Households in Conflict Network
hicn.org

Do Military Deployments Reduce Crime? Evidence from the 2025 Washington, DC National Guard Deployment

Robert M. Gonzalez*

HiCN Working Paper 456

July 2026

Abstract
The deployment of military personnel in response to urban crime has become increasingly salient in the United States. Yet the effectiveness and efficiency of such deployments remain largely unknown. This paper studies the effect of the August 2025 deployment of National Guard troops in Washington, DC, using administrative crime data and a triple differences design that compares changes before and after the August 11 deployment across treated and untreated areas, relative to analogous periods in prior years. The deployment reduced robberies, motor vehicle thefts, and thefts, had no detectable effect on homicides or burglaries, and increased assaults with dangerous weapons. A cost–benefit analysis implies 30-day benefits of $6.45 million against costs of approximately $90 million. For comparison, a community-based monitoring program in Chicago achieved similar crime-reduction benefits at less than four percent of the cost. This underscores the relative inefficiency of large-scale military deployments as a crime-control strategy.

JEL Classifications
K42, H56, D72

Keywords
crime, policing, military, National Guard, Washington DC

[1] School of Economics, Georgia Institute of Technology. E-mail: robert.gonzalez@gatech.edu

1 Introduction

Can large-scale military deployments reduce urban crime in the United States? To our knowledge, this paper provides the first causal evidence on this question by studying the impact of the 2025 National Guard deployment in Washington, DC.

The deployment of military personnel in response to urban crime has become increasingly salient in the United States. While such deployments are more common in countries facing persistent violence, they have historically been rare in the US outside periods of acute civil unrest. Although previous research has examined the militarization of civilian police forces (Bove and Gavrilova, 2017; Harris et al., 2017; Mummolo, 2018; Lowande, 2021; Gunderson et al., 2021), there is little causal evidence on whether the direct deployment of military personnel reduces crime or does so efficiently relative to conventional policing strategies.

In August 2025, President Trump invoked Section 740 of the District of Columbia Home Rule Act to authorize the deployment of National Guard troops and federal agents across the city. The action marked the first use of this provision in US history and effectively placed DC's policing under temporary federal control. The setting provides a clean natural experiment to evaluate the effects of military deployment on crime: the deployment was unanticipated and abruptly implemented, generated a substantial increase in visible security presence across selected areas of the city, and occurred at a well-defined point in time.

We estimate the causal effect of the deployment using a triple-difference design that exploits variation across treated and untreated areas of DC and nearby jurisdictions, before and after the August 11 cutoff, relative to analogous changes in prior years. This specification removes common seasonal patterns by differencing out changes around the same calendar date in prior years, and mitigates concerns about within-DC spillovers by using nearby jurisdictions as external controls. We implement this design using high-frequency administrative data on reported offenses from the DC Metropolitan Police Department (MPD) covering 2022–2025, combined with media-sourced and independently verified information on the location of federal personnel, matched to the city's 57 Police Service Areas (PSAs), and aggregated to a PSA-by-week panel. We harmonize definitions across DC and control jurisdictions to align with the six primary offense categories reported in DC: homicide, assault with a dangerous weapon, robbery, burglary, motor vehicle theft, and theft.[1]

We find a mixed pattern of effects. There are consistent, robust declines in robberies, motor vehicle thefts, and thefts. There are no detectable effects on homicides or burglaries. And there

[1] We use Montgomery County, Prince George's County, and the cities of Alexandria and Baltimore as control jurisdictions for our main analysis.

is a statistically significant increase in assaults with a dangerous weapon (ADW). These findings hold across Poisson and difference-in-differences estimators, monthly aggregations, crime-rate specifications, and a synthetic difference-in-differences estimator. Placebo tests assigning the deployment date to August 11 of non-treatment years confirm the estimates are not artifacts of seasonality. We also test directly for spillovers within the city and find evidence of declines in motor vehicle thefts and thefts in non-deployed PSAs.

We interpret these results through two mechanisms: a deterrence channel operating through changes in the perceived probability of apprehension, and an activity suppression channel whereby deployment reduces public and commercial activity, lowering the frequency of potential victim–offender interactions. The documented declines in robbery, theft, and motor vehicle theft are consistent with both mechanisms. Our empirical setting does not allow us to disentangle deterrence from activity suppression. However, analysis of mobility and activity data—OpenTable restaurant reservations, public transit ridership, ride-hailing and taxi trips–show evidence of a contraction in local economic activity following deployment, consistent with activity suppression. The increase in ADW is harder to reconcile with deterrence or activity suppression. We consider two competing explanations: increased civilian–security interactions during enforcement, protests, or arrests; and disruption of local criminal dynamics triggering intergroup conflict. These mechanisms have distinct empirical signatures: the disruption channel predicts firearm-related violence; the enforcement-interaction channel predicts non-firearm assaults. The evidence is consistent with the latter: the increase in ADW is driven entirely by non-firearm incidents, firearm-related assaults are unchanged, and while detected gunfire rises following deployment, gunshot-related emergency room visits do not.[2] This pattern is consistent with demonstrative or expressive violence–e.g., warning shots–rather than an escalation in gun-related conflict.

Finally, we conduct a cost–benefit analysis that monetizes estimated changes in robberies, thefts, and motor vehicle thefts using contingent valuation estimates from the literature. Over a 30-day window, the implied social benefits of crime reduction amount to \$6.45 million against an estimated \$89.86 million in deployment costs. These cost figures cover only the National Guard component and therefore understate total fiscal costs and intangible costs such as reduced commercial activity and disruptions to residents' sense of safety. We benchmark these estimates against Chicago's Safe Passage Program (SPP), a community monitoring initiative designed to deter crime near public schools. Applying the same valuation framework, the SPP

[2] Detected gunfire is measured using ShotSpotter, an acoustic detection system that uses a network of sensors to identify and geolocate gunfire in real time (SoundThinking, Inc., 2026; Open Data DC, 2026). We combine these data with administrative records on gunshot-related emergency department visits from DC Firearm Injury Surveillance Dashboard (DC Health, 2026).

generated comparable social benefits at a cost of $3.3 million, implying a benefit–cost ratio of 1.60. When comparing benefits per dollar spent, the SPP was 23 times more cost effective than the DC deployment. The modest aggregate impact is consistent with our finding that deployment locations were selected on proximity to central landmarks rather than on pre-existing crime, suggesting the intervention was designed for visibility rather than targeted public-safety impact. This highlights the relative inefficiency of large-scale military deployments compared to localized, civilian-led deterrence models.

This paper speaks to four broad literatures. For the economics of crime, the setting offers a clean test of deterrence at scale and speaks to whether the crime-reducing effects of police presence generalize to military personnel. For political economy, the deployment represents a rare observable instance of the federal government assuming direct control over local public safety—a setting that speaks to broader questions about whether centralized state capacity can substitute for, or complement, local governance. For public economics, the cost–benefit analysis speaks directly to questions of efficient public expenditure: with a benefit–cost ratio of 0.07, and a community-based benchmark that is 23 times more cost effective, the deployment is a case study in the misallocation of public safety resources. For urban economics, the activity suppression we document—the contraction in restaurant reservations, transit ridership, and ride-hailing trips—connects the deployment directly to questions about how enforcement-intensive policies affect local commercial activity and the spatial distribution of economic life within cities.

Specifically within the economics of crime, this paper lies at the intersection of research on police presence and on military deployments for policing functions. The literature on police presence has primarily focused on hot-spots policing (Sherman and Weisburd, 1995; Braga et al., 2019; Braga and Weisburd, 2020), proactive policing (Zhao and Zhang, 2022; Masera, 2021), and problem-oriented policing (Hinkle et al., 2024; Bullock et al., 2021; Taylor et al., 2022). Hot-spots and problem-oriented strategies show meaningful, sustained crime reductions; findings for proactive policing are more mixed, with concerns about diminishing returns and legitimacy costs.[3] Evidence on militarized policing through the federal 1033 Program—which transferred surplus equipment to civilian agencies—is similarly divided: some studies find deterrence effects on street crime (Bove and Gavrilova, 2017; Harris et al., 2017), while others document little public-safety gain alongside increases in use of force or erosion of public trust (Mummolo, 2018; Lowande, 2021; Gunderson et al., 2021). Critically, this literature studies the militarization of civilian police, not the deployment of military personnel themselves.[4] This paper fills that gap

[3] Other papers studying the relationship between policing and crime include (Di Tella and Schargrodsky, 2004; Evans and Owens, 2007; Klick and Tabarrok, 2007; Sherman and Weisburd, 1995; Braga and Weisburd, 2020). For a comprehensive review, see Chalfin and McCrary (2017).

[4] Evidence from Colombia's *Plan Fortaleza* finds limited crime-reduction effects from large-scale army patrols

by (i) providing the first causal evidence on a large-scale federal military deployment used for domestic policing, (ii) examining the mechanisms through which such deployments affect crime, and (iii) benchmarking their cost-effectiveness against community-oriented interventions that emphasize sustained local engagement over highly visible force deployment.

2 Conceptual Framework

This section presents a conceptual framework that outlines how military deployments can affect crime. The analysis is based on a standard deterrence model (Becker, 1968), in which potential offenders compare the expected utility from committing an offense to the utility of an outside legal option. Formally, individual i commits offense j if

$$(1 - p_{ij}(D))\, u(B_{ij}) + p_{ij}(D)\, u(B_{ij} - F_{ij}) \ \geq \ u(W_i),$$

where B_{ij} denotes the payoff from offense j, $p_{ij}(D)$ is the perceived probability of apprehension as a function of deployment D, F_{ij} is the sanction if caught, W_i is the payoff from the outside option, and $u(\cdot)$ is a concave utility function.

Deterrence Mechanism Deployment may reduce crime by increasing the perceived probability of apprehension, $p_{ij}(D)$. A larger visible presence of military personnel raises the expected cost of offending, thereby lowering the expected utility from crime and reducing offending in equilibrium.

This deterrence mechanism may also generate several equilibrium responses. First, offenders may substitute across crime types, shifting toward offenses for which $p_{ij}(D)$ is less affected by deployment (e.g., less visible crimes such as burglary). Second, if deployment intensity varies spatially, crime may be displaced to areas with lower security presence. Third, offenders may postpone criminal activity until periods when the perceived probability of apprehension is lower. In our empirical setting, we are able to examine cross-crime substitution and spatial displacement, but our focus on short-run responses during the deployment period limits our ability to assess intertemporal substitution.

We note that the deterrence mechanism and its equilibrium responses are not unique to military deployments. Conventional police deployments can be interpreted within the same framework. We next discuss an additional mechanism that is more specific to large-scale military deployments.

despite substantial troop presence (Blair and Weintraub, 2023).

Activity Suppression Mechanism Military deployments may reduce overall economic and social activity by discouraging mobility, tourism, and public gatherings. In the DC context, contemporaneous reports document declines in restaurant visits, retail foot traffic, and consumer spending in the weeks following the deployment.[5] This contrasts with conventional police deployments, which are often perceived as improving public safety and may not generate comparable reductions in economic activity.

This mechanism can affect crime through a primary subchannel: lower economic and social activity reduces the frequency of interactions between potential offenders and potential victims, decreasing the expected returns to crime B_{ij}, particularly for opportunistic offenses. Because this channel operates independently of $p_{ij}(D)$, it can generate declines in crime even in the absence of deterrence effects. A second plausible subchannel is that reduced economic activity may lower labor demand in sectors such as hospitality, retail, and food services which can serve as outside options for potential offenders. In the model, this corresponds to a reduction in W_i, which increases the relative attractiveness of crime and may partially offset the crime-reducing effects of deployment.

While our empirical analysis cannot directly isolate the causal effect of activity suppression, we examine several indicators of mobility and economic activity to assess whether the deployment was associated with a broader contraction in local economic activity.

Overall, the conceptual framework highlights two broad channels through which military deployments may affect crime: a deterrence channel operating through changes in the perceived probability of apprehension, and an activity suppression channel operating through changes in economic and social activity. These mechanisms are not mutually exclusive and may operate simultaneously. In the discussion of the empirical results, we return to these channels and consider how they help explain the observed pattern of effects across crime categories.

3 The 2025 DC National Guard Deployment

In August 2025, President Trump declared a crime emergency in Washington, DC and invoked Section 740 of the District of Columbia Home Rule Act. This marked the first time a president had used the provision to assert federal control over the District's policing apparatus. The executive order authorized the deployment of National Guard units and federal agents from agencies including the U.S. Marshals Service, the Bureau of Alcohol, Tobacco, Firearms and Explosives, and Customs and Border Protection in coordination with the Metropolitan Police Department

[5] See for example, Crumley (2025) for a report on restaurant visits and retail foot traffic, Smith (2025) for a report on restaurant reservations, and (Loh and Haskins, 2025) for a report on hotel stays in the DC region (Loh and Haskins, 2025).

(MPD) (Washington Post Staff, 2025). From an empirical perspective, the intervention generated a sharp and highly visible increase in security presence concentrated in selected areas of the city.

The deployment was justified by the administration as a response to an alleged crisis of violent crime in the capital. President Trump described DC as "plagued by gangs and lawlessness" and argued that extraordinary measures were necessary to restore order. Yet the order coincided with historically low crime levels: official MPD statistics indicated that violent crime had declined by more than 30 percent relative to the previous year, while independent analyses reported that overall crime was at its lowest point in several decades. This contrast between official crime statistics and the administration's characterization of public safety conditions became central to public and legal debates surrounding the intervention (Washington Post Staff, 2025; ACLU of the District of Columbia, 2025; Greenblatt, 2025).

The 2025 deployment also occurred in a unique legal and political context. Unlike U.S. states, the District of Columbia lacks full autonomy over its policing institutions, and Congress retains constitutional authority to override local decision-making. While federal involvement in DC policing has occurred during past crises—most notably following the 1968 riots after Martin Luther King Jr.'s assassination and during the 2020 George Floyd protests—the 2025 episode represented the first instance of proactive federalization absent widespread civil unrest. Relative to prior episodes of federal intervention, the deployment was also unusual in its explicit focus on routine crime control rather than emergency response or riot containment. The episode renewed debate over the relationship between federal authority, local democratic control, and public-safety policy in the District (Brennan Center for Justice, 2025; Levitz, 2025; Ducharme, 2025; Forgey and Schor, 2025).

4 Data

We combine four main datasets in the analysis: (i) administrative incident-level crime records from the Metropolitan Police Department (MPD) in Washington, DC; (ii) geolocated information on National Guard and federal personnel deployments compiled from publicly available media reports; (iii) administrative crime records from neighboring police jurisdictions used to construct external control units; and (iv) ShotSpotter acoustic gunfire detection data and firearm-related emergency room (ER) visits data used to examine gun-related activity independently of police reporting.

4.1 DC Crime Data

Our primary dataset consists of administrative records of reported crime incidents from the Metropolitan Police Department (MPD) in Washington, DC, obtained through the District's Open Data Portal (District of Columbia Metropolitan Police Department, 2025). The dataset covers January 1, 2019, to October 31, 2025 and includes incident-level information on the date and time of the offense, police district, ward, Police Service Area (PSA), offense type, and geocoded coordinates. Washington, DC is divided into 57 PSAs, which represent the smallest operational units in MPD's organizational structure.

Our main crime measures aggregate the data to the PSA–week level. We use PSA-level aggregation because PSAs are the units at which MPD manages deployments, supervises officers, and tracks crime trends internally. Weekly aggregation is sufficiently granular to capture short-run responses to the deployment while also smoothing volatility in low-frequency categories such as homicides.

The dataset includes the following offense categories: assault with a dangerous weapon, robbery, homicide, sex abuse, burglary, theft, motor vehicle theft, and arson. Between January 1, 2019 and August 10, 2025, there were 195,512 reported incidents in DC. Theft constitutes the largest category, accounting for approximately 69% of all incidents, followed by motor vehicle theft (14%) and robbery (8%). Appendix Table A1 presents these figures. For the main analysis, we classify assault with a dangerous weapon, robbery, and homicide as violent crimes, and burglary, theft, and motor vehicle theft as property crimes. Sex abuse and arson occur relatively infrequently and are therefore excluded as stand-alone categories, although they are included in aggregate violent- and property-crime measures.

4.2 Deployment Data

We code the timing of the deployment using the official announcement issued by the White House on August 11, 2025, as well as publicly documented information on the subsequent arrival of National Guard units and federal agencies in the city (Washington Post Staff, 2025; Horton et al., 2025). We geolocate the deployment using publicly available media reports. In particular, we draw on detailed coverage from *The Washington Post*, which identified the presence of federal personnel across specific neighborhoods using visual accounts from reporters on the ground and verified videos posted on social media. We georeference the reported locations and match them to the corresponding Police Service Areas (PSAs). This allows us to construct indicators for whether a given PSA experienced patrols by National Guard units or other federal agencies.

The deployment involved highly visible patrols, military vehicles, and uniformed federal personnel operating in public spaces, making deployment activity relatively observable to both reporters and residents. Importantly, our identification does not rely exclusively on precise deployment geolocation. In additional specifications, we treat all DC PSAs as exposed to the intervention and obtain qualitatively similar results. Figure 1 maps reported deployment locations across PSAs. Figure 3 maps deployment intensity and pre-deployment crime rates at the PSA level. Areas with higher pre-deployment crime rates do not appear to receive systematically greater deployment intensity.

For the main analysis, we omit non-deployed PSAs within DC to avoid potential spillovers from nearby deployments and instead rely on external control units described below. In additional specifications, we retain these PSAs and estimate spillover effects separately.

Figure 2 presents weekly trends in crime totals by offense category between 2019 and 2025. For reference, we normalize the horizontal axis to denote the number of weeks relative to August 11 and overlay the time series for each year. The treatment year, 2025, is highlighted in blue. Across most categories, crime levels in 2025 are lower than in prior years (2019–2024). For theft and motor vehicle theft, levels in 2025 fall within the mid-range of previous years, but exhibit a gradual decline following the deployment. Figure 3 maps deployment intensity and pre-deployment crime rates at the PSA level. Notably, areas with higher pre-deployment crime levels do not appear to receive greater deployment intensity.

4.3 External Control Units

We construct our control group using nearby police jurisdictions. Specifically, we compile incident data from police beats in Montgomery County, MD; Prince George's County, MD; city of Baltimore, MD; and the City of Alexandria, VA.[6] Police beats in these control jurisdictions are equivalent to DC's PSAs. To enhance comparability with treated PSAs in DC, we restrict Prince George and Montgomery counties' control pool to beats belonging to police districts that border DC.[7]

Appendix B provides a detailed description of the offense classification and harmonization across the four control jurisdictions and DC. Panel A of Figure 4 depicts the estimation sample.[8]

Figure 5 presents monthly crime rates by offense category for treated PSAs in DC and external control units between January 1, 2022 and November 30, 2025. This is the time period

[6] Comparable beat-level microdata for Arlington County, VA were not publicly available at the time of data collection.

[7] We obtained Alexandria incident data at the police reporting district (PRD) level. Because PRDs do not align one-to-one with the Alexandria beats used in our analysis, we harmonize geographies by assigning each PRD to the beat that contains its polygon centroid and then aggregating incidents to the beat level. We present results using all units in control jurisdictions in the Robustness section below.

[8] Baltimore units are excluded from the figure for ease of interpretation.

for which we have common support across jurisdictions. Several patterns emerge. First, treated PSAs in DC exhibit higher theft rates while assault with a deadly weapon and burglary tend to be higher in control jurisdictions. Second, trends between treated and control units are broadly similar prior to the August 2025 deployment, providing visual support for the comparability of the two groups. A notable exception is violent crime (especially robbery and homicide) that saw an uptick in the summer of 2023 in DC but not in control jurisdictions. For this reason, we exclude 2023 from the main analysis, as it raises concerns about differential pre-trends between DC and control units.

To construct population estimates at the PSA level, we use 2020 Census block data from the District of Columbia Open Data Portal (District of Columbia Office of the Chief Technology Officer, 2020). We compute the centroid of each census block and assign it to the corresponding PSA based on spatial overlay with PSA shapefiles, then aggregate the population counts of all census blocks whose centroids fall within each PSA boundary. For control units outside of DC, we obtain 2020 Census block shapefiles from the U.S. Census Bureau's TIGER/Line files (U.S. Census Bureau, 2020), merge them with block-level population counts from the 2020 Public Law 94-171 Redistricting Data using the block GEOID identifier, and apply the same centroid overlay and aggregation procedure. This ensures that population is measured consistently across both treated (DC) and control units.

4.4 Shotspotter and Emergency Room data

Lastly, we complement our main crime data with two additional datasets that capture gun-related activity and outcomes. First, we use novel data from ShotSpotter, an acoustic gunfire detection system that records the date, time, and geolocation of gunshots in real time (SoundThinking, Inc., 2026; Open Data DC, 2026).[9] These data provide a measure of gunfire that is independent of police reporting and may capture incidents that do not result in reported crimes. Second, we use data on gun-related emergency department (ER) visits from the DC FASTER firearm injury surveillance system maintained by DC Health (DC Health, 2026). These data record firearm-related injuries treated in hospitals and are available at the ward-month level. Together, these datasets allow us to examine gun-related activity from both a detection-based and a health-outcome perspective.

[9]We obtain ShotSpotter data from the DC Open Data Portal. Available at: https://opendata.dc.gov/datasets/DCGIS::shot-spotter-gun-shots/about.

5 Empirical Strategy

Our main empirical strategy relies on a triple-differences (DDD) design that builds on a standard difference-in-differences (DID) framework. DID compares changes in crime after the Aug. 11, 2025 deployment between policing units that experienced deployments and those that did not, using control units from outside Washington, DC. Our DDD design introduces an additional layer of differencing across years. Specifically, it differences out changes in crime that occur around the deployment date in prior years for both treated units in DC and control units outside DC. Intuitively, the approach stacks all years around the August 11 cutoff and compares the change in crime in 2025 to the corresponding changes observed around the same cutoff in prior years. In doing so, the DDD design isolates the 2025-specific treatment effect from recurring unit-level seasonality around the deployment period. We note, however, that the timing of the deployment was unexpected and unrelated to underlying crime trends; thus, we adopt the DDD framework as a conservative approach to account for any residual differential seasonality around the cutoff. We treat this specification using external control units as our preferred design, as it mitigates bias from spatial crime and policing spillovers that may contaminate PSAs within DC without a reported deployment. We also report estimates using within-DC controls as a complementary specification.

Formally, we estimate:

$$y_{its} \;=\; \beta \left(\text{Deployed}_i \times \text{Post}_t \times Y2025_s\right) \;+\; \alpha_{it} \;+\; \gamma_{is} \;+\; \delta_{ts} \;+\; \varepsilon_{its}. \tag{1}$$

where y_{its} denotes the number of incidents of a given offense in police unit i, in calendar week t, and year s. Deployed_i equals one if unit i is a PSA in Washington, DC that experienced deployments of federal personnel; Post_t equals one for calendar weeks after August 11 (deployment date); and $Y2025_s$ equals one for observations from year 2025. The coefficient of interest, β, captures the change in crime in deployed PSAs after the deployment in 2025, relative to control units outside DC, netting out analogous differences around the same period in previous years. Fixed effects α_{it} (unit$\times$calendar week), γ_{is} (unit$\times$year), and δ_{ts} (calendar week$\times$year) absorb all main effects and two-way interactions in the triple-difference.[10] More generally, these fixed effects flexibly account for multiple sources of confounding variation: α_{it} captures unit–

[10] Equation (1) is equivalent to the fully interacted model:

$$\begin{aligned} y_{its} = \beta_0 + \beta_1\,\text{Deployed}_i + \beta_2\,\text{Post}_t + \beta_3\,Y2025_s + \beta_4\left(\text{Deployed}_i \times \text{Post}_t\right) \\ + \beta_5\left(\text{Deployed}_i \times Y2025_s\right) + \beta_6\left(\text{Post}_t \times Y2025_s\right) + \beta_7\left(\text{Deployed}_i \times \text{Post}_t \times Y2025_s\right) + \varepsilon_{its}, \end{aligned}$$

where the coefficient on the triple interaction term $\text{Deployed}_i \times \text{Post}_t \times Y2025_s$ corresponds to the DDD estimate β in Equation (1).

calendar-week specific characteristics that do not vary across years (e.g., systematically higher crime in central relative to residential areas during summer weeks). γ_{is} absorbs unit-specific annual trends in crime and other characteristics. δ_{ts} accounts for shocks that are common across all units. Standard errors are clustered at the unit level to allow for arbitrary serial and spatial correlation within units.

Given our DDD specification, identification does not rely on parallel trends in levels between treated and control units, as in a standard DiD design. Instead, it requires parallel trends *in differences*. That is, in the absence of deployment, the difference in crime trends between treated and control units around the August 11 cutoff,

$$\begin{aligned} &\Big(\mathbb{E}[y_{its} \mid \text{Deployed}_i = 1, \text{Post}_t = 1] - \mathbb{E}[y_{its} \mid \text{Deployed}_i = 1, \text{Post}_t = 0]\Big) \\ -&\Big(\mathbb{E}[y_{its} \mid \text{Deployed}_i = 0, \text{Post}_t = 1] - \mathbb{E}[y_{its} \mid \text{Deployed}_i = 0, \text{Post}_t = 0]\Big), \end{aligned} \tag{2}$$

is the same across years. This condition is weaker than standard parallel trends, as it allows treated and control units to follow different underlying trends over time, as long as the difference in their pre–post changes around the cutoff remains stable in the absence of deployment. Figure 5 nonetheless shows that trends between treated and control units are broadly parallel prior to the deployment. In our setting, this assumption is plausible for several reasons. First, we use geographically proximate control units. Second, the design differences out recurring seasonal patterns around the same calendar date across years. Third, the timing of the deployment was not anticipated and not driven by any visible uptick in crime (Figure 5) or clear seasonal pattern.

To study the evolution of effects over time and the existence of pre-trends, we also estimate a dynamic (event-study) version of the DDD specification:

$$y_{its} = \sum_{k \neq -1} \beta_k \left(\text{Deployed}_i \times \mathbf{1}\{\tilde{t} = k\} \times Y2025_s\right) + \alpha_{it} + \gamma_{is} + \delta_{ts} + \varepsilon_{its}. \tag{3}$$

where $\tilde{t}$ indexes the number of calendar weeks relative to August 11, with $k = -1$ (the week immediately preceding deployment) as the reference category. The coefficients β_k trace the dynamic response of crime relative to the week before deployment, with $k < 0$ capturing pre-deployment differences and $k \geq 0$ capturing post-deployment effects.

We complement the main specification with several robustness exercises. For comparison, we estimate a more conventional difference-in-differences (DiD) specification that compares changes in crime across deployed PSAs in DC and control units outside DC in 2025 alone:

$$y_{it} = \beta\,(\text{Deployed}_i \times \text{Post}_t) + \alpha_i + \gamma_t + \varepsilon_{it}, \tag{4}$$

where t indexes weeks within 2025.

Second, we estimate a synthetic difference-in-differences (SDID) model that constructs weighted combinations of control units to better match pre-deployment trends in treated PSAs, providing a complementary approach that flexibly accounts for differential pre-trends across treated and control units.

Third, we use monthly rather than weekly aggregation to ensure that results are not driven by short-term fluctuations. Fourth, we re-estimate our DDD model using (i) Poisson pseudo-maximum likelihood (PPML) to account for the count nature of the dependent variable; (ii) crime rates (per 1,000 residents) instead of counts to account for population differences across units; (iii) district-level aggregation to test robustness to spatial aggregation of treatment and outcomes, and (iv) aggregating offense-specific outcomes into broader violent and property crime categories.

6 Results

6.1 Main Results

This section presents our triple-differences estimates from Equation (1) comparing treated PSAs within DC to control policing units from neighboring jurisdictions: Prince George's County, Montgomery County, the City of Alexandria, and the City of Baltimore. We harmonize offense categories across jurisdictions for the six categories available in the DC data: assault with a dangerous weapon, robbery, homicide, burglary, theft, and motor vehicle theft.[11] We exclude non-treated DC PSAs to mitigate potential within-city spillovers, and examine spillovers directly in subsection 6.5. We further restrict the control group to units located in districts adjacent to the DC boundary to ensure geographic comparability and reduce unobserved heterogeneity across jurisdictions.[12]

The resulting panel covers 274 external control units and 25 treated units (PSAs) within DC, aggregated weekly between 2022 and 2025.[13] We exclude 2023 from the main analysis, as

[11] Refer to Appendix B for detailed information on the harmonization across categories and jurisdictions.

[12] We present results using all units in control jurisdictions in the Robustness section below.

[13] The breakdown for the 274 control units is: 193 police posts from the City of Baltimore, 45 police beats from Prince George's County, 20 police beats from Montgomery County, and 16 police beats from the City of Alexandria.

it represents an atypical year for violent crime in DC. As shown in Figure 5, robberies exhibit a pronounced spike during 2023 that is not present in external control units, raising concerns about differential trends between DC and control units. Lastly, we restrict the sample to a symmetric window of 24 weeks before and 16 weeks after August 11 in each year, corresponding to the November 30 end of deployment. Panel A of Figure 4 depicts the estimation sample.[14] Table 1 reports the corresponding DDD estimates, and Figure 6 shows the dynamic event-study coefficients.

Results indicate statistically significant declines in robberies, thefts, and motor vehicle thefts (MVT) in treated DC PSAs relative to external controls, alongside a statistically significant increase in assaults. Aggregated across all treated PSAs, these estimates correspond to a reduction of approximately 5 robberies, 44 thefts, and 20 motor vehicle thefts per week, and an increase of about 3 assaults with a dangerous weapon. To put these magnitudes in perspective, the treated PSAs cover a population of 292,529 residents (2020 Census), implying weekly changes of approximately 1.7 fewer robberies, 15 fewer thefts, and 6.8 fewer motor vehicle thefts per 100,000 residents, alongside an increase of about 1 assault per 100,000 residents.[15]

For all crime categories, the event-study coefficients are largely centered around zero in the pre-deployment period, suggesting stable differences in trends between treated and control units prior to the August 11 cutoff (Figure 6). Thefts and MVTs show evidence of a gradual decline post-deployment. Assault with a dangerous weapon exhibits a more nuanced increase post-deployment, although the coefficients are not statistically significant. The dynamics for robbery are less clear. While the aggregate coefficient in Table 1 is negative and statistically significant, the event study suggests a declining pattern prior to the cutoff and no clear decline afterward. To address this, we complement our baseline specification with a synthetic difference-in-differences (SDID) approach that better matches pre-deployment trends in treated PSAs.

6.2 Robustness Checks

As a robustness check, we estimate treatment effects using the synthetic difference-in-differences (SDID) estimator of Arkhangelsky et al. (2021). This estimator combines features of difference-in-differences and synthetic control methods by constructing unit and time weights that best reproduce the pre-treatment trajectory of treated units using donor jurisdictions. To align the

[14] Baltimore units are excluded from the figure for ease of interpretation.

[15] A potential concern is that the null homicide results reflect limited statistical power due to the rarity of these events. However, several patterns suggest that this is unlikely. Crimes with much higher frequency—such as burglaries—also exhibit no statistically significant response (Table 1, column 4). In addition, aggregating outcomes to the monthly level, which reduces sparsity in the data, yields similarly null estimates for homicides (Table A4). Results are also unchanged when we use PPML as the estimator, which is better suited for count outcomes with many zeros (Table A2).

estimator with the identification strategy of our triple-difference design, we restrict the sample to weeks following August 11 in each year. In this specification, post-August 11 weeks from earlier years serve as the pre-treatment period, while post-August 11 weeks in 2025 constitute the treatment period, ensuring that treated and donor units are compared at comparable points in the seasonal crime cycle.

Table 2 presents the SDID estimates. The estimates closely resemble the main DDD results in Table 1. The direction of the estimated effects is consistent across outcomes and the magnitudes are broadly similar. The primary differences are that the estimated increase in assaults with dangerous weapons is no longer statistically significant, while the estimated decline in burglaries becomes marginally significant at the 10 percent level. Figure 7 presents the event study graphs for this method. Note that the event study for robbery presents a clearer pattern now, stable differences in trends prior to deployment and a clear drop in robberies post deployment.

Our main DDD results are robust across a range of alternative specifications, with robberies exhibiting the most consistent and statistically robust declines. Declines in robberies remain statistically significant across all specifications we consider, including (i) PPML estimation (Table A2); (ii) a conventional difference-in-differences model (Table A3);[16] (iii) monthly aggregation (Table A4); (iv) expressing outcomes as rates per 100,000 residents (Table A5); (v) monthly rates (Table A6); (vi) excluding police beats from the City of Baltimore (Table A7); and (vii) expanding the control pool to include all units within neighboring counties rather than only those adjacent to DC (Table A8). By contrast, the statistical significance of motor vehicle thefts, thefts, and assaults is more sensitive to outcome definition, although the direction of effects remains consistent across specifications. For each of these outcomes, coefficients are statistically significant in specifications using levels—such as PPML estimation (Table A2), monthly aggregation (Table A4), and alternative control samples excluding Baltimore or expanding the control pool (Tables A7 and A8)—but become statistically insignificant when outcomes are expressed as rates (Tables A5 and A6). As an additional robustness check, Table A9 aggregates offenses into violent (assault with a dangerous weapon, robbery, homicide) and property (theft, burglary, MVT) crime categories. Each column corresponds to an alternative estimation approach (OLS, PPML, crime-rate outcomes, and monthly aggregation). Across specifications, estimates for violent crime remain statistically insignificant, while property crimes exhibit negative and significant coefficients.

[16]The DiD specification also produces a weakly significant (10%) decrease in homicide. However, this result does not appear in any other specification or in the more robust triple-difference specification; thus we interpret this as spurious.

6.3 Placebo Test

We also conduct a falsification exercise that uses August 11 of each non-deployment year (2019–2024) as a placebo intervention date. For each placebo year, we estimate Equation (3) using August 11 of that year as the deployment cutoff while omitting 2025 from the sample. As with previous results, we use units outside of DC as the control group. Figure A2 plots the estimated triple-difference coefficient along with the estimated treatment effect for the true intervention year, 2025 (red marker).

Overall, the placebo estimates provide little evidence of systematic effects in untreated years. Crime categories for which we find no statistically significant effects in the main analysis likewise show no meaningful placebo effects. More importantly, categories for which the main analysis detects meaningful effects—assault with a dangerous weapon, robbery, theft, and motor vehicle theft—display no consistent patterns, are smaller in magnitude, and are statistically insignificant in most placebo years.

We note a small number of statistically significant placebo estimates. In particular, the placebo coefficients for robberies and homicides display jumps in 2023, reflecting the sharp spike in violent crime observed in Washington, DC during the summer of that year (Figure 5). This surge was largely concentrated in DC rather than in neighboring jurisdictions (Dale, 2025). For this reason, we exclude 2023 from the main analysis, as it would otherwise generate differential trends in violent crime between DC and the non-DC control units. More broadly, the presence of few placebo effects underscores the importance of our triple-difference design. A simple difference-in-differences specification using August 11 as the cutoff could attribute these seasonal shifts to the deployment. By contrast, the triple-difference estimator differences out baseline August patterns observed in other years, isolating the change unique to the 2025 deployment.

6.4 Within-DC Analysis

We also estimate Equation (1) restricting the analysis to within DC by using non-deployed PSAs as control units. As with our main results, we use weekly data and a 24-week period prior to the deployment date. Table 3 reports these results. We find a decline in robberies following the deployment, although the estimated effects are imprecise and not statistically distinguishable from zero. Other violent crimes, such as assaults and homicides, show no systematic response, with coefficients that are small in magnitude and statistically insignificant. Burglary and theft display no meaningful changes after deployment, and the coefficient for motor vehicle theft is positive but also statistically insignificant. Taken together, these results indicate no robust statistical evidence that deployments affected crime levels across PSAs. Figure 8 presents the dynamic DDD estimates from Equation (3). The event-study coefficients show no evidence of differential pre-trends prior to the deployment and reveal only modest and short-lived fluctuations in crime categories immediately afterward.

This conclusion is robust across a wide range of alternative specifications. We obtain similar qualitative results when re-estimating the weekly triple-differences specification using a Poisson pseudo-maximum likelihood (PPML) estimator (Table A14). The findings are also unchanged when using a standard difference-in-differences (DiD) specification as in Equation (4) (Table A15), when using crime rates per 100,000 residents as the outcome (Table A16), and when aggregating the data to the police district level (Table A17).[17] Results are similarly unchanged when aggregating the data monthly rather than weekly (Table A18), and when collapsing offenses into broader violent (assault with dangerous weapon, robbery, homicide) and property (theft, burglary, MVT) crime categories (Table A19).

Overall, compared to our main results using external control units, these findings suggest that deployment spillovers likely attenuate estimates when using within-DC controls, as PSAs without reported deployments may nonetheless be indirectly affected by nearby deployments.

6.5 Deployment Spillovers within DC

We next assess whether the effects of the deployment extended beyond treated PSAs. The specification distinguishes between three groups of units: (i) deployed PSAs within DC, (ii) non-deployed PSAs within DC, and (iii) external control units from Montgomery County, Prince George's County, the City of Alexandria, and the City of Baltimore. This "ring" design allows us to estimate both the direct impact of deployment and potential spillovers into nearby, non-deployed PSAs, relative to areas outside of DC. Panel C of Figure 4 illustrates the three groups in the estimation sample. Specifically, we estimate the following regression model:

$$\begin{aligned} y_{its} &= \beta_1 \left(\text{Deployed}_i \times \text{Post}_t \times Y2025_s\right) \\ &\quad + \beta_2 \left(\text{NonDeployedDC}_i \times \text{Post}_t \times Y2025_s\right) \\ &\quad + \alpha_{it} + \gamma_{is} + \delta_{ts} + \varepsilon_{its}. \end{aligned} \tag{5}$$

where NonDeployedDC_i is an indicator equal to one if there is no reported deployment in PSA i; the remaining terms are defined as in Equation (1). Table 4 reports the corresponding estimates. Results indicate some evidence of spillover effects across most crime categories. Coefficients for

[17] Because each district includes at least one PSA with deployments, a binary "Deployed" indicator has no between-district variation. We therefore construct a continuous measure of treatment intensity—the *deployment share*—defined as the fraction of PSAs within a district that experienced deployments. Statistical inference relies on wild-cluster bootstrap p-values given the small number of clusters ($G = 7$).

assault, robbery, burglary, and homicide are small in magnitude and statistically insignificant for the non-deployed PSAs in DC. By contrast, motor vehicle thefts (MVT) exhibit a negative and statistically significant coefficient (column 6), consistent with a meaningful decline in vehicle thefts in PSAs that were not directly deployed but border treated PSAs. The magnitude of the MVT effect is comparable to that estimated for directly treated PSAs, suggesting that reductions in vehicle theft extended beyond the immediate deployment zones. There is weaker evidence of policing spillovers in thefts (column 5) as the coefficient on non-deployed PSAs is marginally significant or indistinguishable from zero when using other specifications.[18]

While we find evidence of policing spillovers for motor vehicle thefts, we do not find similar results for violent crimes, including robberies. Since vehicles can be moved across space, even modest increases in monitoring within certain areas can reduce theft risk in nearby, non-deployed PSAs. For example, common deployment features such as checkpoints and license-plate checks likely extend the perceived risk of detection beyond the immediate deployment zones. In contrast, robberies are more localized and depend on the presence of both potential victims and offenders within specific street segments. Deterrence in this case relies more heavily on the visible presence of personnel within a given area.

6.6 Selection into Deployment

A large empirical literature finds that police presence can deter crime, but the effects are highly dependent on where and how resources are deployed. Evidence synthesized by Chalfin and McCrary (2017) shows that police resources are most effective when targeted to high-crime areas or hot spots where the elasticity of crime with respect to police presence is highest. This section examines the determinants of PSA selection into deployment. In particular, we assess whether pre-existing crime patterns, demographic composition, or geographic proximity to central landmarks were systematically related to the likelihood of deployment.

We estimate regressions of the form:

$$\text{Deployed}_i = \alpha + \boldsymbol{\beta}' \,\text{Crime}_i + \gamma_1 \,\text{Population}_i + \gamma_2 \,\text{ShareNonWhite}_i + \gamma_3 \,\text{DistanceWH}_i + \varepsilon_i, \quad (6)$$

where Deployed_i is an indicator equal to one if PSA i received deployment on August 11, 2025. Crime_i is a vector of lagged crime prior to deployment, constructed over different windows: one month (July 12–August 10, 2025), three months (May 12–August 10, 2025), and twelve

[18] Statistical significance disappears when using a Poisson Pseudo-ML estimator and it becomes less significant when using an expanded definition of spillovers using four rings, treated PSAs in DC, non-treated PSAs within DC, units outside of DC but bordering DC in Prince George and Montgomery counties, and control units outside of DC. Table A20 in the Appendix presents these results.

months (August 12, 2024–August 10, 2025). Depending on the specification, we include either aggregate totals for violent and property crimes or a vector of subcategories (robbery, assault with a dangerous weapon, homicide, burglary, motor vehicle theft, and theft). Each regression also controls for PSA population (in thousands), the share of non-White residents, and the distance from the PSA centroid to the White House.

Table 5 presents the results. Several patterns emerge. First, lagged crime is generally a weak predictor of deployment. Coefficients on property crime are consistently close to zero and statistically insignificant. For violent crime, most coefficients are small and imprecisely estimated, although there is some evidence of a positive association in the three-month specification (Column 3). The joint F-tests on the crime variables tell a similar story: in most specifications the p-values are large. In a few cases (Columns 3, 4, and 6) the p-values fall below conventional thresholds, suggesting that crime patterns may have played some role in shaping deployment. However, this result is not consistent across robustness checks described below.

By contrast, geography is a consistent predictor. Across all specifications, the distance from the White House enters with a negative and statistically significant coefficient, indicating that PSAs located closer to central landmarks were much more likely to host deployments. The demographic variables are less systematically related.

Results are robust to alternative specifications using deployment rate (number of deployments per 10,000 population) as the outcome variable (Table A21), using a Probit model (Table A23), redefining geographic proximity based on other landmarks (the Capitol, the Washington Monument, and the Lincoln Memorial) (Table A24), and using changes in crime rather than average crime levels prior to deployment (Table A22).

The regression results are visually supported by the spatial pattern in Figure 3. Note that there is a clear contrast between deployment intensity across PSAs (panels a and b) and crime intensity (panels c and d). Specifically, PSAs with higher crime rates prior to deployment do not seem to be targeted with more intense deployment. This is particularly true for violent crime.

Taken together, these results suggest that deployment selection was primarily driven by symbolic or strategic considerations tied to geography—proximity to landmarks and tourist areas—rather than by local crime conditions. At the same time, the significant coefficients on crime in some windows indicate that crime may have been one factor considered, albeit not the dominant one. For instance, in column 3, while crime variables explain about 8% of the variation in deployment across PSAs, proximity to the White House explains 27%.

7 Mechanisms

7.1 Heterogeneous Effects across Crime Categories

We next examine heterogeneity across crime categories to shed light on the mechanisms through which the deployment affected crime. Guided by the conceptual framework, we focus on two primary channels. The first is a deterrence mechanism, whereby the visible presence of military personnel increases the perceived probability of apprehension. The second is an activity suppression mechanism, whereby the deployment reduces public and commercial activity, lowering the frequency of interactions between potential offenders and potential victims.

The pattern of effects across offense categories is informative because these mechanisms are expected to have their largest impacts on crimes that are both opportunistic and highly dependent on public interactions. Consistent with this prediction, we find significant and robust reductions in robberies, thefts, and motor vehicle thefts following the deployment. These offenses typically occur in public settings and are particularly sensitive to both increases in perceived detection risk and declines in victim–offender interactions.

By contrast, we find no detectable effects on homicides or burglaries. Homicides are often relational in nature, arising within established social networks or private settings, and may therefore be less responsive to changes in visible enforcement.[19] Burglaries similarly tend to occur in lower-visibility contexts and may be less affected by either mechanism. Taken together, these heterogeneous effects are consistent with a large literature showing that visible policing disproportionately reduces crimes of opportunity, such as robbery, theft, and motor vehicle theft, while having more limited effects on interpersonal or less visible offenses (Wilson and Boland, 1978; Sampson and Cohen, 1988; Kubrin et al., 2010).

Given our empirical setting, and the fact that deterrence and activity suppression generate similar predictions across crime categories, we cannot separately identify the relative importance of these two channels. We therefore interpret the heterogeneous effects as reflecting a combination of deterrence and activity suppression. However, we can still assess whether the activity suppression mechanism is operative by examining the effect of the deployment on several high-frequency measures of mobility and consumer activity.

[19] The incident-level data do not indicate whether an offense occurred in a public or private location, which limits our ability to directly test whether deployment effects were concentrated in settings where military visibility was highest.

7.2 Activity Suppression

We examine the activity suppression mechanism using three sources of data. First, we use monthly public transit ridership from the Federal Transit Administration's National Transit Database monthly module Federal Transit Administration (2026). The transit outcomes are unlinked passenger trips (UPT), which count passenger boardings. We use UPT for WMATA heavy rail, WMATA Metrobus, Maryland commuter rail, and Maryland commuter bus. The commuter rail and commuter bus series capture travel flows from Maryland into Washington, DC. Second, we use data from the DC Department of For-Hire Vehicles on taxi and ride-sharing trips District of Columbia Department of For-Hire Vehicles (2026). Specifically, we use monthly counts of taxi and ride-sharing trips. Third, we use daily restaurant reservation data from OpenTable's State of the Industry (OpenTable, 2025). OpenTable reports the year-over-year percentage change in seated diners from online reservations among restaurants active on its platform. The data used in this analysis include two aggregate series: the 2025 versus 2024 percentage change for DC and for the broader United States excluding DC. OpenTable compares each day to the same day of the week in the corresponding week of the previous year to account for day-of-week seasonality in restaurant demand (OpenTable, 2025).

Several caveats are worth noting. The OpenTable data include only restaurants that use OpenTable and only reservations made through the platform. As a result, they exclude walk-in customers, reservations made through other platforms, and restaurants that do not rely on reservation systems. Similarly, the transportation outcomes are aggregate time series of trips or boardings. As a result, the analysis lacks a comparison group and therefore relies primarily on time-series variation for identification.[20] We therefore interpret these results as suggestive evidence on changes in overall mobility and economic activity rather than as causal estimates of the deployment on specific behavioral margins.

Given the lack of cross-sectional variation in the transportation data, we estimate a difference-in-differences specification that uses previous years as a control group. Specifically, we compare the change around August 2025 to the corresponding change around August in prior years (analogous to the final difference in the triple-difference design in Equation (1)). This accounts for recurring seasonality around August of every year. We estimate:

$$y_{mt} = \alpha_t + \delta_m + \beta\left(\text{PostAug}_m \times \text{Year2025}_t\right) + \varepsilon_{mt}, \tag{7}$$

[20] For two outcomes—rail and Metrobus—comparable Baltimore transit series are available. Appendix Table A10 reports robustness checks using a small-panel comparison between Washington, DC and Baltimore. These estimates should also be interpreted cautiously because the number of comparison units is small.

where y_{mt} is the log outcome in calendar month m and year t, PostAug_m is an indicator equal to one for August and later months, and Year2025_t is an indicator for 2025. The specification includes year fixed effects and month-of-year fixed effects. The coefficient β measures whether the post-August change in 2025 differs from the typical post-August change in prior years. This design is equivalent to a comparative interrupted time-series design in which earlier years serve as historical controls. We report heteroskedasticity- and autocorrelation-consistent standard errors with a six-month lag. We focus on the 2022–2025 period to avoid confounding from COVID-related disruptions to transportation patterns.

We begin by discussing the OpenTable results graphically. Figure 9 plots the 2025 versus 2024 percentage change in seated diners for DC and outside DC over the 30 days before and after the August 11, 2025 deployment. Restaurant activity in Washington, DC declines sharply relative to the rest of the country immediately after the deployment. A simple difference-in-differences estimate (the average difference between the two series before and after the deployment) suggests a decline of approximately 17 percentage points in seated diners in DC. The effect is short-lived, however: by August 18, the DC trend returns to its pre-deployment range.[21] This pattern is consistent with a temporary reduction in local consumer activity following the deployment.

Table 6 presents estimates of Equation (7). Across the transportation outcomes, the estimates generally indicate declines in mobility after August 2025 relative to the same period in prior years. Metrorail and Metrobus ridership decline by approximately 17% and 15%, respectively (columns 1 and 2). Commuter rail and commuter bus ridership from the Maryland Transit Administration (MTA) into DC also decline by 12.7% and 8.8%, respectively (columns 3 and 4). Ride-sharing and taxi trips likewise decline in the post-deployment period relative to the same months in previous years (columns 5 and 6).[22] Taken together, the transportation and restaurant data suggest that the deployment temporarily reduced local economic activity, providing some support for the activity suppression mechanism.

7.3 Enforcement Interactions and Disruption

The increase in assaults with dangerous weapons (ADW) deserves special attention, as it is the only crime category that rises following the deployment. Two mechanisms may account for this pattern. First, increased contact between civilians and enforcement personnel may generate additional reported physical offenses through confrontations during enforcement activ-

[21] This rebound likely reflects the start of Washington, DC's Summer Restaurant Week on August 18, 2025, a citywide promotional event that may have boosted dining activity (Fischer, 2025).

[22] Appendix Figure A3 plots each outcome for 2025 alongside the comparison years (2022–2024).

ities, protests, or arrests. Second, the deployment may disrupt local crime dynamics, triggering conflict among criminal groups such as conflict over new territory. These mechanisms imply different empirical predictions. If the increase reflects criminal disruption, we would expect a rise in firearm-related violence. In contrast, if enforcement interactions are the primary mechanism, increases should be concentrated in non-firearm assaults.

To distinguish between these explanations, we estimate our triple-difference specification separately for firearm and non-firearm ADW incidents. Table A11 presents the results.[23] We find that the estimated increase in ADW is driven entirely by non-firearm assaults in deployed areas, while firearm assaults remain unchanged. This pattern is more consistent with the enforcement interaction mechanism.

The deployment may affect crime dynamics in ways not fully captured by police reports. To examine this possibility, we use ShotSpotter data from the DC Open Data Portal.[24] Because gunfire associated with criminal activity may not always result in reported offenses, ShotSpotter provides a complementary measure of underlying gun activity independent of police reporting. Using geocoded ShotSpotter alerts, we estimate our triple-difference specification with the number of detected shots in each PSA-week as the outcome. Table A12 shows a statistically significant increase in detected gunfire following the deployment.

We complement this analysis with data on firearm injury emergency department visits from DC Health's DC-FASTER surveillance system (DC Health, 2026). Because these data are available at the ward-month level, we estimate our triple-difference specification using a ward-month panel. Table A13 presents the results. Despite the increase in detected gunfire in Table A12, we find no corresponding increase in gunshot-related injuries. This evidence suggests that both mechanisms are theoretically plausible, but the overall pattern is more consistent with the enforcement interaction channel. The increase in ADW is concentrated entirely in non-firearm incidents, while firearm-related assaults remain unchanged. Although we detect more gunfire following the deployment, the absence of a corresponding increase in firearm-related injuries suggests that this additional gunfire likely reflects demonstrative or expressive violence—such as warning shots or signaling among criminal groups—rather than an increase in severe violent crime.

Taken together, the evidence suggests that the deployment affected crime through multiple channels. The reductions in opportunistic crimes are consistent with both deterrence and ac-

[23] This analysis is limited to Washington, DC, Baltimore, and Montgomery County, as these jurisdictions are the only ones in our sample that report weapon type.

[24] ShotSpotter is an acoustic detection system that uses a network of sensors to identify and geolocate gunfire in real time. We obtain these data from the DC Open Data Portal. Available at: https://opendata.dc.gov/datasets/DCGIS::shot-spotter-gun-shots/about.

tivity suppression effects, while the increase in assaults with dangerous weapons appears more consistent with increased enforcement interactions than with an escalation in violent crime due to disruptions. More broadly, the findings highlight that large-scale military deployments may generate a broader set of behavioral and social responses than conventional policing interventions.

8 Cost-Benefit Analysis

We conduct a benefit–cost analysis over a 30-day window that aligns with how deployments are typically authorized. To calculate the benefits of crime reductions, we rely on the DDD coefficients for treated and spillover PSAs (Table 4). We monetize these benefits using contingent-valuation (CV) values from Cohen et al. (2004) as compiled in Chalfin (2016).[25] All values are adjusted to 2025 USD (from the 2010 USD used in Chalfin (2016)) using the August 2025 CPI index.

We adopt contingent valuation (CV) as our preferred approach to valuing crime costs because it provides an *ex ante*, preference-based measure of willingness to pay to reduce victimization risk, thereby capturing both tangible and intangible costs.[26] The literature also recommends CV for policy formulation under uncertainty (Chalfin, 2016). Moreover, because CV generally yields higher valuations than accounting-based ("bottom-up") estimates, our calculations should be interpreted as an upper bound on the policy's total social benefits (Miller et al., 2021).

Deployment Benefits Let $N_T = 25$ and $N_S = 32$ denote the number of treated and spillover PSAs, respectively. Let $\hat{\beta}_{T,c}$ and $\hat{\beta}_{S,c}$ denote the DDD coefficients (per PSA–week) for crime category $c \in \{\text{Robbery}, \text{Theft}, \text{MVT}\}$ estimated in treated and spillover PSAs, respectively (estimates obtained from Table 4).

We focus on robberies, motor vehicle thefts, and thefts, which exhibit consistent declines across specifications. Assault with a dangerous weapon also shows statistically significant effects when using external controls; however, we estimate an increase in these incidents following deployment. Given that this increase appears to be driven by enforcement-related interactions rather than conventional criminal activity, we exclude assaults from our baseline benefit calcu-

[25] We rely on Cohen et al. (2004) because their estimates tend to yield higher monetary valuations of averted crimes than comparable CV studies (e.g., Cohen and Piquero, 2009) or more recent "bottom-up" incidence-based estimates (e.g., Miller et al., 2021). Therefore, our benefit estimates should be interpreted as upper bounds on the potential social gains from crime reduction.

[26] In contrast, "bottom-up" methods tabulate tangible, ex post resource costs (e.g., medical and mental-health spending, lost wages, property losses, and criminal-justice expenditures for policing, courts, and corrections). Intangible harms (pain, suffering, diminished quality of life) are not directly observed in bottom-up tallies and are typically proxied via jury-award–based valuations.

lations. Including assaults provides a more conservative lower bound on the welfare effects of deployment since it mechanically reduces estimated benefits. Nonetheless, we provide a discussion when including assaults at the end of the section. Let V_c denote the CV dollar value per incident (in 2025 USD). The total 30-day benefit for crime c is:

$$\text{Benefit}_c = \left(\hat{\beta}_{T,c}\, N_T + \hat{\beta}_{S,c}\, N_S\right) \times \frac{30}{7} \times V_c. \tag{8}$$

Column 4 of Table 7 reports the estimates of total benefits Benefit_c for each crime type along with the coefficient estimates $\hat{\beta}_{T,c}$, $\hat{\beta}_{S,c}$ (columns 1 and 2), and the CV value per incident V_c (column 3) used in the calculation. Using $V_{\text{rob}} = \$133{,}426$ per avoided robbery, $V_{\text{theft}} = \$1{,}229$ per avoided theft, and $V_{\text{mvt}} = \$8{,}121$ per avoided motor-vehicle theft (MVT), the implied 30-day benefits are \$4.39 million for robbery, \$0.35 million for theft, and \$1.71 million for MVT, for a total net benefit of \$6.45 million (2025 USD).

Deployment costs We compute deployment costs by separately estimating expenditures for the DC–based National Guard and for out-of-state Guard units. For the DC Guard, we rely on the projected total cost of \$201 million reported in Mayes-Osterman (2025) for the period August 11 to November 30 (111 days). Scaling this figure to a 30-day window implies an estimated cost of approximately \$54 million ($201/111 \times 30$).

For out-of-state personnel, we begin with the per–Guard-day cost of \$530 reported for the 2020 DC deployment (Ali and Brice, 2020) and inflate it to 2025 dollars using the CPI factor of 1.252, yielding \$664 per Guard-day. Consistent with prior practice, we apply a 33% overhead to account for mobilization and demobilization time, so that 30 calendar days correspond to roughly 40 effective payroll days (Shumway, 2025). We estimate that 1,342 of the 2,300 deployed personnel were out-of-state Guard members (2,300 total minus 958 from the DC Guard) (Babb, 2025; Mayes-Osterman, 2025). Multiplying personnel, per-day cost, and effective days produces an estimated cost of roughly \$36 million. Combining the DC and out-of-state components yields a total 30-day deployment cost of approximately \$90 million, reported in Column 5 of Table 7.

Cost–benefit assessment Over a 30-day window, the crime-reduction benefits in Table 7 sum to \$6.45 million, while deployment costs amount to \$89.86 million. The implied benefit–cost ratio is around 0.07, meaning that each dollar of spending generated about seven cents in social benefits. Put differently, total costs exceeded benefits by more than a factor of ten. Since we are using contingent-valuation estimates to value avoided crime; these totals should be interpreted as an upper bound on benefits; alternative methods (e.g., bottom-up valuations) can further widen the gap. Moreover, the deployment cost figures are conservative: they exclude losses borne by local businesses, reduced economic activity due to fear and disruption, and the mental-health costs imposed on residents by the presence of armed military personnel (Chapin, Chapin; Horton, Horton). Incorporating these additional social costs would further exacerbate the net welfare loss associated with the intervention.

If we include assault with a dangerous weapon, the gap between benefits and costs becomes even starker due to the relatively high CV estimates for assaults. Using a value of \$233,039 per avoided assault, the implied 30-day effect for this category is negative, reflecting the estimated increase in assaults following deployment. Incorporating assaults reduces total crime-reduction benefits from \$6.45 million to \$1.73 million.[27] The implied benefit–cost ratio is therefore 0.02 (1.72/89.86), meaning that each dollar of spending generated approximately two cents in social benefits. Total costs exceeded benefits by a factor of more than fifty.

8.1 Comparison with a Community-Based Intervention

As a benchmark, we compare our estimates to benefits implied by Chicago's *Safe Passage Program* (SPP). The SPP is a community monitoring initiative launched by CPS in 2010/11 that assigns community monitors to designated street routes around schools during arrival and dismissal times. The program's primary objective is to deter crime near public schools through visible presence and reporting of suspicious activity. Community monitors are stationed along designated "safe passage" routes on the streets, sidewalks, and grounds surrounding participating schools, typically positioned on street corners with broad sight lines to maximize visibility. They wear brightly colored vests identifying them as Safe Passage personnel The program is implemented through partnerships with local nonprofit organizations contracted by CPS to manage community monitoring activities. They recruit, employ, and supervise Safe Passage monitors—typically residents of the local community—who work between five and six hours per day at a wage of roughly \$10 per hour. Monitors receive training on first aid, CPR, and conflict de-escalation, and follow standardized protocols for observing and reporting criminal activity. They are equipped with CPS-provided cell phones or two-way radios that allow direct communication with Safe Passage supervisors, school officials, and emergency services (Zubrzycki, 2013; Schools, 2017).

The SPP shares with the DC deployment the same overarching objective of reducing street-level crime through heightened visibility but differs in implementation and scale: while the

[27] We compute benefits using equation (8). For assaults, $\hat{\beta}_{T,\text{assault}} = 0.106$ and $\hat{\beta}_{S,\text{assault}} = 0.065$, with $N_T = 25$ and $N_S = 32$. This yields $(0.106 \times 25 + 0.065 \times 32) \times (30/7) \times 233{,}039 \approx -\$4.72$ million, where the negative sign reflects the increase in assaults.

DC initiative relied on federal law enforcement and National Guard personnel for a short-term emergency deployment, the SPP relies on community-based civilian monitors who operate continuously in violence-prone areas of Chicago. In this sense, the SPP more closely resembles hotspot policing strategies that concentrate deterrence efforts in persistent high-crime areas.

We replicate the DC valuation approach for Chicago using the 2025 USD contingent valuations above and the same crime categories. We rely on Gonzalez and Komisarow (2020) for estimates of averted robberies, thefts, and MVTs as a result of SPP. We assume the SPP operates over 176 school days (i.e., $176/30 = 5.87$ effective months).[28] Gonzalez and Komisarow (2020) estimate SPP averted 207 robberies, 465 motor vehicle thefts (MVTs), while increasing thefts by 396 due to theft crime displacement.[29] Using CV values per averted crime in column 3 of Table 7, total school-year benefits are \$27.62M (robbery) −\$0.49M (theft), and \$3.78M (MVT). Normalizing by duration yields monthly benefits of \$4.71M (robbery), -\$0.08M (theft), and \$0.64M (MVT) for a combined \$5.27M per month (2025 USD).

Chicago's Safe Passage Program employed 955 civilian monitors at an annual cost of approximately \$20,275 per worker in 2025 dollars.[30] This implies a program cost of about \$19.36 million per school year. To maintain consistency with our benefit calculations expressed per 30 operating days, we convert the 176 school days into $176/30 = 5.87$ effective months. This yields a cost of \$3.30 million per 30 operating days.

The SPP's total monthly benefits of approximately \$5.27 million exceed its monthly operating costs of \$3.30 million for a benefit–cost ratio of roughly 1.6. In other words, every dollar spent on the community-based program generated about \$1.60 in social benefits under the same valuation framework.

Taken together, the two interventions reveal stark differences in cost effectiveness. The DC federal deployment generated an estimated \$6.45 million in social benefits per 30-day window at a cost of \$89.86 million, yielding a benefit–cost ratio of 0.07. In contrast, Chicago's community-based Safe Passage Program produced \$5.27 million in social benefits per 30 operating days at a cost of \$3.30 million, corresponding to a benefit–cost ratio of 1.60. Measured by benefits per

[28] This estimate is consistent with Illinois state law requiring a minimum of 176 instructional days per year for public schools (Illinois School Code, 105 ILCS 5/10–19.05). It is also the modal number of school days in Chicago Public Schools published school year calendars.

[29] The robbery and theft estimates are taken directly from Table 7 of Gonzalez and Komisarow (2020), which reports the total number of averted crimes within 2 miles of SPP blocks over the school year. The MVT estimate is based on the authors' calculations combining the block-level treatment effects for motor vehicle thefts reported in Table A5 of the same paper with the number of blocks by distance band reported in Table 7. Specifically, for each band $b \in \{\text{SPP}, 0.25, 0.50, 0.75, 1.00, 1.25, 1.50, 1.75, 2.00\}$, the number of averted MVTs is computed as $\hat{\beta}_b \times N_b$, where $\hat{\beta}_b$ is the coefficient for MVTs (Table A5) and N_b is the number of blocks in that band (Table 7). Summing across all bands yields an estimated 465 MVTs averted citywide during the school year.

[30] This figure converts the reported 2015 cost of \$14,833 per worker in Gonzalez and Komisarow (2020) to 2025 dollars using the CPI adjustment factor $\text{CPI}_{2025}/\text{CPI}_{2015} = 323.976/237.017 = 1.3669$.

dollar spent, the Chicago program was therefore about 23 times more cost effective than the DC deployment. While both initiatives share a deterrence objective, the SPP program achieved broadly comparable crime-reduction benefits at a fraction of the cost, highlighting the efficiency gains from localized, community-based monitoring relative to large-scale federal deployments.

9 Conclusion

This paper examines whether large-scale deployments of military personnel can reduce crime in urban settings. For this purpose, we provide the first causal evidence on the effects of the 2025 federal deployment of National Guard troops and federal agents in Washington, DC. Using high-frequency administrative crime data and a triple-differences design, we find that the intervention produced declines in robberies, thefts, and motor-vehicle thefts—offenses that are public, opportunistic, and highly visible—while other violent and property crimes show little meaningful response and assaults with dangerous weapons increase.

These results are consistent with a visibility-based deterrence mechanism, whereby uniformed presence alters offender behavior in public spaces but has little effect on private or relational forms of violence. More unique to military deployments, however, these findings are also consistent with an activity suppression mechanism whereby deployment reduces mobility and commercial activity and thus potential offender-victim interactions. While we cannot cleanly disentangle deterrence from activity suppression, we document evidence of reduced mobility and economic activity following the deployment.

Our findings also point to important limits in the cost-effectiveness of large-scale military deployments for crime control. The cost–benefit analysis underscores this inefficiency: estimated social benefits amount to roughly seven cents per dollar spent. To benchmark these figures, we compare the DC deployment to Chicago's community-based Safe Passage program—a long-running initiative that employs trained local residents to monitor school routes and deter street-level offenses through consistent, visible presence. Despite operating on a much smaller budget, the Safe Passage program achieves comparable reductions in robberies, thefts, and vehicle thefts at roughly 4% of the cost. While both interventions rely on visibility to discourage public and opportunistic crimes, the analysis illustrates that sustained, community-embedded models can deliver similar public-safety benefits with substantially lower fiscal and institutional costs than short-term military deployments.

We note several limitations with this study. First, the analysis focuses on a restricted set of index offenses. These represent the only categories publicly reported by the DC Metropolitan Police Department. As a result, we cannot assess whether deployments affected other types of crimes such as drug offenses, or quality-of-life violations. This limitation implies that the estimated effects may not capture broader behavioral responses to the deployment.

A related concern is that with more personnel on the street, the likelihood that offenses are observed and officially recorded may increase even if the underlying incidence of crime decreases. This can bias coefficients toward zero. Although this is a plausible argument, we believe this explanation is unlikely to account for some of the null effects. National victimization surveys show that aggravated assaults and robberies have very similar reporting rates—around 60 percent (Thompson and Tapp, 2022)—yet we find a decline only in robberies. If reporting bias were driving the results, we would expect both categories to respond similarly since both occur as frequently and have near identical reporting rates. Moreover, the effect of deployment on reporting behavior can be theoretically ambiguous: while greater police presence increases the probability that officers detect or record offenses directly, it may also reduce the likelihood that citizens report incidents if they perceive police to be already aware or nearby.

Another point relates to the relatively short post-treatment window in our analysis. Federal or National Guard deployments of this type are typically short-lived, often lasting between 30 and 60 days (Shumway, 2025), and in the case of Washington, DC, the 2025 deployment was formally authorized for 30 days, although troops remained deployed beyond that time frame (Mitchell, 2025). Our research design is therefore intentionally focused on identifying the contemporaneous impact of the deployment, rather than long-term or persistent effects. While this limits our ability to assess whether the observed changes in crime endure after the withdrawal of federal personnel, it aligns with the policy's short-term deterrence objective and captures the immediate behavioral response such interventions are intended to generate.

Although this paper focuses on Washington, DC, our insights can extend to similar deployments recently implemented in other U.S. cities. In 2025, the National Guard was formally deployed to Los Angeles (Rodriguez, 2025), Memphis (Sainz, 2025), Portland (Raymond, 2025), and Chicago (NewsHour, 2025). Because these deployments often apply a similar rationale and tactics–proactive and visible military deployment–our DC case offers insight into their likely mechanisms and effects elsewhere.

To conclude, the 2025 DC deployment highlights both the potential and the limits of large-scale federal interventions for urban crime control. While highly visible deployments may reduce certain opportunistic street crimes, their effects appear concentrated in offenses most responsive to public visibility and disruption. At the same time, the substantial fiscal costs and increase in assaults with dangerous weapons underscore that such interventions can generate important tradeoffs. Taken together, the findings suggest that visibility alone is unlikely to produce broad-based reductions in crime absent more targeted, locally embedded, and sustained public-safety strategies.

References

ACLU of the District of Columbia (2025, August). Trump seizes d.c. under false pretenses. Accessed: 2025-08-21.

Ali, I. and M. Brice (2020). What was the cost for the national guard to deploy in D.C.? up to $2.6 million a day. *Reuters*. U.S. officials estimated about $530 per Guard member per day; peak cost about $2.65M/day.

Arkhangelsky, D., S. Athey, D. A. Hirshberg, G. W. Imbens, and S. Wager (2021). Synthetic difference-in-differences. *American Economic Review 111*(12), 4088–4118.

Babb, C. (2025). Troops in dc encounter few crises, but plenty of walking and yard work. *Military Times*.

Becker, G. S. (1968). Crime and punishment: An economic approach. *Journal of Political Economy 76*(2), 169–217.

Blair, R. A. and M. Weintraub (2023). Little evidence that military policing reduces crime or improves human security. *Nature Human Behaviour 7*(6), 861–873.

Bove, V. and E. Gavrilova (2017, August). Police officer on the frontline or a soldier? the effect of police militarization on crime. *American Economic Journal: Economic Policy 9*(3), 1–18.

Braga, A. A., B. S. Turchan, A. V. Papachristos, and D. M. Hureau (2019). Hot spots policing and crime reduction: An update of an ongoing systematic review and meta-analysis. *Journal of Experimental Criminology 15*(3), 289–311.

Braga, A. A. and D. L. Weisburd (2020). Does hot spots policing have meaningful impacts on crime? findings from an alternative approach to estimating effect sizes from place-based program evaluations. *Journal of Quantitative Criminology 38*(1), 1–22.

Brennan Center for Justice (2025, August). One week into trump's d.c. takeover attempt. Accessed: 2025-08-21.

Bullock, K., A. Sidebottom, R. Armitage, M. P. Ashby, C. Clemmow, S. Kirby, G. Laycock, and N. Tilley (2021). Problem-oriented policing in england and wales: Barriers and facilitators. *Policing and Society 32*(9), 1087–1102.

Chalfin, A. (2016). Economic costs of crime. In W. G. Jennings (Ed.), *The Encyclopedia of Crime and Punishment* (First ed.). John Wiley & Sons, Inc.

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

Chapin, J. Downtown dc businesses hope for increased foot traffic after end to federal takeover. Quotes DowntownDC BID on major drop in foot traffic; mixed consumer sentiment with troops present.

Cohen, M. A. and A. R. Piquero (2009). New evidence on the monetary value of saving a high risk youth. *Journal of Quantitative Criminology 25*, 25–49.

Cohen, M. A., R. T. Rust, S. Steen, and S. T. Tidd (2004). Willingness-to-pay for crime control programs. *Criminology 42*(1), 89–110.

Crumley, B. (2025). D.c. business owners say they're losing customers as troops patrol the streets. https://www.inc.com/bruce-crumley/d-c-business-owners-say-theyre-losing-customers-as-troops-patrol-the-streets/91231303. Accessed: 2026-04-24.

Dale, D. (2025, August). Fact check: Violent crime in dc has fallen in 2024 and 2025 after a 2023 spike. CNN.

DC Health (2026). Dc firearm injury surveillance dashboard (dc-faster). https://dchealth.dc.gov/dc_faster. Accessed May 20, 2026.

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

District of Columbia Department of For-Hire Vehicles (2026). Dfhv dashboard and statistical data sets. https://dfhv.dc.gov/page/dfhv-dashboard-and-statistical-data-sets. Accessed: May 19, 2026.

District of Columbia Metropolitan Police Department (2025). District of columbia crime incidents dataset. https://opendata.dc.gov/datasets/74d924ddc3374e3b977e6f002478cb9b_7. Accessed August 2025.

District of Columbia Office of the Chief Technology Officer (2020). 2020 census blocks with population. https://opendata.dc.gov/datasets/a6f76663621548e1a039798784b64f10_0/explore. Accessed: September 23, 2025.

Ducharme, J. (2025, August). White house backs off 'hostile takeover' of d.c. police after lawsuit. *Time*. Accessed: 2025-08-21.

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

Federal Transit Administration (2026). Complete monthly ridership with adjustments and estimates. https://www.transit.dot.gov/ntd/data-product/monthly-module-adjusted-data-release. Accessed: May 19, 2026.

Fischer, J. (2025). Dc restaurants see signs of support amid 'ongoing challenges' of federal takeover. https://www.wusa9.com/article/news/local/dc/restaurant-reservations-rally-as-dc-summer-restaurant-week-kicks-off-amid-federal-takeove 65-e83cc2e7-a921-401a-ba35-960c77f8da14. Accessed: 2026-05-15.

Forgey, Q. and E. Schor (2025, August). Supermajority of washington residents oppose trump's police takeover, poll finds. *Politico*. Accessed: 2025-08-21.

Gonzalez, R. and S. Komisarow (2020). Community monitoring and crime: Evidence from chicago's safe passage program. *Journal of Public Economics 191*, 104250.

Greenblatt, A. (2025, August). Trump federalizes d.c. police, declares public safety emergency. *Governing*. Accessed: 2025-08-21.

Gunderson, A., E. Cohen, K. J. Schiff, and T. S. Clark (2021). Counterevidence of crime-reduction effects from federal grants of military equipment to local police. *Nature Human Behaviour 5*(2), 194–204.

Harris, M. C., J. Park, D. J. Bruce, and M. N. Murray (2017, August). Peacekeeping force: Effects of providing tactical equipment to local law enforcement. *American Economic Journal: Economic Policy 9*(3), 291–313.

Hinkle, J. C., D. Weisburd, C. W. Telep, and K. Petersen (2024). When is problem-oriented policing most effective? a systematic examination of heterogeneity in effect sizes for reducing crime and disorder. *Policing: A Journal of Policy and Practice*.

Horton, A. National guard documents show public 'fear,' veterans' 'shame' over d.c. presence. *The Washington Post*. Internal assessments report majority negative public sentiment and concerns about mission legitimacy.

Horton, A., K. Elwood, and O. George (2025, August). Gop states pour national guard troops into d.c. as trump tightens control. *The Washington Post*. Accessed: 2025-08-21.

Klick, J. and A. Tabarrok (2007). Using terror alert levels to estimate the effect of police on crime. *Journal of Law and Economics 48*(1), 267–279.

Kubrin, C. E., S. F. Messner, G. Deane, K. McGeever, and T. D. Stucky (2010). Proactive policing and robbery rates across u.s. cities. *Criminology 48*(1), 57–97.

Levitz, E. (2025, August). Trump's federalization of d.c. police: Legal limits and risks. Accessed: 2025-08-21.

Loh, T. H. and G. Haskins (2025). Consumer spending and visitor demand in the washington, dc region are dropping. https://www.brookings.edu/articles/consumer-spending-and-visitor-demand-in-the-washington-dc-region-are-dropping/. Accessed: 2026-04-24.

Lowande, K. (2021). Police demilitarization and violent crime. *Nature Human Behaviour 5*(2), 262–269.

Masera, F. (2021). Police safety, killings by the police, and the militarization of us law enforcement. *Journal of Urban Economics 124*, 103365.

Mayes-Osterman, C. (2025, sep). Dc national guard deployment to cost 200$million, assoldierspickuptrash, blowleaves. USAToday. Accessed: 11-17-2025$.

Miller, T. R., M. A. Cohen, D. I. Swedler, B. Ali, and D. V. Hendrie (2021). Incidence and costs of personal and property crimes in the usa, 2017. *Journal of benefit-cost analysis 12*(1), 24–54.

Mitchell, E. (2025). Most states with national guard troops in d.c. plan to withdraw this fall. *The Hill*. The Hill, October 12, 2025. Accessed [insert date of access].

Mummolo, J. (2018). Militarization fails to enhance police safety or reduce crime but may harm police reputation. *Proceedings of the National Academy of Sciences 115*(37), 9181–9186.

NewsHour, P. (2025). What to know about trump's national guard deployments in chicago and portland.

Open Data DC (2026). Shotspotter gunshot detection alerts. https://opendata.dc.gov/search?q=shotspotter. Accessed May 20, 2026.

OpenTable (2025). State of the industry: Seated diners. https://www.opentable.com/c/state-of-industry/#seated-diners-chart. Accessed: 2026-05-15.

Raymond, N. (2025). Us judge extends block on trump deploying national guard in portland, oregon. *Reuters*.

Rodriguez, O. (2025). Judge rules trump administration broke law in deploying national guard soldiers to la this summer. Online; accessed TBD.

Sainz, A. (2025). National guard troops seen patrolling in memphis alongside local police. *PBS NewsHour*.

Sampson, R. J. and J. Cohen (1988). Deterrent effects of the police on crime: A replication and theoretical extension. *Law and Society Review 22*(1), 163–189.

Schools, C. P. (2017). Success starts here: Three year vision, 2016-2019. *Chicago Public Schools*.

Sherman, L. W. and D. Weisburd (1995). General deterrent effects of police patrol in crime "hot spots": A randomized, controlled trial. *Justice Quarterly 12*(4), 625–648.

Shumway, J. (2025). Paying national guard troops to deploy to portland would cost millions. State official estimated $3.8M pay for 200 Guard over 60 days; excludes meals, lodging, transportation; 80 payroll days including training/demob.

Smith, A. (2025). D.c. small businesses reel amid troop deployment. https://www.yahoo.com/news/articles/dc-small-businesses-reel-amid-193000424.html. Accessed: 2026-04-24.

SoundThinking, Inc. (2026). Shotspotter. https://www.soundthinking.com/law-enforcement/shotspotter/. Accessed May 20, 2026.

Taylor, B. G., W. Liu, P. Maitra, C. S. Koper, J. Sheridan, and W. Johnson (2022). The effects of community-infused problem-oriented policing in crime hot spots based on police data: A randomized controlled trial. *Journal of Experimental Criminology 20*(2), 317–345.

Thompson, A. and S. N. Tapp (2022). Criminal victimization, 2021. *NCJ 305101*(1).

U.S. Census Bureau (2020). Tiger/line shapefiles: 2020 census blocks. https://www.census.gov/cgi-bin/geo/shapefiles/index.php?year=2020&layergroup=Blocks+%282020%29. Accessed: September 23, 2025.

US Inflation Calculator (2025). Consumer price index data from 1913 to 2025. Aggregates CPI-U data from the U.S. Bureau of Labor Statistics.

Washington Post Staff (2025, August). Where the national guard and federal agents are patrolling in d.c. *The Washington Post*. Accessed: 2025-08-21.

Wilson, J. Q. and B. Boland (1978). The effect of the police on crime. *Law and Society Review 12*(3), 367–390.

Zhao, J. S. and Y. Zhang (2022). Proactive policing embedded in two models: A geospatial analysis of proactive activities by patrol officers and cop officers. *Journal of Criminal Justice 82*, 101972.

Zubrzycki, J. (2013). Chicago school opening tests new ‘safe passage’routes. *Education Week 30*.

Tables and Figures

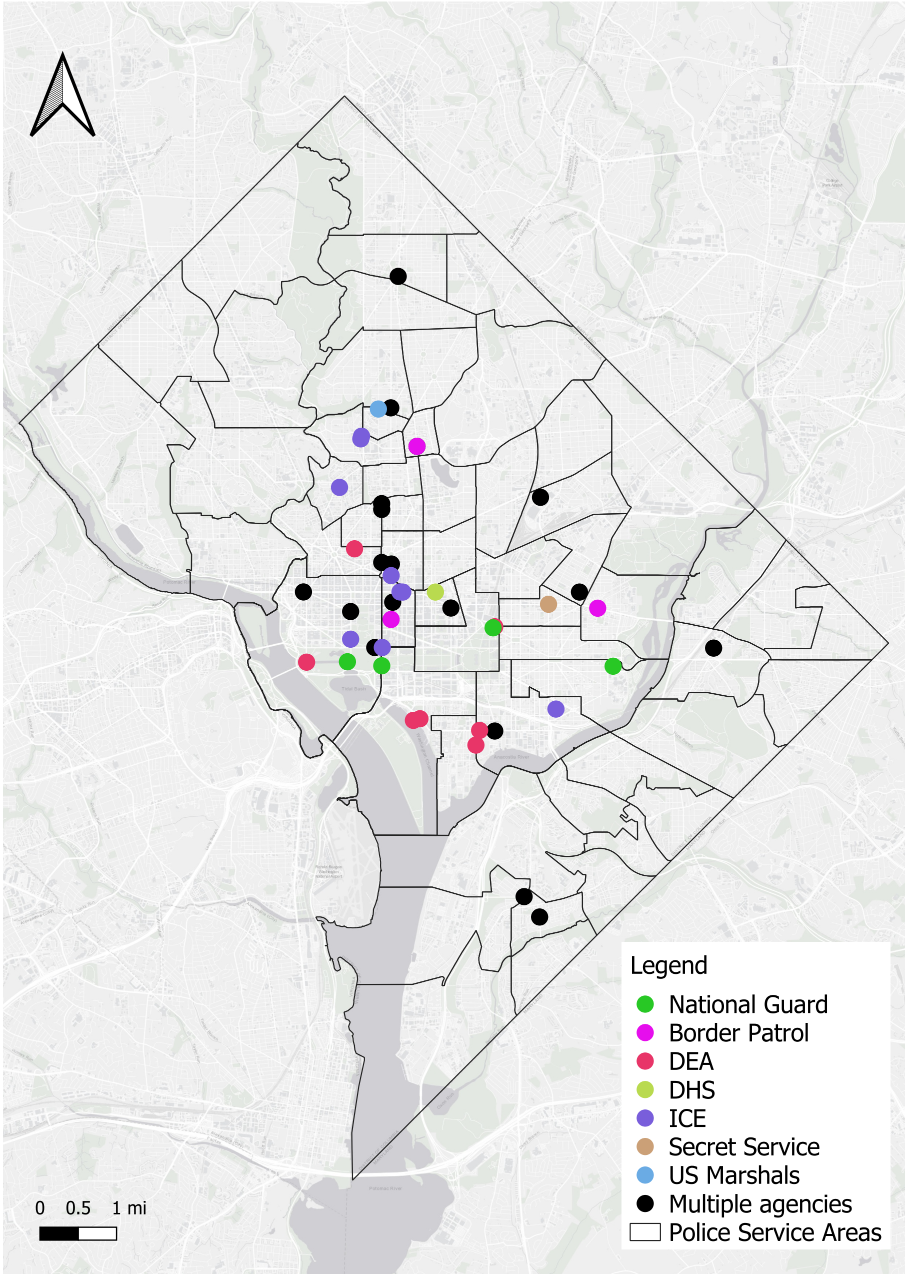


Figure 1: Reported Location of Troops and other Deployed Agencies

Notes: Reported location of troops and other deployed agencies. Source: (Washington Post Staff, 2025).

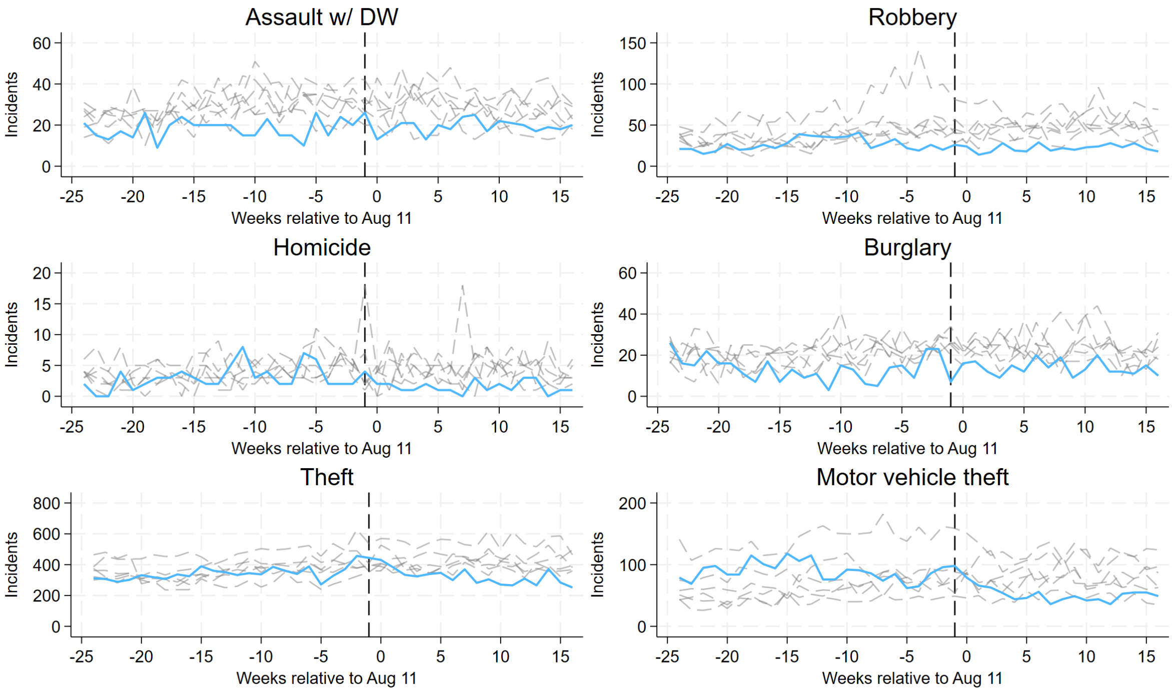
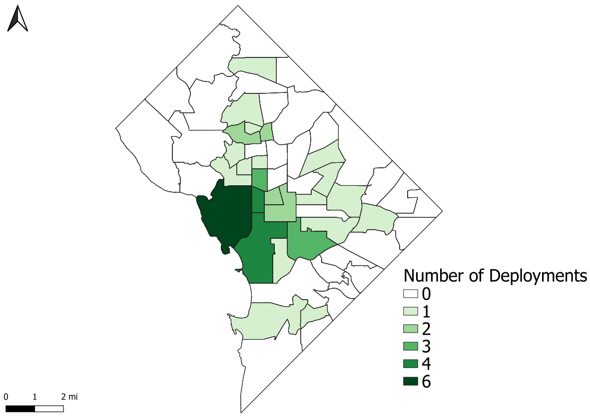
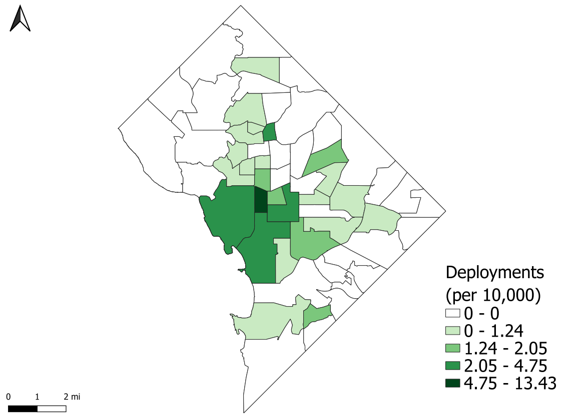


Figure 2: Weekly Crime Trends, 2019-2025, 2025 Highlighted

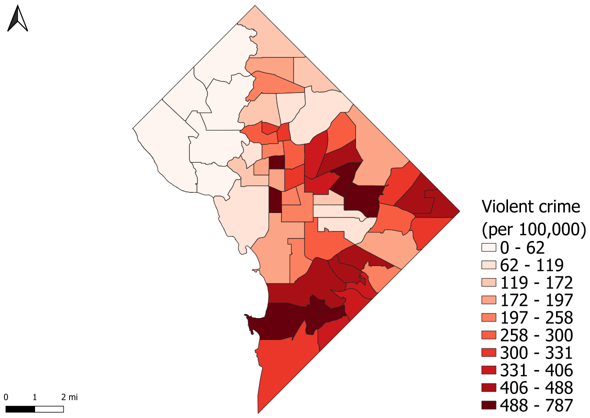
Notes: Weekly citywide crime incidents by offense category. Blue line depicts year 2025. Dashed lines depict years 2019-2024. Sample restricted to a 24-week window around deployment cutoff. The dashed vertical line marks the week prior to August 11, 2025 (National Guard deployment). Violent crime includes assault with a dangerous weapon, robbery, sex abuse, and homicide. Property crime includes burglary, arson, theft (auto and other), and motor vehicle theft. Theft combines thefts from autos and other thefts. Arson and sex abuse are excluded due to low frequency. Data from the Metropolitan Police Department (District of Columbia Metropolitan Police Department, 2025).



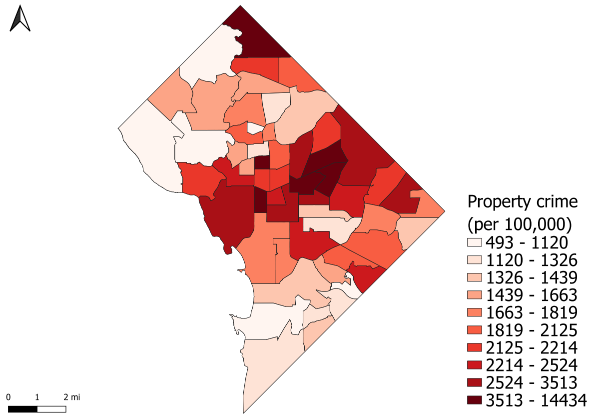
(a) PSA deployment



(b) PSA deployment rate



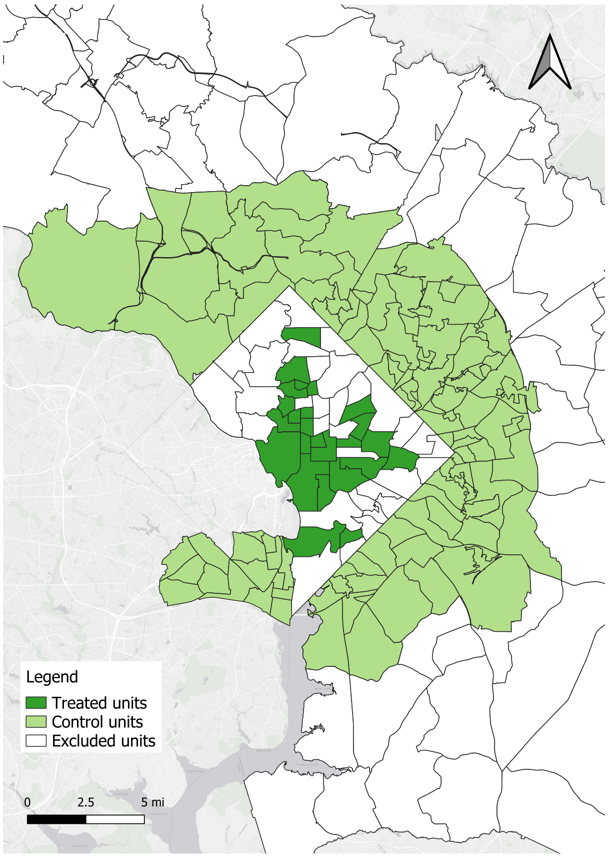
(c) Violent crimes



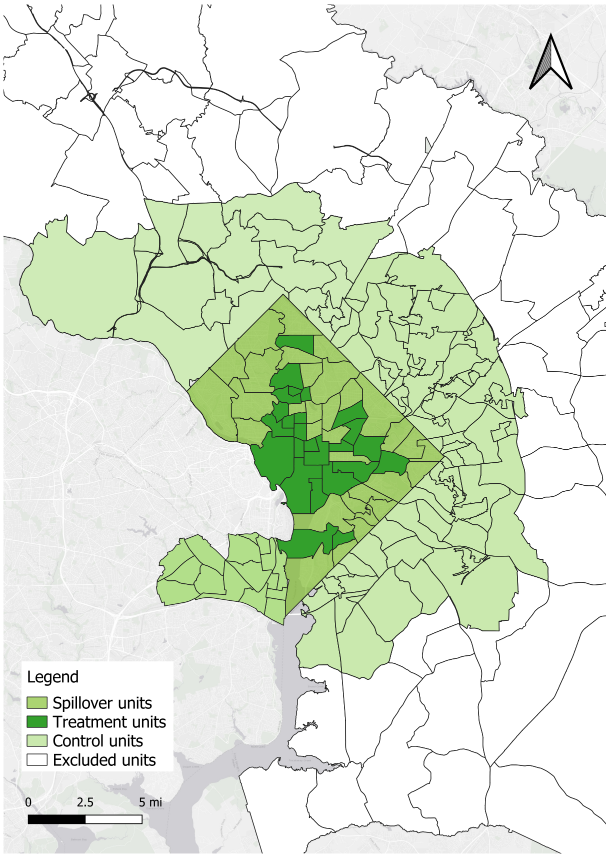
(d) Property crimes

Figure 3: Agency Deployment and Crime Rates across PSAs

Notes: Refer to Figure 1 for list of agencies. Violent crime includes assault with a dangerous weapon, robbery, sex abuse, and homicide. Property crime includes burglary, theft, arson, and motor vehicle theft. Crime figures use crime in 2025 prior to deployment date Aug. 11, 2025. Crime rates are calculated per 100,000 population in the PSA. Analysis uses population data from 2020 Census.



(a) Reported deployment



(b) Spillovers

Figure 4: Treatment and Control Unit Definitions

Notes: Control units created using police beats from Montgomery County, Prince George's County, and City of Alexandria. Control units restricted to beats in districts adjacent to DC for Montgomery County, Prince George's County. Crime data from Arlington county not available for study period. Panel A defines treatment as all PSAs in DC with reported deployment by Washington Post Staff (2025). Panel B defines three groups: treatment PSAs, control PSAs *without* any reported deployment in order to perform spillover estimations, and control units outside DC.

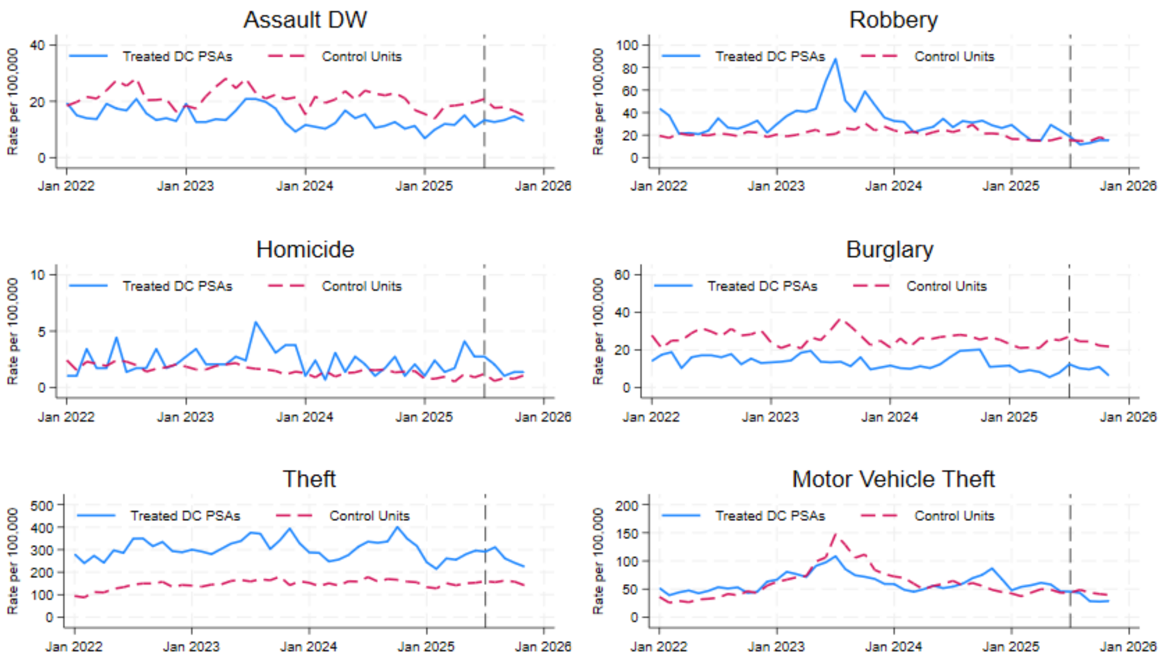
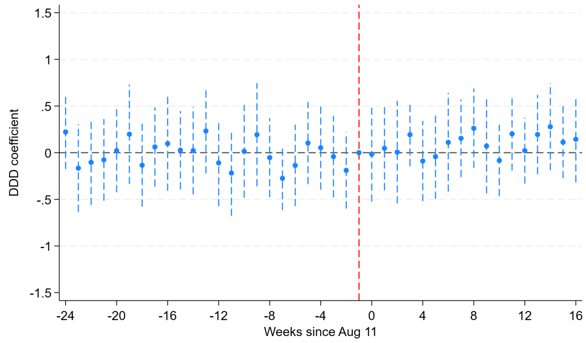
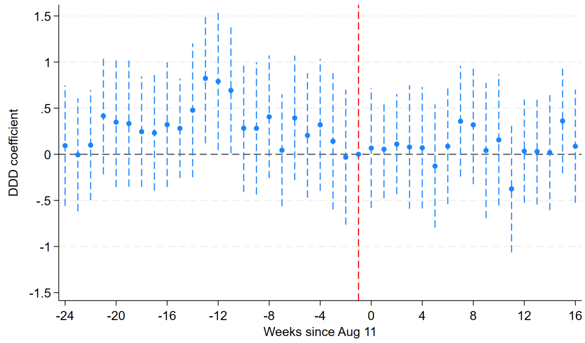


Figure 5: Monthly Crime Trends by Treatment Status (2022-2025)

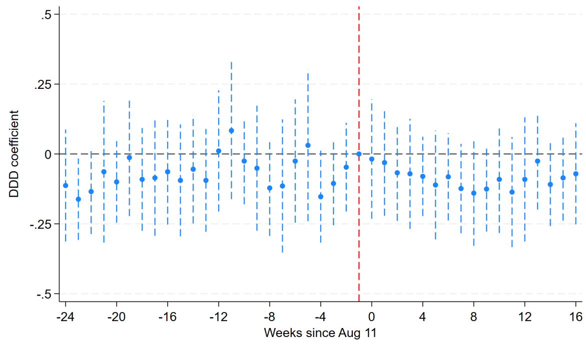
Notes: Monthly crime rate by offense category and treatment status. Treatment status refers to whether unit reports a deployment (PSAs in DC with reported deployment) and control units. Control units created using police beats from Montgomery County, Prince George's County, City of Alexandria, and City of Baltimore. Control units restricted to beats in districts adjacent to DC for Montgomery County, Prince George's County. Crime rates in each group calculated as the total incidents in the group divided by total population in the group per 100,000 population. Analysis uses population data from 2020 Census. The dashed vertical line marks the month prior to August 11, 2025 (National Guard deployment). Theft combines thefts from autos and other thefts. Arson and Sex abuse is excluded due to low frequency. Data from the Metropolitan Police Department (District of Columbia Metropolitan Police Department, 2025).



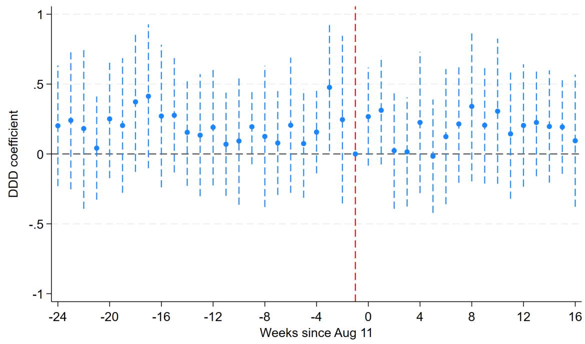
(a) Assault with a dangerous weapon



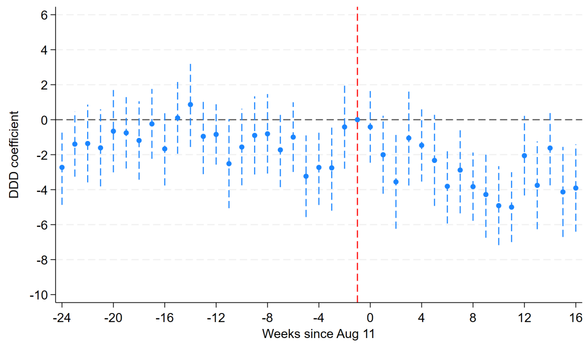
(b) Robbery



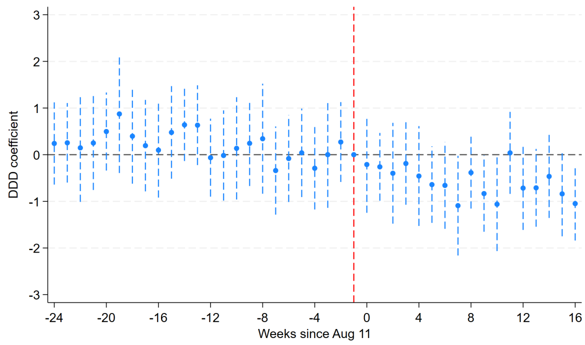
(c) Homicide



(d) Burglary



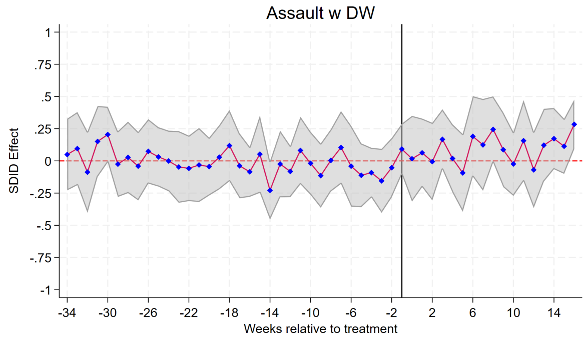
(e) Theft



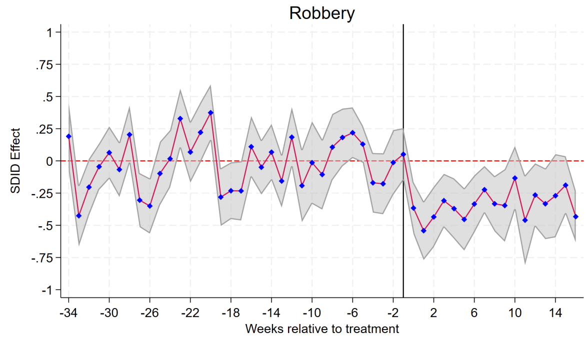
(f) Motor vehicle theft

Figure 6: The Effect of Deployment on Crime, Triple Differences (DDD) Event Study plots, non-DC controls

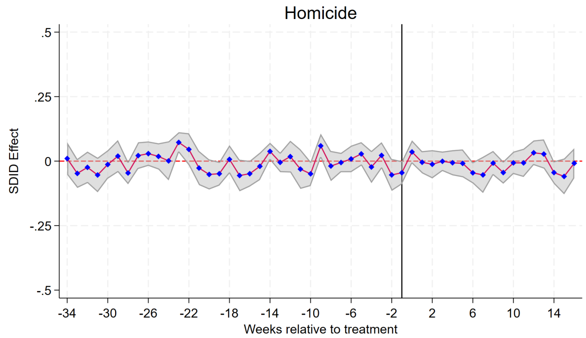
Notes: Each dot gives the coefficient estimates β_k from Equation (3). Dashed lines show 95% confidence intervals. Week of August 10 is the reference category. Vertical line marks reference date. All models include unit×calendar week, unit×year, and calendar week×year fixed effects. Control units created using police beats from Montgomery County, Prince George's County, City of Baltimore, and City of Alexandria. Control units restricted to beats in districts adjacent to DC for Montgomery County and Prince George's County. Crime data from Arlington county not available for study period. Unit refers to a PSA in DC, a police post in Baltimore, and a police beat in remaining control jurisdictions. Control mean refers to mean in control units, before Aug. 11. Analysis from January 2022 through December 2025. Year 2023 omitted from analysis. Refer to Appendix B for a description of outcome creation and harmonization. Standard errors are clustered at the unit level.



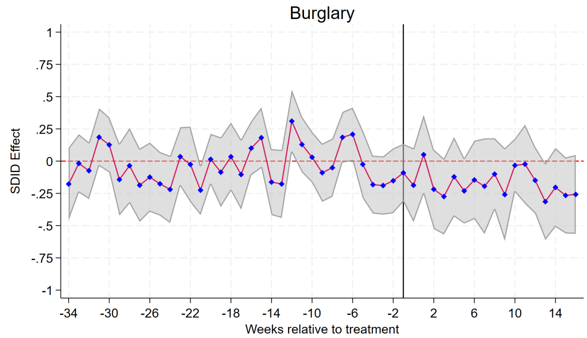
(a) Assault with DW



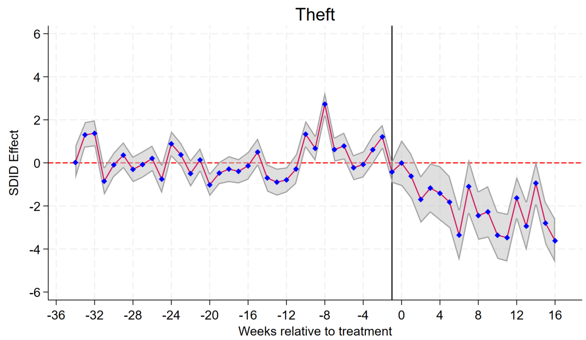
(b) Robbery



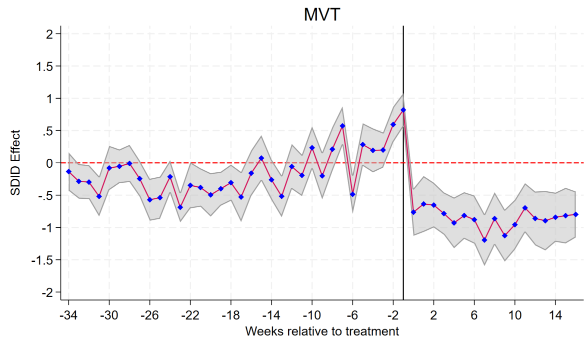
(c) Homicide



(d) Burglaries



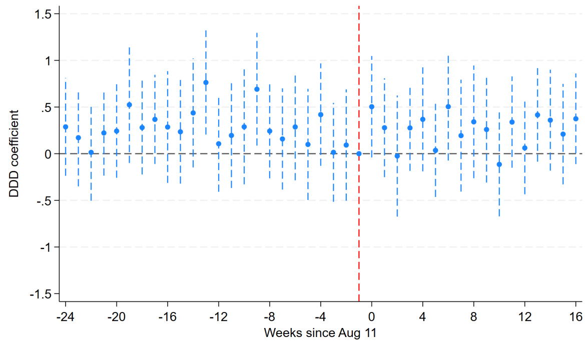
(e) Theft



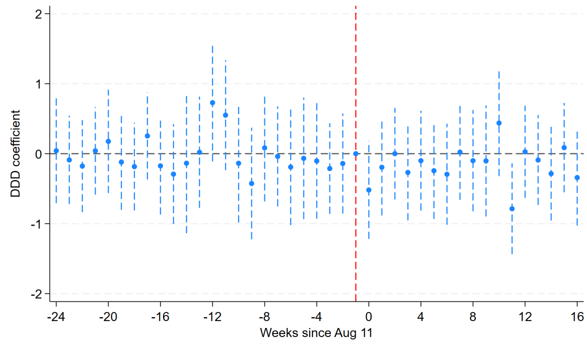
(f) Motor Vehicle Theft

Figure 7: The Effect of Deployment on Crime, Synthetic DID, non-DC controls

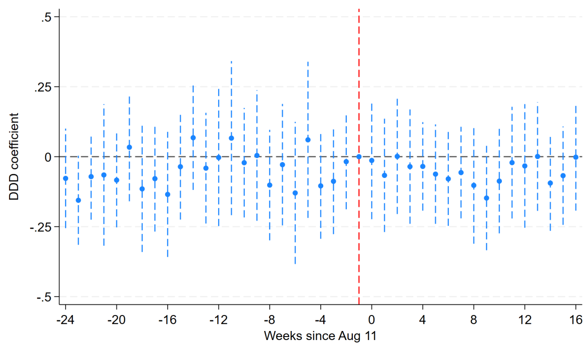
Notes: This figure reports synthetic difference-in-differences (SDID) estimates of the effect of the August 11, 2025 deployment on weekly crime counts. The treated units are PSAs in Washington, D.C., while the donor pool consists of jurisdictions in Montgomery County, Prince George's County, and the cities of Alexandria and Baltimore. Sample is restricted to weeks following August 11 in each year. Post-August 11 weeks from earlier years serve as the pre-treatment period, while post-August 11 weeks in 2025 constitute the treatment period. Treatment variable *Deployed* $\times$ *Post* equals one for deployed D.C. PSAs in 2025 and zero otherwise. Standard errors are computed using placebo-based inference with 500 replications following Arkhangelsky et al. (2021).



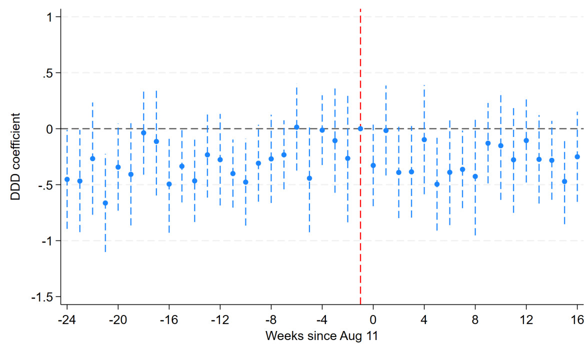
(a) Assault with a dangerous weapon



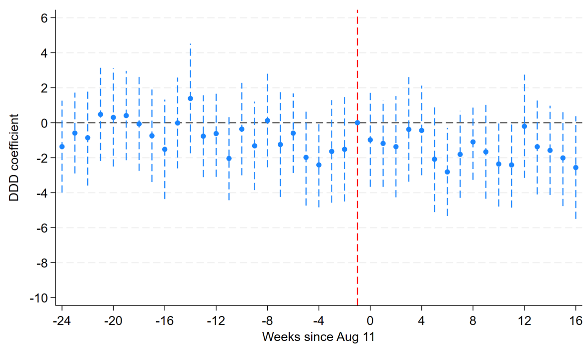
(b) Robbery



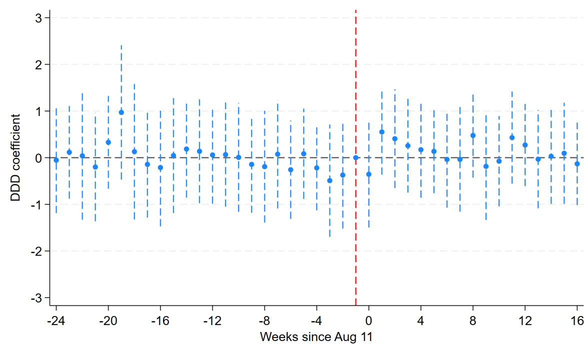
(c) Homicide



(d) Burglary



(e) Theft



(f) Motor vehicle theft

Figure 8: The Effect of Deployment on Crime, Triple Differences (DDD) Event Study plots, within-DC Analysis

Notes: Each dot gives the coefficient estimates β_k from Equation (3). Dashed lines show 95% confidence intervals. Sample is at the PSA $\times$ week level. Week of August 10 is the reference category. Vertical line marks reference date. Estimates include PSA and week fixed effects. Violent crime includes assault with a dangerous weapon, robbery, sex abuse, and homicide. Property crime includes burglary, theft, arson, and motor vehicle theft. Theft combines thefts from autos and other thefts. Results for Arson and Sex abuse excluded due to low frequency.

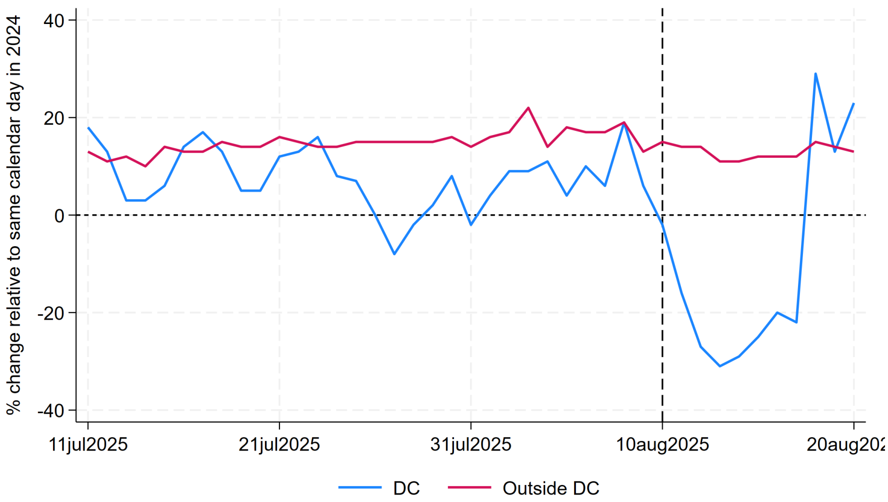


Figure 9: Daily Percent Change in Seated Diners in 2025 Relative to 2024

Notes: Trend lines plot the percentage change in seated diners in 2025 relative to the same calendar day in 2024 from OpenTable's State of the Industry dataset for Washington, DC and for other metros in the US excluding Washington, DC (OpenTable, 2025). Any metro with more than 50 restaurants in the OpenTable network is included in the dataset. A seated diner is a customer who made a reservation through OpenTable and was subsequently seated at the restaurant. OpenTable compares each day to the same day of the week in the corresponding week of the previous year. The sample covers the 30 days before the deployment date (August 11, 2025). The vertical dashed line marks the day before deployment.

Table 1: The Effect of Deployment on Crime, DDD Design, non-DC controls

	(1) Assault	(2) Robbery	(3) Homicide	(4) Burglary	(5) Theft	(6) MVT
Post × 2025 × Deployed	0.103**	-0.219**	-0.020	-0.013	-1.747***	-0.790***
	(0.047)	(0.096)	(0.020)	(0.061)	(0.617)	(0.186)
Control mean	0.375	0.282	0.022	0.349	1.845	0.587
Observations	36777	36777	36777	36777	36777	36777
Clusters	299	299	299	299	299	299
Unit×WOY FE	Yes	Yes	Yes	Yes	Yes	Yes
Unit×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Year×WOY FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates of β from Equation (1). Sample is at the unit × week level. All models include unit×calendar week, unit×year, and calendar week×year fixed effects. Control units created using police beats from Montgomery County, Prince George's County, City of Baltimore, and City of Alexandria. Control units restricted to beats in districts adjacent to DC for Montgomery County and Prince George's County. Unit refers to a PSA in DC, a police post in Baltimore, and a police beat in remaining control jurisdictions. Control means refers to mean in control units, before Aug. 11. Analysis from January 2022 through December 2025. Year 2023 omitted from analysis. Refer to Appendix B for a description of outcome creation and harmonization. Standard errors are clustered at the unit level.
* p<.1, ** p<.05, *** p<.01

Table 2: The Effect of Deployment on Crime, Synthetic Difference-in-Differences Design, Non-DC controls

	(1) Assault w/ DW	(2) Robbery	(3) Homicide	(4) Burglary	(5) Theft	(6) MVT
Deployed × Post	0.092	-0.341***	-0.012	-0.172*	-2.039***	-0.854***
	(0.074)	(0.071)	(0.016)	(0.089)	(0.451)	(0.128)
Control mean	0.358	0.300	0.021	0.367	2.050	0.644
Observations	15249	15249	15249	15249	15249	15249
Treated units	25	25	25	25	25	25
Donor units	274	274	274	274	274	274
Pre-Treatment weeks	34	34	34	34	34	34
Post-Treatment weeks	17	17	17	17	17	17

Notes: This table reports synthetic difference-in-differences (SDID) estimates of the effect of the August 11, 2025 deployment on weekly crime counts. The treated units are PSAs in Washington, D.C., while the donor pool consists of jurisdictions in Montgomery County, Prince George's County, and the cities of Alexandria and Baltimore. To align the estimator with the identification strategy of the main triple-difference specification, the sample is restricted to weeks following August 11 in each year. Post-August 11 weeks from earlier years serve as the pre-treatment period, while post-August 11 weeks in 2025 constitute the treatment period. The variable *Deployed × Post* equals one for deployed D.C. PSAs in 2025 and zero otherwise. Standard errors are computed using placebo-based inference with 500 replications following Arkhangelsky et al. (2021). Control means are calculated using non-deployed donor jurisdictions in the pre-treatment period.
* p<.1, ** p<.05, *** p<.01

Table 3: The Effect of Deployment on Crime, DDD Design, Within-DC Analysis

	(1) Assault	(2) Robbery	(3) Homicide	(4) Burglary	(5) Theft	(6) MVT
Post × 2025 × Deployed	-0.010	-0.137	-0.006	0.010	-0.842	0.118
	(0.057)	(0.104)	(0.020)	(0.056)	(0.645)	(0.203)
Control mean	0.512	0.676	0.080	0.335	5.343	1.427
Observations	14022	14022	14022	14022	14022	14022
Clusters	57	57	57	57	57	57
PSA×WOY FE	Yes	Yes	Yes	Yes	Yes	Yes
PSA×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Year×WOY FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates of β from Equation (1). Sample is at the PSA × week level. All models include PSA×calendar week, PSA×year, and calendar week×year fixed effects. Control means refers to mean in non-deployed PSAs, before Aug. 11. Analysis from year 2019-2025. Year 2020 omitted from analysis. Standard errors are clustered at the PSA level.
* p<.1, ** p<.05, *** p<.01

Table 4: The Spillover Effects of Deployment on Crime, DDD Design, non-DC controls

	(1) Assault	(2) Robbery	(3) Homicide	(4) Burglary	(5) Theft	(6) MVT
Post × 2025 × Deployed	0.106**	-0.219**	-0.020	-0.013	-1.747***	-0.790***
	(0.046)	(0.096)	(0.020)	(0.061)	(0.617)	(0.186)
Post × 2025 × Non-deployed	0.065*	-0.069	-0.024	-0.005	-0.728**	-0.915***
	(0.033)	(0.066)	(0.016)	(0.048)	(0.305)	(0.158)
Control mean	0.298	0.282	0.022	0.349	1.845	0.587
Observations	40713	40713	40713	40713	40713	40713
Clusters	331	331	331	331	331	331
Unit×WOY FE	Yes	Yes	Yes	Yes	Yes	Yes
Unit×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Year×WOY FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates of β_1 and β_2 from Equation (5). Sample is at the unit × week level. All models include unit×calendar week, unit×year, and calendar week×year fixed effects. Post×2025×Deployed refers to DC PSAs with reported deployment. Post× 2025×Non-deployed refers to DC PSAs without reported deployment. Control units created using police beats from Montgomery County and Prince George's County, City of Baltimore, and City of Alexandria. Control units restricted to beats in districts adjacent to DC for Montgomery County and Prince George's County. Unit refers to a PSA in DC, a police post in Baltimore, and a police beat in remaining control jurisdictions. Control means refers to mean in control units, before Aug. 11. Analysis from January 2022 through December 2025. Year 2023 omitted from analysis. Refer to Appendix B for a description of outcome creation and harmonization. Standard errors are clustered at the unit level.
* p<.1, ** p<.05, *** p<.01

Table 5: Predictors of PSA Selection into Deployment

	1 Month		3 Months		12 Months	
	(1)	(2)	(3)	(4)	(5)	(6)
	Deployed	Deployed	Deployed	Deployed	Deployed	Deployed
Violent	0.021		0.018**		0.003	
	(0.020)		(0.008)		(0.002)	
Property	0.002		0.000		0.000	
	(0.003)		(0.001)		(0.000)	
Robbery		0.006		0.009		0.002
		(0.033)		(0.012)		(0.003)
Assault w/ DW		0.016		0.017		0.008
		(0.039)		(0.024)		(0.011)
Homicide		0.036		0.058		0.012
		(0.139)		(0.050)		(0.043)
Burglary		0.062		0.032		-0.001
		(0.058)		(0.027)		(0.009)
MVT		-0.011		-0.005		-0.001
		(0.015)		(0.007)		(0.001)
Theft		0.003		0.001		0.001
		(0.004)		(0.001)		(0.000)
Population	-0.020	-0.018	-0.019	-0.017	-0.022*	-0.023*
	(0.013)	(0.013)	(0.012)	(0.012)	(0.013)	(0.013)
Share non-White	0.003	0.136	-0.224	-0.126	-0.156	-0.175
	(0.336)	(0.391)	(0.353)	(0.396)	(0.324)	(0.371)
Dist. White House (km)	-0.110***	-0.112***	-0.098***	-0.101***	-0.098***	-0.092***
	(0.026)	(0.029)	(0.028)	(0.029)	(0.029)	(0.029)
Outcome mean	0.439	0.439	0.439	0.439	0.439	0.439
Observations	57	57	57	57	57	57
R-squared	0.315	0.348	0.348	0.390	0.330	0.367
p-val: Crime F-test	0.315	0.312	0.050	0.015	0.162	0.050

Notes: Each observation is a PSA. Each column reports estimates from Equation (6). Each estimate gives the association between the probability that there was a deployment in the PSA on Aug. 11, 2025, and pre-deployment crime levels and demographic controls. Columns (1)–(2) use crime totals from the 1-month window prior to deployment (July 12–Aug. 10, 2025), Columns (3)–(4) use crime totals from the 3-month window (May 12–Aug. 10, 2025), and Columns (5)–(6) use crime totals from the 12-month window (Aug. 12, 2024–Aug. 10, 2025). Columns 1, 3, and 5 use aggregate crime categories (violent and property). Columns 2, 4, and 6 use crime subcategories (robbery, assault with a dangerous weapon, homicide, burglary, motor vehicle theft, and theft). Population refers to PSA population (in thousands) calculated from 2020 Census, share of non-White residents obtained from 2020 Census, and distance to the White House (km) as controls. Robust standard errors are reported in parentheses. "p-val: Crime F-test" refers to the p-value from a joint test of the significance of the crime variables.
* p<.1, ** p<.05, *** p<.01

Table 6: The Effect of Deployment on Transportation and Mobility

	(1) Rail	(2) Bus	(3) Commuter Rail	(4) Commuter Bus	(5) Ridesharing	(6) Taxi
Post-August × 2025	-0.176**	-0.155***	-0.127**	-0.088	-0.055**	-0.056
	(0.066)	(0.040)	(0.052)	(0.083)	(0.026)	(0.068)
Control mean (000s)	11966	8975	323	107	3786	243
Observations	48	48	48	48	48	48
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes

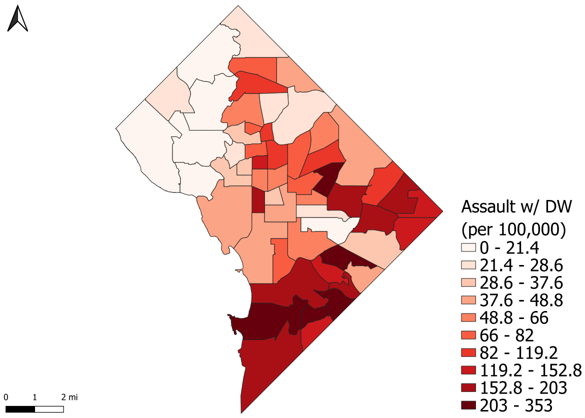
Notes: This table reports estimates from monthly difference-in-differences specifications comparing the post-August change in 2025 to the corresponding post-August change in prior years. Sample period is 2022-2025. The dependent variable is the log monthly outcome. Rail, Bus, Commuter Rail, and Commuter Bus denote monthly unlinked passenger trips (UPT) for Washington Metropolitan Area Transit Authority (WMATA) Metrorail, WMATA Metrobus, Maryland Transit Administration (MTA) commuter rail (MARC), and MTA commuter bus, respectively. UPT measures the total number of passenger boardings and counts each boarding separately, including transfers. Ridesharing and Taxi are monthly trips from the DC Department of For-Hire Vehicles. All specifications include year fixed effects and month-of-year fixed effects. Standard errors in parentheses are heteroskedasticity- and autocorrelation-consistent with a six-month lag. Control mean (in thousands) refers to the average number of trips in all pre-August months. The sample covers January 2022 through December 2025. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Cost-Benefit Analysis (30-day window)

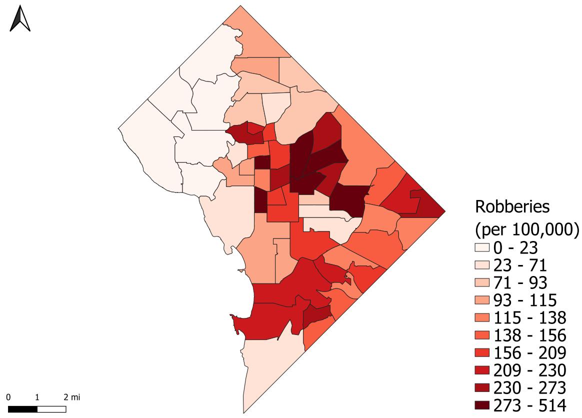
Offense	Treated ($N_T = 25$) (1)	Spillover ($N_S = 32$) (2)	Value (USD) (3)	Total benefit (Million USD) (4)	Deployment cost (Million USD) (5)
Robbery	-0.219	-0.069	$133,426$	4.39	
MVT	-0.790	-0.915	$8,121$	1.71	
Theft	-1.747	-0.728	$1,229$	0.35	
Total	—	—	—	6.45	89.86

Notes: DDD coefficients in columns 1 and 2 are per PSA–week and come from Table 4, respectively. $N_T = 25$ gives the number of treated PSAs; $N_S = 32$ gives the number of spillover PSAs. Per-incident contingent valuation values in column 3 come from Cohen et al. (2004) as tabulated in Chalfin (2016) (Table 1). Values are adjusted to 2025 dollars using a 2010 annual CPI index of 218.056 and an August 2025 index of 323.976 (US Inflation Calculator, 2025). Robbery \$89,804→\$133,426; MVT \$5,466→\$8,121; Theft \$827→\$1,229. Total benefits in column 4 are computed as $(\hat{\beta}_{\text{T}} \times N_T + \hat{\beta}_{\text{S}} \times N_S) \times (30/7) \times$ (Column 3), where $\hat{\beta}_T$ and $\hat{\beta}_S$ are DDD coefficients per PSA–week for treated and spillover groups, respectively. Total deployment costs in column 5 are computed separately for the D.C. National Guard and the out-of-state Guard units. For the D.C. National Guard, we use the projected \$201 million total cost reported in Mayes-Osterman (2025) for the period August 11 to November 30 (111 days). This yields approximately \$54 million ($201/111 \times 30$) for a 30-day window. For out-of-state Guard units, we use the per–Guard-day cost estimate of \$530 from Ali and Brice (2020), adjusted to 2025 dollars using a CPI factor of 1.252 (\$664 per Guard-day). We apply a 33% overhead in deployment time (30 days $\times$ 1.33 $\approx$ 40 effective days), following (Shumway, 2025). We use 1,342 deployed out-of-state personnel (calculated as 2,300 total personnel minus 958 D.C. Guard (Babb, 2025; Mayes-Osterman, 2025)). The resulting cost is (1,342 $\times$ 664 $\times$ 40 $\approx$ \$36 million). Total deployment costs are approximately \$90 million (54 million from D.C. Guard and 36 million from out-of-state Guard).

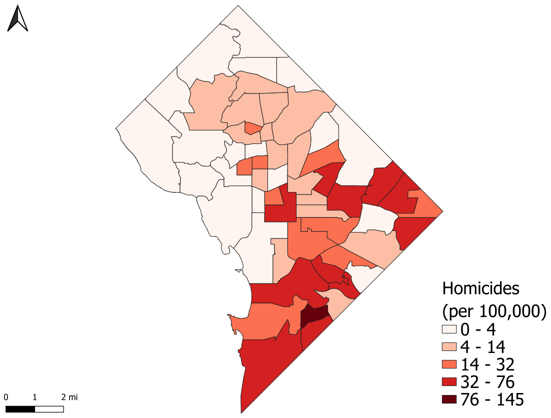
A Appendix Tables and Figures



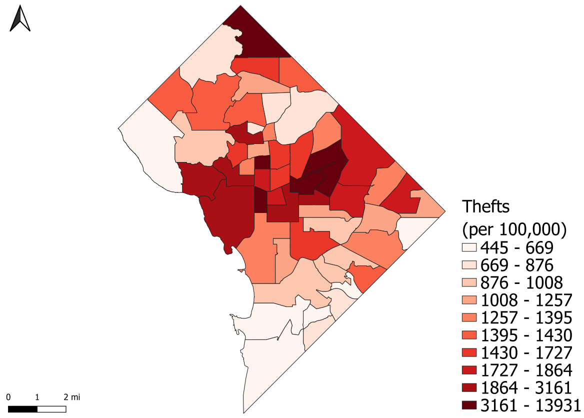
(a) Assault with DW



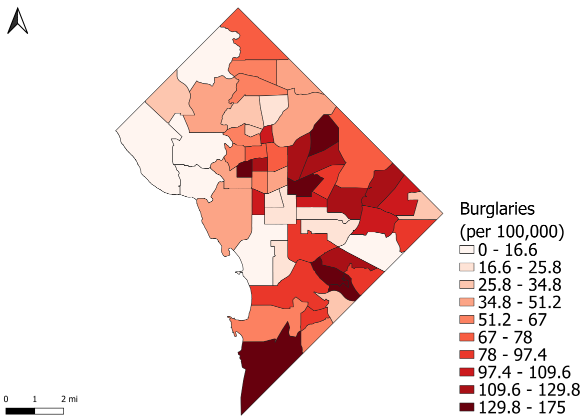
(b) Robbery



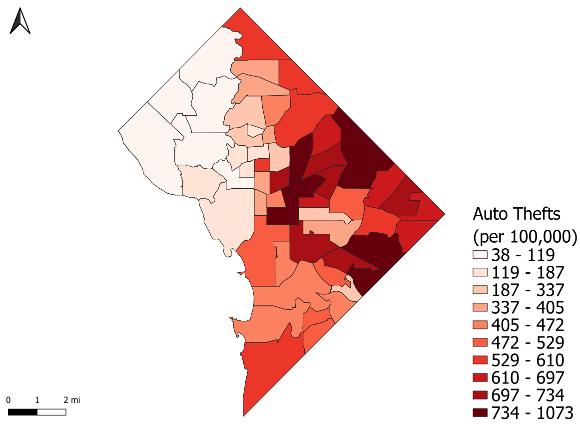
(c) Homicide



(d) Theft



(e) Burglaries



(f) Motor Vehicle Theft

Figure A1: Crime Rates across PSAs: Crime Subcategories

Notes: Assault with a dangerous weapon, robbery, and homicide are considered violent crimes. Burglary, theft, and motor vehicle theft are considered property crimes. Crime figures use crime in 2025 prior to deployment date Aug. 11, 2025. Crimes rates are calculated per 100,000 population in the PSA. Analysis uses population data from 2020 Census.

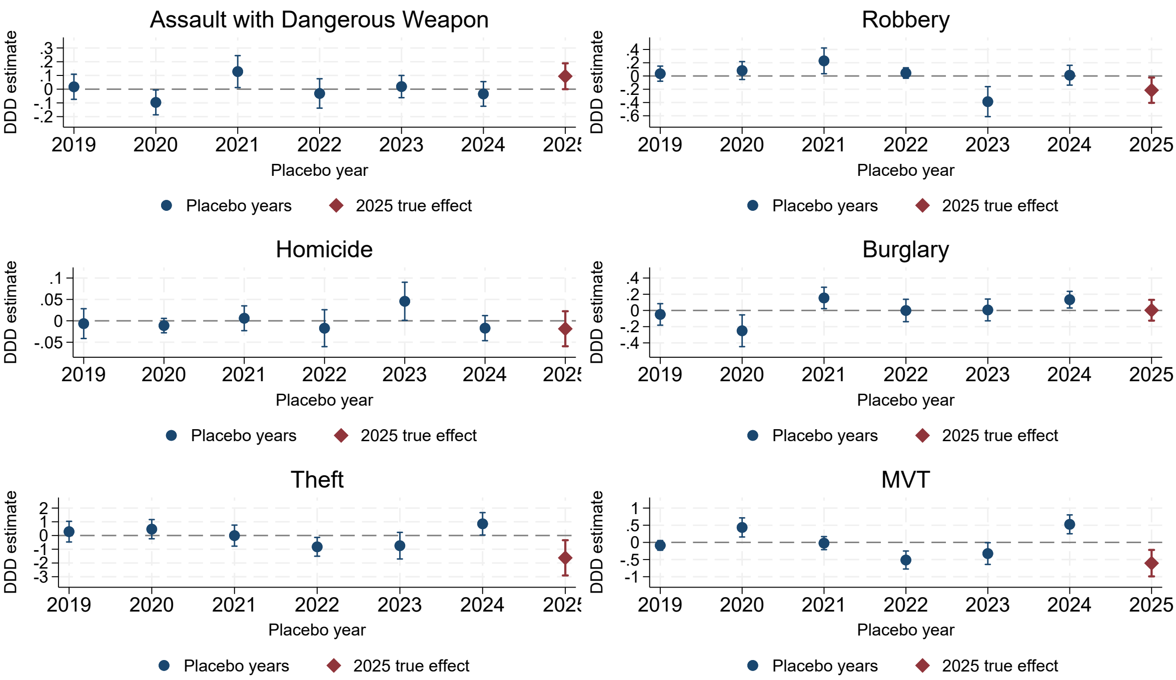


Figure A2: Placebo Deployment Dates, DDD estimates, non-DC controls

Notes: Each blue dot gives the coefficient estimates β_k from Equation (3) estimated using Aug. 11 of the specified year as the placebo cutoff instead of Aug. 11, 2025. Capped lines show the 95% confidence intervals. All estimates omit 2025. Control units created using police beats from Montgomery county, Prince George county, and City of Alexandria. Control units restricted to beats in districts adjacent to DC for Montgomery county and Prince George county. City of Baltimore omitted from this analysis because data starts in 2022. Red dot shows DDD estimate on true treatment year (2025) corresponding to Table A7. Unit refers to a PSA in DC, a police and a police beat in remaining control jurisdictions. Refer to Appendix B for a description of outcome creation and harmonization. Standard errors are clustered at the unit level.

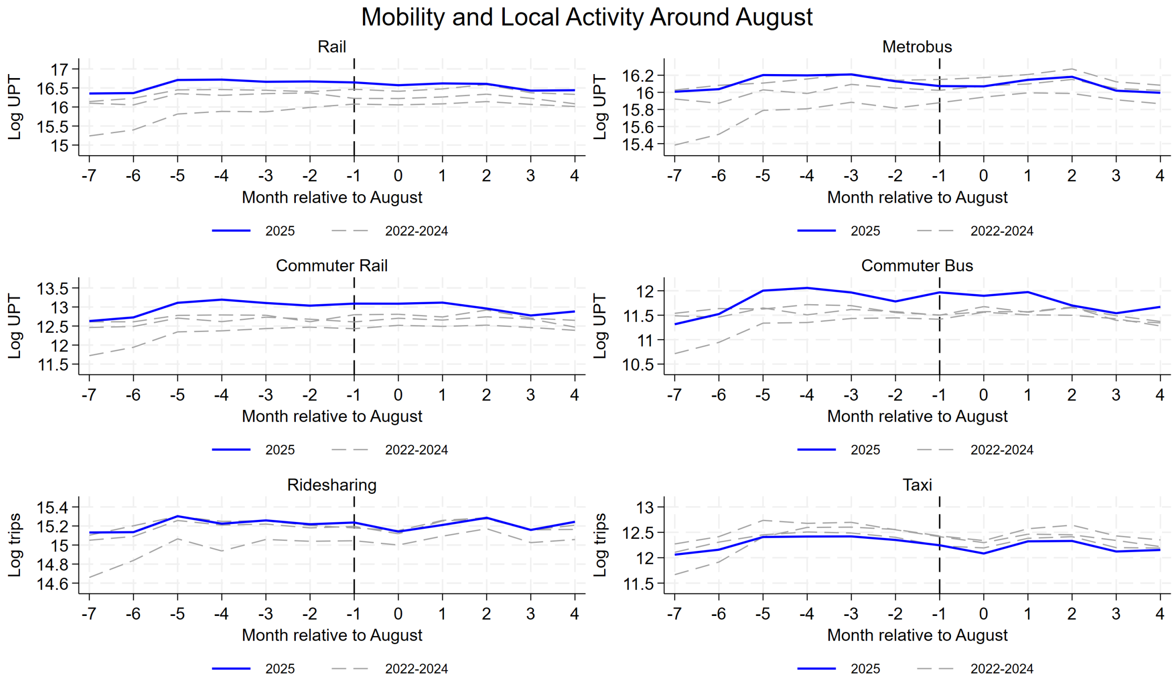


Figure A3: Trends in Mobility and Transportation Around Deployment

Notes: Each panel plots the log monthly outcome by month (relative to August). The figure overlays 2022, 2023, 2024, and 2025. The 2025 series is shown as a solid blue line, while prior years are shown as dashed gray lines. The vertical dashed line marks July (the month before deployment). Transit outcomes are unlinked passenger trips (UPT) from the Federal Transit Administration's National Transit Database. Taxi and ridesharing outcomes are trips from the DC Department of For-Hire Vehicles.

Table A1: Distribution of Reported Offenses before Deployment, 2019–2025 (Annualized Rates)

Offense Type	Count	Percent	Annual Rate per 100,000
Theft	134,199	68.64	2,945
Motor vehicle theft	27,378	14.00	601
Robbery	14,783	7.56	324
Assault with a dangerous weapon	9,178	4.69	201
Burglary	7,446	3.81	163
Homicide	1,349	0.69	30
Sexual assault	1,131	0.58	25
Arson	48	0.02	1
Total	195,512	100.00	4,290

Notes: Table reports the number of incidents, share of total, and annualized rates per 100,000 population by offense category for January 1, 2019 - August 10, 2025 (day before deployment). Data from MPD administrative records (District of Columbia Metropolitan Police Department, 2025). Population is 689,545 from the 2020 Census. Rates are annualized by dividing cumulative totals by the number of years. Number of years calculated as number of days between January 1, 2019 and August 2025 divided by 365.

Table A2: The Effect of Deployment on Crime, DDD Design, non-DC controls, PPML

	(1) Assault	(2) Robbery	(3) Homicide	(4) Burglary	(5) Theft	(6) MVT
Post × 2025 × Deployed	0.339**	-0.414**	-0.359	-0.004	-0.158**	-0.571***
	(0.139)	(0.171)	(0.456)	(0.186)	(0.078)	(0.122)
Control mean	0.298	0.282	0.022	0.349	1.845	0.587
Observations	36777	36777	36777	36777	36777	36777
Clusters	299	299	299	299	299	299
Unit×WOY FE	Yes	Yes	Yes	Yes	Yes	Yes
Unit×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Year×WOY FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates of β from Equation (1) using Poisson Pseudo Maximum Likelihood estimator. Sample is at the unit × week level. All models include unit×calendar week, unit×year, and calendar week×year fixed effects. Control units created using police beats from Montgomery county, Prince George county, City of Baltimore, and City of Alexandria. Control units restricted to beats in districts adjacent to DC for Montgomery county and Prince George county. Unit refers to a PSA in DC, a police post in Baltimore, and a police beat in remaining control jurisdictions. Control mean refers to mean in control units, before Aug. 11. The analysis covers 2022–2025. Refer to Appendix B for a description of outcome creation and harmonization. Standard errors are clustered at the unit level.
* p<.1, ** p<.05, *** p<.01

Table A3: The Effect of Deployment on Crime, non-DC controls, DID

	(1) Assault	(2) Robbery	(3) Homicide	(4) Burglary	(5) Theft	(6) MVT
Deployed × Post	0.067	-0.176***	-0.030*	0.018	-0.555	-0.528***
	(0.047)	(0.068)	(0.015)	(0.033)	(0.355)	(0.137)
Control mean (pre, non-dep.)	0.252	0.208	0.012	0.315	1.982	0.602
Observations	12259	12259	12259	12259	12259	12259
Clusters	299	299	299	299	299	299
Unit FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates of β from Equation (4). Sample is at the unit × week level. Outcome is the number of incidents for the specified crime category. Control units include beats from Montgomery County and Prince George County, MD, and the City of Alexandria. Sample of analysis presented in Figure 4. In the case of DC, a unit is a PSA. For the remaining controls, a unit is a police beat. Standard errors are clustered at the unit level.
* p<.1, ** p<.05, *** p<.01

Table A4: The Effect of Deployment on Crime, DDD Design, Monthly, non-DC controls

	(1) Assault	(2) Robbery	(3) Homicide	(4) Burglary	(5) Theft	(6) MVT
Post × 2025 × Deployed	0.486***	-0.958**	-0.086	-0.180	-6.201**	-3.527***
	(0.175)	(0.381)	(0.090)	(0.272)	(2.445)	(0.821)
Control mean	1.242	1.226	0.097	1.495	7.734	2.650
Observations	9867	9867	9867	9867	9867	9867
Clusters	299	299	299	299	299	299
Unit×MOY FE	Yes	Yes	Yes	Yes	Yes	Yes
Unit×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Year×MOY FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates of β from Equation (1). Sample is at the unit × month level. All models include unit×calendar month, unit×year, and calendar month×year fixed effects. Control units created using police beats from Montgomery county, Prince George county, City of Baltimore, and City of Alexandria. Control units restricted to beats in districts adjacent to DC for Montgomery county and Prince George county. Unit refers to a PSA in DC, a police post in Baltimore, and a police beat in remaining control jurisdictions. Control mean refers to mean in control units, before Aug. 11. The analysis covers 2022–2025. Refer to Appendix B for a description of outcome creation and harmonization. Standard errors are clustered at the unit level.
* p<.1, ** p<.05, *** p<.01

Table A5: The Effect of Deployment on Crime, DDD Design, non-DC controls, Rate

	(1) Assault	(2) Robbery	(3) Homicide	(4) Burglary	(5) Theft	(6) MVT
Post × 2025 × Deployed	0.762	-7.497**	-2.139	-0.820	-10.313	-1.562
	(6.139)	(3.763)	(1.382)	(1.056)	(22.427)	(3.044)
Control mean	18.832	12.837	2.513	15.995	75.668	22.120
Observations	29643	29643	29643	29643	29643	29643
Clusters	241	241	241	241	241	241
Unit×WOY FE	Yes	Yes	Yes	Yes	Yes	Yes
Unit×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Year×WOY FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates of β from Equation (1). Sample is at the unit × week level. Outcome is crime rate per 100,000 population. Population at the PSA level calculated using 2020 Census block population data. All models include unit×calendar week, unit×year, and calendar week×year fixed effects. Control units created using police beats from Montgomery county, Prince George county, City of Baltimore, and City of Alexandria. Control units restricted to beats in districts adjacent to DC for Montgomery county and Prince George county. Unit refers to a PSA in DC, a police post in Baltimore, and a police beat in remaining control jurisdictions. Control mean refers to mean in control units, before Aug. 11. The analysis covers 2022–2025. Refer to Appendix B for a description of outcome creation and harmonization. Standard errors are clustered at the unit level.
* p<.1, ** p<.05, *** p<.01

Table A6: The Effect of Deployment on Crime, DDD Design, Monthly, non-DC controls, Rate

	(1) Assault	(2) Robbery	(3) Homicide	(4) Burglary	(5) Theft	(6) MVT
Post × 2025 × Deployed	3.363	-33.443**	-7.401	4.639	-7.586	-4.579
	(23.921)	(15.612)	(4.814)	(6.153)	(80.373)	(21.536)
Control mean	78.390	55.281	9.310	65.631	296.776	99.421
Observations	7953	7953	7953	7953	7953	7953
Clusters	241	241	241	241	241	241
Unit×MOY FE	Yes	Yes	Yes	Yes	Yes	Yes
Unit×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Year×MOY FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates of β from Equation (1). Sample is at the unit × month level. Outcome is crime rate per 100,000 population. Population at the PSA level calculated using 2020 Census block population data. All models include unit×calendar month, unit×year, and calendar month×year fixed effects. Control units created using police beats from Montgomery county, Prince George county, City of Baltimore, and City of Alexandria. Control units restricted to beats in districts adjacent to DC for Montgomery county and Prince George county. Unit refers to a PSA in DC, a police post in Baltimore, and a police beat in remaining control jurisdictions. Control mean refers to mean in control units, before Aug. 11. The analysis covers 2022–2025. Refer to Appendix B for a description of outcome creation and harmonization. Standard errors are clustered at the unit level.
* p<.1, ** p<.05, *** p<.01

Table A7: The Effect of Deployment on Crime, DDD Design, non-DC controls, Omitting Baltimore

	(1) Assault	(2) Robbery	(3) Homicide	(4) Burglary	(5) Theft	(6) MVT
Post × 2025 × Deployed	0.094*	-0.214**	-0.019	0.002	-1.626**	-0.605***
	(0.048)	(0.097)	(0.021)	(0.065)	(0.655)	(0.197)
Control mean	0.177	0.227	0.013	0.339	2.931	0.883
Observations	13038	13038	13038	13038	13038	13038
Clusters	106	106	106	106	106	106
Unit×WOY FE	Yes	Yes	Yes	Yes	Yes	Yes
Unit×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Year×WOY FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates of β from Equation (1). Sample is at the unit × week level. All models include unit×calendar week, unit×year, and calendar week×year fixed effects. Control units created using police beats from Montgomery county, Prince George county, and City of Alexandria. Control units restricted to beats in districts adjacent to DC for Montgomery county and Prince George county. Unit refers to a PSA in DC, and a police beat in remaining control jurisdictions. Control mean refers to mean in control units, before Aug. 11. The analysis covers 2022–2025. Refer to Appendix B for a description of outcome creation and harmonization. Standard errors are clustered at the unit level.
* p<.1, ** p<.05, *** p<.01

Table A8: The Effect of Deployment on Crime, DDD Design, non-DC controls, including all units in Montgomery and Prince George counties

	(1) Assault	(2) Robbery	(3) Homicide	(4) Burglary	(5) Theft	(6) MVT
Post × 2025 × Deployed	0.095**	-0.219**	-0.020	0.005	-1.651***	-0.765***
	(0.046)	(0.095)	(0.020)	(0.060)	(0.616)	(0.185)
Control mean	0.267	0.260	0.020	0.354	1.919	0.624
Observations	44157	44157	44157	44157	44157	44157
Clusters	359	359	359	359	359	359
Unit×WOY FE	Yes	Yes	Yes	Yes	Yes	Yes
Unit×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Year×WOY FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates of β from Equation (1). Sample is at the unit × week level. All models include unit×calendar week, unit×year, and calendar week×year fixed effects. Control units created using police beats from Montgomery county, Prince George county, City of Baltimore, and City of Alexandria. Unit refers to a PSA in DC, a police post in Baltimore, and a police beat in remaining control jurisdictions. Control mean refers to mean in control units, before Aug. 11. The analysis covers 2022–2025. Refer to Appendix B for a description of outcome creation and harmonization. Standard errors are clustered at the unit level.
* p<.1, ** p<.05, *** p<.01

Table A9: The Effect of Deployment on Crime, DDD Design, non-DC controls, Aggregate Crime

	OLS		PPML		Rate		Monthly	
	(1) Violent	(2) Property	(3) Violent	(4) Property	(5) Violent	(6) Property	(7) Violent	(8) Property
Post×2025×Deployed	-0.13	-2.55***	-0.13	-0.15**	-8.87	-12.70	-0.59	-10.96***
	(0.10)	(0.64)	(0.12)	(0.07)	(7.70)	(23.93)	(0.43)	(2.66)
Control mean	0.60	2.78	1.05	3.35	34.18	113.78	2.41	11.12
Observations	36777	36777	20114	29629	29643	29643	8970	8970
Clusters	299	299	236	277	241	241	299	299
Unit×Calendar FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Unit×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year×Calendar FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates of β from Equation (1). Sample is at the unit × week level. All models include unit×calendar week, unit×year, and calendar week×year fixed effects. Columns 1–2 estimate effects in levels using OLS, columns 3–4 use PPML, columns 5–6 use crime rates (per 100,000 population), and columns 7–8 aggregate data at the monthly level. Violent crimes include homicide, robbery, and assault with a deadly weapon; property crimes include burglary, theft, and motor vehicle theft. Control units created using police beats from Montgomery County, Prince George's County, the City of Baltimore, and the City of Alexandria. Control units restricted to beats in districts adjacent to D.C. for Montgomery and Prince George's counties. "Unit" refers to a PSA in D.C., a police post in Baltimore, and a police beat in other control jurisdictions. Control mean refer to the mean in control units before August 11. Analysis covers the period 2022–2025. Refer to Appendix B for details on outcome construction and harmonization. Standard errors clustered at the unit level.

Table A10: The Effect of Deployment on Mobility and Transportation, Panel Data for Rail and Metrobus Ridership

	(1) Rail	(2) Metrobus
Treated × Post-August 2025	-0.041	-0.161***
	(0.075)	(0.046)
Control mean (000s)	11810	9168
Observations	96	96
Agency FE	Yes	Yes
Month FE	Yes	Yes

Notes: This table reports robustness specifications of columns 1 and 2 of Table 6 using panel comparisons for rail and bus outcomes. The rail specification compares Washington Metropolitan Area Transit Authority (WMATA) Metrorail to Maryland Transit Administration (MTA) combined rail ridership (MTA heavy rail and light rail). The bus specification compares WMATA Metrobus to City of Baltimore bus service. The dependent variable is the log monthly unlinked passenger trips (UPT), where UPT measures total passenger boardings and counts each boarding separately. The reported coefficient corresponds to the interaction between the treated Washington, DC series and an indicator for August 2025 and later. All specifications include agency fixed effects and month fixed effects. Standard errors in parentheses are heteroskedasticity- and autocorrelation-consistent with a six-month lag. The sample covers January 2022 through December 2025. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A11: Decomposing the Effect on Assaults with Dangerous Weapons, DDD Design, non-DC controls

	(1) Weapon: All	(2) Weapon: Firearm	(3) Weapon: Non-Firearm
Post × 2025 × Deployed	0.101**	0.001	0.099**
	(0.049)	(0.026)	(0.041)
Control mean	0.440	0.150	0.290
Observations	29274	29274	29274
Clusters	238	238	238
Unit×WOY FE	Yes	Yes	Yes
Unit×Year FE	Yes	Yes	Yes
Year×WOY FE	Yes	Yes	Yes

Notes: Estimates of β from Equation (1). Sample is at the unit × week level. All models include unit×calendar week, unit×year, and calendar week×year fixed effects. Post×2025×Deployed refers to DC PSAs with reported deployment. Control units created using police beats from Montgomery county, and City of Baltimore. Prince George county and City of Alexandria data does not distinguish type of weapon in assault data. Unit refers to a PSA in DC, a police post in Baltimore, and a police beat in Montgomery county. Control mean refers to mean in control units, before Aug. 11. The analysis covers 2022–2025. Year 2023 omitted from analysis. Refer to Appendix B for a description of outcome creation and harmonization. Standard errors are clustered at the unit level.
* p<.1, ** p<.05, *** p<.01

Table A12: Shots detected via ShotSpotter Alerts, DDD Design, within DC

	(1) Shots detected	(2) Shots detected (per 100K)
Post×2025×Deployed	0.721**	7.750**
	(0.318)	(3.174)
Control mean	3.12	32.18
Observations	10944	10944
Clusters	57	57
PSA×Calendar FE	Yes	Yes
PSA×Year FE	Yes	Yes
Year×Calendar FE	Yes	Yes

Notes: Estimates of β from Equation (1). Sample is at the PSA × week level. All models include PSA×calendar week, unit×year, and calendar week×year fixed effects. Post×2025×Deployed refers to DC PSAs with reported deployment. Control units are other PSAs within DC without reported deployment. Control mean refers to mean in control units, before Aug. 11. Analysis from year 2019-2025. Year 2020 omitted from analysis. Standard errors are clustered at the PSA level.
* p<.1, ** p<.05, *** p<.01

Table A13: Gun-related Emergency Room Visits, DDD Design, Monthly, within DC

	(1) Visits	(2) Visits	(3) Visits (per 100K)	(4) Visits (per 100K)
Post × 2025 × N. of agencies	-0.121		-0.185	
	(0.120)		(0.140)	
Post × 2025 × N. of agencies (per 100K)		-0.112		-0.172
		(0.103)		(0.115)
Wild cluster p-value	0.722	0.723	0.658	0.661
Control mean	10.552	10.552	9.152	9.152
Observations	531	531	472	472
Clusters	9	9	8	8
Ward×MOY FE	Yes	Yes	Yes	Yes
Ward×Year FE	Yes	Yes	Yes	Yes
Year×MOY FE	Yes	Yes	Yes	Yes

Notes: Estimates of β from Equation (1). Sample is at the ward × month level. All models include ward×calendar month, ward×year, and calendar month×year fixed effects. Post×2025×Number of agencies refers to the number of reported federal agencies deployed in a given DC ward. Post×2025×Deployed share refers to number of agencies per 100 thousand population in the ward. Control mean refers to mean before Aug. 11, 2025. Analysis from year 2021-2025 for which ER visits data available. Standard errors are clustered at the ward level. Wild cluster p-value refers to the p-values obtained from wild bootstrap clustered inference given the small number of clusters.
* p<.1, ** p<.05, *** p<.01

Table A14: The Effect of Deployment on Crime, DDD Design, PPML, Within-DC analysis

	(1) Assault w/ DW	(2) Robbery	(3) Homicide	(4) Burglary	(5) Theft	(6) MVT
Post × 2025 × Deployed	0.008	-0.313*	0.204	0.034	-0.026	0.022
	(0.156)	(0.186)	(0.462)	(0.202)	(0.083)	(0.123)
Control mean	0.512	0.676	0.080	0.335	5.343	1.427
Observations	14022	14022	14022	14022	14022	14022
Clusters	57	57	57	57	57	57
PSA×WOY FE	Yes	Yes	Yes	Yes	Yes	Yes
PSA×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Year×WOY FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates of β from Equation (1). Sample is at the PSA × week level. All models include PSA×calendar week, PSA×year, and calendar week×year fixed effects. Estimation using Poisson Pseudo Maximum Likelihood estimator. Control mean refers to mean in non-deployed PSAs, before Aug. 11, in non-2025 years. Analysis from year 2019-2025. Year 2020 omitted from analysis. Standard errors are clustered at the PSA level.
* p<.1, ** p<.05, *** p<.01

Table A15: The Effect of Deployment on Crime, DiD Design, Within-DC analysis

	(1) Assault	(2) Robbery	(3) Homicide	(4) Burglary	(5) Theft	(6) MVT
Deployed × Post	0.035	-0.171**	-0.007	-0.004	-0.090	0.177
	(0.052)	(0.080)	(0.018)	(0.044)	(0.449)	(0.176)
Control mean	0.315	0.409	0.043	0.230	5.031	1.691
Observations	2337	2337	2337	2337	2337	2337
Clusters	57	57	57	57	57	57
PSA FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates of β from Equation (4). Sample is at the PSA × week level. Outcome is the number of incidents for the specified crime category. All models include PSA×calendar week, PSA×year, and calendar week×year fixed effects. Standard errors are clustered at the PSA level.
* p<.1, ** p<.05, *** p<.01

Table A16: The Effect of Deployment on Crime, DDD Design, Crime rate, Within-DC analysis

	(1) Assault	(2) Robbery	(3) Homicide	(4) Burglary	(5) Theft	(6) MVT
Post × 2025 × Deployed	-0.327	-1.481*	-0.108	0.329	-14.029	1.292
	(0.698)	(0.871)	(0.223)	(0.518)	(12.066)	(1.512)
Control mean	0.490	0.768	0.072	0.364	6.728	1.399
Observations	14022	14022	14022	14022	14022	14022
Clusters	57	57	57	57	57	57
PSA×WOY FE	Yes	Yes	Yes	Yes	Yes	Yes
PSA×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Year×WOY FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates of β from Equation (1). Sample is at the PSA × week level. All models include PSA×calendar week, PSA×year, and calendar week×year fixed effects. Estimation using Poisson Pseudo Maximum Likelihood estimator. Control mean refers to mean in non-deployed PSAs, before Aug. 11, in non-2025 years. Analysis from year 2019-2025. Year 2020 omitted from analysis. Standard errors are clustered at the PSA level. Outcome is crime rate per 100,000 population. Population at the PSA level calculated using 2020 Census block population data.
* p<.1, ** p<.05, *** p<.01

Table A17: The Effect of Deployment on Crime, DDD Design, District level, Within-DC analysis

	(1) Assault	(2) Robbery	(3) Homicide	(4) Burglary	(5) Theft	(6) MVT
Post × 2025 × Deployment share	0.312	-1.837	-0.449	0.600	3.041	-0.449
	(0.756)	(1.469)	(4.298)	(1.025)	(10.123)	(4.298)
Wild cluster p-value	0.690	0.147	0.924	0.750	0.822	0.924
Control mean	3.99	6.25	11.39	2.96	54.78	11.39
Observations	1680	1680	1680	1680	1680	1680
Clusters	7	7	7	7	7	7
District×WOY FE	Yes	Yes	Yes	Yes	Yes	Yes
District×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Year×WOY FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates of β from Equation (1). Sample is at the District × week level. All models include District×calendar week, District×year, and calendar week×year fixed effects. Control mean refers to mean in non-deployed PSAs, before Aug. 11, in non-2025 years. Analysis from year 2019-2025. Year 2020 omitted from analysis. Standard errors are clustered at the District level. Deployed refers to the share of PSAs within a district with deployment. Table presents clustered wild-bootstrap p-values given the low number of clusters (7 districts). Asterisks based on wild-bootstrap p-values.

* p<.1, ** p<.05, *** p<.01

Table A18: The Effect of Deployment on Crime, DDD Design, Monthly, Within-DC analysis

	(1) Assault	(2) Robbery	(3) Homicide	(4) Burglary	(5) Theft	(6) MVT
Post × 2025 × Deployed	-0.016	-0.850**	0.019	0.123	-3.408	0.580
	(0.212)	(0.413)	(0.088)	(0.239)	(2.588)	(0.914)
Control mean	2.130	2.991	0.336	1.447	22.988	6.174
Observations	3762	3762	3762	3762	3762	3762
Clusters	57	57	57	57	57	57
PSA×MOY FE	Yes	Yes	Yes	Yes	Yes	Yes
PSA×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Year×MOY FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates of β from Equation (1). Sample is at the PSA × month level. All models include PSA×calendar month, PSA×year, and calendar month×year fixed effects. Control mean refers to mean in non-deployed PSAs, before Aug. 11, in non-2025 years. Analysis from year 2019-2025. Year 2020 omitted from analysis. Standard errors are clustered at the PSA level.

* p<.1, ** p<.05, *** p<.01

Table A19: The Effect of Deployment on Crime, DDD Design, Aggregate Crime Categories, Within-DC analysis

	OLS		PPML		Rate		Monthly	
	(1) Violent	(2) Property	(3) Violent	(4) Property	(5) Violent	(6) Property	(7) Violent	(8) Property
Post×2025×Deployed	-0.15	-0.71	-0.15	0.01	-1.93	-12.43	-0.62	-3.31
	(0.11)	(0.71)	(0.13)	(0.07)	(1.22)	(12.57)	(0.48)	(2.96)
Control mean	1.33	7.11	1.41	7.11	12.61	60.14	5.31	28.43
Observations	14022	14022	13599	14022	14022	14022	3420	3420
Clusters	57	57	57	57	57	57	57	57
PSA×Calendar FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
PSA×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year×Calendar FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates of β from Equation (1). Sample is at the unit $\times$ week level in columns 1-6 and include unit×calendar week, unit×year, and calendar week×year fixed effects. Results in columns 7-8 use unit $\times$ month level and unit×calendar month, unit×year, and calendar month×year fixed effects. Columns 1–2 estimate effects in weekly counts using OLS, columns 3–4 use PPML, columns 5–6 use crime rates per 100,000 population, and columns 7–8 aggregate outcomes to the monthly level. Violent crimes include homicide, robbery, and assault with a deadly weapon; property crimes include burglary, theft, and motor vehicle theft. Control mean refer to the mean in non-deployed PSAs before August 11. Analysis covers 2019–2025, excluding 2020. Standard errors are clustered at the PSA level.
* p<0.10, ** p<0.05, *** p<0.01.

Table A20: The Spillover Effects of Deployment on Crime, DDD Design, non-DC controls

	(1) Assault	(2) Robbery	(3) Homicide	(4) Burglary	(5) Theft	(6) MVT
Post × 2025 × Deployed	0.093**	-0.221**	-0.020	0.006	-1.659***	-0.816***
	(0.047)	(0.096)	(0.021)	(0.061)	(0.615)	(0.185)
Post × 2025 × Non-deployed	0.053	-0.070	-0.024	0.014	-0.640**	-0.941***
	(0.034)	(0.066)	(0.016)	(0.048)	(0.301)	(0.157)
Post × 2025 × Adj. DC	-0.009	-0.006	-0.001	0.004	-0.033	-0.211***
	(0.021)	(0.025)	(0.006)	(0.037)	(0.231)	(0.072)
Control mean	0.267	0.260	0.020	0.354	1.919	0.624
Observations	48093	48093	48093	48093	48093	48093
Clusters	391	391	391	391	391	391
Unit×WOY FE	Yes	Yes	Yes	Yes	Yes	Yes
Unit×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Year×WOY FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates of β from Equation (1). Sample is at the unit × week level in columns 1-6. All models in columns 1-6 include unit×calendar week, unit×year, and calendar week×year fixed effects. Results in columns 7-8 use unit × month level and unit×calendar month, unit×year, and calendar month×year fixed effects. Post×2025×Deployed refers to DC PSAs with reported deployment. Post× 2025×Non-deployed refers to DC PSAs without reported deployment. Post×2025×Adj. DC refers to units outside of DC (Prince George and Montgomery counties) but bordering DC. Control units created using police beats from Montgomery county and Prince George county (not bordering DC), City of Baltimore, and City of Alexandria. Unit refers to a PSA in DC, a police post in Baltimore, and a police beat in remaining control jurisdictions. Control mean refers to mean in control units, before Aug. 11. The analysis covers 2022–2025. Year 2023 omitted from analysis. Refer to Appendix B for a description of outcome creation and harmonization. Standard errors are clustered at the unit level.
* p<.1, ** p<.05, *** p<.01

Table A21: Predictors of PSA Selection into Deployment: Deployments per 10,000 population

	1 Month		3 Months		12 Months	
	(1)	(2)	(3)	(4)	(5)	(6)
	Deployed	Deployed	Deployed	Deployed	Deployed	Deployed
Violent	-0.069		-0.031		-0.010	
	(0.080)		(0.034)		(0.008)	
Property	0.023		0.009		0.002	
	(0.018)		(0.007)		(0.002)	
Robbery		-0.222		-0.050		-0.012
		(0.144)		(0.059)		(0.015)
Assault w/ DW		0.087		0.017		0.046
		(0.163)		(0.064)		(0.037)
Homicide		0.592		-0.005		-0.062
		(0.402)		(0.136)		(0.096)
Burglary		-0.059		-0.009		-0.050*
		(0.166)		(0.088)		(0.027)
MVT		-0.018		-0.003		-0.000
		(0.034)		(0.017)		(0.005)
Theft		0.042		0.011		0.004*
		(0.029)		(0.009)		(0.002)
Population	-0.236*	-0.248*	-0.226*	-0.223*	-0.232*	-0.233*
	(0.134)	(0.144)	(0.128)	(0.128)	(0.133)	(0.124)
Share non-White	-0.723	-0.462	-0.480	-0.331	-0.478	-1.081
	(1.220)	(1.450)	(1.216)	(1.353)	(1.285)	(1.566)
Dist. White House (km)	-0.304***	-0.278***	-0.325***	-0.314***	-0.288***	-0.256***
	(0.088)	(0.088)	(0.095)	(0.098)	(0.087)	(0.088)
Outcome mean	0.888	0.888	0.888	0.888	0.888	0.888
Observations	57	57	57	57	57	57
R-squared	0.425	0.481	0.425	0.441	0.429	0.515
p-val: Crime F-test	0.409	0.592	0.407	0.739	0.326	0.601

Notes: Each observation is a PSA. Each column reports estimates from Equation (6). Each estimate gives the association between the number of deployments per 10,000 population in the PSA on Aug. 11, 2025, and pre-deployment crime levels and demographic controls. Columns (1)–(2) use crime totals from the 1-month window prior to deployment (July 12–Aug. 10, 2025), Columns (3)–(4) use crime totals from the 3-month window (May 12–Aug. 10, 2025), and Columns (5)–(6) use crime totals from the 12-month window (Aug. 12, 2024–Aug. 10, 2025). Columns 1, 3, and 5 use aggregate crime categories (violent and property). Columns 2, 4, and 6 use crime subcategories (robbery, assault with a dangerous weapon, homicide, burglary, motor vehicle theft, and theft). Population refers to PSA population (in thousands) calculated from 2020 Census, share of non-White residents obtained from 2020 Census, and distance to the White House (km) as controls. Robust standard errors are reported in parentheses. "p-val: Crime F-test" refers to the p-value from a joint test of the significance of the crime variables.
* p<.1, ** p<.05, *** p<.01

Table A22: Predictors of PSA Selection into Deployment: Changes in Crime

	1 Month		3 Months		12 Months	
	(1)	(2)	(3)	(4)	(5)	(6)
	Deployed	Deployed	Deployed	Deployed	Deployed	Deployed
Violent	-0.007		0.020		-0.001	
	(0.022)		(0.014)		(0.004)	
Property	-0.000		0.002		0.000	
	(0.007)		(0.003)		(0.001)	
Robbery		-0.003		0.026*		-0.001
		(0.034)		(0.013)		(0.005)
Assault w/ DW		-0.000		-0.003		-0.004
		(0.039)		(0.022)		(0.013)
Homicide		-0.065		0.029		0.014
		(0.072)		(0.040)		(0.021)
Burglary		0.013		0.038**		0.003
		(0.043)		(0.018)		(0.008)
MVT		-0.005		0.003		0.004*
		(0.015)		(0.011)		(0.002)
Theft		-0.000		0.001		-0.000
		(0.007)		(0.003)		(0.001)
Population	-0.011	-0.013	-0.011	-0.011	-0.012	-0.009
	(0.014)	(0.015)	(0.014)	(0.014)	(0.014)	(0.015)
Share non-White	0.148	0.146	0.221	0.127	0.111	0.178
	(0.305)	(0.335)	(0.341)	(0.333)	(0.324)	(0.364)
Dist. White House (km)	-0.122***	-0.123***	-0.125***	-0.120***	-0.121***	-0.133***
	(0.025)	(0.026)	(0.028)	(0.031)	(0.026)	(0.025)
Outcome mean	0.439	0.439	0.439	0.439	0.439	0.439
Observations	57	57	57	57	57	57
R-squared	0.290	0.305	0.335	0.403	0.290	0.342
p-val: Crime F-test	0.948	0.949	0.100	0.043	0.938	0.562

Notes: Each observation is a PSA. Each column reports estimates from Equation (6). Each estimate gives the association between the probability of a deployment in the PSA on Aug. 11, 2025, and pre-deployment changes in crime levels. Columns (1)–(2) use changes in crime totals from the 1-month window prior to deployment (July 12–Aug. 10, 2025) relative to the month before that, Columns (3)–(4) use changes in crime totals from the 3-month window (May 12–Aug. 10, 2025) relative to the previous three-month window, and Columns (5)–(6) use changes in crime totals from the 12-month window (Aug. 12, 2024–Aug. 10, 2025) relative to the prior year. Columns 1, 3, and 5 use aggregate crime categories (violent and property). Columns 2, 4, and 6 use crime subcategories (robbery, assault with a dangerous weapon, homicide, burglary, motor vehicle theft, and theft). Population refers to PSA population (in thousands) calculated from 2020 Census, share of non-White residents obtained from 2020 Census, and distance to the White House (km) as controls. Robust standard errors are reported in parentheses. "p-val: Crime F-test" refers to the p-value from a joint test of the significance of the crime variables.
* p<.1, ** p<.05, *** p<.01

Table A23: Predictors of PSA Selection into Deployment: Probit Model

	1 Month		3 Months		12 Months	
	(1)	(2)	(3)	(4)	(5)	(6)
	Deployed	Deployed	Deployed	Deployed	Deployed	Deployed
Violent	0.077		0.063**		0.009	
	(0.082)		(0.032)		(0.008)	
Property	0.010		0.002		0.001	
	(0.012)		(0.005)		(0.001)	
Robbery		0.017		0.031		0.010
		(0.122)		(0.052)		(0.014)
Assault w/ DW		0.079		0.084		0.051
		(0.122)		(0.066)		(0.035)
Homicide		0.229		0.243		0.021
		(0.445)		(0.175)		(0.119)
Burglary		0.149		0.093		-0.023
		(0.159)		(0.092)		(0.033)
MVT		-0.035		-0.018		-0.006
		(0.050)		(0.025)		(0.005)
Theft		0.016		0.005		0.004**
		(0.015)		(0.006)		(0.002)
Population	-0.076	-0.071	-0.072	-0.070	-0.082	-0.153**
	(0.059)	(0.054)	(0.059)	(0.059)	(0.063)	(0.071)
Share non-White	0.178	0.713	-0.590	-0.416	-0.307	-1.333
	(1.419)	(1.519)	(1.459)	(1.490)	(1.397)	(1.626)
Dist. White House (km)	-0.368***	-0.374***	-0.328***	-0.335***	-0.321***	-0.282**
	(0.117)	(0.119)	(0.125)	(0.126)	(0.122)	(0.113)
Outcome mean	0.439	0.439	0.439	0.439	0.439	0.439
Observations	57	57	57	57	57	57
Pseudo R-squared	0.268	0.296	0.298	0.342	0.278	0.351
p-val: Crime F-test	0.464	0.406	0.111	0.077	0.310	0.018

Notes: Each observation is a PSA. Each column reports estimates from Equation (6). Each estimate gives the association between the probability of a deployment in the PSA on Aug. 11, 2025, and pre-deployment crime levels. Columns (1)–(2) use crime totals from the 1-month window prior to deployment (July 12–Aug. 10, 2025), Columns (3)–(4) use crime totals from the 3-month window (May 12–Aug. 10, 2025), and Columns (5)–(6) use crime totals from the 12-month window (Aug. 12, 2024–Aug. 10, 2025). Columns 1, 3, and 5 use aggregate crime categories (violent and property). Columns 2, 4, and 6 use crime subcategories (robbery, assault with a dangerous weapon, homicide, burglary, motor vehicle theft, and theft). Population refers to PSA population (in thousands) calculated from 2020 Census, share of non-White residents obtained from 2020 Census, and distance to the White House (km) as controls. Robust standard errors are reported in parentheses. "p-val: Crime F-test" refers to the p-value from a joint test of the significance of the crime variables.

* p<.1, ** p<.05, *** p<.01

Table A24: Predictors of PSA Selection into Deployment: Other Landmarks

	1 Month			3 Months			12 Months		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Deployed	Deployed	Deployed	Deployed	Deployed	Deployed	Deployed	Deployed	Deployed
Violent	0.077	0.022	0.018	0.017*	0.019**	0.017**	0.002	0.003	0.003
	(0.082)	(0.019)	(0.020)	(0.009)	(0.008)	(0.008)	(0.002)	(0.002)	(0.002)
Property	0.010	0.004	0.003	0.001	0.000	0.000	0.000	0.000	0.000
	(0.012)	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
Population	-0.076	-0.025**	-0.018	-0.021	-0.022*	-0.017	-0.024*	-0.026**	-0.020
	(0.059)	(0.012)	(0.012)	(0.013)	(0.012)	(0.012)	(0.013)	(0.012)	(0.012)
Share non-White	0.178	0.011	-0.073	-0.676**	-0.215	-0.292	-0.578*	-0.157	-0.216
	(1.419)	(0.330)	(0.307)	(0.305)	(0.336)	(0.313)	(0.303)	(0.315)	(0.290)
Dist. Capitol (km)				-0.058**			-0.061**		
				(0.028)			(0.026)		
Dist. Lincoln Mem (km)		-0.110***			-0.100***			-0.100***	
		(0.025)			(0.025)			(0.026)	
Dist. Wash. Mon (km)			-0.109***			-0.096***			-0.097***
			(0.023)			(0.023)			(0.024)
Outcome mean	0.439	0.439	0.439	0.439	0.439	0.439	0.439	0.439	0.439
Observations	57	57	57	57	57	57	57	57	57
R-squared		0.321	0.325	0.299	0.358	0.350	0.289	0.341	0.337
p-val: Crime F-test	0.464	0.157	0.242	0.061	0.020	0.044	0.175	0.083	0.157

Notes: Each observation is a PSA. Each column reports estimates from Equation (6). Each estimate gives the association between the probability of a deployment in the PSA on Aug. 11, 2025, and pre-deployment crime levels. Columns (1)–(2) use crime totals from the 1-month window prior to deployment (July 12–Aug. 10, 2025), Columns (3)–(4) use crime totals from the 3-month window (May 12–Aug. 10, 2025), and Columns (5)–(6) use crime totals from the 12-month window (Aug. 12, 2024–Aug. 10, 2025). Columns 1, 3, and 5 use aggregate crime categories (violent and property). Columns 2, 4, and 6 use crime subcategories (robbery, assault with a dangerous weapon, homicide, burglary, motor vehicle theft, and theft). Population refers to PSA population (in thousands) calculated from 2020 Census, share of non-White residents obtained from 2020 Census, and distance to landmarks (km) as controls. Robust standard errors are reported in parentheses. "p-val: Crime F-test" refers to the p-value from a joint test of the significance of the crime variables.

* p<.1, ** p<.05, *** p<.01

B Data Appendix

B.1 Offense Harmonization Across Jurisdictions.

The crime data for the District of Columbia include the following primary offense categories: *Homicide*, *Assault with a Dangerous Weapon (ADW)*, *Robbery*, *Burglary*, *Motor Vehicle Theft (MVT)*, and *Theft*. To enable consistent comparisons across jurisdictions, we harmonize the classification of these offenses using incident-level data from Baltimore, Prince George's County, Montgomery County, and Alexandria. This section details how each of these categories was mapped to the most comparable local offense definitions available in each control jurisdiction. Table B1 summarizes the specific offense categories used to construct each of the main crime variables across Baltimore, Prince George's County, Montgomery County, and Alexandria.

B.1.1 Assault with a Dangerous Weapon (ADW) Harmonization

Because participating jurisdictions classify assaults differently, we harmonize assault with a dangerous weapon (ADW) using the closest available weapon-based aggravated assault categories in each dataset.

For Baltimore, we defined an ADW-equivalent offense as any incident recorded as *Aggravated Assault* where the reported weapon type corresponded to a conventional dangerous weapon. Specifically, we included incidents involving:

- *Firearm*, *Handgun*, *Rifle*, *Shotgun*, or *Other firearm*
- *Automatic firearm*, *Automatic handgun*, *Automatic rifle*, *Automatic shotgun*, or *Automatic other firearm*
- *Knife/cutting instrument*
- *Blunt object*, *Motor vehicle/vessel*, *Explosives*, *Fire/incendiary device*, *Poison*, or *Other*

This restriction excludes assaults involving only *Personal Weapons* (hands or feet) and other ambiguous categories.

For Prince George's County, the dataset classifies incidents by type rather than by weapon. We therefore defined ADW-equivalent assaults as those labeled:

- *Assault, Shooting*
- *Assault, Weapon*

These categories explicitly denote the presence of a firearm or other dangerous weapon. Simple assaults labeled only as *Assault* were excluded.

For Montgomery County, we restricted the broader *Aggravated Assault* category to subtypes explicitly referencing weapon use. Specifically, we included:

- *Assault – aggravated – gun*
- *Assault – aggravated – other*
- *Assault – aggravated – family-gun*
- *Assault – aggravated – family-other weapon*
- *Assault – aggravated – non-family-gun*
- *Assault – aggravated – non-family-other weapon*
- *Assault – aggravated – pol off-gun*
- *Assault – aggravated – pol off-other weapon*
- *Assault – aggravated – pub off-gun*
- *Assault – aggravated – pub off-other weapon*

All *strong-arm* aggravated assaults were excluded. Because Montgomery County does not separately identify knife-based aggravated assaults, knife incidents are included within the *other weapon* category.

For Alexandria, detailed weapon information was unavailable. We therefore used all incidents classified as *Aggravated Assault* as the closest available analogue to DC's ADW category.

B.1.2 Burglary Harmonization.

In Baltimore, burglary corresponds to incidents labeled *Burglary*. For Prince George's County, we include all "breaking and entering" categories, namely *B & E, Commercial*, *B & E, Other*, and *B & E, Residential*. In Montgomery County, burglary is identified from the category *Burglary/Breaking and Entering*. In Alexandria, we include all incidents labeled *Burglary*.

B.1.3 Motor Vehicle Theft (MVT) Harmonization.

In Baltimore, motor vehicle theft corresponds to offenses labeled *Auto theft*. For Prince George's County, we include *Auto, stolen* and *Auto, stolen & recovered*. In Montgomery County, the equivalent category is *Motor Vehicle Theft*. In Alexandria, we include incidents labeled *Stolen Auto*.

B.1.4 Robbery Harmonization.

In Baltimore, robbery corresponds to offenses labeled *Robbery*. For Prince George's County, we include the categories *Robbery, Commercial*, *Robbery, Other*, and *Robbery, Residential*. In Montgomery County, robbery is identified from the category *Robbery*. In Alexandria, we include incidents labeled *Robbery*.

B.1.5 Theft Harmonization.

In Baltimore, we distinguish between *Theft from Auto* (*Larceny from Auto*) and *Theft/Other* (*Larceny*, *Larceny of Motor Vehicle Parts or Accessories*, and *Shoplifting*). For Prince George's County, *Theft from Auto* corresponds to *Theft from Auto*, while *Theft/Other* is defined as *Theft*. In Montgomery County, *Theft from Auto* corresponds to *Theft From Motor Vehicle*, while *Theft/Other* includes *All other larceny*, *Theft from Building*, *Theft of Motor Vehicle Parts of Accessories*, and *Shoplifting*. In Alexandria, the available category *Larceny* encompasses all thefts. For each jurisdiction where both subcategories are available, we also construct a composite variable *Theft* equal to the union of *Theft from Auto* and *Theft/Other*.

B.1.6 Homicide Harmonization.

In Baltimore and Prince George's County, homicide corresponds to incidents labeled *Homicide*. In Montgomery County, we include *Murder and Nonnegligent Manslaughter*, *Negligent Manslaughter*, and *Justifiable Homicide*. In Alexandria, we include incidents labeled *Homicide*.

Table B1: Harmonization of Offense Categories Across Jurisdictions

Offense Type	**Baltimore**	**Prince George's County**	**Montgomery County**	**Alexandria**
Homicide	Homicide	Homicide	Murder and Nonnegligent Manslaughter; Negligent Manslaughter; Justifiable Homicide	Homicide
Assault w/ DW	Agg. Assault where weapon ∈ {Firearm, Handgun, Rifle, Shotgun, Other Firearm, Automatic Firearm, Automatic Handgun, Automatic Rifle, Automatic Shotgun, Automatic Other Firearm, Knife/Cutting Instrument, Blunt Object, Motor vehicle/vessel, Explosives, Fire/incendiary device, Poison, or Other}	Assault, Shooting; Assault, Weapon	Assault – Aggravated – Gun; Assault – Aggravated – Family-Gun; Assault – Aggravated – Family-Other Weapon; Assault – Aggravated – Non-Family-Gun; Assault – Aggravated – Non-Family-Other Weapon; Assault – Aggravated – Pol Off-Gun; Assault – Aggravated – Pol Off-Other Weapon; Assault – Aggravated – Pub Off-Gun; Assault – Aggravated – Pub Off-Other Weapon	Aggravated Assault
Robbery	Robbery	Robbery, Commercial; Robbery, Other; Robbery, Residential	Robbery	Robbery
Burglary	Burglary	B & E, Commercial; B & E, Other; B & E, Residential	Burglary/Breaking and Entering	Burglary
Motor Vehicle Theft (MVT)	Auto Theft	Auto, Stolen; Auto, Stolen & Recovered	Motor Vehicle Theft	Stolen Auto
Theft	Larceny from Auto; Larceny; Larceny of Motor Vehicle Parts or Accessories; Shoplifting	Theft from Auto; Theft	Theft From Motor Vehicle; All Other Larceny; Theft from Building; Theft of Motor Vehicle Parts of Accessories; Shoplifting	Larceny

Notes: The main offense categories are based on the classification used by the Metropolitan Police Department of the District of Columbia: *Homicide*, *Assault with a Dangerous Weapon (ADW)*, *Robbery*, *Burglary*, *Motor Vehicle Theft (MVT)*, and *Theft*. The table lists the corresponding local offense categories used to construct comparable variables for the four control jurisdictions—Baltimore, Prince George's County, Montgomery County, and Alexandria. Category names appear as reported in each dataset. "Agg. Assault" denotes aggravated assault; "B & E" denotes breaking and entering; and "MVT" refers to motor vehicle theft. Weapon subcategories (e.g., firearm, knife, blunt object) follow the reporting conventions of each jurisdiction. Each mapping reflects the most specific and consistent incident labels available in the underlying administrative datasets.