodsky, 2004). Alternatively, officers may engage in pretextual policing in PSPO-designated zones, *de facto* expanding not only the legal threshold for suspicion but also the discretionary space within which stop and search occur. Following Knowles, Persico and Todd (2001*a*), if PSPO designation lowers the *de facto* cost of initiating a stop, officers may optimise over stop volume rather than productive outcomes.

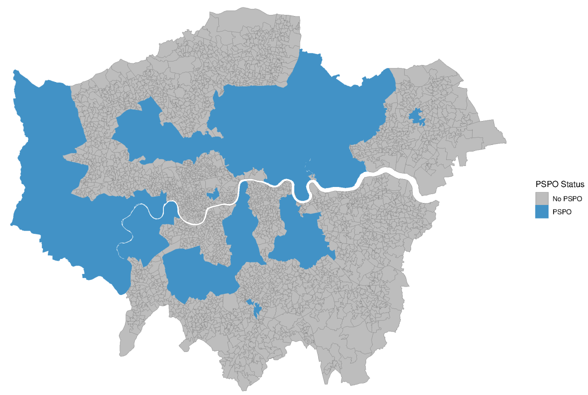
4 Data

We compile two main datasets. The first is an LSOA–month panel combining stop-and-search incidents, recorded crime, and PSPO treatment indicators, covering all London LSOAs over the period 2010–2024. Figure A1 shows a full map of the LSOAs and PSPOs distribution across the entire period of analysis. The second is a ward–election panel linking council election results to PSPO exposure, covering all London wards across five electoral cycles. Both datasets exploit the staggered rollout of PSPOs across London to identify causal effects, with all LSOAs, electoral wards, and PSPO boundaries geocoded to 2011 geography as reference. This Section describes the main data sources we employ.

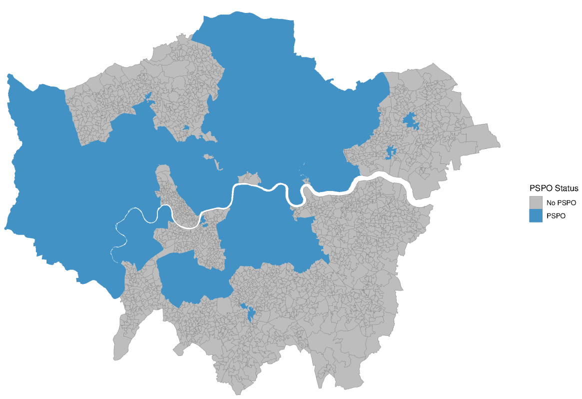
Public Space Protection Order (PSPO) Data. To study the effects of localised expansions in police authority, we construct a novel database including all existing PSPO-designated areas from 2014 to 2024. We collect the information about the area designated for each PSPO, the date of the formulation and eventual extension of each order from the website of each local authority located within the Greater London Area. Each Council is required to publish online the details of the written order with each map attached. We webscrape the text of the orders and geolocate each map, resulting in a total of 94 orders covering 87% of the Greater London Area in 2024. After geolocating each PSPO area, we mapped them onto 2011 census tracts' boundaries, which are standardised geographic units typically containing 400–1,200 households and 1,000–3,000 residents. Figure 2 offers a visualisation of the staggered treatment implementation for the entire GLA area across all periods.

Figure 2: PSPO treatment status across years

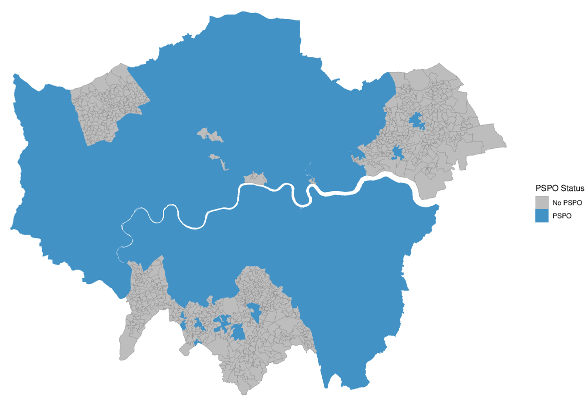
(a) PSPO treatment 2015



(b) PSPO treatment 2017



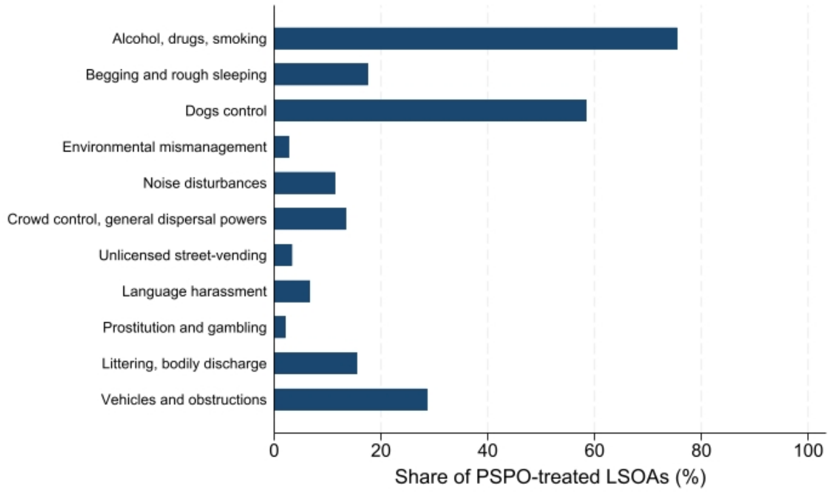
(c) PSPO treatment 2019



(d) PSPO treatment 2022

Importantly, PSPOs vary by the type of anti-social behaviour targeted, as shown in Figure 3. The PSPOs categories by type are not exclusive, as the same area can be covered by multiple PSPOs across years. We find that this is the case for around 50% of the sample. We also account for areas which were previously covered by any DPPO, Dog Control Orders, and Gating Orders which were automatically transformed into PSPOs after 2014.

Figure 3: PSPOs by the type of ASB targeted



Last, we built a continuous measure of PSPO exposure at the census block level that combines spatial coverage with policy overlap. This construction follows the continuous local exposure approach in which a pre-determined geographic weight is interacted with a time-varying common shock to produce a unit-level intensity measure (Autor, Dorn and Hanson, 2013; Nunn and Qian, 2011; Fetzer, 2019). For each census block–month observation, we compute the share of each census block's area covered by active PSPOs by summing the geometric intersection of the census block boundary with each PSPO order in force up to that month. In our application, the geographic weight is the area-intersection share, $|A_i \cap P_j|/|A_i|$, and the time variation is driven by PSPO enactment dates. This time-varying measure captures the intensive margin of treatment, namely, what percentage of each census block area is subject to PSPO restrictions, and how many orders cover the same area. Formally, for each census block c in month t:

$$C_{it} = \sum_{j \in \mathcal{J}_t} \frac{|A_i \cap P_j|}{|A_i|} \tag{1}$$

where C_it is the PSPO's coverage of census block c in month t, A_i is the area of census block c, and P_j is the geographic boundary of PSPO order j. $|A_i \cap P_j|$ denotes the area of their geometric intersection, and $\mathcal{J}_t = \{j : t_j^{\text{start}} \leq t\}$ is the set of all PSPO orders that are implemented by month t. The measure C_{it} equals to 0 for untreated census blocks[8], it ranges between 0 and 1 when a single PSPO partially or fully covers the census block, and is higher than 1 when multiple PSPO orders overlap within the same census block.

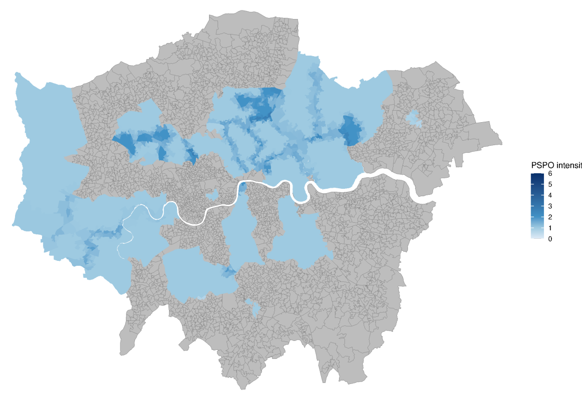
Figure 4 shows how the intensity of PSPOs' coverage changes across our time window. Coverage expands

[8]The measure is set to zero for LSOAs whose centroid does not fall within any active PSPO boundary, consistent with the binary treatment definition, ensuring that the continuous measure is a strict intensification of the binary indicator rather than an independent spatial assignment.

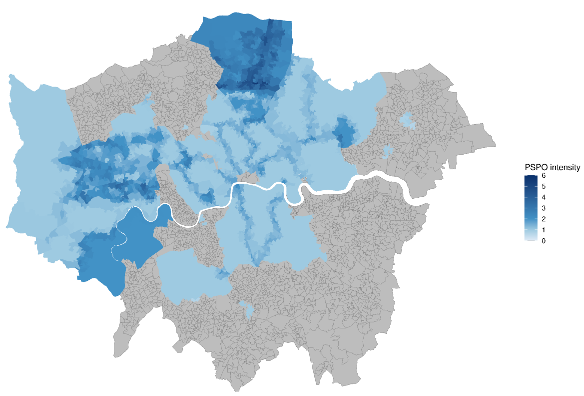
substantially by 2019, with darker shading indicating areas subject to multiple overlapping orders. Unlike a simple binary treatment indicator, this measure captures meaningful heterogeneity in treatment intensity, as two census blocks both coded as treated may face very different regulatory environments depending on how many orders overlap within their boundaries and what share of their area is covered. We use this variable alongside the binary indicator to assess whether the estimated effects scale with the geographic intensity of PSPO coverage.

Figure 4: PSPO intensity treatment across years

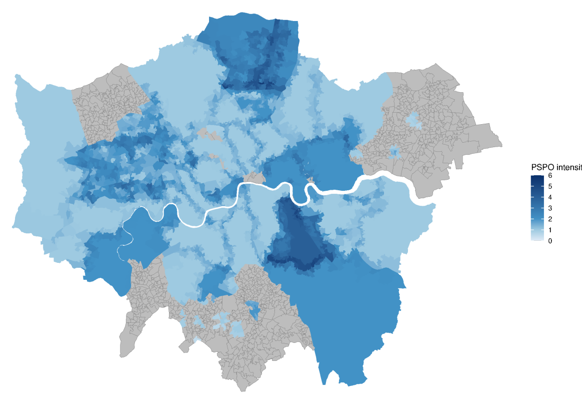
(a) PSPO intensity 2015



(b) PSPO intensity 2017



(c) PSPO intensity 2019



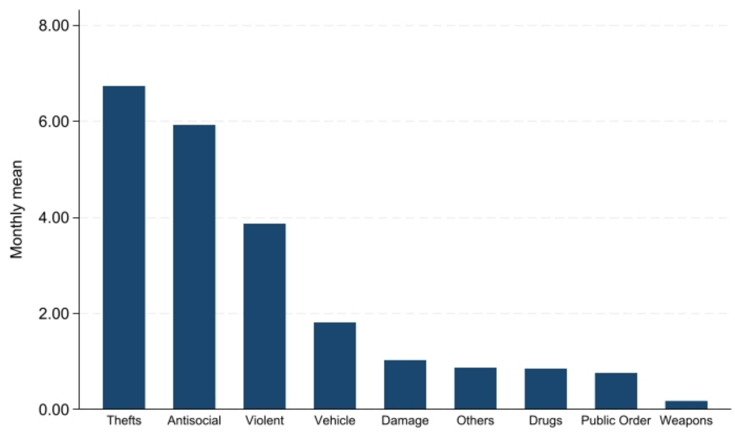
(d) PSPO intensity 2022

Crime Data. We employ the Metropolitan police database of crime incidents recorded between January 2010 and October 2024.[9] The data are the result of matching records from the MPS and the Ministry of

[9] We exclude from our sample the data released by the City of London Police for two main reasons: first, crime records without a specific location recorded are usually attributed to the CoL police by MPS (Facchetti, 2025), second restricting the sample to a single police force ensures that all observations are subject to the same institutional recording practices, discretionary standards, and organisational policies throughout the sample period.

Justice, providing information about the LSOA where the crime occurred, the number of crimes recorded, the type of crime reported, and the last investigation outcome of each report. The categories the records map onto regard both victim-related offences, such as thefts, violent crimes, vehicle crime, and criminal damage, and offences against the State, such as drugs, weapons, and public order-related reports. All crime incidents are mapped to their 2011 census tracts.[10] The final dataset we employ contains around 15 million reported incidents, with an average of 18 crimes per census tract per month. Figure 5 shows that the majority of crimes relates to theft and ASB, with an average of 6 incidents per census tract per month, while the least reported crimes are those related to drugs, public order, and weapons possession.

FIGURE 5: TYPES OF CRIMES



Stop-and-Search Data. Along with crime data, we employ data about stop-and-search patterns from the MPS.[11] The database contains almost 1.5 million of people or vehicles searched from June 2016 to October 2024, for which we know the place and the time of the search, the age range of the person searched, and the gender and self-defined ethnicity of both the person searched and the searching officer.[12] All stop and search records are mapped to their 2011 census tracts, for which we follow the same procedure adopted with crime records (Goodchild and Siu-Ngan Lam, 1980). Figure A2 shows the localised variation of the number of people stopped and searched across the full period. On average, 2 people get searched every month per census tract. We also have information about the last investigation outcome of each report and whether the search required the removal of more than just outer clothing. Figure 6 shows some descriptive statistics by the type of investigation outcome across years. Interestingly, we notice a clear decrease in the

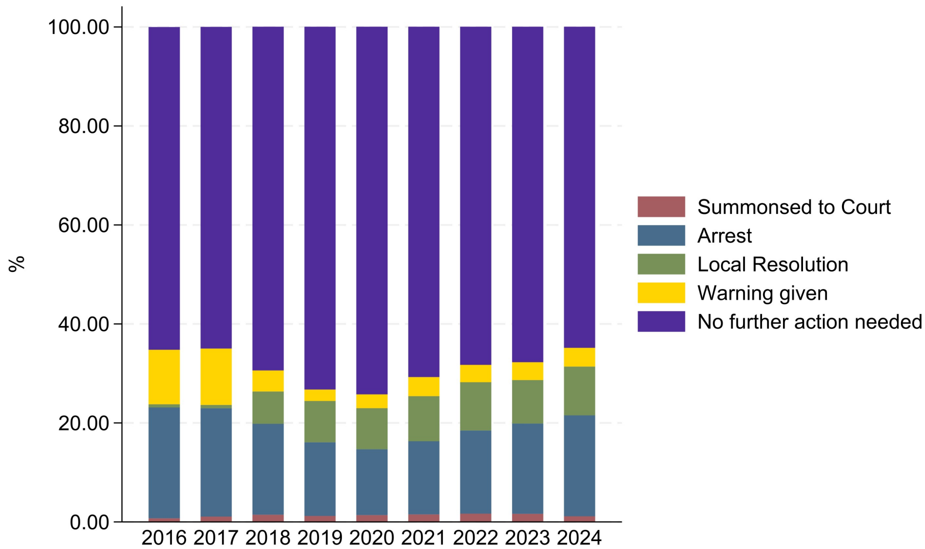
[10] We account for changes in LSOA boundaries between the 2011 and 2021 censuses by crosswalking all geographic information to 2011 LSOAs as the reference geography. For LSOAs whose boundaries changed, we compute weights capturing how much of each 2021 LSOA falls within each 2011 LSOA, and rescale crime and stop-and-search counts accordingly. While we cannot observe the exact location of incidents within an LSOA, this approach mitigates the concern that apparent changes in crime or policing intensity reflect boundary redrawings rather than real shifts in activity. Under the assumption of homogeneity within each source zone (Goodchild and Siu-Ngan Lam, 1980), the rescaled counts provide an unbiased mapping to the 2011 geography.

[11] We follow the same practice employed for crime records, and we exclude from our sample the City of London.

[12] The MPS anonymises the precise location of each stop-and-search incident by slightly shifting the recorded coordinates. While this could introduce measurement error at fine spatial scales, it does not directly affect our analysis, as we aggregate incidents to the LSOA level, which is directly reported by the MPS and is not subject to any anonymisation procedure.

share of stop-and-searches ended with a warning, and an increase in the share of stop-and-searches with a local resolution achieved. For the purpose of our analysis, we aggregate the searches ending with no further actions and those ending with any further action to track different effects on searches' outcomes.

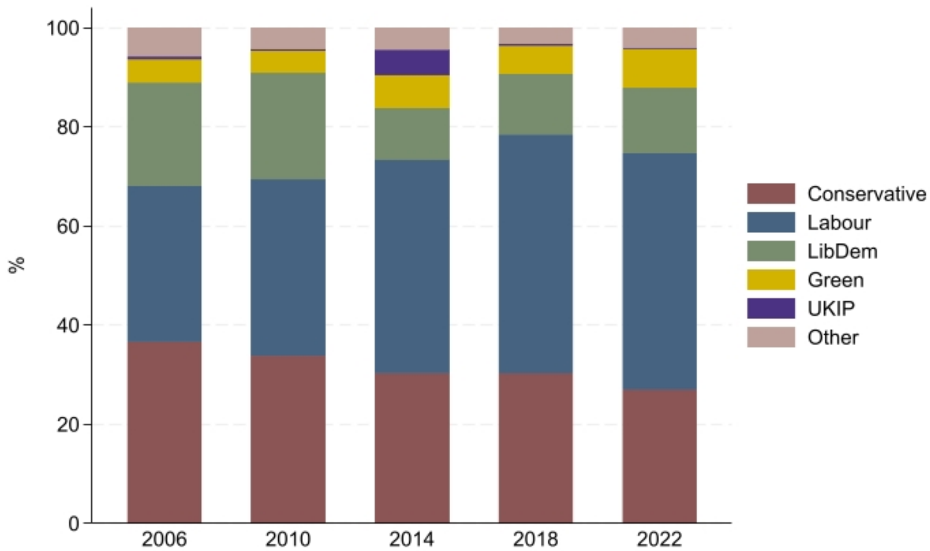
Figure 6: S&S Outcomes by Year



Electoral Data. We compile a second dataset covering the council-level administrative electoral results for each electoral ward in the GLA for the years 2006, 2010, 2014, 2018, and 2022. The data are obtained from London Datastore (2024) and report the total number of votes, party-level and candidate-level number of votes for each electoral ward[13]. For the purpose of our analysis, we compute the simple share of the votes for the main five parties for the available electoral cycles: conversative Party, Labour Party, Green Party, Liberal-democratic Party, and UKIP. Figure 7 shows the trends in electoral outcomes. The distribution of vote shares remains relatively stable across election cycles, with Labour and Conservative parties accounting for the largest shares, while minor parties exhibit more limited variation.

[13]Wards represent the basic electoral units within London boroughs, with each ward typically electing two or three councillors representative of the residents in local governance decisions.

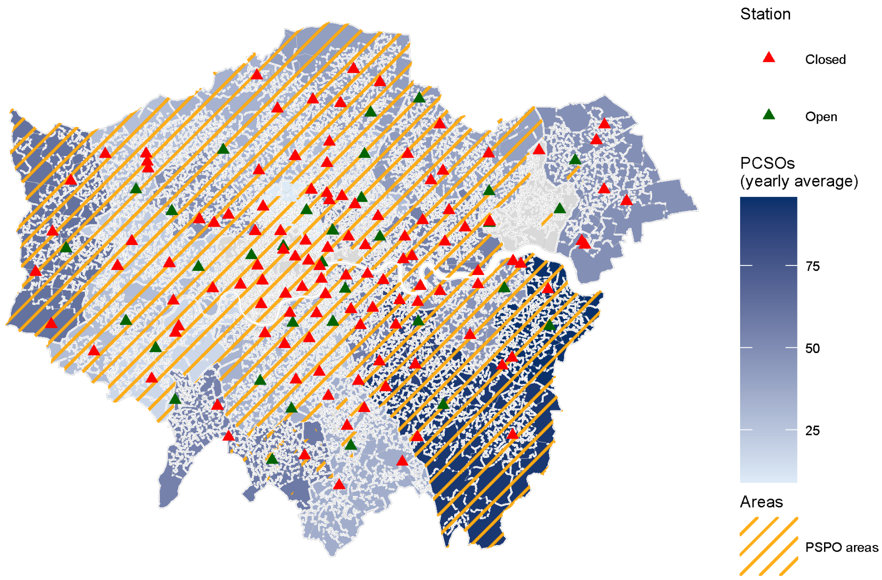
Figure 7: Electoral Outcomes by Year



Police Stations and Officers Data. Following Facchetti (2025), we employ data about the universe of MPS police stations to run several robustness checks on the interaction between the timing of PSPOs implementations and the effect of austerity cuts on the MPS budget. We download the data from the MPS archive of FOI requests, were police officers provide data requested by citizens online. We access information about the location of police stations, the date of their closure, whether a specific station remained open, and the number of police officers working for each MPS' Basic Command Unit (BCU). We compute the time-varying distance from each census block to each open police station. Additionally, we compute a proxy for the level of police strength as the number of police officers (PCSOs) operating in each BCU divided by the current number of open police stations in each BCU (Facchetti, 2025)[14] Figure 8 shows the spatial distribution of police stations and the average number of operating police officers for treated and never-treated LSOAs.

[14]As in Facchetti (2025), officer-level data are available only at the BCU level rather than at the LSOA level. Until 2018, BCU boundaries coincided with borough boundaries, providing a clean mapping between officer counts and our unit of analysis at a higher level of aggregation. From 2018 onwards, however, the MPS reorganised into 12 larger BCUs, each aggregating multiple boroughs. To maintain a consistent borough-level proxy throughout the sample period, we disaggregate post-2018 BCU officer counts back to constituent boroughs using the pre-2018 BCU-to-borough crosswalk, weighting each borough by its share of total officers in the BCU observed in the pre-reorganisation period. While this procedure may introduce measurement error in the post-2018 period, it is unlikely to bias our estimates systematically: the 2018 reorganisation was driven by administrative cost-saving considerations rather than by local crime or disorder patterns, and is therefore plausibly orthogonal to the timing and location of PSPO introductions.

Figure 8: Police stations, PCSOs, PSPOs areas



House Prices Data. We access data about the LSE-Real Estate Economics and Finance (REEF) index from Ahlfeldt, Heblich and Seidel (2023), which provides information about the LSOA-level of housing prices in England and Wales from 2010 to 2020. Following Ahlfeldt, Carozzi and Makovsky (2021), the authors employ pooled cross-sections of housing transactions from the Land Registry transaction records with Energy Performance Certificate (EPC) data and use both parametric and non-parametric estimation to produce balanced panel price series. Therefore, the computed index ensures that housing prices are adjusted for specific features of individual houses.

Other Data. We employ publicly available information from the 2011 and 2021 UK Census Data. We combine information about demographic characteristics, such as population density, age distribution, ethnicity, educational, and labour market trends. We employ this information as a part of our analysis to control for baseline demographic characteristics.

4.1 Descriptive Statistics

The final sample we employ to investigate the effect of PSPOs on police behaviour covers the universe of GLA census blocks, excluding the City of London, observed from January 2010 to October 2024. The second sample, which we use to measure how PSPOs affect electoral outcomes, follows GLA electoral wards across all council elections from 2006 to 2022. Tables 1 and 2 present the descriptive statistics for the two samples, respectively. The designation of where to implement a PSPO and its timing are non-random by nature. Columns from (1) to (4) of Table 1 show that treated census blocks had substantially higher pre-treatment crime levels than never-treated areas: on average, 55% more anti-social behaviour incidents, 49% more thefts, and 32% more violent crimes. Treated areas are also characterised by denser and more diverse populations and higher housing prices, consistent with PSPOs being deployed in urban, mixed-tenure areas experiencing visible disorder. Columns from (5) to (8) show that early treated census blocks display slightly higher baseline crime levels compared to later-treated ones, suggesting that areas with greater disorder may have been prioritised for earlier adoption. Early treated areas also display slightly lower baseline police presence and strength relative to later treated ones, suggesting that areas with weaker policing capacity may have been faster to adopt PSPOs as an alternative regulatory tool. Distance from the closest open police station follows broadly similar patterns across all groups. We also observe differences in political composition across treated and untreated wards. Treated wards tend to have higher Labour vote shares and lower Conservative and UKIP vote shares compared to never-treated wards, alongside relatively higher support for smaller parties such as the Greens. These differences are broadly consistent with the urban character of treated areas and their higher population density. When comparing early- and later-treated wards, early adopters exhibit somewhat higher Labour support and lower Conservative vote shares than later adopters, although differences across timing cohorts are generally smaller than those between treated and never-treated wards. Overall, these patterns suggest that PSPO adoption is more prevalent in politically urban and Labour-leaning areas, reinforcing the importance of accounting for differential baseline characteristics in our empirical strategy.

Although PSPOs' designation is not random, our identification strategy does not require similarity in levels. It only assumes that, absent the PSPO, treated and control census blocks would have followed parallel trends in policing and crime outcomes. Figure A3 shows that never-treated and treated units were following similar trends before any PSPO was implemented, and we employ event studies to support this assumption in Section 5. We further investigate the determinants of PSPO adoption in Appendix B.

Table 1: Descriptive Statistics and Selection Bias: Crime and Stop & Search

	All census blocks				Treated census blocks			
	Never treated		Treated		Later treated		Early treated	
	(1) N	(2) mean	(6) N	(7) mean	(1) N	(2) mean	(6) N	(7) mean
Baseline characteristics								
Count anti-social behaviour	106,379	2.963	700,565	4.578	328,489	4.145	372,076	4.961
Count of thefts	106,379	3.622	700,565	5.411	328,489	5.356	372,076	5.459
Count of violent crimes	106,379	2.229	700,565	2.951	328,489	2.677	372,076	3.193
Housing price index	106,379	3,874	700,899	6,066	328,489	6,359	372,410	5,809
Density per sqkm	106,379	6,052	700,899	10,126	328,489	9,437	372,410	10,733
Share of white residents	106,379	0.719	700,899	0.589	328,489	0.640	372,410	0.544
Unemployment share	106,379	0.0475	700,899	0.0526	328,489	0.0490	372,410	0.0558
Share of over 65 residents	106,379	0.121	700,899	0.0938	328,489	0.103	372,410	0.0854
Distance from closest open police station	106,379	7.231	700,899	6.945	328,489	6.979	372,410	6.914
Operating police officers per borough	106,379	4.668	700,899	4.429	328,489	5.043	372,410	3.887
Police strength	105,377	1.163	700,398	0.975	328,322	1.157	372,076	0.814

Table 2: Descriptive Statistics and Selection Bias: Electoral Results

	All electoral wards				Treated electoral wards			
	Never treated		Treated		Later treated		Early treated	
	(1) N	(2) mean	(6) N	(7) mean	(1) N	(2) mean	(6) N	(7) mean
Baseline characteristics								
Votes: conservaties	315	0.307	2,773	0.301	1,175	0.384	1,422	0.238
Votes: greens	315	0.0477	2,773	0.0698	1,175	0.0518	1,422	0.0859
Votes: liberal-democrats	315	0.132	2,773	0.101	1,175	0.0744	1,422	0.113
Votes: labour	315	0.293	2,773	0.448	1,175	0.416	1,422	0.482
Votes: UKIP	315	0.126	2,773	0.0415	1,175	0.0470	1,422	0.0312
Votes: Other	315	0.0930	2,773	0.0392	1,175	0.0272	1,422	0.0496
Housing price index	315	3,813	2,773	6,079	1,176	6,654	1,422	5,765
Density per sqkm	315	500,270	2,773	933,737	1,176	900,030	1,422	978,432
Share of white residents	315	0.738	2,773	0.595	1,176	0.641	1,422	0.551
Unemployment share	315	0.0461	2,773	0.0517	1,176	0.0482	1,422	0.0546
Share of over 65 residents	315	0.123	2,773	0.0948	1,176	0.104	1,422	0.0863

5 Empirical Framework

Estimating the causal effect of PSPOs on local outcomes presents several empirical challenges. As shown in the descriptive statistics, PSPOs tend to be implemented in more densily populated areas, with larger unemployment and a slightly younger population. While those areas appear similar in distance to the nearest police station, the average police strength seems to be lower, while the baseline level of crime tends to be higher. Among them, areas with a higher level of violent crime and lower housing prices tend to be treated earlier. To relax the concern of differential pre-trends driven by selection bias, we investigate the local determinants of the introduction of PSPOs reported in Table B1. We systematically control for a rich set of demographic characteristics which are strongly correlated with the treatment, exploiting only within-borough variation in the treatment timing; we demonstrate that, conditional on these factors, the timing of PSPO introduction is plausibly uncorrelated with other determinants of stop and search and crime. [15]
When a PSPO is introduced, the range of behaviours that officers can formally act upon expands within the designated area, while officer numbers and legal search powers remain virtually unchanged. Any resulting increase in stop and search, therefore, is more likely to reflect a change in officer behaviour rather than a change in legal mandate or resource allocation according to the legislation mandate. This makes stop and search a direct empirical window onto discretionary policing: under our identification assumptions, a positive

[15]It is still possible that areas covered by at least one PSPO differ significantly from uncovered ones within the same borough based on other unobserved characteristics which confound the effect. In Section 7 we rule out that contemporaneous changes in police strength or a gentrification process drive the results. If census tracts covered by PSPOs are systematically located in areas where local councils deploy higher numbers of private security agents, one might worry that private enforcement activity independently increases stop and search rates. However, stop and search powers are reserved exclusively for sworn police constables under PACE 1984 and cannot be exercised by private security personnel. Beyond this, two residual sources of bias operate in opposite directions. On the upward side, organised resident groups may simultaneously petition councils for PSPOs and maintain informal pressure on BCU commanders for increased patrol activity, which represents a channel of hyper-local civic mobilisation that is not fully captured by our controls. On the downward side, anticipation effects represent a more credible source of attenuation: the mandatory consultation period between PSPO formulation and implementation period means officers may informally begin increasing stop and searches in designated areas differently before our treatment indicator switches on, driving our estimated effect toward zero. On balance, the direction of remaining bias is more plausibly downward than upward, suggesting our estimates are very likely to be conservative.

effect of PSPO introduction on search rates isolates the behavioural response of officers to the order, net of any concurrent changes in police infrastructure. A potential threat to this interpretation arises from the overlap between the timing of PSPO introductions and concurrent reforms to police organisation and deployment. Most notably, the austerity-driven wave of station closures reorganised police resources across London in ways that independently affected patrol intensity and officer deployment patterns during the same time window of our analysis (Facchetti, 2025). If PSPOs were systematically introduced in areas simultaneously experiencing changes in station proximity or officer concentration, our estimates would conflate the effect of PSPOs with infrastructure-driven shifts in policing. We address this by conditioning on time-varying measures of police deployment following Facchetti (2025).

Beyond enforcement activity, we expect PSPO introduction to affect recorded crime through two competing forces: a deterrence effect that suppresses targeted conduct within the designated area, and a displacement effect that relocates it just beyond the boundary. The net direction is theoretically ambiguous, and we treat it as an empirical question. Finally, the expansion of police contact in designated areas may erode the relationship between residents and law enforcement. Increased enforcement exposure, particularly where searches lead to no further action, reduces perceived procedural fairness and may diminish residents' willingness to cooperate with police, generating costs that recorded crime statistics do not capture (Tyler, Fagan and Geller, 2014; Laniyonu, 2019). This Section is structured as follows. First, we discuss our difference-in-differences design to estimate the effects of PSPOs on police behaviour and criminal patterns, comparing LSOAs exposed to a PSPO to those that remain unexposed. Second, we investigate dynamic treatment effects and assess the robustness of our estimates to heterogeneous treatment timing. Since we exploit a continuous measure of treatment intensity, we follow de Chaisemartin, D'Haultfœuille and Vazquez-Bare (2024), whose estimator accommodates heterogeneous treatment effects under continuous and staggered treatment designs. Third, we present the models used to examine criminal displacement effects. Last, we present the model used to examine how citizens respond at the polling station following PSPO introduction.

5.1 DiD Model

Our main TWFE model estimates the effect of PSPOs on stop and search and crime under a robust DiD design with staggered treatment adoption. We define treated LSOAs as those for which their centroid falls under a PSPO area, while control LSOAs' centroids are not, and we compare treated LSOAs to never and not-yet-treated ones[16] Our final sample includes 4,197 treated census blocks across all periods, and 637 never-treated census blocks. Since our outcome variables are recorded at the monthly level, we aggregate the panel into six-month periods (semesters) to reduce noise and improve statistical power, yielding two

[16]We also estimate the model under an Intention-to-Treat (ITT) approach by restricting the control group to not-yet-treated units, excluding never-treated census blocks. Figure 18 shows that, although the smaller sample yields less precisely estimated coefficients, the post-treatment estimates are virtually unchanged relative to our main specification, and pre-treatment coefficients remain small and statistically indistinguishable from zero, supporting the parallel trends assumption within the set of eventually-treated units.

observations per year for each census block. The model is specified as follows:

$$Y_{c,b,t} = \beta_1 PSPO_{c,t} + \alpha_c + \delta_t + \mu_{b,t} + X'_{c,t} + \epsilon_{c,b,t} \tag{2}$$

where $Y_{c,b,t}$ is the outcome of interest for each census block c within borough b in semester t. $PSPO_{c,t}$ is equal to 1 if at least one PSPO has been implemented within the census block c by semester t.[17] We include census blocks' fixed effects α_c, semester fixed effects δ_t, and borough-by-semester fixed effects $\mu_{b,t}$, with standard errors clustered at the census block level. We rely on the classic difference-in-differences parallel trend assumption (PTA), stating that in the absence of treatment, treated and control census blocks within the same borough would have followed similar trends. Additionally, we include $X'_{c,t}$ as time trends for the key demographic characteristics which predict a PSPO implementation as discussed in Appendix B. For this purpose, we consider the baseline levels of population density, unemployment share, residents over 65 years old, the share of non-white residents, and the presence at the baseline of an existing alcohol or dog control Order. This way, we allow places with different baseline characteristics to follow differential trends in policing outcomes, ensuring that our estimates capture the effect of PSPO adoption rather than pre-existing divergences in stop and search activity across demographically heterogeneous areas.

We employ a standard event-study approach exploiting variation in the timing and location of treatment adoption to identify causal effects. Our primary estimator is the interaction-weighted (IW) estimator proposed by Sun and Abraham (2021), which addresses well-documented concerns about two-way fixed effects (TWFE) estimators in staggered adoption settings. The main advantage of the Sun and Abraham (2021) framework is its compatibility with the introduction of conservative fixed effects as well as demographic-specific linear trends, which we include to absorb pre-existing differential trajectories across census blocks.[18] To assess the validity of our identifying assumptions, we examine pre-treatment event-study coefficients and formally test for parallel pre-trends. Finally, for robustness, we replicate our main results in Section 7 using the alternative heterogeneity-robust estimator of de Chaisemartin, D'Haultfœuille and Vazquez-Bare (2024), which relies on a distinct identification approach based on first differences, providing an independent check on our findings.

5.2 DiD Dynamic Model

Our binary treatment specification rests on the assumption that census blocks are homogeneously treated once a PSPO is active within their boundaries. This assumption is unlikely to hold in practice for two

[17] We use an absorbing treatment, as only 2 PSPOs were not renewed after the first 3 years. As a more conservative choice, we exclude the LSOAs under these PSPOs from the analysis.

[18] The event study estimating equation is reported in Appendix C.

reasons. First, PSPOs vary considerably in geographic size and rarely align with census block boundaries, meaning that a block whose centroid falls within an active Order may only be partially covered, introducing measurement error into the binary indicator and potentially attenuating our estimates. Second, the same census block may fall under multiple PSPO orders at different points in time, or under several simultaneously active orders, generating variation in treatment intensity that a binary indicator cannot capture. To relax these concerns, we complement our main results by exploiting the continuous PSPO's coverage measure we introduced in Section 4, which records the share of each census block's area subject to active PSPO restrictions in each month and accumulates across overlapping orders. This measure addresses both sources of misclassification simultaneously: it corrects for partial geographic overlap between PSPO boundaries and census block areas, and it records the full intensity of exposure when multiple orders coincide within the same block. A key feature of this measure is that treatment intensity varies both across census blocks and over time as new Orders are enacted and implemented. This dual variation renders standard binary event-study approaches unsuitable, since they assume a single discrete adoption date per unit and cannot accommodate treatment that intensifies gradually and differentially across units. Beyond this, the canonical TWFE estimator imposes a linear dose-response relationship between the treatment and the outcome, which in staggered adoption settings produces biased estimates when treatment effects are heterogeneous across units or time (Goodman-Bacon, 2021).

To address both concerns simultaneously, we adopt the heterogeneity-robust estimator of de Chaisemartin, D'Haultfœuille and Vazquez-Bare (2024), which is explicitly designed for settings with continuous and time-varying treatment. Rather than comparing treated and untreated units, the estimator identifies causal effects by comparing census blocks experiencing different changes in coverage intensity over time, using units whose treatment dose remains stable in a given period as the control group. The model equation is specified in Appendix C.

5.3 Estimating the Spillover Effect of PSPOs

To estimate the indirect effects of PSPOs on stop-and-search activity and crime, we disentangle the direct effects of the Orders on treated areas from the spillover effects on neighbouring areas. As PSPO implementation may affect census blocks not formally covered by any Order, spillover effects could operate in either direction[19]. Spillover effects from place-based enforcement interventions are well documented in the crime literature and could operate in either direction. On the one hand, a displacement effect could relocate antisocial behaviour and crime into adjacent census blocks as targeted individuals move beyond Order boundaries (Weisburd et al., 2006; Blattman et al., 2021; Zambiasi, 2022). Police behaviour may similarly spill over into nearby areas, as officers might intervene within their assigned Basic Command Unit rather

[19] For this reason, our main specification may violate the Stable Unit Treatment Value Assumption (SUTVA), which requires that each unit's potential outcomes are independent of other units' treatment status (Cunningham, 2021). This further motivates us to test for spillover effects.

than strictly within PSPO boundaries.[20] On the other hand, border census blocks may experience reductions in antisocial behaviour through deterrence spillovers, as visible enforcement concentration within designated areas generates a broader suppression effect (Pinotti, 2015; Daniele and Dipoppa, 2017; Macdonald, 2016). Officers may also reduce searches in nearby areas by concentrating discretionary intervention within directly covered tracts.

We adopt the standard approach in the difference-in-differences literature (Kline and Moretti, 2014; Butts, 2023) and perform three complementary exercises. We first define spillover-affected LSOAs as those sharing at least one boundary segment with a treated LSOA at any point in time, and we introduce such an indicator directly into our main specification alongside the treatment indicator, allowing us to separately identify direct and indirect effects within the same estimating equation. Second, we exclude neighbouring areas potentially affected by spillovers from the control group entirely, providing a conservative estimate of the direct treatment effect that is robust to contamination of the comparison group. We take a further step and restrict the spillover analysis to a comparison between potentially spillover-affected LSOAs and never-treated LSOAs with no boundary contact with any active Order (Blattman et al., 2021). This design allows us to assess whether any observed changes in neighbouring areas reflect genuine displacement or deterrence diffusion, rather than pre-existing differences between treated and untreated neighbourhoods.

The estimating equation of the first exercise is described as:

$$Y_{c,b,t} = \beta_1 PSPO_{c,t} + \beta_2 \text{Spillover}_{c,t} + \alpha_c + \delta_t + \mu b, t + X'_{c,t} + \varepsilon_{c,b,t} \tag{3}$$

where $Y_{c,d,t}$ is the outcome of interest measured in census block c, district d and semester t. Spilloverc,t is an indicator equal to 1 if census block c shares at least one boundary segment with a PSPO-treated census track in semester t but it is not itself covered by any active Order. β_1 identifies the direct effect on treated blocks and β_2 captures the indirect effect on immediately neighbouring untreated blocks. This way, we effectively measure spillover effects while controlling for the direct effects of PSPOs.

Second, we re-estimate Equation (1) excluding all spillover-adjacent census blocks from the sample:

$$Y_{c,b,t} = \beta_1 PSPO_{c,t} + \alpha_c + \delta_t + \mu_{b,t} + X'_{c,t} + \varepsilon_{c,b,t} \quad \text{for } c \notin \mathcal{S} \tag{4}$$

where $\mathcal{S}$ denotes the set of spillover-adjacent census blocks. This provides a conservative estimate of the direct treatment effect robust to any contamination of the comparison group.

Third, to directly measure the magnitude and direction of spillover effects, we restrict the sample to spillover-adjacent and never-treated census tracts only, excluding all directly treated blocks:

$$Y_{c,b,t} = \beta_1 \text{Spillover}_{c,t} + \alpha_c + \delta_t + \mu b, t + X'_{c,t} + \varepsilon_{c,b,t} \quad \text{for } c \notin \mathcal{T} \tag{5}$$

[20] As documented by Facchetti (2025), fewer than 1% of officers are deployed outside their assigned Basic Command Unit (BCU), suggesting that spillover effects on stop-and-search activity are most plausibly contained within BCU boundaries. We therefore restrict our spillover analysis for stop-and-search to LSOAs within the same BCU as the treated area.

where $\mathcal{T}$ denotes the set of directly treated census blocks. β_1 here identifies whether spillover-adjacent areas experience displacement or deterrence diffusion relative to never-treated areas with no boundary contact with any active Order. All remaining terms follow the notation of Equation (1) throughout.

5.4 Estimating the Effects of PSPOs on Political Participation

A growing literature in political economy and criminology documents that interactions with law enforcement shape individuals' political attitudes and behaviour (Weaver and Lerman, 2010; Tyler, Fagan and Geller, 2014; Morris and Shoub, 2024). In this context, the introduction of Public Space Protection Orders (PSPOs) provides a natural setting to study how expansions in discretionary policing authority translate into political responses. By increasing the grounds of everyday police interactions without changing formal legal powers, PSPOs may influence political behaviour through changes in institutional trust, perceived fairness, and exposure to enforcement. We therefore examine whether and how PSPO exposure affects electoral participation and party preferences.

To provide suggestive evidence of how the expansion of discretionary policing affects political behaviour, we estimate the impact of PSPO exposure on electoral outcomes at the ward level. Our main outcomes are party-level vote shares in local council elections over five electoral cycles from 2006 to 2022. Since electoral data are observed at discrete election years, we construct a panel of electoral wards across election cycles and exploit variation in PSPO exposure between elections. The model is specified as:

$$Y_{w,b,t} = \beta_1 PSPO_{c,t} + \alpha_w + \delta_t + \mu_{b,t} + X'_{w,t} + Z'_{w,t} + Y'_{b,t} + \epsilon_{w,b,t} \tag{6}$$

where $Y_{w,b,t}$ denotes the political outcome of interest in ward w at election year t. $PSPO_{c,t}$ is the measure of PSPO exposure in ward w up to the election year t, turning 1 whenever a ward's centroid is covered by a PSPO onwards. We include ward fixed effects α_w to control for time-invariant differences in political preferences, and election fixed effects δ_t to absorb common shocks across election cycles. Moreover, we control for borough-election year time shocks by including borough-time fixed effects $\mu_{b,t}$, and $X'_{w \cdot t \cdot \gamma}$ includes specific baseline demographics time trends. Standard errors are clustered at the ward level.

We interpret the results presented in Section 6.3 as suggestive evidence rather than definitive causal estimates, as the identification strategy faces a potential endogeneity concern: local councils may strategically implement PSPOs in anticipation of upcoming electoral outcomes, introducing endogeneity between treatment and the outcomes of interest. If councils implement PSPOs in response to perceived shifts in the local political environment, the estimated coefficients would capture both the causal effect of PSPO exposure and the pre-existing political dynamics that prompted adoption. Our outcome variable partially addresses this concern: since we employ the *change* in vote shares between elections rather than their level, time-invariant differences in partisan composition across wards are absorbed by the ward fixed effects, and the identifying variation comes from within-ward shifts in political preferences following PSPO adoption. Nevertheless, anticipatory behaviour operating through time-varying partisan trends would remain a threat to identification. To partially address this, we construct $X'_{w,t}$, $Z'_{w,t}$, and $Y'_{b,t}$ which are controls for ward-level and borough-level party-specific and incumbent vote shares for the preceding Greater London Authority Mayor and Assembly elections; we emply these controls as a proxy for the partisan environment at the time of the PSPO adoption decision and capture the information available to local councils when choosing whether and when to implement an Order.

6 Effects of PSPOs on crime and police behaviour

This Section presents the results of the direct and unintended effects of PSPOs on reported anti-social behaviour crimes, police behaviour, and officer-detected crimes[21]. We also investigate police discretionary power by examining the effect of PSPOs on stop and searches broken down by outcome type and by the ethnicity of people stopped. Lastly, we investigate the effect of introducing PSPOs on electoral outcomes.

6.1 Direct Effects: Deterrence and Displacement

We begin by investigating whether PSPOs achieved their intended purpose of reducing localised anti-social behaviour. Table A1 presents estimates of the effect of PSPOs on recorded ASB incidents per 1,000 inhabitants. Columns (1) and (2) report standard TWFE estimates without and with baseline demographic trends, respectively, all including census track, semester, and borough-by-semester FEs. These specifications yield small negative coefficients statistically indistinguishable from zero. Columns (3) and (4) report the estimates of the interaction-weighted estimator of Sun and Abraham (2021), which yield a positive but insignificant effect of PSPOs on the level of reported ASB. As shown by our preferred specification, the results suggest that PSPOs had no clear or meaningful impact on reported anti-social behaviour.

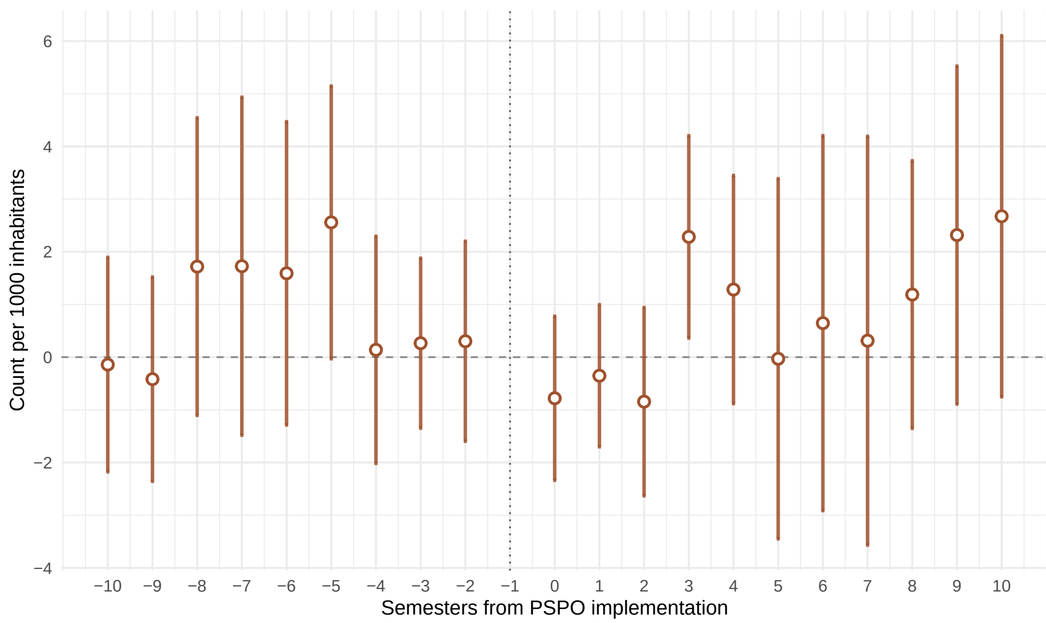
The event study shown in Figure 9 corroborates this interpretation. Pre-treatment coefficients are, on average, small and jointly indistinguishable from zero[22], supporting the identifying assumption of parallel trends. Post-treatment coefficients remain close to zero and statistically insignificant for year 1. From approximately the second year onwards, point estimates drift modestly upward, though they remain on average statistically indistinguishable from zero at conventional levels. On one hand, PSPO implementation may increase reported anti-social behaviour by raising residents' propensity to report ASBs to the police; indeed, as the set of formally prohibited behaviours expands, individuals may become more likely to report incidents they previously tolerated. On the other hand, genuine deterrence may operate through potential offenders internalising the prohibitions of the Order and adjusting their behaviour accordingly. If both mechanisms

[21] These include drug and weapons-related offences and public order offences which are usually recorded based on officer-initiated contact rather than victim reports (Home Office, 2026).

[22] We reject the hypothesis that pre-treatment coefficients are jointly equal to zero by reporting a F-statistic = 0.93 and a p-value of 0.52.

are simultaneously at work, they would offset each other, producing a precisely estimated zero rather than an absence of any effect.[23] The upward pattern of post-treatment coefficients suggests that deterrence is unlikely to be driving the null effect, and that the true underlying incidence of anti-social behaviour is unlikely to be falling. Our null result is therefore a lower bound on the ineffectiveness of PSPOs: if the increased propensity to report is generating modest upward pressure on observed counts, correcting for it would push the estimates closer to zero, reinforcing rather than undermining the conclusion that PSPOs had no meaningful deterrent effect.

Figure 9: Effect of PSPO on anti-social behaviour reports per 1000 inhabitants - Sun and Abraham (2021)



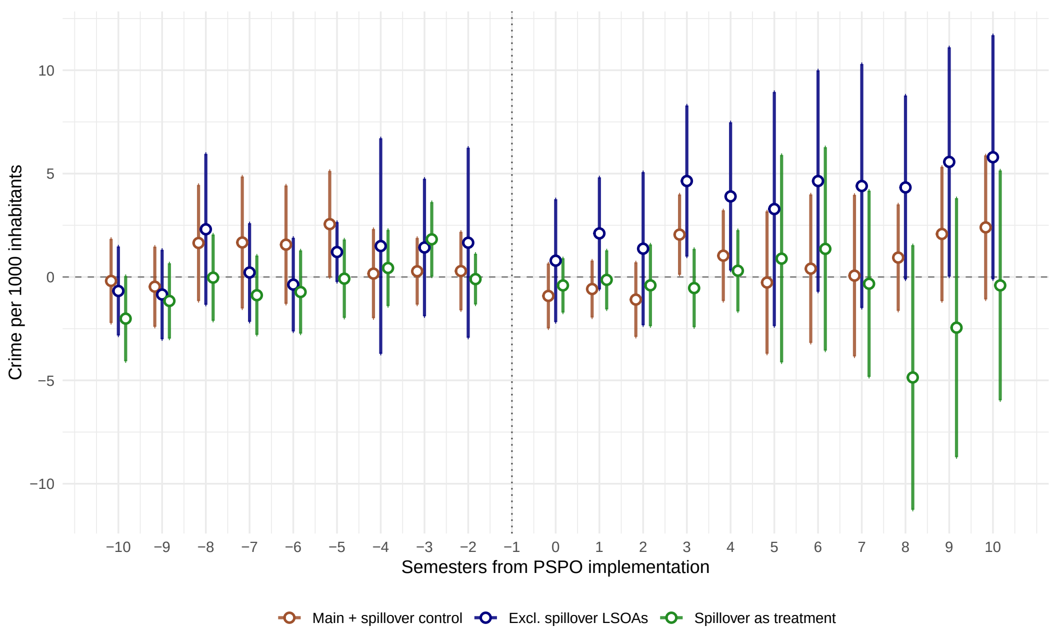
A natural question is whether this reflects a genuine absence of any effect, or rather a displacement of anti-social behaviour into adjacent untreated areas. We examine this possibility directly by testing whether neighbouring untreated census blocks experience differential changes in ASB following PSPO introduction in adjacent census blocks. Table A2 presents the results from the three complementary spillover exercises introduced in Section 5.3. The direct treatment effect is stable and insignificant across all specifications: whether we use the baseline specification, we include a spillover indicator alongside the treatment, or exclude spillover-adjacent LSOAs from the control group. The spillover coefficient itself remains negative and statistically insignificant across all specifications, ruling out meaningful displacement of anti-social behaviour into

[23] We present suggestive evidence against both mechanisms in Section 6.2, though we interpret it cautiously. First, we report that crimes likely detected exclusively by police officers also show no significant change following PSPO adoption, suggesting the null result is not driven by a reporting offset. Second, the pretextual enforcement we document may suppress reporting through a different route. Communities exposed to sustained unproductive stop-and-search activity over multiple semesters may become less willing to initiate contact with police, including to report anti-social behaviour (Tarling and Morris, 2010; Kirk and Papachristos, 2011; Desmond, Papachristos and Kirk, 2016), which would work against any deterrence effect becoming visible in the data.

neighbouring areas.

Figure 10 plots the event study estimates for all three spillover specifications. Pre-treatment coefficients are small and jointly indistinguishable from zero across all three models with p-values between 0.25 and 0.69, supporting the parallel trends assumption. Post-treatment coefficients for the main specification controlling for spillover effects and the spillover-as-treatment specification remain centred on zero and insignificant. However, when we exclude spillover-affected areas, we see a slight increase in the level of ASB in the long run. This aligns with the interpretation that PSPO adoption slightly raises reporting propensity as residents become more aware of formally proscribed behaviours. Importantly, the coefficients are positive rather than negative, which rules out deterrence as the dominant force and reinforces the conservative lower-bound interpretation of the main null result.

Figure 10: Spillover effect of PSPO on anti-social behaviour reports per 1000 inhabitants - Sun and Abraham (2021)



6.2 Unintended Effects: Police Discretion

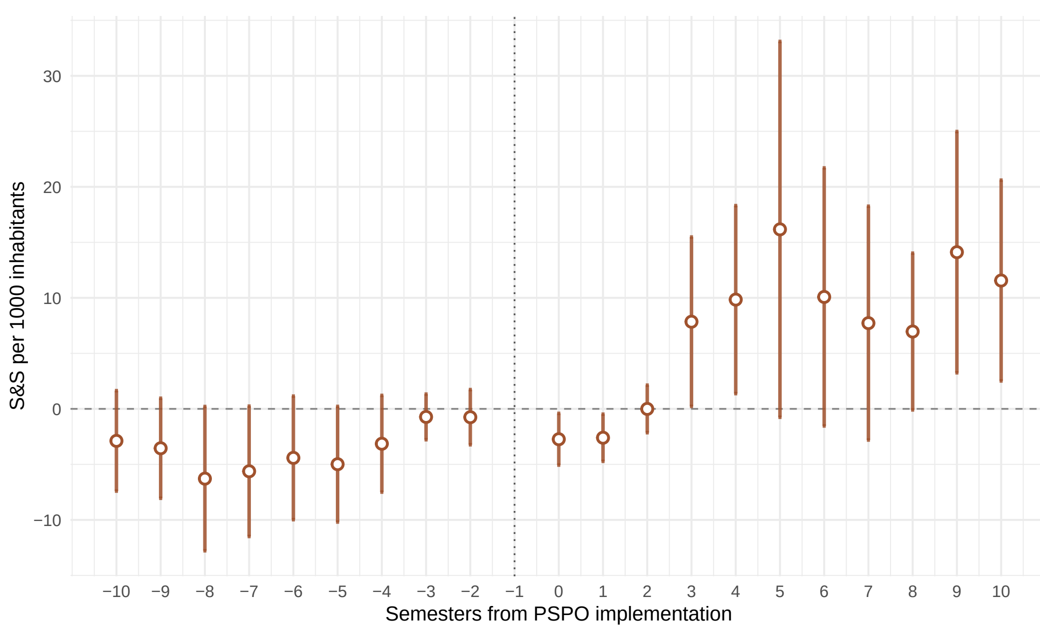
We then turn to the unintended consequences of PSPOs on officers' behaviour, especially focusing on the searching powers. As detailed in Section 2.4, PSPOs do not grant officers new stop-and-search powers and do not mandate additional patrol resources; assuming that implementing a PSPO is not contemporaneous to changes in police strength and deployment[24], any change in enforcement activity in treated areas therefore reflects discretionary officer behaviour rather than a legal- or resource-related response. We find robust evidence that PSPOs substantially increased stop-and-search activity. Table A3 presents the estimates of

[24]We test whether changes in police deployment and strength confound our effect in Section 7.

the effect of PSPOs on total stop-and-search rates per 1,000 inhabitants. Across all specifications, the effect is positive and statistically significant. Our preferred interaction-weighted estimator yields an ATT of 7.075 additional searches per 1,000 inhabitants per semester, which is statistically significant at 5%. Given a pre-treatment mean of approximately 9 searches per 1,000 inhabitants, this represents a roughly 79% increase in stop-and-search activity relative to control areas.

The event study in Figure 11 reveals that pre-treatment coefficients are flat and jointly zero with a p-value of 0.11, and the post-treatment increase emerges gradually, becoming consistently positive after one year of implementation and growing through the full post-treatment window. This delayed onset is inconsistent with a mechanical change in officer behaviour at the moment of designation; instead, it suggests a progressive adaptation of enforcement practices as officers and BCU commanders become familiar with the new Order in place[25].

Figure 11: Effect of PSPO on Stop and Search per 1000 inhabitants - Sun and Abraham (2021)



As discussed in Section 3, PSPOs may lower the suspicion threshold required to initiate a search, with ambiguous implications for search productivity: a lower threshold could either improve enforcement efficiency by exposing genuinely suspicious individuals to greater scrutiny, or expand officer discretion in ways that generate unproductive searches. To assess which channel dominates, we decompose total searches by their recorded outcome.[26] In a context of productive policing, we would expect the level of S&S ending with

[25]Figure A4 confirms that the results hold when we measure the effect of PSPOs on the raw count of stop and searches.

[26]To ensure comparability across outcome variables with different baseline levels, we standardise each outcome by subtracting its mean and dividing by its standard deviation before estimation. Coefficients are therefore expressed in standard deviation

further action to increase alongside the total volume of searches. On the contrary, in a context of expanded discretion and pretextual policing, we would observe the number of S&S ending with no further action moving together with the total number of searches.

Figure 12 shows the effect of PSPOs on searches ending with further and no further action, respectively. The pattern of the effect of S&S with no further action closely mirrors total S&S: pre-treatment coefficients are jointly indistinguishable from zero, and from approximately the second year of treatment, unproductive searches rise substantially and persistently. The pattern of the effect of S&S with further action tells the opposite story: the effect on productive searches is centred on zero throughout the entire event window, with no significant increase at any point. The decomposition therefore points unambiguously toward expanded discretion rather than productive enforcement.

This conclusion is further corroborated by the absence of any effect on officer-detected crimes. Table A4 and Figure 13 report the effect of PSPOs on officer-detected crimes (ODCs), namely drugs, weapons, and public order offences, that are recorded exclusively based on officer-initiated contact and require no victim report (Home Office, 2026). Under Section 1 of PACE 1984, officers must hold reasonable grounds for suspecting an individual of carrying weapons, controlled drugs, or articles for use in crime before initiating a search. Under Section 60 of the Criminal Justice and Public Order Act 1994, a senior officer may authorise suspicion-free searches within a designated area in anticipation of violence or disorder. If PSPO-induced searches were grounded in legitimate suspicion under either framework, uncovering contraband or disorder should generate a corresponding rise in recorded ODCs.[27][28]. Instead, the ODC effect is small and insignificant across all specifications; we report in Figure 13 the event study of Column (5), which is our preferred specification, showing coefficients uniformly distributed around zero both before and after treatment.

units and can be directly compared across the two series.

[27]Under the Home Office Counting Rules, officer-detected crimes must be recorded within 72 hours of coming to the attention of the officer (Home Office, 2026). Given that our outcome variable is aggregated at the semester level, any recording lag is negligible and cannot account for the absence of an effect on ODCs.

[28]This result is also free from the concern that residents are more likely to report incidents.

FIGURE 12: EFFECT OF PSPO ON STOP AND SEARCH PER 1000 INHABITANTS - SUN AND ABRAHAM (2021)

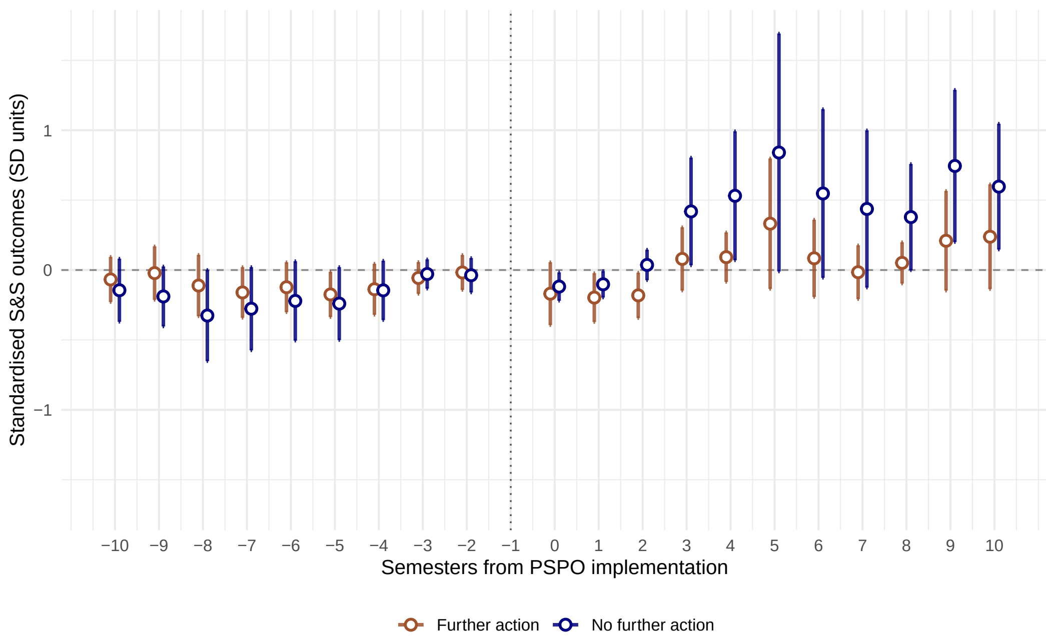
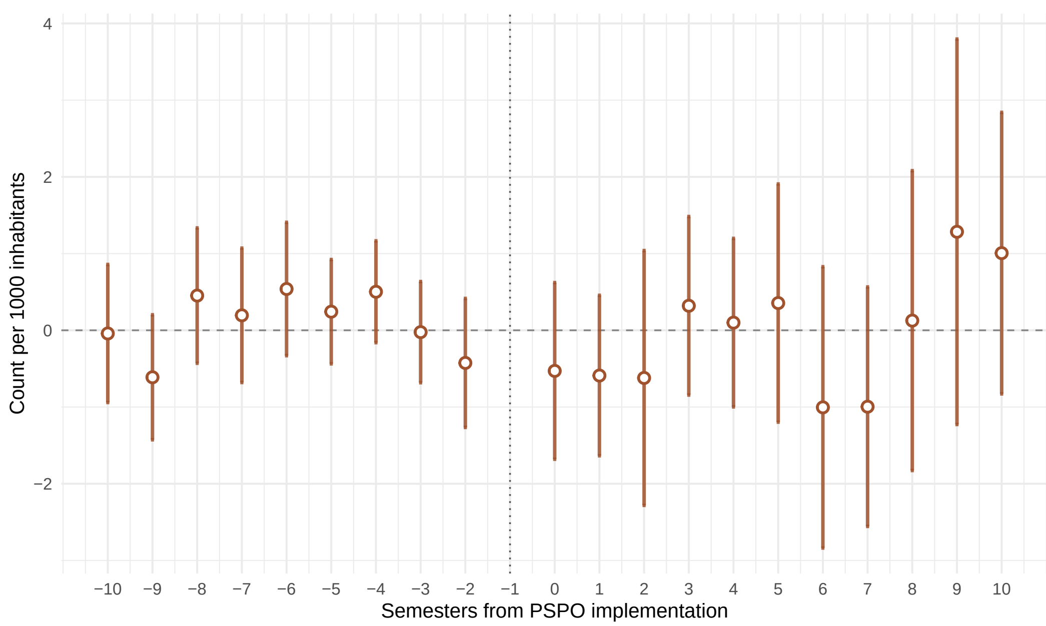


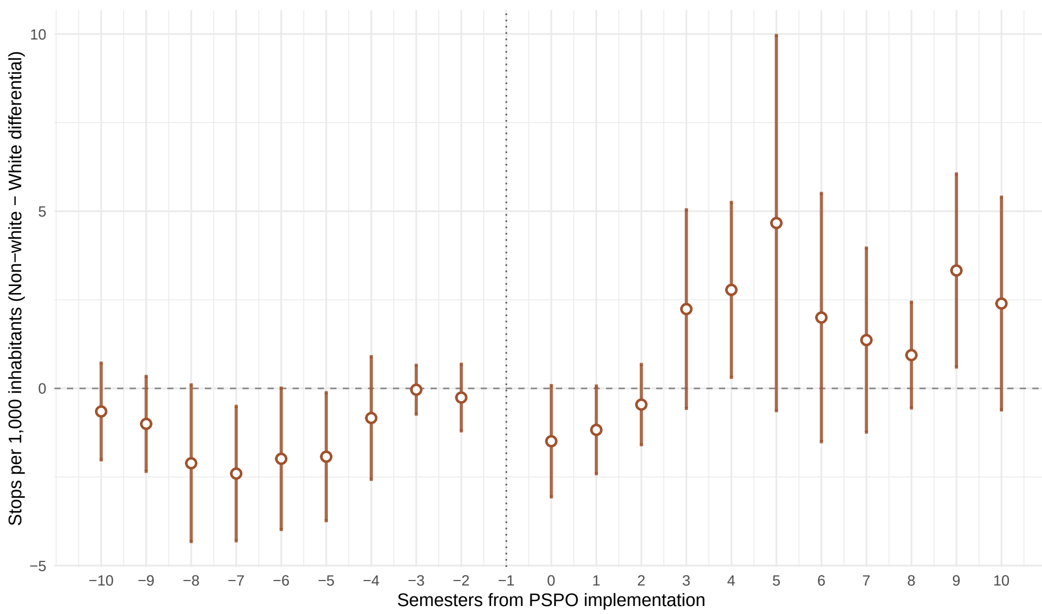
FIGURE 13: EFFECT OF PSPO ON ODCS PER 1000 INHABITANTS - SUN AND ABRAHAM (2021)



We provide suggestive evidence that the expansion of police discretion documented above might affect all residents equally. Table A6 presents triple-difference estimates of the differential effect of PSPOs on searches of non-white individuals relative to white individuals, exploiting within-LSOA variation across ethnic groups over time.[29] The DDD term *PSPO* × *Non-white* reported in Column (4) is positive and significantly estimated at 1.36 additional searches per 1,000 inhabitants per semester. This stability across specifications is strong evidence that the differential is not driven by borough-level confounders or pre-existing demographic trends. By contrast, the DiD PSPO coefficient is small and statistically insignificant from Columns (2) to (4), suggesting that the overall S&S increase documented in Section 6.2 is concentrated almost entirely among non-white residents.[30]

The mean of the dependent variable is 3.46 searches per 1,000 inhabitants per semester, implying that PSPOs generate a differential increase of approximately 39% in the non-white search rate relative to the white search rate.[31] This evidence suggests that minority status might act as a proxy for officers' suspicion when the *de facto* costs of initiating stops are reduced by PSPO designations (Knowles, Persico and Todd, 2001*a*; Anwar and Fang, 2006).

Figure 14: Effect of PSPO on S&S per 1000 inhabitants between White and Non-White - Sun and Abraham (2021)



6.3 Effects on Electoral Outcomes

Table 3 presents the TWFE estimates of the effect of PSPOs on ward-level vote shares across the five major party groups. Two important caveats apply. First, the electoral panel contains only three election cycles

[29] The estimating equation is reported in Appendix C.

[30] Column (1), which includes no demographic time trends, yields a significant DiD coefficient and an insignificant DDD term. We do not interpret this as evidence against differential targeting. This specification is a standard TWFE estimator, which is known to produce biased estimates under staggered adoption due to negative weighting of some cohort-period comparisons (Goodman-Bacon, 2021).

[31] The event study underlying these estimates displays some pre-treatment variation in the early part of the window, particularly between 3 and 4 years before the treatment. We interpret this cautiously as suggesting evidence. Importantly, pre-treatment coefficients become progressively smaller and closer to zero as the treatment timing approaches.

with limited post-treatment variation, which does not allow us to identify the effect under our preferred heterogeneity-robust estimator (Sun and Abraham, 2021); we therefore treat these results as suggestive evidence rather than definitive causal estimates. Second, our estimates may be subject to endogeneity if local councils strategically time PSPO implementation to influence upcoming electoral outcomes. Importantly, our outcome variable is the change in vote share between elections rather than the level of vote share itself. This specification absorbs any time-invariant partisan composition of wards and means that our estimates capture shifts in voting behaviour relative to the pre-PSPO baseline, rather than differences in absolute support levels across wards. This partly addresses the strategic timing concern: even if councils with particular partisan profiles are more likely to adopt PSPOs, the first-differenced outcome ensures that pre-existing differences in vote shares do not confound our estimates. What would remain problematic is if councils strategically time adoption to coincide with anticipated changes in the partisan environment. To relax this concern, in Panel B of Table 3, we control for ward-level and borough-level incumbent vote shares in the preceding Greater London Authority Mayor and Assembly elections. Since GLA elections occur one to two years before the council elections we observe as our outcome, they provide a plausible proxy for the prevailing partisan environment at the time of the implementation decision, capturing the information available to incumbents when choosing whether and when to adopt a PSPO.

The results are consistent across both panels. In Panel A, which includes ward and election-year fixed effects alongside borough-by-time fixed effects, PSPOs are associated with a differential increase in Labour vote share of approximately 8.5 percentage points, significant at the 1% level, and a differential decline in Liberal Democrat vote share of approximately 9.1 percentage points, significant at the 10% level. Effects on Conservative, Green, and UKIP vote shares are small and statistically indistinguishable from zero. In Panel B, the Labour and Liberal Democrat effects remain stable in both magnitude and significance once GLA incumbent controls are included, suggesting they are not driven by pre-existing partisan trends. The UKIP coefficient shows a significant differential increase of approximately 2.5 percentage points, while the Conservative and Green effects remain negligible throughout. The stability of the main results across Panels provides some reassurance against the strategic timing concern.

The direction of the political response is suggestive of a broad-based legitimacy backlash against PSPO adoption. The simultaneous increase in Labour support and marginal rise in UKIP vote share is difficult to reconcile as a simple partisan sorting story, as Labour and UKIP draw from very different parts of the political spectrum; instead, it is more consistent with a diffuse disaffection effect operating across different voter types. The decline in Liberal Democrat support reinforces this backlash interpretation, as the Liberal Democrats were junior coalition partners in the government that introduced the 2014 Act, and their vote share falls precisely in the wards most exposed to its enforcement consequences.

Table 3: TWFE Effects of PSPOs on electoral outcomes

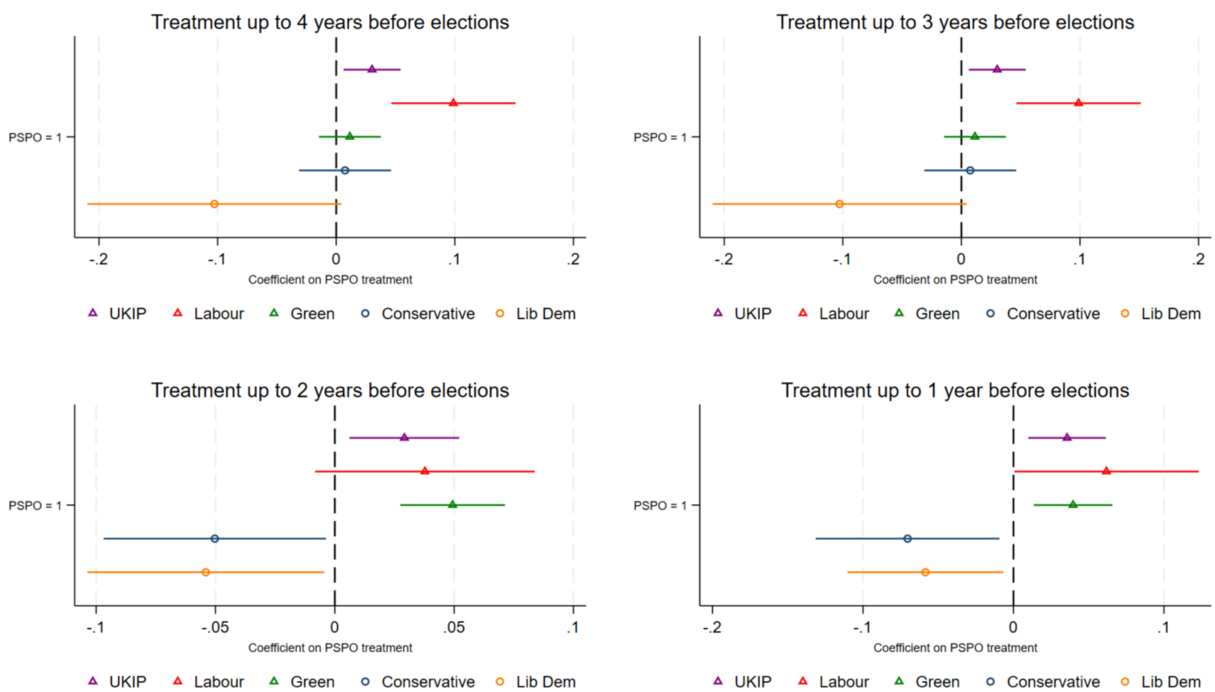
	Greens	**Labours**	**LibDems**	**Conservatives**	**UKIP**
Panel A: Baseline					
PSPO = 1	0.0301	0.0854***	-0.0908*	0.00376	0.0218
	(0.0219)	(0.0247)	(0.0476)	(0.0190)	(0.0151)
Observations	2,464	2,464	2,464	2,464	2,464
R-squared	0.482	0.564	0.673	0.525	0.705
clustered SE	yes	yes	yes	yes	yes
ward FE	yes	yes	yes	yes	yes
election year FE	yes	yes	yes	yes	yes
borough X time FE	yes	yes	yes	yes	yes
GLA party votes	no	no	no	no	no
GLA incumbent votes	no	no	no	no	no
Mean dep. var.	0.00755	0.0397	-0.0197	-0.0220	-0.00113
	Greens	**Labours**	**LibDems**	**Conservatives**	**UKIP**
Panel B: Pre-trends controls					
PSPO = 1	0.0306	0.0858***	-0.0910*	0.00446	0.0252*
	(0.0216)	(0.0247)	(0.0479)	(0.0181)	(0.0137)
Observations	2,464	2,464	2,464	2,464	2,464
R-squared	0.483	0.565	0.674	0.527	0.715
clustered SE	yes	yes	yes	yes	yes
ward FE	yes	yes	yes	yes	yes
election year FE	yes	yes	yes	yes	yes
borough X time FE	yes	yes	yes	yes	yes
GLA party votes	yes	yes	yes	yes	yes
GLA incumbent votes	yes	yes	yes	yes	yes
Mean dep. var.	0.00755	0.0397	-0.0197	-0.0220	-0.00113

Figure 15 provides further nuance by examining how the electoral effects we estimate vary with the time elapsed between PSPO adoption and the end of each electoral cycle. We restrict the sample to wards where treatment occurred within a given window before the election, namely up to 4 years, 3 years, 2 years, and 1 year, respectively, allowing us to assess whether the backlash intensifies or fades as the policy becomes more or less salient to voters[32]. First, this exercise suggests that UKIP and Labour Party differential increases are stable and positive across all four panels, indicating that the shift toward UKIP and Labour is not sensitive to the timing of exposure and reflects a durable realignment rather than a short-term signal from the voters. Second, the timing of exposure relative to the year of elections provides suggestive additional evidence. When treatment occurred within one year before the elections, the Conservative and Liberal Democrat coefficients are negative and somewhat larger in magnitude than in the longer-horizon panels, which is consistent with voters being more likely to move away from the parties that introduced the policy when its effects are most

[32]The electoral cycle for council elections lasts four years, which is why we restrict our sample to wards treated within four years of the election.

visible and recent. This short-run pattern attenuates as the treatment horizon expands, suggesting that the direct accountability effect fades over time as other electoral considerations might become more relevant and the policy implementation becomes less recent. We interpret this as broadly consistent with a suggestive political backlash narrative.

Figure 15: Effect of PSPO on changes in votes shares by treatment timing



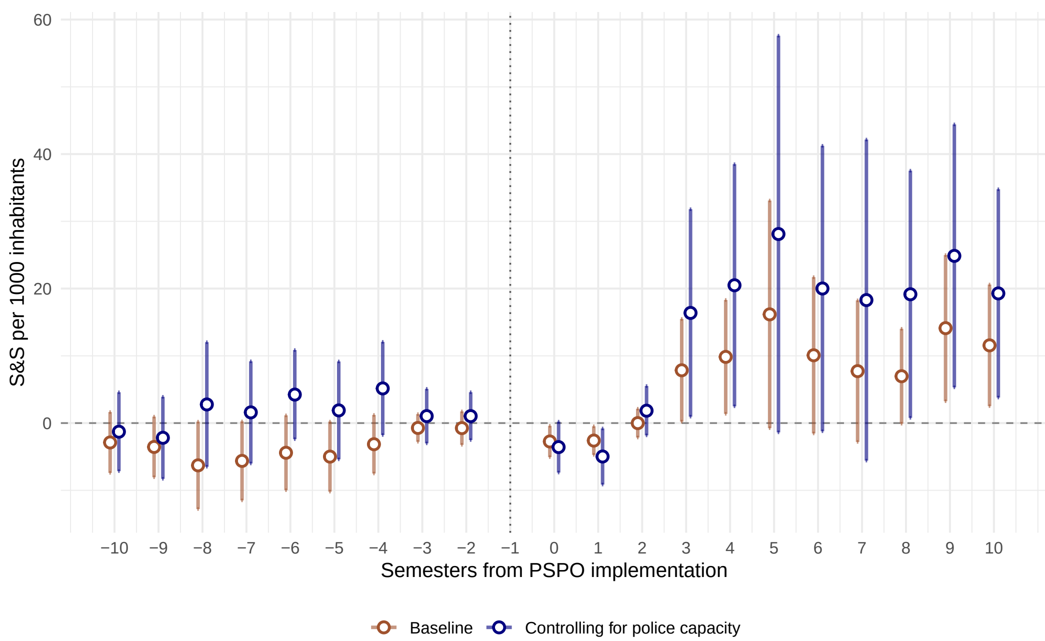
7 Robustness Checks

The effects we measure are robust to several sources of identification threats and changes in treatment assumptions. We organise the robustness checks as follows. First, we rule out the possibility that contemporaneous changes in police strength, officer deployment, or neighbourhood gentrification confound our estimates. Second, we test for anticipation effects, examining whether enforcement activity increased before formal PSPO designation in areas with predecessor orders. Third, we assess sensitivity to alternative outcome and treatment definitions: we test whether the measured increase in stop-and-search reflects genuine local enforcement or partial displacement of searches from adjacent areas, and we replace ASB with public order offences as the outcome variable to rule out administrative reclassification.

7.1 Identification Threats

The primary identification threat to our stop-and-search results is that PSPOs may have been introduced contemporaneously with changes in police deployment. If treated areas also experienced increases in officer numbers or the distance from the closest open station increases during the same period, the observed rise in the rate of S&S could reflect a resource-driven increase in enforcement capacity rather than a behavioural response to the Order. Figure 16 addresses this directly by augmenting our main stop-and-search specification with a time-varying control for local police strength, measured as the ratio of PCSOs to open stations within the BCU, and the distance from the closest open police station (Facchetti, 2025). The event study pattern is nearly identical to the baseline: pre-treatment coefficients remain flat and jointly indistinguishable from zero with a p-value of 0.48, and the post-treatment trajectory tracks the baseline closely throughout the event window. The magnitude of the effect is unchanged, suggesting that austerity-driven variation in police deployment does not account for the PSPO-induced rise S&S activity.

Figure 16: Effect of PSPO on Stop and Search per 1000 inhabitants controlling for changes in police capacity - Sun and Abraham (2021)

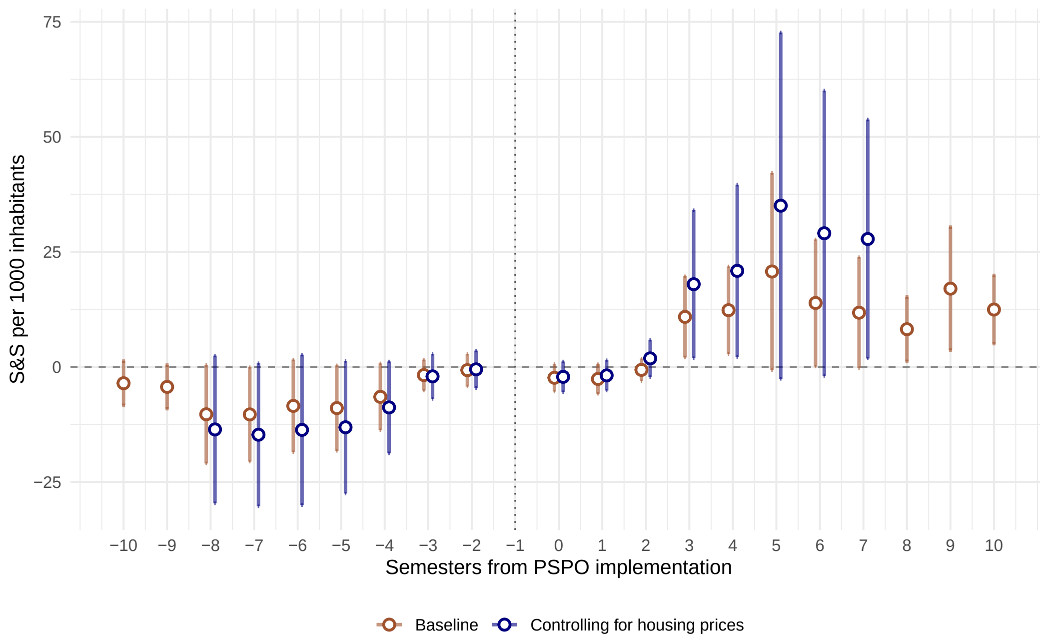


A related concern is that PSPO adoption may coincide with neighbourhood gentrification, a process through which rising housing values attract more police attention and change the demographic composition of treated areas, both of which could independently alter enforcement activity. Figure 17 presents the event study for our main specification augmented with the LSOA-level housing price index[33]. The post-treatment pattern is very close to the baseline, but we note that pre-treatment coefficients in this specification are jointly

[33]When we control for housing prices, we find a restricted event-study window, given the fact that housing prices data are only available until 2020.

significant. We interpret this as evidence that conditioning on housing price dynamics introduces rather than removes pre-existing differential trends. Our preferred specification excludes this control; Figure 17 serves as a specification check illustrating this sensitivity rather than a robustness exercise supporting the baseline.

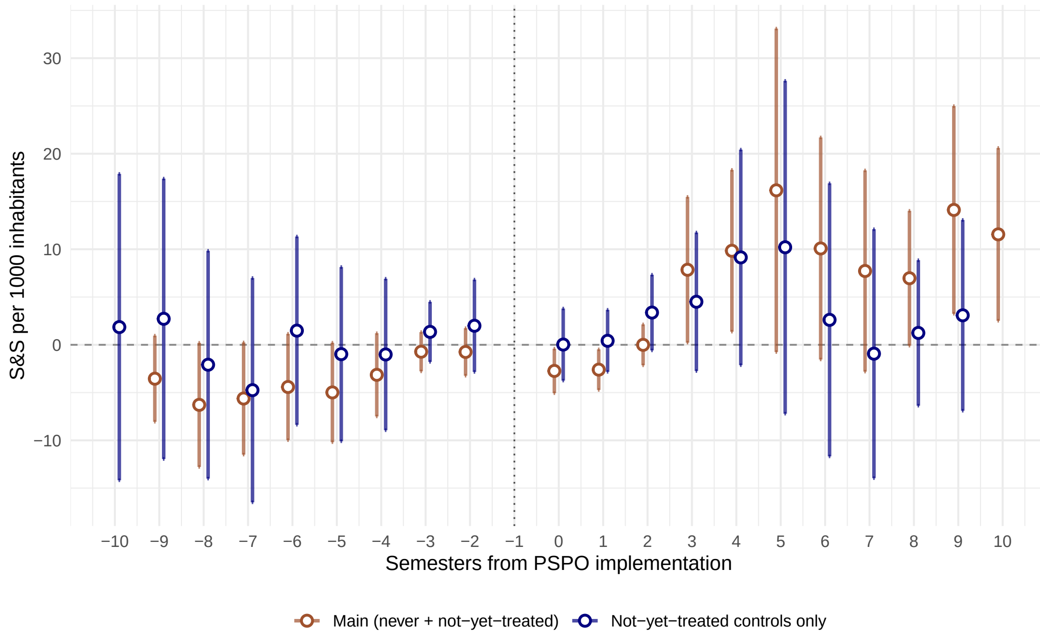
Figure 17: Effect of PSPO on Stop and Search per 1000 inhabitants controlling for housing price index - Sun and Abraham (2021)



7.2 Alternative Treatment Definitions

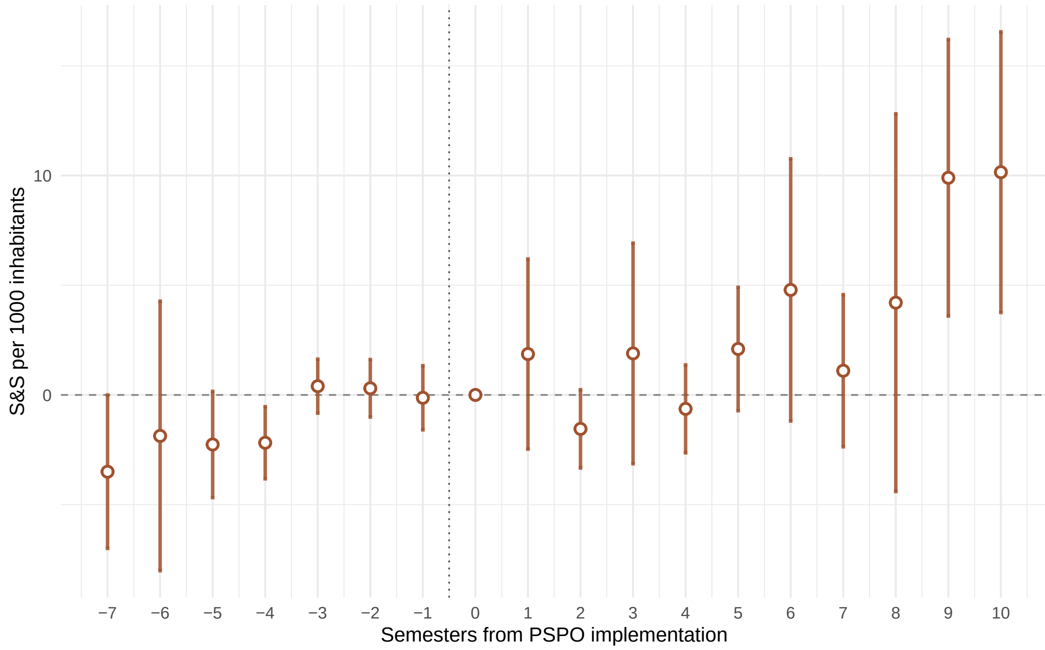
Figure 18 replicates the main event study for stop and search under an alternative control group definition, restricting the comparison group to not-yet-treated census blocks and excluding never-treated units entirely. This ITT specification addresses the concern that never-treated areas may be structurally different from treated ones in ways that violate the parallel trends assumption, relying instead on variation in the timing of adoption among eventually-treated units. The post-treatment estimates from this specification, shown in blue, are broadly consistent with those from the main specification in terms of direction and magnitude, though confidence intervals are wider as expected given the smaller effective sample. Pre-treatment coefficients remain centred on zero and jointly insignificant across both specifications, supporting the parallel trends assumption within the set of eventually-treated units. The broad alignment between the two sets of estimates suggests that the main results are not driven by the inclusion of never-treated areas in the control group, and that the positive effect of PSPO introduction on stop-and-search activity is robust to this alternative treatment definition.

Figure 18: Effect of PSPO on the count of Stop and Search - Sun and Abraham (2021) - Full sample and ever treated only



Our binary treatment indicator classifies a census block as treated as long as the centroid of the block falls under a PSPO, abstracting from variation in geographic coverage intensity. Figure 19 presents event-study estimates from the continuous treatment specification using the area-share exposure measure constructed in Section 4, estimated via the heterogeneity-robust estimator of de Chaisemartin, D'Haultfœuille and Vazquez-Bare (2024). The pattern closely mirrors the binary estimates: pre-treatment coefficients are small and indistinguishable from zero, and the effect accumulates gradually in the post-treatment window, consistent with a progressive adaptation of enforcement practices. The dose-response gradient confirms that the stop-and-search increase scales with the geographic intensity of PSPO exposure, ruling out the possibility that results are driven by partially covered blocks at the boundary of PSPO zones.

Figure 19: Effect of PSPO continuous coverage on Stop and Search per 1000 inhabitants - Sun and Abraham (2021)



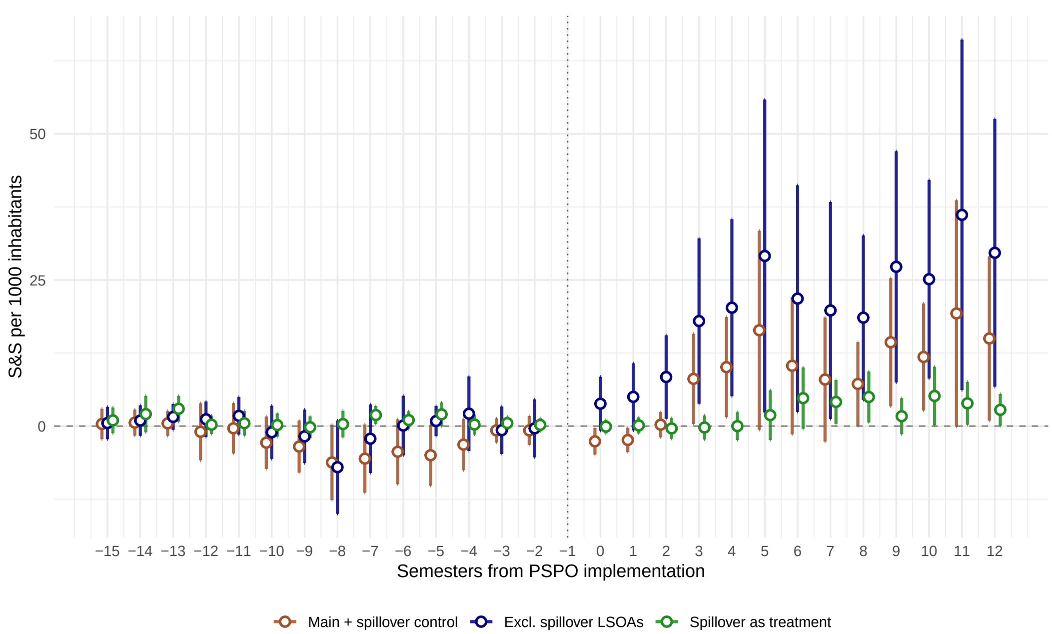
7.3 SUTVA

Our main specification may violate the Stable Unit Treatment Value Assumption (SUTVA) if officers concentrate enforcement within PSPO zones by drawing activity away from neighbouring areas, generating a mechanical redistribution of stops rather than a net increase. Table 4 and Figure 20 replicate the three spillover exercises of Section 5.3 for stop-and-search outcomes. Across all specifications the direct treatment effect is stable, large, and statistically significant. The spillover coefficient for stop-and-search is positive but small, while the spillover-as-treatment event study is centred on zero throughout the post-treatment window. This pattern rules out the possibility that our main result are driven by displacement of officer activity from neighbouring areas into PSPO zones rather than a genuine net increase in enforcement.

Table 4: DiD and Robust DiD estimates of the effect of PSPO on S&S: Spillover robustness

	S&A ATT			
Dep var: S&S per 1000 in.	(1)	(2)	(3)	(4)
PSPO = 1	7.075**	7.307**	18.527***	
	(3.019)	(3.08)	(6.741)	
Spillover = 1		0.543		0.969
		(0.472)		(0.753)
clustered SE	yes	yes	yes	yes
census block FE	yes	yes	yes	yes
time FE	yes	yes	yes	yes
borough-time FE	yes	yes	yes	yes
time trends	yes	yes	yes	yes
model	baseline	control spill	excl. spill	spillover
Observations	83,404	83,404	77,450	11,125
R-squared	0.711	0.731	0.732	0.686
Mean dep. var.	9.26	9.262	9.426	5.307

Figure 20: Effect of PSPO on Stop and Search per 1000 inhabitants controlling for spillover - Sun and Abraham (2021)



8 Conclusion

This paper provides causal evidence on the consequences of expanding police discretionary authority through place-based legislation. Using the staggered rollout of Public Space Protection Orders across London from

2015 to 2024, we trace the effects of a policy designed to reduce anti-social behaviour on three interconnected outcomes: ASBs, police enforcement practices, and community electoral behaviour. Our findings reveal a systematic gap between the stated rationale for these orders and their observed effects.

First, PSPOs do not significantly affect ASBs' reports. Our preferred estimates, drawn from the interaction-weighted estimator of Sun and Abraham (2021) with demanding within-borough fixed effects, show no meaningful effect of PSPO introduction on recorded anti-social behaviour, either coming from a deterrence or a displacement effect. The null result for anti-social behaviour is robust to reclassification checks and alternative treatment definitions. Post-treatment event-study coefficients remain close to zero throughout the entire post-treatment window, providing no evidence of a deterrent effect at any horizon. If anything, the reporting-propensity concern operates in a conservative direction: communities subjected to repeated unproductive enforcement encounters may become progressively less willing to contact police, biasing recorded ASB estimates downward relative to true incidence and making PSPOs appear more effective than they are. The flat event-study profile therefore reinforces, rather than undermines, our conclusion of no meaningful deterrent effect on disorder.

We investigate the unintended consequences of the policy, and we find that PSPOs generate a large and persistent increase in discretionary stop-and-search activity. Treated areas experience approximately a doubling of search rates relative to control areas, with the effect accumulating gradually over the first year of implementation and scaling with the geographic intensity of PSPO coverage. This pattern suggests that officers progressively incorporate spatial designations into their enforcement routine as familiarity with the Order grows and BCU commanders begin directing resources accordingly. The composition of these searches reveals whether the rise in activity reflects genuinely productive targeting or a broader and less discriminate exercise of officer discretion. The entire increase is accounted for by stops ending with no further action; productive searches, resulting in arrest, warnings, show no discernible change, while officer-detected crimes remain uniformly null across all specifications. This joint pattern is consistent with pretextual policing: PSPO designations function as a contextual opportunity for discretionary stops not grounded in specific suspicion and yielding no enforcement benefit. Furthermore, our triple-difference estimates show that the stop-and-searches increase falls almost entirely on non-white residents, generating a differential of approximately 12% in the non-white search rate relative to the white search rate that is remarkably stable across all specifications. This pattern is consistent with minority status serving as a proxy for suspicion when the *de facto* cost of initiating a stop is lowered by policy design (Knowles, Persico and Todd, 2001*b*; Livingston, 1997), and with a broader literature showing that vaguely defined institutional mandates create structural conditions for discriminatory enforcement (Livingston, 1997; Archer, 2023).

Last, we provide preliminary evidence suggesting that these policing patterns might carry political costs. Treated electoral wards experience a shift away from the parties that formulated the policy. These results contribute to a growing literature on the political consequences of policing (Weaver and Lerman, 2010; Tyler, Fagan and Geller, 2014; Morris and Shoub, 2024) by showing that the political costs of police authority can accumulate through the everyday exercise of discretionary power, without requiring highly visible incidents of force.

Taken together, our results suggest that PSPOs expanded the contextual grounds for discretionary enforcement without reducing the behaviour they were designed to target, concentrated that enforcement on non-white residents in a manner consistent with statistical discrimination, and generated measurable electoral backlash. The entire mechanism operates through officer discretion rather than any formal expansion of legal powers: PSPOs do not amend PACE 1984, do not mandate additional patrol resources, and carry no prescribed enforcement protocol.

The findings carry important policy implications. The Anti-Social Behaviour, Crime and Policing Act 2014 grants local councils the authority to define prohibited behaviours in broad and often vague terms and designate enforcement areas without judicial oversight. Our findings suggest that this design generates unproductive enforcement activity, racially concentrated targeting, and political disaffection among affected communities. The absence of any effect on reported anti-social behaviour calls into question whether PSPOs achieve their stated objective. The enforcement patterns they enable, which are concentrated on non-white residents and predominantly unproductive, are instead more likely to erode the community relations and institutional trust on which effective policing depends. The evidence presented here suggests that the social costs, borne disproportionately by non-white communities and detectable even in electoral outcomes, may substantially outweigh the public order benefits that justify the policy's adoption.

SUPPLEMENTARY APPENDICES

Appendix A: Additional Figures and Tables

Figure A1: LSOA boundaries and centroids, and PSPO covarage across the entire period.

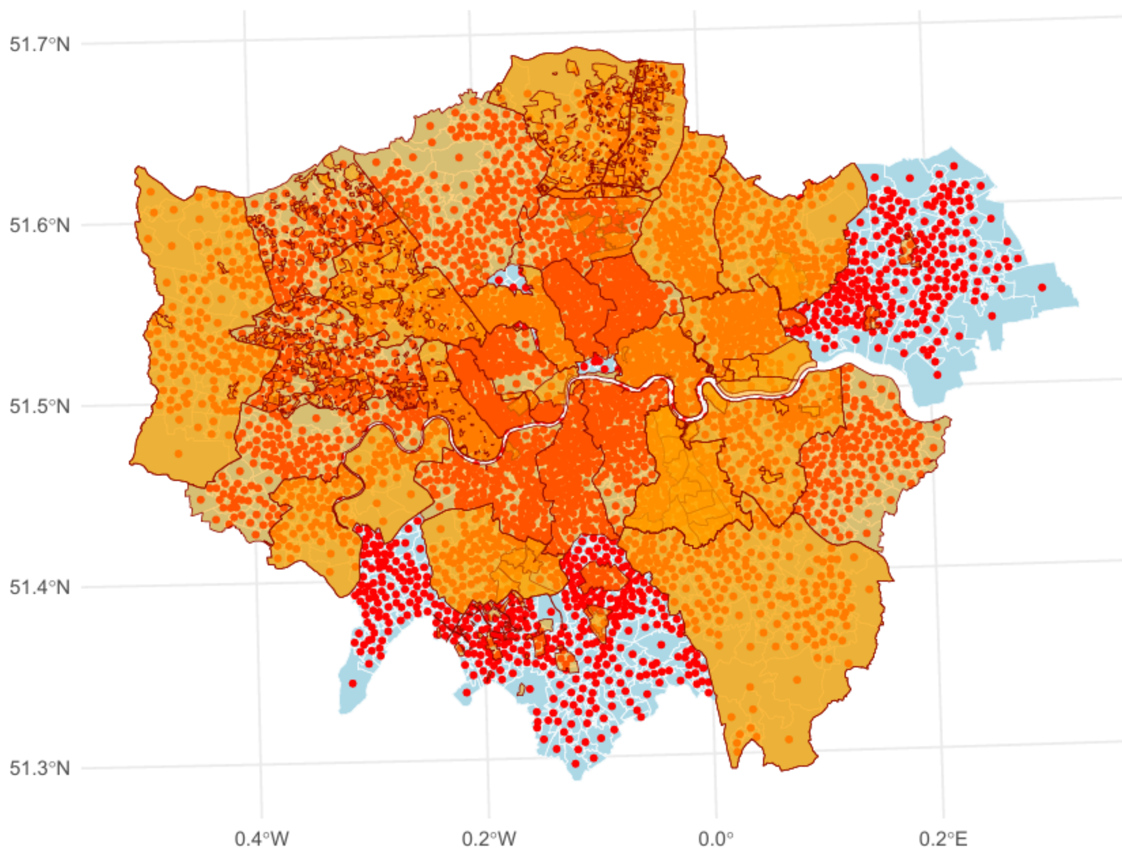
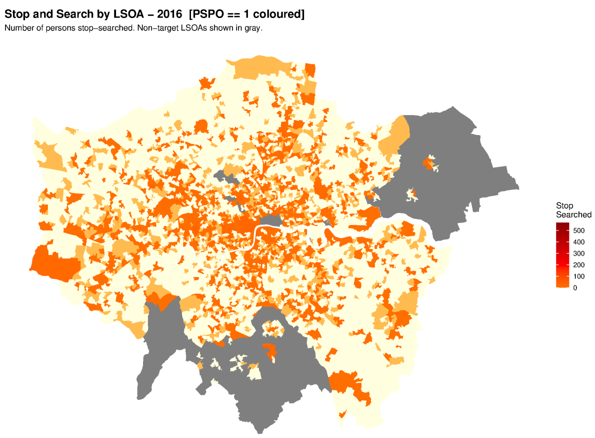
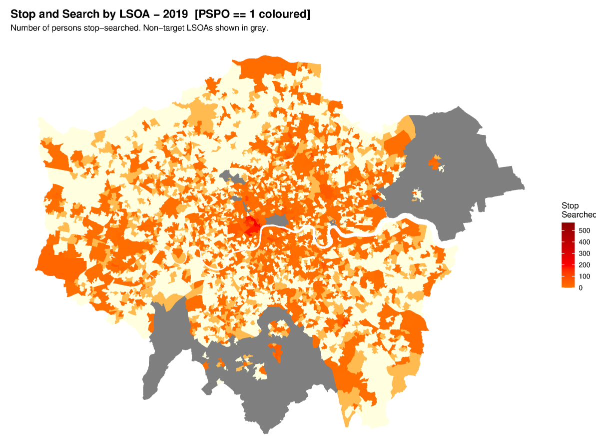


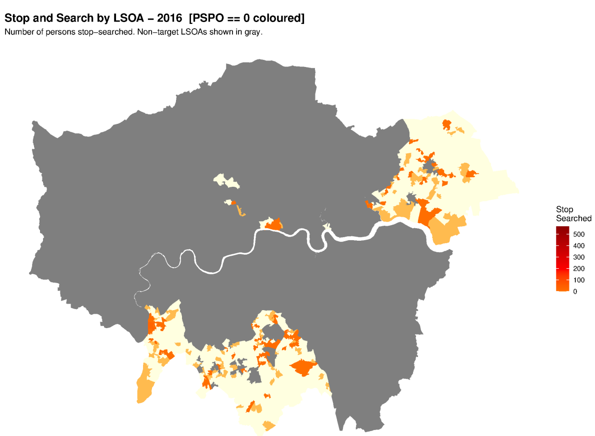
Figure A2: Stop and Search concentration in treated and never treated census blocks across time



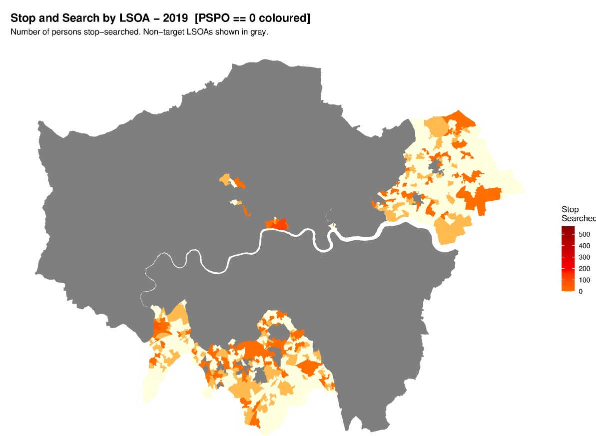
(a) Stop and Search in treated blocks, 2016



(b) Stop and Search in treated blocks, 2019



(c) Stop and Search in never treated blocks, 2016



(d) Stop and Search in never treated blocks, 2019

Figure A3: Trends in the total number of crimes and anti-social behaviour for treated and never treated census blocks before the introduction of PSPOs.

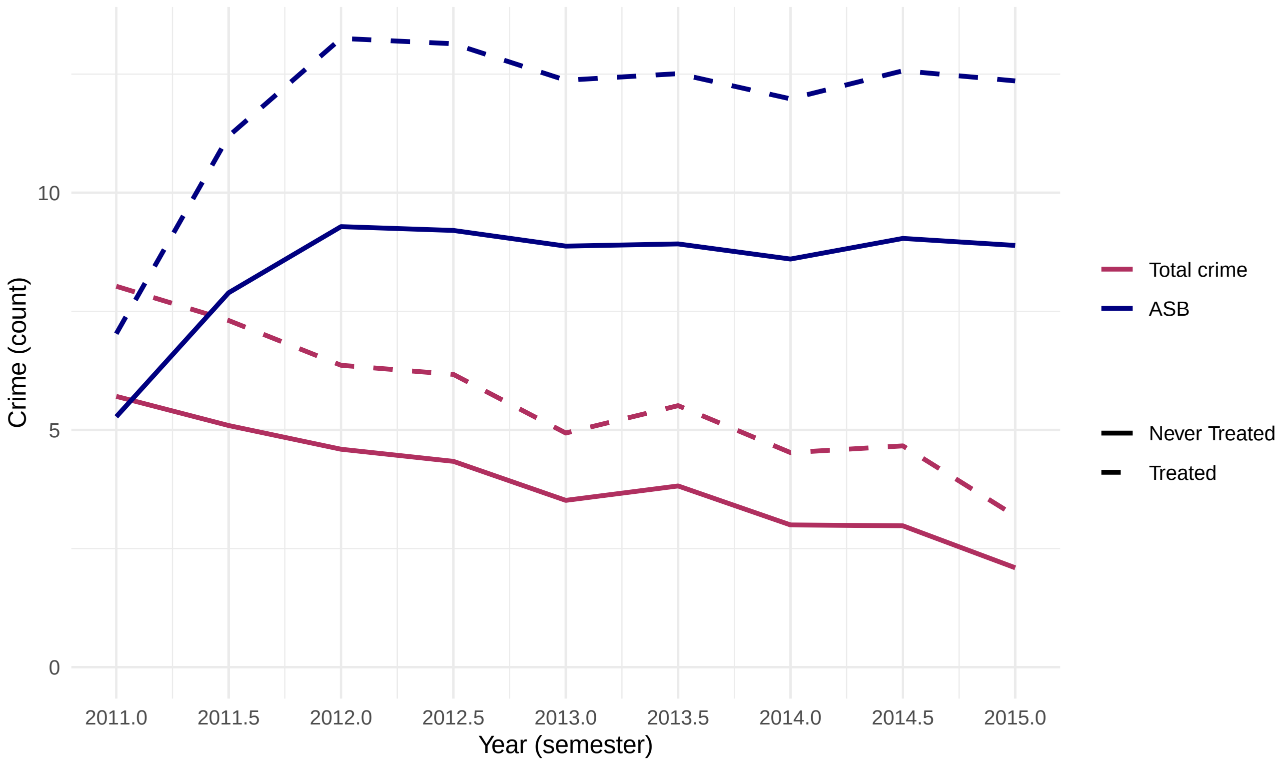


Table A1: DiD and Robust DiD estimates of the effect of PSPO on ASB

	TWFE		**S&A ATT**	
Dep var: ASB per 1000 in.	(1)	(2)	(3)	(4)
PSPO = 1	-0.813	-0.859	0.913	0.856
	(1.144)	(1.156)	(1.036)	(1.224)
clustered SE	yes	yes	yes	yes
census block FE	yes	yes	yes	yes
time FE	yes	yes	yes	yes
borough-time FE	yes	yes	yes	yes
time trends	no	yes	no	yes
Observations	84,970	84,970	83,404	83,404
R-squared	0.824	0.824	0.823	0.825
Mean dep. var.	15.55	15.55	15.35	15.35

Notes:

Table A2: DiD and Robust DiD estimates of the effect of PSPO on ASB

	S&A ATT			
Dep var: ASB per 1000 in.	(1)	(2)	(3)	(4)
PSPO = 1	0.856	0.404	3.594	
	(1.224)	(1.083)	(2.256)	
Spillover = 1		-0.534		-0.278
		(0.357)		(1.147)
clustered SE	yes	yes	yes	yes
census block FE	yes	yes	yes	yes
time FE	yes	yes	yes	yes
borough-time FE	yes	yes	yes	yes
time trends	yes	yes	yes	yes
model	baseline	control spill	excl. spill	spillover
Observations	83,404	83,404	77,450	11,125
R-squared	0.825	0.825	0.827	0.786
Mean dep. var.	15.35	15.347	15.55	10.12

Notes:

Table A3: DiD and Robust DiD estimates of the effect of PSPO on S&S

	TWFE		S&A ATT	
Dep var: S&S per 1000 in.	(1)	(2)	(3)	(4)
PSPO = 1	4.784**	5.127*	7.235**	7.075**
	(2.421)	(2.867)	(3.053)	(3.019)
clustered SE	yes	yes	yes	yes
census block FE	yes	yes	yes	yes
time FE	yes	yes	yes	yes
borough-time FE	yes	yes	yes	yes
time trends	no	yes	no	yes
Observations	84,970	84,970	83,404	83,404
R-squared	0.709	0.710	0.704	0.711
Mean dep. var.	9.060	9.060	9.26	9.26

Notes:

Figure A4: Effect of PSPO on the count of Stop and Search - Sun and Abraham (2021)

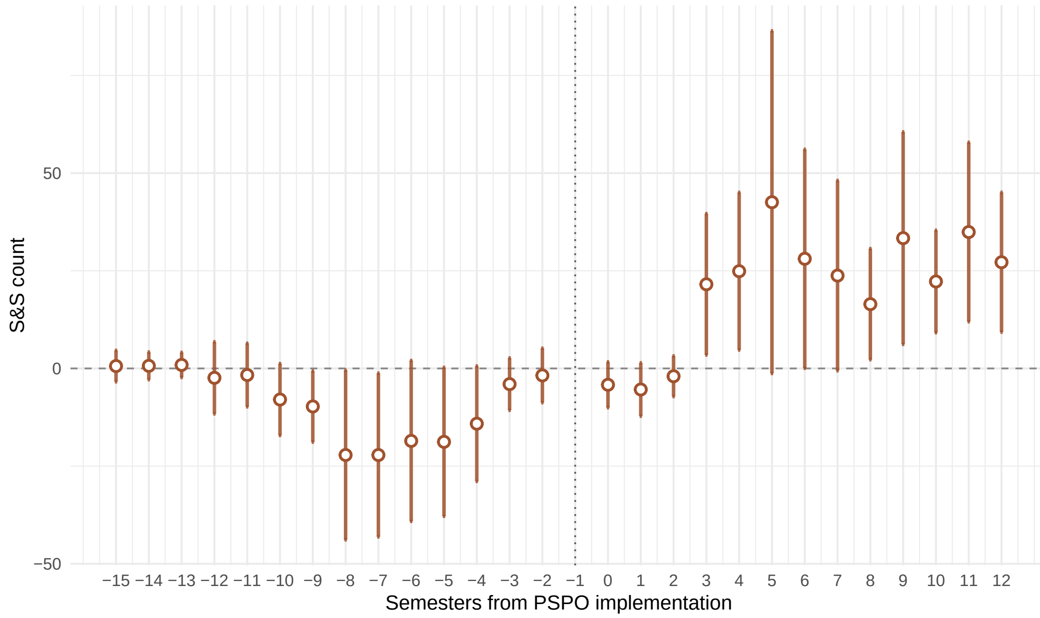


Table A4: DiD and Robust DiD estimates of the effect of PSPO on ODCs

	TWFE		**S&A ATT**	
Dep var: ODC per 1000 in.	(1)	(2)	(3)	(4)
PSPO = 1	-0.221	-0.263	0.112	0.134
	(0.409)	(0.419)	(0.748)	(0.743)
clustered SE	yes	yes	yes	yes
census block FE	yes	yes	yes	yes
time FE	yes	yes	yes	yes
borough-time FE	yes	yes	yes	yes
time trends	no	yes	no	yes
Observations	84,970	84,970	83,404	83,404
R-squared	0.878	0.878	0.86	0.861
Mean dep. var.	5.689	5.689	5.567	5.567

Notes:

Figure A5: Effect of PSPO on the reports of crimes related to theft - Sun and Abraham (2021)

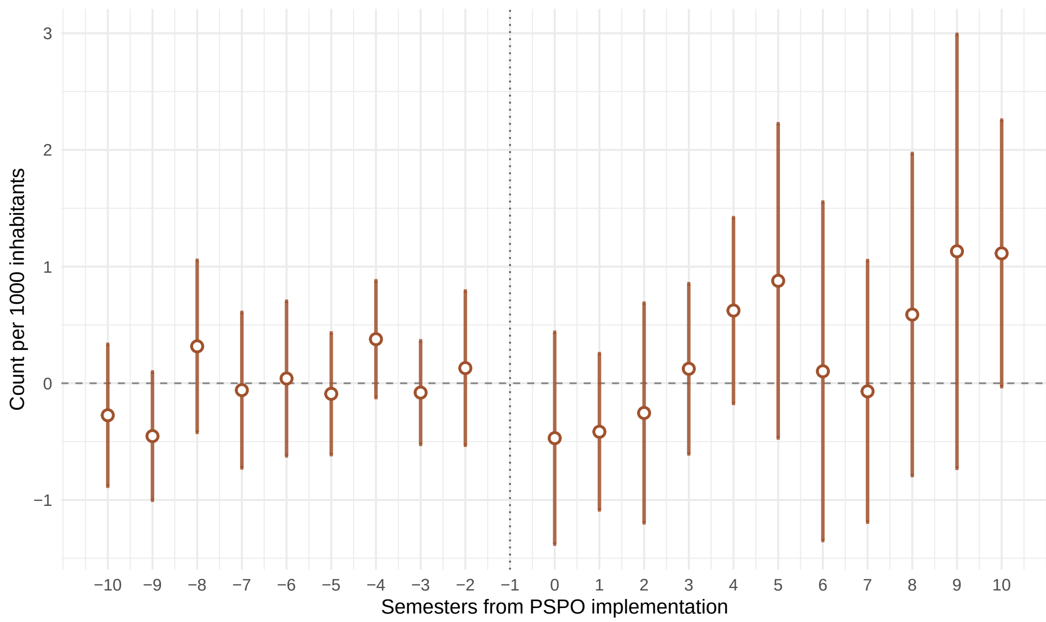


Figure A6: Effect of PSPO on the reports of crimes related to public damage - Sun and Abraham (2021)

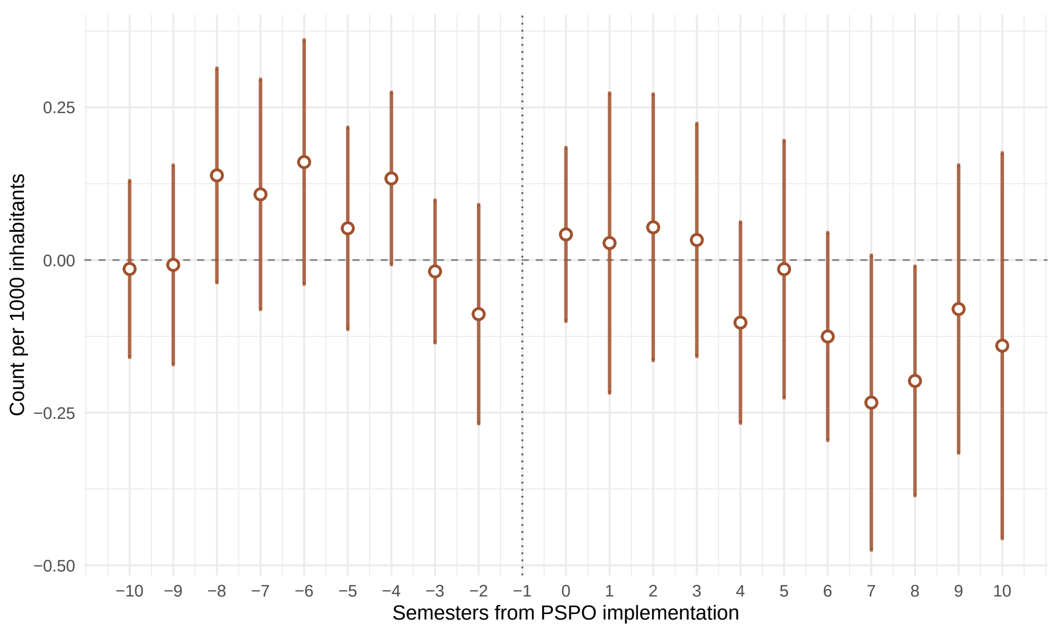


Table A5: DiD and Robust DiD estimates of the effect of PSPO on ODCs

	S&A ATT			
Dep var: ODC per 1000 in.	(1)	(2)	(3)	(4)
PSPO = 1	0.134 (0.743)	0.193 (0.754)	1.318 (1.83)	
Spillover = 1		0.132 (0.131)		-0.156 (0.273)
clustered SE	yes	yes	yes	yes
census block FE	yes	yes	yes	yes
time FE	yes	yes	yes	yes
borough-time FE	yes	yes	yes	yes
time trends	yes	yes	yes	yes
model	baseline	control spill	excl. spill	spillover
Observations	83,404	83,404	77,450	11,125
R-squared	0.861	0.861	0.862	0.816
Mean dep. var.	5.567	5.567	5.605	3.941

Notes:

Figure A7: Effect of PSPO on officer-detected crimes per 1000 inhabitants - Sun and Abraham (2021)

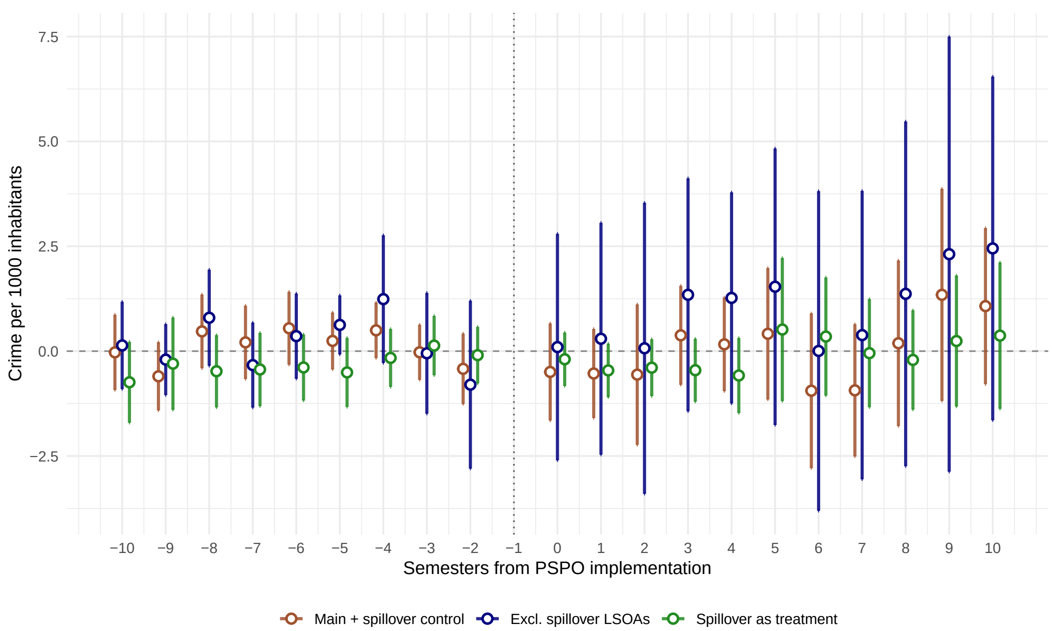
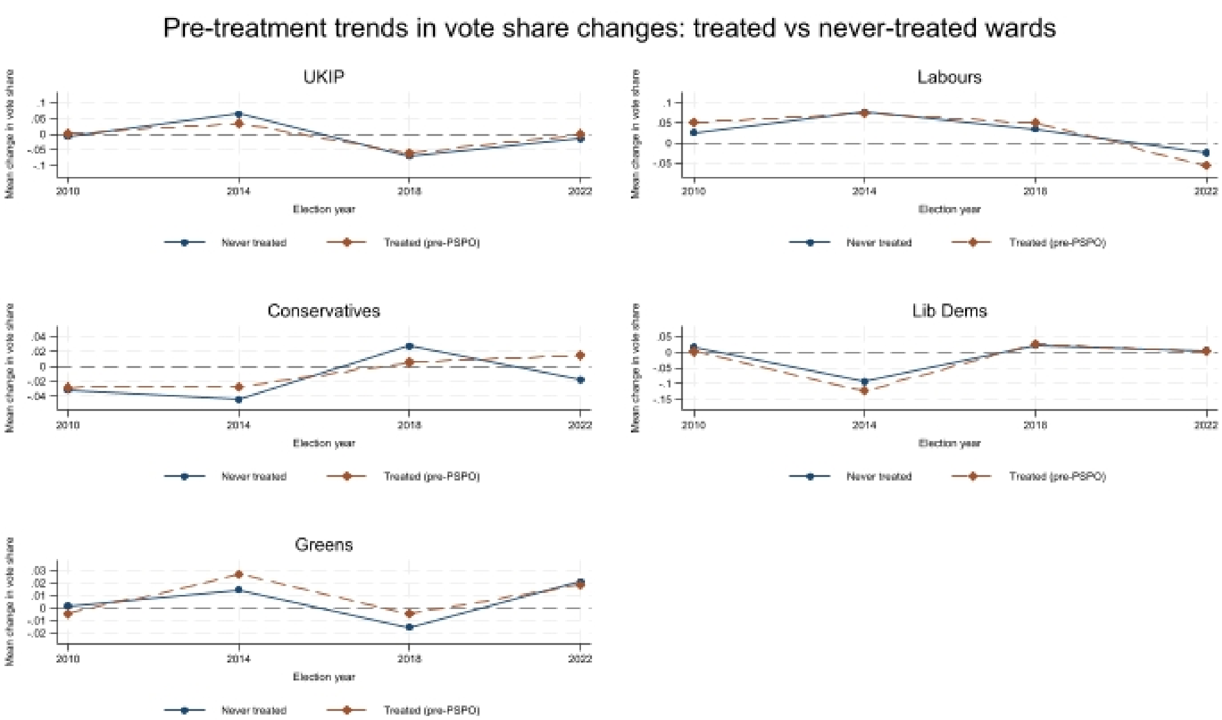


Table A6: DDD - Additional effect of PSPO on non-white stops relative to white stops.

	TWFE		S&A ATT	
Dep var: S&S per 1,000 inh.	(1)	(2)	(3)	(4)
PSPO = 1	1.019*	0.928	0.269	0.269
	(0.612)	(0.606)	(0.471)	(0.471)
PSPO × Non-white	0.369	0.483*	1.425*	1.363*
	(0.296)	(0.290)	(0.468)	(0.471)
Clustered SE	Yes	Yes	Yes	Yes
Census block FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Borough-time-race FE	Yes	Yes	Yes	Yes
Time trends	No	Yes	No	Yes
Observations	169,940	169,940	166,809	166,809
R-squared	0.681	0.690	0.673	0.665
Mean dep. var.	3.373	3.373	3.462	3.462

Notes: Standard errors clustered at the LSOA level in parentheses. All specifications include LSOA × race and time × race fixed effects. Borough-time-race FE denotes borough × time × race fixed effects. TWFE estimates reported in columns (1)–(2); Sun & Abraham (2021) ATT estimates in columns (3)–(4). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A8: Pre-trends of change in each party's vote share by treatment



Appendix B: PSPOs determinants and local attributes

This Appendix investigates the local determinants of PSPO adoption. Table B1 reports the results of a linear probability model in which the dependent variable is an indicator for whether a census block receives at least one PSPO, estimated using a Linear Probability Model (LPM). The purpose of this exercise is to identify the local attributes that predict PSPO adoption and to support the claim that, conditional on these covariates, the timing of treatment is plausibly exogenous within borough. The results confirm the patterns described in the descriptive statistics. The share of non-white residents and the share of residents aged over 65 are the most consistent predictors across all columns, with a large and precisely estimated coefficient that remains stable when additional controls are introduced, suggesting that PSPOs are systematically more likely to be implemented in more ethnically diverse areas mainly populated by young people. The unemployment share is imprecisely estimated in most specifications and is only marginally significant in column 6, while housing prices do not independently predict adoption once demographic composition is controlled for. Finally, the prior presence of an Alcohol or Dog Control Order is a strong positive predictor of PSPO adoption, consistent with PSPOs replacing or extending earlier regulatory instruments in areas already subject to previous local enforcement activity. These baseline characteristics are the same variables we include as demographic-specific linear time trends in the main DiD specification in Section 5. By interacting each variable's baseline level with a linear time trend, we allow census blocks with different pre-treatment profiles to follow differential trajectories in policing outcomes.

Table B1: LPM model of the determinants of PSPO implementation

	Pr (PSPO = 1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Baseline characteristics						
Population density per 1,000/sqkm	0.008***	0.004***	0.004***	-0.001	-0.002	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Share of non-white residents		0.559***	0.583***	0.472***	0.488***	0.495***
		(0.037)	(0.046)	(0.048)	(0.05)	(0.049)
Unemployment share			-0.238	-0.439	-0.376	-0.704***
			(0.284)	(0.285)	(0.288)	(0.281)
Share of over 65 residents				-1.802***	-1.782***	-1.491***
				(0.179)	(0.181)	(0.181)
Housing price index					0.025	-0.002
					(0.025)	(0.023)
Alcohol and Dog Control Orders						0.312***
						(0.015)
Observations	4197	4197	4197	4197	4197	4197
Mean dep. var.	0.531	0.531	0.531	0.531	0.531	0.531

Notes:

Appendix C: Construction of Additional Variables and Specifications

Robust DiD event study specification. As reported in Section 4.1, we employ an event study specification underlying the interaction-weighted (IW) estimator of Sun and Abraham (2021). The model is specified as:

$$Y_{c,d,t} = \sum_{g\in\mathcal{G}} \sum_{\ell\neq -1} \delta_{g,\ell} \left(\mathbf{1}[G_c = g] \times \mathbf{1}[t - G_c = \ell]\right) + \alpha_c + \delta_t + \mu_{b,t} + X'_{c,t}\gamma + \epsilon_{c,d,t} \tag{7}$$

where G_c denotes the treatment cohort of census block c, defined as the first semester in which a PSPO was implemented within that census block, and $\mathcal{G}$ is the set of all cohorts. The term $\mathbf{1}[t - G_c = \ell]$ is an indicator equal to 1 when census block c is ℓ periods relative to its treatment date, with $\ell = -1$ normalised to zero and omitted as the reference period. The cohort-specific coefficients $\delta_{g,\ell}$ capture the average treatment effect for cohort g at relative time ℓ, and are identified by comparing treated census blocks within cohort g to clean control units at the same calendar time[34]. The aggregate ATT reported in the main results is a weighted average of the $\delta_{g,\ell}$ estimates across all cohorts and post-treatment periods ($\ell \geq 0$), using cohort-size weights. All remaining terms are as defined in equation (2): α_c are census block fixed effects, δ_t are semester fixed effects, $\mu_{b,t}$ are borough-by-semester fixed effects, and $X'_{c,t}\gamma$ are demographic-specific linear trends. Pre-treatment coefficients ($\ell < -1$) are reported in the event study figures, and they should be jointly indistinguishable from zero to rule out any difference in the trends of treated an controls groups before the treatment starts.

DiD dynamic model specification. To complement our main binary treatment results, we present the DiD dynamic model specification estimated using the heterogeneity-robust estimator of de Chaisemartin, D'Haultfœuille and Vazquez-Bare (2024). To relax concerns related to treatment allocation within census tracts, we exploit the continuous coverage measure C_{ct} introduced in Section 3, which records the share of each census block's area subject to active PSPO restrictions in each semester, which is cumulative for overlapping orders. C_{ct} equals zero for untreated census blocks, ranges between 0 and 1 when a single PSPO partially or fully covers the block, and exceeds 1 when multiple orders overlap within the same block, directly capturing the cumulative intensity of treatment exposure.

We estimate the following event-study specification:

$$Y_{c,b,t} = \sum_{\ell\neq -1} \beta_\ell \cdot C_{c,t-\ell} + \alpha_c + \delta_t + \mu_{b,t} + X'_{c\cdot t\cdot\gamma} + \varepsilon c, b, t \tag{8}$$

The coefficient β_ℓ measures the dynamic dose-response relationship at each period ℓ relative to the first semester in which census block c receives any positive PSPO coverage ($C_{ct} > 0$), with $\ell = -1$ normalised to zero. α_c are census block fixed effects absorbing all time-invariant differences across blocks, δ_t are semester

[34] The estimator employs as control units both never and not-yet treated units (Sun and Abraham, 2021).

fixed effects capturing aggregate time trends, $\mu_{b,t}$ are borough-by-semester fixed effects absorbing time-varying shocks common to census blocks within the same borough, and $X'_{c \cdot t \cdot \gamma}$ captures demographic-specific linear trends as defined in Section 4.1. Standard errors are clustered at the census block level.

The key identifying assumption is that in the absence of PSPOs, the evolution of the outcomes $Y_{c,b,t}$ would have been the same across census blocks receiving different levels of coverage intensity C_{ct}[35]. Pre-treatment coefficients β_ℓ for $\ell < -1$ provide the standard empirical check on this assumption and should be jointly indistinguishable from zero if treatment intensity is uncorrelated with pre-existing differential trends across census blocks.

Triple difference-in-differences (DDD) specification. The DDD estimating equation takes the following form:

$$Y_{it}^{e} = \sum_{g \neq -1} \delta_g \cdot \mathbf{1}[G_i = g] \cdot \mathbf{1}[t - g = \ell] \cdot \mathbf{1}[e = \text{Non-white}] + \mathbf{X}_{it}^{e} \boldsymbol{\beta} + \alpha_i^e + \lambda_t^e + \psi_{bt}^e + \varepsilon_{it}^e \tag{9}$$

where Y_{it}^e denotes stop-and-search counts per 1,000 inhabitants for ethnic group $e \in \{\text{Non-white}, \text{White}\}$ in census tract i at semester t. The dataset is reshaped to long format, yielding two observations per census tract per semester — one for non-white stops and one for white stops. The coefficient of interest, δ_g, captures the differential effect of PSPO adoption on non-white stops relative to white stops for cohort g, estimated using the interaction-weighted approach of Sun and Abraham (2021). The identifying variation comes from within-LSOA differences across ethnic groups over time, net of common shocks. $\mathbf{X}_{it}^e$ is a vector of baseline demographic characteristics interacted with semester dummies, allowed to differ across ethnic groups. α_i^e denotes LSOA×ethnicity fixed effects, absorbing time-invariant differences in stop rates across ethnic groups within each census tract. λ_t^e denotes semester×ethnicity fixed effects, absorbing common time shocks that affect each ethnic group differently. ψ_{bt}^e denotes borough×semester×ethnicity fixed effects, absorbing borough-level policing trends that may differ across ethnic groups. Standard errors are clustered at the LSOA level.

[35]PSPOs are designed by each borough council and are required to fall within borough boundaries, but they are not allocated to LSOAs nor do councils observe or follow LSOA boundaries when drawing the geographic extent of an Order. The overlap between PSPO boundaries and census block boundaries is therefore determined purely by geometric coincidence rather than by any deliberate targeting of specific LSOAs, strengthening the case that $\frac{|A_i \cap P_j|}{|A_i|}$ is predetermined with respect to local trends in outcomes.

References

Ahlfeldt, Gabriel M., Felipe Carozzi and Lukas Makovsky. 2021. A micro-geographic house price index for England and Wales. Technical Report CEPOP61 Centre for Economic Performance, LSE.
https://cep.lse.ac.uk/_new/publications/abstract.asp?index=10580

Ahlfeldt, Gabriel M., Stephan Heblich and Tobias Seidel. 2023. "Micro-geographic property price and rent indices." *Regional Science and Urban Economics* 98:103836.
https://www.sciencedirect.com/science/article/pii/S0166046222000746

Anwar, Shamena and Hanming Fang. 2006. "An Alternative Test of Racial Prejudice in Motor Vehicle Searches: Theory and Evidence." *American Economic Review* 96(1):127–151.
https://www.aeaweb.org/articles?id=10.1257/000282806776157579

Archer, Bejamin. 2023. Investigating the Implementation of Public Spaces Protection Orders PhD thesis Shieffield Hallam University.

Autor, David H., David Dorn and Gordon H. Hanson. 2013. "The China Syndrome: Local Labor Market Effects of Import Competition in the United States." *American Economic Review* 103(6):2121–2168.
https://www.aeaweb.org/articles?id=10.1257/aer.103.6.2121

Barilari, Francesco and Diego Zambiasi. 2025. "Political Rhetoric and Racial Discrimination in Arrests for Drugs." *The Economic Journal* p. ueaf055.

Blattman, Christopher, Donald P Green, Daniel Ortega and Santiago Tobón. 2021. "Place-Based Interventions at Scale: The Direct and Spillover Effects of Policing and City Services on Crime." *Journal of the European Economic Association* 19(4):2022–2051.
https://doi.org/10.1093/jeea/jvab002

Bowling, Benjamin, Robert Reiner and James Sheptycki. 2019. The police role, function and effects. In *The Politics of the Police*. Oxford University Press.

Bridges, Lee. 2015. The Legal Powers and their Limits. In *Stop and Search: The Anatomy of a Police Power*, ed. Rebekah Delsol and Michael Shiner. London: Palgrave Macmillan UK pp. 9–30.
https://doi.org/10.1057/9781137336101_2

Brown, Kevin J. 2017. "The hyper-regulation of public space: the use and abuse of public spaces protection orders in England and Wales." *Legal Studies* 37(3):543–568.

Burney, Elizabeth. 2009. *Making People Behave: Anti-social Behaviour, Politics and Policy*. 2nd edition. ed. United Kingdom: Willan.

Butts, Kyle. 2023. "Difference-in-Differences Estimation with Spatial Spillovers.". arXiv:2105.03737 [econ].
http://arxiv.org/abs/2105.03737

Cameron, David. 2010. "Big Society Speech.".
https://www.gov.uk/government/speeches/big-society-speech

Chalfin, Aaron and Justin McCrary. 2018. "Are U.S. Cities Underpoliced? Theory and Evidence." *The Review of Economics and Statistics* 100(1):167–186.
https://ideas.repec.org//a/tpr/restat/v100y2018i1p167-186.html

Cunningham, Scott. 2021. *Causal Inference: The Mixtape*. Yale University Press. Google-Books-ID: PSE-MEAAAQBAJ.

de Chaisemartin, Clément, Xavier D'Haultfœuille and Gonzalo Vazquez-Bare. 2024. "Difference-in-Difference Estimators with Continuous Treatments and No Stayers." *AEA Papers and Proceedings* 114:610–613.
https://www.aeaweb.org/articles?id=10.1257/pandp.20241049

Desmond, Matthew, Andrew V. Papachristos and David S. Kirk. 2016. "Police violence and citizen crime reporting in the Black community." *American Sociological Review* 81(5):857–876.

Di Tella, Rafael and Ernesto Schargrodsky. 2004. "Do Police Reduce Crime? Estimates Using the Allocation of Police Forces After a Terrorist Attack." *American Economic Review* 94(1):115–133.
https://www.aeaweb.org/articles?id=10.1257/000282804322970733

Draca, Mirko, Stephen Machin and Robert Witt. 2011. "Panic on the Streets of London: Police, Crime, and the July 2005 Terror Attacks." *American Economic Review* 101(5):2157–2181.

Facchetti, Elisa. 2024. "Police infrastructure, police performance, and crime: Evidence from austerity cuts." *IFS Working Paper 10.1920* .

Facchetti, Elisa. 2025. "Police infrastructure, police performance, and crime: evidence from austerity cuts.".
https://ifs.org.uk/publications/police-infrastructure-police-performance-and-crime-evidence-austerity

Fetzer, Thiemo. 2019. "Did Austerity Cause Brexit?" *American Economic Review* 109(11):3849–3886.
https://www.aeaweb.org/articles?id=10.1257/aer.20181164

Finnane, Mark. 1990. "Police and Politics in Australia — The Case for Historical Revision." *Australian & New Zealand Journal of Criminology* 23(4):218–228.
https://doi.org/10.1177/000486589002300402

Goodchild, M. F. and N. Siu-Ngan Lam. 1980. "Areal interpolation: a variant of the traditional spatial problem." *Geo-Processing* 1(3):297–312.
https://www.scopus.com/pages/publications/0019095230

Goodman-Bacon, Andrew. 2021. "Difference-in-differences with variation in treatment timing." *Journal of Econometrics* 225(2):254–277.
https://www.sciencedirect.com/science/article/pii/S0304407621001445

Heap, Vicky and Jill Dickinson. 2018. "Public Spaces Protection Orders: a critical policy analysis." *Safer Communities* 17(3):182–192.

Hoekstra, Mark and CarlyWill Sloan. 2022. "Does Race Matter for Police Use of Force? Evidence from 911 Calls." *American Economic Review* 112(3):827–60.
https://www.aeaweb.org/articles?id=10.1257/aer.20201292

Home Office. 1984. "Police and Criminal Evidence Act (PACE).".

Home Office. 1994. "Criminal Justice and Public Order Act.".

Home Office. 1998. "Crime and Disorder Act 1998.".

Home Office. 2018. Public Spaces Protection Orders: Guidance for councils. Technical report.

Home Office. 2026. Crime Recording Rules for Front line Officers and Staff. Technical report.

Italian Council of Ministers. 2025. "Law No. 80/2025: Conversion into law, with amendments, of Decree-Law No. 48/2025.". Issue: 80.

Kelling, George L. and James Q. Wilson. 1982. "Broken Windows." *The Atlantic* . Section: U.S. Volume Title: March 1982.
https://www.theatlantic.com/magazine/archive/1982/03/broken-windows/304465/

Kirk, David S. and Andrew V. Papachristos. 2011. "Cultural Mechanisms and the Persistence of Neighborhood Violence." *American Journal of Sociology* 116(4):1190–1233.
https://www.jstor.org/stable/10.1086/655754

Kline, Patrick and Enrico Moretti. 2014. "Local Economic Development, Agglomeration Economies, and the Big Push: 100 Years of Evidence from the Tennessee Valley Authority *." *The Quarterly Journal of Economics* 129(1):275–331.
https://doi.org/10.1093/qje/qjt034

Knowles, John, Nicola Persico and Petra Todd. 2001*a*. "Racial bias in motor vehicle searches: Theory and evidence." *Journal of Political Economy* 109(1):203–229.

Knowles, John, Nicola Persico and Petra Todd. 2001*b*. "Racial bias in motor vehicle searches: Theory and evidence." *Journal of Political Economy* 109(1):203–229.

Laniyonu, Ayobami. 2019. "The political consequences of policing: Evidence from New York City." *Political Behavior* 41(2):527–558.

LeRoy, William. 2024. "Understanding policing in the aftermath of gun violence: Examining investigatory stops and crime in Chicago." *Journal of Public Economics* 234:105117.

Livingston, Debra. 1997. "Police Discretion and the Quality of Life in Public Places: Courts, Communities, and the New Policing." *Columbia Law Review* 97(3):551–672.
https://www.jstor.org/stable/1123359

Mastrobuoni, Giovanni. 2019. "Police disruption and performance: Evidence from recurrent redeployments within a city." *Journal of Public Economics* 176:18–31.
https://www.sciencedirect.com/science/article/pii/S0047272719300684

Mastrorocco, Nicola and Arianna Ornaghi. 2025. "Who Watches the Watchmen? Local News and Police Behavior in the United States." *American Economic Journal: Economic Policy* 17(2):285–318.
https://www.aeaweb.org/articles?id=10.1257/pol.20230356

Metropolitan Police Service. 2020. MPS Stop and Search Policy. Technical report.

Morris, Kevin T and Kelsey Shoub. 2024. "Contested killings: The mobilizing effects of community contact with police violence." *American Political Science Review* 118(1):458–474.

Nunn, Nathan and Nancy Qian. 2011. "The Potato's Contribution to Population and Urbanization: Evidence From A Historical Experiment*." *The Quarterly Journal of Economics* 126(2):593–650.
https://doi.org/10.1093/qje/qjr009

Owens, Emily. 2020. "The Economics of Policing." *The Economics of Policing* .

Police, Metropolitan. 2026*a*. "Applying Problem-Solving Methods to Reduce Crime - Anti-Social Bheaviour.".

Police, Metropolitan. 2026*b*. "Officer Guidance on ASB Warning Notices.".

Ratcliffe, Jerry H., Travis Taniguchi, Elizabeth R. Groff and Jennifer D. Wood. 2011. "The Philadelphia Foot Patrol Experiment: A Randomized Controlled Trial of Police Patrol Effectiveness in Violent Crime Hotspots." *Criminology* 49(3):795–831. _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.1111/j.1745-9125.2011.00240.x.
https://onlinelibrary.wiley.com/doi/abs/10.1111/j.1745-9125.2011.00240.x

Skogan, Wesley G. 1992. *Disorder and Decline: Crime and the Spiral of Decay in American Neighborhoods*. University of California Press. Google-Books-ID: ASrAMJh7LngC.

Sun, Liyang and Sarah Abraham. 2021. "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects." *Journal of Econometrics* 225(2):175–199.
https://www.sciencedirect.com/science/article/pii/S030440762030378X

Tarling, Roger and Katie Morris. 2010. "Reporting Crime to the Police." *The British Journal of Criminology* 50(3):474–490.
https://doi.org/10.1093/bjc/azq011

Tyler, Tom R, Jeffrey Fagan and Amanda Geller. 2014. "Street stops and police legitimacy: Teachable moments in young urban men's legal socialization." *Journal of Empirical Legal Studies* 11(4):751–785.

UK Home Office. 2014. "Anti-Social Behaviour, Crime and Policing Act 2014.".

UK Home Office. 2023. "Public Order Act 2023.". Issue: c. 15.
https://www.legislation.gov.uk/ukpga/2023/15/contents

Weaver, Vesla M and Amy E Lerman. 2010. "Political consequences of the carceral state." *American Political Science Review* 104(4):817–833.

Weisburd, David, Laura A. Wyckoff, Justin Ready, John E. Eck, Joshua C. Hinkle and Frank Gajewski. 2006. "Does crime just move around the corner? A controlled study of spatial displacement and diffusion of crime control benefits." *Criminology: An Interdisciplinary Journal* 44(3):549–592.

White House. 2025. "Executive Order 14288: Strengthening and Unleashing America's Law Enforcement to Pursue Criminals and Protect Innocent Citizens.". Published: 90 Fed. Reg. 18765.
https://www.whitehouse.gov/presidential-actions/2025/04/executive-order-14288-strengthening-and-unlea

Yao, Wang. 2023. Police Discretion: A Power that Can Be Abused and Should Be Regulated. Atlantis Press pp. 620–628.
https://www.atlantis-press.com/proceedings/cdsd-22/125984901

Zambiasi, Diego. 2022. "Drugs on the Web, Crime in the Streets. The Impact of Shutdowns of Dark Net Marketplaces on Street Crime." *Journal of Economic Behavior & Organization* 202(C):274–306.
https://ideas.repec.org//a/eee/jeborg/v202y2022icp274-306.html